Learning the Major: The Role of Early Specialization in Educational and Labor Market Outcomes*

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Abstract

The timing of field specialization involves trade-offs. Early specialization may provide students with more information about their match quality with a chosen field, but it can also impose greater constraints on their ability to switch fields later. To examine how the timing of college field specialization affects educational and labor market outcomes, I exploit a 2011 policy change at a Midwestern flagship university that incentivized students in the College of Engineering to declare an engineering major earlier. Using a difference-in-differences design, I find that students became significantly more likely to take major-specific high-level coursework following earlier declaration, which may help them better assess fit across engineering majors but also reduce flexibility to switch fields—particularly when accumulated credits do not transfer easily to more distant disciplines. Consistent with this, students became less likely to exit the School of Engineering altogether, but more likely to switch from one engineering major to another. The policy also improved academic performance, as reflected in higher GPAs and increased honors graduation rates. LinkedIn-based career data further suggest improved early-career outcomes: greater persistence in engineering roles, longer initial job tenure, and a higher likelihood of attaining managerial positions. These findings are consistent with a dynamic framework I develop on how the timing of specialization affects match quality. Taken together, the results suggest that, in settings where students retain flexibility in major selection, incentivizing earlier major declaration can improve student-major match quality.

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1 Introduction

The US college system is particularly flexible, allowing students considerable freedom in choosing their academic paths: Students can take a variety of courses before declaring a major and are often free to switch majors if their initial choice no longer aligns with their interests or strengths. This flexibility is rare compared to the more rigid systems in many other countries, such as China and Brazil, where students typically specialize at the start of college admission and are unable to switch majors later on (Lovenheim and Smith, 2023). This flexibility is commonly used in practice, as 33% of degree-seeking students switch majors within three years of initial enrollment (NCES, 2017). It may also be beneficial: Students who switch majors graduate at higher rates (BPS, 2009). However, flexibility comes with costs, as students who switch two or more times take an average of an additional 0.5 years to complete their degrees, imposing significant time and financial burdens through delayed graduation (BPS, 2009). This trade-off raises important questions about the appropriate degree of flexibility in students' major choice.

In this paper, I examine one specific margin of flexibility: the timing of initial major declaration, which reflects how long students can spend prior to committing to a field of study and may reduce flexibility in exploring or further changing academic paths. Specifically, I ask: How does incentivizing early major declaration affect educational and labor market outcomes?

To investigate this question, I exploit a 2011 policy change at a Midwestern flagship university that incentivized students in the College of Engineering, all of whom entered undeclared, to select a major earlier. The reform urged students to declare one of the engineering majors by the start of their third term. Additionally, undeclared students were required to meet with an advisor and develop a plan to select and declare a major within a reasonable timeframe in order to register for a fourth term in the college. Before the reform, the timing of major declaration was flexible, with no specific deadline in place.

To analyze this setting, I develop a dynamic framework in which students form beliefs about match quality across majors by observing course grades as they accumulate credits. The key insight is that requiring students to declare earlier increases their exposure to major-specific coursework—including high-level courses—making them better informed about their relative fit across majors but more specialized in their chosen field. When credits transfer

easily within the same umbrella field, as in switching between aerospace and mechanical engineering, the model predicts that this improved information facilitates switching within the umbrella of Engineering majors and raises the likelihood of ending up in a better-fitting major. However, early declaration may be less advantageous when accumulated credits are not transferable across umbrella fields, such as when switching from aerospace engineering to economics. In such cases, students may lack sufficient credits to graduate in the new field, making such switches costly or infeasible. Students may therefore remain in a poorly matched major even after learning about their mismatch, which is predicted to result in fewer exits from Engineering and potentially lower match quality in the final major choice. As both types of switching—within and across umbrella fields—occur simultaneously across students, the overall impact of early declaration on major match quality remains theoretically ambiguous.

I examine this theoretical ambiguity, along with the major choice dynamics described above, by estimating the impact of incentivizing early declaration on educational and labor market outcomes. I use a difference-in-differences design that compares freshmen who entered undeclared in the College of Engineering with those in the College of Arts, Literature, and Sciences (the largest college at the Midwestern flagship university, hereafter CALS) from Fall 2005 to Fall 2015. Importantly, the declaration policy for CALS students remained unchanged, serving as a valid control group.

I combine two data sources: (i) school administrative records that track students' course-taking, major choices, and academic performance, and (ii) LinkedIn data, which provides detailed CV-style information on individuals' self-reported employment histories. I first document that the policy change leads students to declare their majors earlier: the share of engineering students declaring a major before their third term doubles. By the start of the fourth enrolled term, approximately 80% have declared a major, up from 61% prior to the reform.

Following the earlier declaration, the policy change has an immediate effect on students' course selection. During the fourth term—the first after the declaration deadline—students significantly increase their enrollment in high-level, major-specific courses. The policy leads to an average increase of approximately one major-specific credit—a statistically significant 17% rise relative to the baseline average of 5.8 credits—and a corresponding decrease of about one credit in non-major coursework. It also shifts enrollment toward high-level courses, with

credits from 300-level and above increasing by 40%.

These shifts could shape how students learn about their match quality across majors. Indeed, consistent with the framework's predictions, incentivizing early major declaration leads to an increase in switching within the umbrella of Engineering majors after initial declaration of 5 percentage points, a 75% increase relative to the pre-policy mean. This increase in switching may reflect both a higher incidence of initial mismatches—more likely when pre-declaration exploration is shortened—and improved learning about relative fit, as earlier and more intensive engagement with major-specific coursework, particularly high-level courses, enables students to identify better-fitting majors and switch into them.

The framework also predicts that when early specialization substantially increases the difficulty of satisfying graduation credit requirements in alternative fields, the probability of exiting from Engineering should decline. In line with this prediction, the policy reduces the exit rate by 80% relative to the 2010 baseline mean of 10%. I also find that the reduction is larger when the destination major is further from Engineering—for example, switching to non-STEM or non-business/economics (BE) fields rather than to other STEM or BE majors that share similar quantitative requirements and training. This pattern provides additional evidence that early specialization may raise barriers to switching: the less transferable the credits, the higher the barrier and the larger the decline in exits.

Overall, do these changes help students arrive at a better fit with their chosen major? I first examine educational outcomes at graduation as proxies for match quality. Incentivizing early major declaration increases cumulative GPA by 0.04 grade points and raises the probability of graduating with honors by 14%. Moreover, the increase in academic performance persists when restricting to major-specific GPA (0.07 grade points) and across a series of checks that account for major difficulty or changes in coursetaking. These improvements in academic performance suggest better student-major matches, on average.

To further examine whether incentivizing early major declaration has longer-term effects, I construct a five-year post-graduation panel using detailed employment and educational histories from students' publicly available LinkedIn profiles and implement an alternative difference-in-differences design that compares engineering students at the treated Midwestern flagship university to engineering students at a similarly ranked Midwestern flagship that did not change its major declaration policy. This engineering-to-engineering comparison addresses concerns that engineering-specific labor market conditions may confound comparisons

between engineering and non-engineering students.

The results show that incentivizing early major declaration has persistent effects on labor market outcomes. Using standard proxies for student–major match quality—whether graduates work in engineering roles and in positions aligned with their specific major (see, e.g., Robst (2007); McGuinness, Pouliakas and Redmond (2018))—I find that early declaration increases the likelihood of working as an engineer both in the first job and of remaining in an engineering role five years after graduation. The latter effect represents a 12% increase relative to the pre-reform baseline, indicating improved retention in engineering careers. Similar improvements are observed when further restricting the analysis to whether students work in positions that match their declared engineering subfield (e.g., civil engineering graduates working as civil engineers). I also find positive effects on broader early-career outcomes for engineers, reflecting improved fit between students and their chosen careers: It increases the duration of the first engineering job by approximately 140 days, and raises the probability of attaining a managerial or senior engineering position.

Taken together, the evidence suggests that incentivizing early major declaration enhances students' academic performance and strengthens the alignment between their academic training and early labor market trajectories, reflecting improved student–major match quality at graduation and beyond.

The findings in this paper have broader implications for policy and practice. Despite the frequent use of major declaration deadlines as a policy lever to support academic planning, to the best of my knowledge, there is no empirical evidence on how the timing of such decisions affects students. By identifying the effects of early major declaration, this research highlights the trade-offs inherent in policies that promote early specialization. These novel insights can inform the design of college curricula and major declaration policies to foster more effective human capital development and improve long-term student outcomes.

This research contributes to two distinct strands of the literature: the role of learning in college major choices and the impact of the timing of specialization. First, I contribute to the literature that models college major choice as a learning process. College major choice is a critical human capital decision that significantly shapes future labor market outcomes (Altonji, Blom and Meghir, 2012; Altonji, Kahn and Speer, 2014). The value of learning in educational choice has been well-documented (Arcidiacono, 2004; Stange, 2012; Arcidiacono et al., 2016). Prior studies show that learning about match quality is crucial, as biased be-

liefs influence students' decisions (Zafar, 2011; Arcidiacono, Aucejo and Spenner, 2012), and mismatch can affect dropout and persistence rates (Stinebrickner and Stinebrickner, 2012; Arcidiacono et al., 2016). While research highlights that coursetaking can play an important role in shaping student-major match quality (Fricke, Grogger and Steinmayr, 2018; Patterson, Pope and Feudo, 2019), the micro-level learning process through coursework remains underexplored. My research extends this literature by explicitly examining how students learn through coursetaking—gaining signals about match quality while accumulating credits—and how they navigate the trade-off between fit and progress toward graduation. Hsu (2018) also carefully examines this trade-off using a structural approach and finds that credit accumulation can create path dependency that inhibits action informed by learning.

Second, I contribute to the literature on the timing of specialization. The timing of education investment has attracted long-standing attention since Ben-Porath (1967), who emphasizes the high returns to early investment due to longer payoff horizons. In secondary education, studies show that earlier tracking tends to widen dispersion and socio-economic gaps without clear gains in average achievement, whereas delaying selection raises attainment and mobility (e.g., Meghir and Palme (2005); Hanushek and W ößmann (2006); Pekkarinen, Uusitalo and Kerr (2009)). There are very few papers that study field specialization in the college setting, and prior studies have largely relied on cross-sectional comparisons (Malamud, 2010) and structural approaches (Bordon and Fu, 2015). I advance this literature by leveraging a quasi-random experiment to provide the first causal estimates of how incentivizing early major declaration affects educational and labor market outcomes in the U.S. context. Two contemporary papers in the context of Asian higher education also evaluate policy-induced changes in the timing of specialization (Han, Lee and Yoon, 2025; Kang et al., 2025). Unlike in the U.S., where students often begin undeclared and have greater flexibility to switch majors, Asian systems typically admit students directly into a declared major and impose strict limits on switching. In these more rigid settings, the studies find that earlier specialization leads to worse labor market outcomes. They also document student preferences for delayed specialization. These patterns suggest that the degree of institutional flexibility may play an important role in explaining cross-context variation in the effects of early specialization.

The remainder of the paper is structured as follows. Section 2 describes the policy change and the data. Section 3 introduces a framework that yields testable predictions for

the empirical analysis. Section 4 examines the policy's impact on educational outcomes, presenting a sequence of proximate effects and outcomes measured at graduation. Section 5 analyzes longer-term effects in the labor market. Section 6 assesses broader impacts of the policy that extend beyond the primary focus of the framework, including examining the characteristics of students who fail to comply with the declaration deadline. Finally, Section 7 concludes.

2 Background and data

2.1 Major declaration policy reform

At many institutions, students are required to declare a major by a certain deadline, usually by the end of their sophomore year, though the timing and enforcement of this policy vary across colleges. Colleges often impose such deadlines to promote timely graduation, facilitate priority access to restricted courses for declared students, and enhance broader academic planning. While colleges frequently use major declaration deadline as a policy lever, there is little empirical evidence on how the timing of these deadlines influences student outcomes.

In this paper, I study a 2011 policy change at a Midwestern flagship university in which the College of Engineering introduced a deadline requiring students to declare a major in one of the engineering fields. The university is large and selective, enrolling approximately 33,000 on-campus undergraduates in Fall 2024, 54% of whom are female. Admitted students exhibit strong academic profiles: In that year, the median admitted student had a high school GPA of 3.9 and an SAT score of 1,460.[1] First-year students are admitted directly into different colleges as undeclared students, with the two largest—the College of Arts, Literature, and Science (hereafter CALS) and the College of Engineering—enrolling approximately 90% of the undergraduate student body.[2]

The reform effectively incentivized engineering students to declare one of the engineering majors earlier. Prior to the reform, engineering students faced no formal deadline and were

[1] Enrollment, gender composition, and academic profile figures are drawn from the university's Common Data Set 2024-2025.

[2] The application process is uniform across colleges, but students specify which college they are applying to. Engineering students are more selective on average, with a 13% admission rate compared to 16% in CALS in the Fall 2024 enrolled cohort.

free to explore different majors at their own pace. The 2011 reform urged students in the College of Engineering to declare an engineering major by the start of their *third* term. Additionally, undeclared students were restricted from registering for a *fourth* term in the College unless they met with an advisor and developed a concrete plan to select and declare a major within a reasonable timeframe. Details of the declaration requirements before and after the policy change are provided in Figure A.1.[3] Meanwhile, the major declaration policy in CALS has remained consistent over the years: While there is no specific deadline, the general guideline listed on the academic requirements webpage suggests that students typically declare a major during the second term of their sophomore year.

To declare a specific engineering major, students must schedule a meeting with an academic advisor, who checks whether the prerequisites are satisfied and approves the declaration.[4] Other requirements for major declaration—such as GPA standing and prerequisites—remain unchanged across years, aside from minor adjustments in GPA thresholds for selective majors, which affect fewer than 5% of students.[5] Students typically spend their first year in courses that satisfy prerequisites, and the vast majority are eligible to declare at that point—consistent with guidance provided on the college's advising website.[6] As shown in Appendix Table A.1, the coursetaking pattern among students enrolled in the College of En-

[3]The detailed proposal regarding this policy change can be found in the Curriculum Committee meeting minutes. Specifically, the policy was proposed and discussed during meetings held on November 23, 2010, and December 7, 2010, and was formally approved by vote on January 11, to take effect beginning in the Fall 2011 term.

[4]Academic advisors play an important role in the declaration process. First, students are encouraged to meet with an advisor at least once each semester, and according to advising center statistics, 70-75% of first-year students have had an advising appointment during their first semester in recent years. Second, while most messaging about the declaration requirement is sent centrally through newsletters, advisors complement these communications by reminding students to declare their major before the deadline: They reach out to students who have not yet declared a few weeks prior, and students approaching the deadline in the upcoming term often inquire about their major plans and receive useful information for major selection during regular advising meetings. Finally, in the meetings required for major declaration, advisors also provide guidance on course selection and broader academic planning.

[5]The general GPA cutoff for major declaration is 2.0 and remained consistent before and after the policy change. Fewer than 4% of students had first-year GPAs low enough to be affected. In most years, Biomedical Engineering imposed higher cutoffs (chosen by about 5% of students), typically requiring a 3.2 GPA for declaration.

[6]In the main analysis sample (defined later), approximately 95% of students satisfy this requirement both before and after the policy change. The prerequisites require that students have completed at least one first-year–level course in each of the following areas: calculus; calculus-based physics or chemistry; and engineering.

gineering during the first year remains stable before and after the policy change. Specifically, students devoted approximately 85% of their coursework to courses satisfying prerequisites, and an additional 13% to humanities or social science courses to meet the intellectual breadth requirement. These findings suggest that the policy change does not appear to affect how or when prerequisites are satisfied, but instead may influence decisions and outcomes after prerequisite courses have been satisfied.

The College of Engineering studied in this paper is large and selective. Appendix Table A.2 lists the various engineering majors available at the time of declaration and compares them by the size of the graduating student body and associated labor market outcomes. Overall, engineering majors tend to be academically selective—average SAT scores across majors are at or above 1,400—and are associated with strong labor market returns. However, there is meaningful variation across majors in both earnings and industry placement.[7]

Figure 1 illustrates the strong impact of the policy in shifting major declaration timing earlier. As the policy pushed students to declare before the third term and imposed a deadline before the fourth term, the fraction of students declaring a major before their third (fourth) term increased significantly from approximately 23% (61%) to 41% (77%).[8] The remaining 23% of students did not meet the policy deadline, but a majority (62%) delayed by only one additional term before declaring, as shown in Panel (c).[9] The effect on earlier declaration is immediate for engineering students following the 2011 policy change, whereas the declaration pattern in CALS remains stable over time, serving as a natural comparison group. Appendix Figure A.2 further illustrates the effect of the policy across specific engineering majors, showing a consistent shift in declaration timing across all fields.[10]

[7]For example, average median earnings at graduation can differ by as much as $40,000 across majors, with computer science graduates earning about $120,000 on average, compared to $80,000 for chemical engineering graduates. The most common industries of employment also vary by major; for instance, aerospace engineering graduates are often employed in the aerospace and defense sector, while mechanical engineering graduates are more likely to work in the automobiles and parts industry.

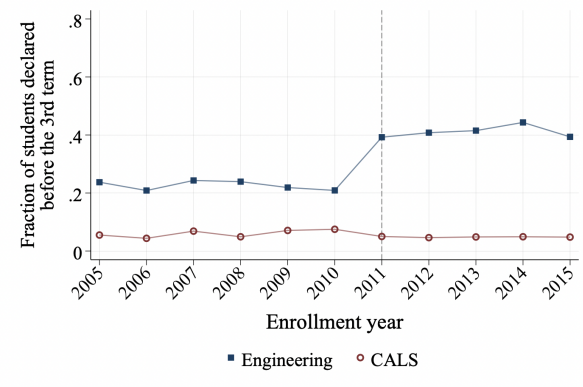
[8]Among all engineering students, about 2% declared a major during the spring/summer term between their first-year winter and second-year fall terms. Counting only the main academic fall and winter terms, the fraction declaring a major before their fourth term increased significantly from approximately 66% to 82%.

[9]I discuss the characteristics of the delayed students further in Subsection 6.1 and find that these students tend to be weaker in academic performance and preparation.

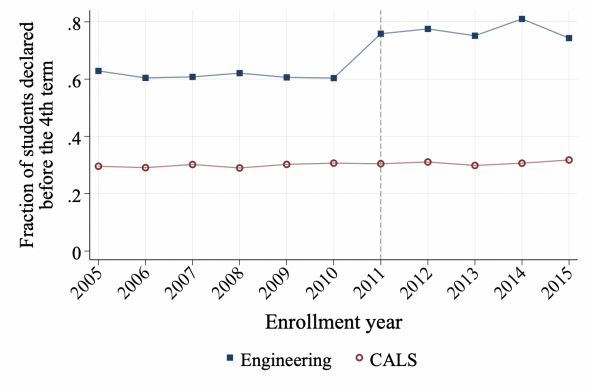
[10]I use the first major that a student declares or graduates with as their primary major when defining outcomes related to declaration or graduation throughout the analysis. Overall, about 13% of students pursue a double major. Among engineering students, both double majors and double degrees are rare—each occurring in less than 2% of students, both before and after the policy change.

Figure 1: Major declaration timing over time in the College of Engineering and CALS

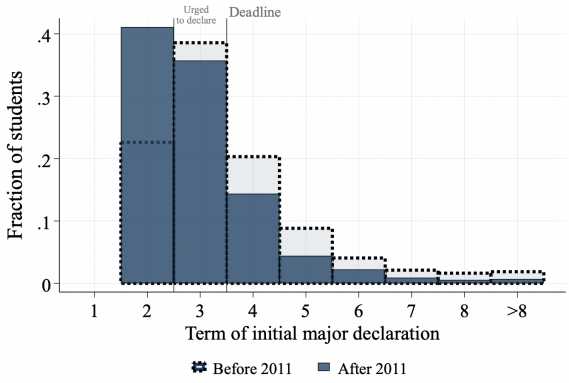
(a) Before the 3rd term



(b) Before the 4th term



(c) Declaration term



Notes: This figure shows changes in major declaration timing in the College of Engineering and CALS over time. Panel (a) plots the fraction of students declaring a major before the third term (the term by which students are urged to declare). Panel (b) shows the fraction declaring before the fourth term (the policy deadline). Panel (c) displays the distribution of the first declaration term for engineering students before (cohorts 2005–2010) and after the policy change (cohorts 2011–2015). The p-value from the Kolmogorov–Smirnov test for a change in distribution is 0.000. Of the 55,850 students in the main sample, 2,471 who dropped out before declaring a major are excluded from the figures.

2.2 Data

I combine two data sources: (i) school administrative records that track students' course-taking, major choices, and academic performance, and (ii) LinkedIn data, which provides detailed CV-style information on individuals' self-reported employment histories.

2.2.1 Administrative data

I obtain student-level administrative records at the course, term, and individual levels from the university registrar. These records include students' major choices, course enrollments, credits earned, grades, GPA, and degrees awarded. Additionally, I observe demographic characteristics, including gender, race, and indicators of family background.

I restrict the sample to first-time freshmen who enrolled between Fall 2005 and Fall 2015, starting in either CALS or the College of Engineering as undeclared students. These students, totaling 55,850, form the main analysis sample.[11] Table A.3 compares the demographic characteristics of students by initial college enrollment. Overall, engineering students are clearly more male-dominated (25% female vs. 56% in CALS) and, on average, have higher overall SAT scores—particularly in math. The average overall SAT score is 1,394 in Engineering compared to 1,357 in CALS, with SAT Math scores averaging 724 in Engineering and 679 in CALS.

2.2.2 LinkedIn profile data

Employment data for the student sample are obtained from Revelio Labs, which collects publicly available information from LinkedIn, a professional networking platform. LinkedIn profiles provide detailed resume-style information, including employer names, job titles, employment start and end dates, job locations, educational institutions attended, degrees earned, and graduation dates.

I link administrative student records to their publicly available LinkedIn profiles for freshmen who enrolled at the Midwestern flagship university between Fall 2005 and Fall 2015. Of the 55,850 anonymized students in the main analysis sample, 55,657 (99.6%) can be linked to name information using registrar-provided crosswalks. Using first name, last name, and graduation year, I successfully match 25,322 students—including 5,739 from the College of Engineering—to LinkedIn profiles, yielding a 60% match rate for engineering students. The time trend in match rates is parallel across Engineering and CALS, as shown

[11] More than 90% of first-year students enroll in the fall, and the College of Engineering admits first-year students exclusively in the fall term. Out of 62,890 first-time freshmen in this period, 13,463 enrolled in the College of Engineering and 42,933 in CALS. The remaining 10% enrolled in other colleges such as Kinesiology, Nursing, and Art & Design. Among Engineering and CALS freshmen (56,396 students), 99% entered as undeclared majors in their first term.

in Appendix Figure C.3, and importantly, there is no divergence in this trend coinciding with the policy change.[12]

While match rates increase over time for both colleges as LinkedIn adoption grows, a potential concern is that unobserved confounding factors—correlated with LinkedIn take-up and varying differentially by college or field—could bias comparisons between Engineering and CALS students in labor market outcomes.[13] More broadly, labor market shocks specific to engineering fields may also confound comparisons between Engineering and CALS graduates. To address these concerns, I collect additional LinkedIn profile data for graduates with bachelor's degrees in engineering from another Midwestern flagship university, which serves as the primary control group for the labor market analysis (hereafter, the control engineering college). The control engineering college shares several key characteristics with the treated engineering college: both are public, located in the Midwest, and similarly ranked among engineering colleges. Importantly, the control engineering college did not undergo any policy change regarding major declaration timing during the study period. As shown in Appendix Figure C.4, trends in the share of engineering students included in the analysis sample are similar across the two universities, suggesting that sample selectivity does not vary systematically over time. Additional data and matching details are provided in Appendix C.

3 Framework

In this section, I present a framework in which students learn about their innate match quality across different majors through course grades while accumulating credits toward graduation. They update their beliefs based on the grades they receive, aiming to choose the major they believe offers the best fit. I then incorporate the policy change into the framework to analyze its impact on major choice—both the dynamics of choice, as reflected in switching behavior, and the final major selected, in terms of realized match quality at graduation. These theoretical insights yield testable predictions, which I examine empirically

[12] Column (1) of Appendix Table C.1 further confirms that there is no statistically significant change in match rates across the two colleges following the reform.

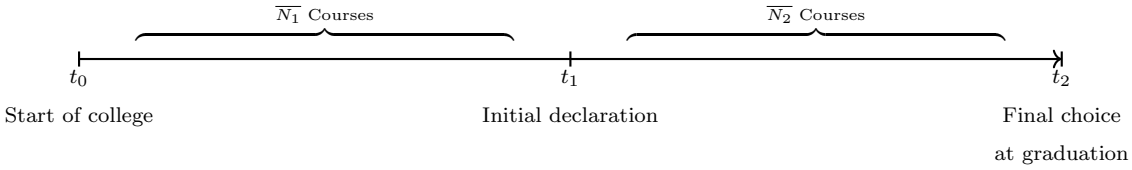
[13] For example, if the labor market for Engineering graduates is more competitive and LinkedIn is a primary tool for job searching, then a similar observed increase in LinkedIn profiles across colleges may mask differential selection, as LinkedIn users from CALS may be more positively selected.

in Sections 4–5.

3.1 Setup

Timing The framework begins at the start of college and unfolds over two primary decision periods, as shown in Figure 2. Students enter college at t_0, face an initial major declaration deadline at t_1, and graduate with a final major choice at t_2. Between these time points, they take courses and receive grades that inform their match quality across majors while accumulating credits toward graduation.

Figure 2: Timeline of college progression in the dynamic framework



Notes: This figure illustrates the timeline in which students enter college at t_0, take courses, and make sequential major-choice decisions at t_1 and t_2. $\overline{N}_1$ denotes the total number of courses a student can take prior to declaration, between t_0 and t_1, while $\overline{N}_2$ denotes the number of courses taken after declaration, between t_1 and t_2.

Setting Students choose between two potential majors, denoted A and B.[14] Each student i has a latent relative match quality with major A compared to B, denoted M_i. A student is better matched with major A if $M_i \geq 0$, and with major B if $M_i < 0$. M_i is unobserved at enrollment but known to follow a normal distribution:

$$M_i \sim \mathcal{N}(0, \sigma_i^2),$$

where σ_i captures student i's ex-ante uncertainty about their relative fit; a larger σ_i reflects greater uncertainty about the true value of M_i. Since M_i is not directly observed, students update their beliefs about relative match quality based on the grades they receive in courses.

Students take multiple courses in both majors, as well as other courses that satisfy general graduation requirements (denoted as type-Z courses). For simplicity, each course is assumed

[14] Empirically, among all students who switch majors in the study, 95% switch only once, justifying this simplification.

to carry one credit. Courses in majors A and B inform students' relative fit, while type-Z courses carry no informational value in this regard. Let n_{1A}, n_{1B}, n_{1Z} denote the number of courses taken in each category prior to the initial declaration at t_1. Coursetaking in this period follows a multinomial distribution:

$$(n_{1A}, n_{1B}, n_{1Z}) \sim \text{Mult}(\overline{N}_1, \mathbf{p}_1 = (p_{A1}, p_{B1}, p_{Z1})),$$

where $\overline{N}_1$ denotes the total number of courses a student can take between t_0 and t_1, which is naturally proportional to the length of time between these two points. The parameters p_{A1}, p_{B1}, and p_{Z1}, which satisfy $p_{A1} + p_{B1} + p_{Z1} = 1$, represent the probabilities of taking a course in major A, major B, and type-Z, respectively.

After declaring a major, students begin taking high-level courses (denoted by AH and BH) aligned with their declared field. Let $n_{2A}, n_{2B}, n_{2AH}, n_{2BH}, n_{2Z}$ denote the number of courses taken after declaration. Coursetaking in this period also follows a multinomial distribution:

$$(n_{2A}, n_{2B}, n_{2AH}, n_{2BH}, n_{2Z}) \sim \text{Mult}(\overline{N}_2, \mathbf{p}_2 = (p_{A2}, p_{B2}, p_{AH2}, p_{BH2}, p_{Z2})),$$

where, as in the pre-declaration period, $\overline{N}_2$ is the total number of courses a student can take after declaration, and p_{c2}, for $c \in \{A, B, AH, BH, Z\}$, denotes the probability of taking each course type (with the probabilities summing to 1).

Specialization occurs after the declaration. The probability of taking courses in the declared major increases relative to before t_1, crowding out the probability of taking courses in other fields. I further assume that students crowd out type-Z courses while maintaining the probability of taking regular courses in the undeclared major. This assumption is reflected as follows:

$$\begin{cases} \text{If } A \text{ is declared at } t_1\text{:} & p_{AH2} > 0,\ p_{A2} + p_{AH2} > p_{A1};\ p_{BH2} = 0,\ p_{B2} = p_{B1},\ p_{Z2} < p_{Z1}, \\ \text{If } B \text{ is declared at } t_1\text{:} & p_{BH2} > 0,\ p_{B2} + p_{BH2} > p_{B1};\ p_{AH2} = 0,\ p_{A2} = p_{A1},\ p_{Z2} < p_{Z1}. \end{cases} \tag{A1}$$

The relative match quality M_i loads onto each course grade in major A or B, with normally distributed i.i.d. noise of mean zero. The grade for student i in course j of major

$X \in \{A, B\}$, which serves as a noisy signal of the relative match quality M_i, is given by:

$$G_{itX}^{(j)} = \tau_X M_i + \epsilon_{itX}^{(j)}, \quad \epsilon_{itX}^{(j)} \sim \mathcal{N}(0, \eta_X^2), \quad j = 1, \ldots, n_{tX} \quad t = 1, 2,$$

where τ_X governs the signal strength, and η_X^2 captures the variance of the noise.

Grades in type-Z courses—those outside majors A and B—do not convey any information about the relative match quality M_i and are therefore independent of it. Specifically, each type-Z course grade is drawn independently from a normal distribution with mean zero:

$$G_{itZ}^{(j)} = \epsilon_{itZ}^{(j)}, \quad \epsilon_{itZ}^{(j)} \sim \mathcal{N}(0, \eta_Z^2), \quad j = 1, \ldots, n_{tZ} \quad t = 1, 2.$$

After the initial declaration in major $X \in \{A, B\}$, students continue to take regular courses in both majors. In addition, they enroll in high-level courses within their declared major (denoted XH), which provide more precise signals, reflected in a lower variance of their noise components:

$$G_{i2XH}^{(j)} = \tau_X M_i + \epsilon_{i2XH}^{(j)}, \quad \epsilon_{i2XH}^{(j)} \sim \mathcal{N}(0, \eta_{XH}^2), \quad j = 1, \ldots, n_{2X}$$

$$\eta_{XH}^2 < \eta_X^2 \tag{A2}$$

Major choice At each decision point $t \in \{t_1, t_2\}$, after receiving grades, students update their beliefs about relative match quality, where $\mathbb{E}(M_i \mid G_{i1})$ and $\mathbb{E}(M_i \mid G_{i1}, G_{i2})$ denote their posterior beliefs (see Appendix D for derivations).

Students then choose the major they believe offers a relatively better match based on their posterior beliefs. Recall that when the relative match quality is positive, major A offers the better match, and vice versa. Therefore, at t_1, the major choice decision is as follows:

$$\text{Choose major} \begin{cases} A, & \text{if } \mathbb{E}(M_i \mid G_{i1}) \geq 0, \\ B, & \text{if } \mathbb{E}(M_i \mid G_{i1}) < 0. \end{cases}$$

At t_2, students again seek the major they believe offers the better match. If this requires revising their initial choice to switch into major $X \in \{A, B\}$ at graduation, the switch is feasible only if the graduation credit requirements for X are satisfied. In particular, after

accounting for transferable credits to X (denoted N_{Xtr}), the student must have accumulated enough credits in X and type-Z such that

$$N_X + N_{Xtr} + N_Z \geq Q,$$

where Q is the minimum number of credits required for graduation. Here, $N_X + N_{Xtr}$ denotes the total number of credits in major X, adjusted for transferable courses, and N_Z denotes credits in type-Z courses, which fulfill general requirements and can be counted toward either major. By construction, students who remain in their initially declared major always satisfy this threshold, as standard academic progress ensures they meet the graduation requirement in that field (e.g., $N_A + N_Z \geq Q$ when A is declared at t_1).

The likelihood of satisfying the constraint $N_X + N_{Xtr} + N_Z \geq Q$ depends on whether majors A and B belong to the same umbrella field. If they do (e.g., both are engineering majors), credits are mutually transferable, ensuring that the constraint is always satisfied.[15] If instead the majors lie in different umbrella fields (e.g., engineering vs. economics), credits are not transferable, and the switch may be infeasible if the student has not accumulated enough credits in X and Z (i.e., if $N_X + N_Z < Q$).

Therefore, a student who declared major A at t_1 will remain in major A at graduation if their posterior belief, updated after observing all grades, remains positive. They will switch to major B if their posterior belief becomes negative and they have accumulated enough credits in B and type-Z such that $N_B + N_{Btr} + N_Z \geq Q$. In summary, for students who declared major A at t_1, their decision rule at t_2 can be written as:

$$\text{Choose major} \begin{cases} A, & \text{if } \mathbb{E}(M_i \mid G_{i1}, G_{i2}) \geq 0, \\ B, & \text{if } \mathbb{E}(M_i \mid G_{i1}, G_{i2}) < 0 \text{ and } N_B + N_{Btr} + N_Z \geq Q. \end{cases}$$

Symmetrically, for students who declared major B at t_1, the decision rule is:

$$\text{Choose major} \begin{cases} B, & \text{if } \mathbb{E}(M_i \mid G_{i1}, G_{i2}) < 0, \\ A, & \text{if } \mathbb{E}(M_i \mid G_{i1}, G_{i2}) \geq 0 \text{ and } N_A + N_{Atr} + N_Z \geq Q. \end{cases}$$

[15] For instance, if the student intends to switch into major A (i.e., $X = A$), this implies that $N_A + N_{Atr} + N_Z = N_A + N_B + N_Z \geq Q$, since $N_B + N_Z \geq Q$ already holds. Therefore, the constraint is always satisfied. An analogous argument holds if X corresponds to major B.

Following this framework, a mismatch is defined as graduating in the major with lower relative match quality. Specifically, a mismatch occurs when major A has higher match quality ($M_i \geq 0$) but the student graduates with major B, or when major B has higher match quality ($M_i < 0$) but the student graduates with major A. In the later empirical analysis, I use GPA and engineering employment as proxies for match quality, assuming that a lower probability of mismatch corresponds to higher values of these proxies.

3.2 Testable predictions of the policy reform

Policy change As discussed in Subsection 2.1, the policy reform incentivizes students enrolled in the College of Engineering to declare an engineering major by the start of their fourth enrollment term. This effectively shifts the timing of the initial major declaration earlier, which can be represented as moving t_1 to an earlier time point t_1' in the framework. As a result, the pre-declaration period is shortened. Students consequently have fewer opportunities to take courses that serve as signals of their relative fit for different majors prior to declaring, reflected by a reduction in $\overline{N}_1$. Meanwhile, the post-declaration period becomes longer, resulting in a corresponding increase in $\overline{N}_2$, the number of courses taken after declaration. This shift alters the timing and distribution of courses taken before and after the initial declaration, affecting the inference process through which students learn about their relative match quality.

This policy change generates two main effects. First, students are now required to declare a major earlier, based on fewer grade signals received before t_1. This increases the likelihood of a mismatch in the initial major choice. In the meantime, however, in place of the reduced number of pre-declaration courses, students gain greater exposure to high-level courses in the extended post-declaration period—courses that provide more precise information about match quality (based on Assumption A2). As a result, the total information accumulated by graduation increases, enabling more informed final major choices. I refer to the policy-induced increase in overall information about relative match quality as the *learning gain*.

Second, from Assumption A1 it follows that earlier declaration leads to earlier specialization in coursetaking, which results in more credits in the initially declared major and fewer in other fields. This does not affect students when switching occurs within engineering, since engineering credits are assumed to be fully transferable and therefore the graduation credit

requirements are always satisfied. However, due to this increased specialization, students seeking to exit engineering may find it infeasible to satisfy the graduation credit requirements in the new field, as they cannot transfer credits from their initially declared major and have accumulated fewer credits that count toward graduation requirements in the destination major. I refer to this effect as the *tightened exiting constraint*.

Combining these effects, the framework yields several predictions about the policy's impact on major choice dynamics-reflected in switching behavior-and on the match quality of the final major choice, as outlined below.

3.2.1 Within-Engineering switching and match quality among engineering graduates

Consider first the case where both major A and major B fall within the umbrella field of Engineering. The policy reform affects coursetaking in two key ways: (i) students complete fewer courses in the pre-declaration period, which increases the likelihood of a mismatch in their initial major choice and, consequently, the chances that updated posterior beliefs will overturn their initial beliefs—prompting a correction through major switching; and (ii) they complete more courses in the post-declaration period, including high-level courses that provide more precise information about the relative match quality. This increased information acquired after declaration further raises the likelihood that students revise their beliefs and re-optimize their major choice. Together, these mechanisms predict an increase in switching within Engineering majors.

Prediction 1 *(Within-Engineering switching) When both majors A and B fall within Engineering, a policy change that reallocates coursework from period $t_0 - t_1$ to period $t_1 - t_2$ increases the probability of switching between majors.*

Proof. See proof in Appendix D.2. ■

Greater exposure to post-declaration coursework also increases the amount of information students accumulate about relative match quality by graduation. Since graduation credit requirements are always satisfied for within-Engineering switches, students are able to fully benefit from the learning gain. As a result, the likelihood of graduating in a mismatched major declines, and the match quality of students' final major choice at graduation improves.

Prediction 2 *(Match quality among engineering graduates) When both majors A and B fall within Engineering, a policy change that reallocates coursework from period $t_0 - t_1$ to period $t_1 - t_2$ reduces the probability of mismatch at graduation.*

Proof. See proof in Appendix D.3. ■

3.2.2 Exits from Engineering and overall match quality

Now consider the case where one major (either A or B) is in Engineering and the other is not. In this case, the policy's impact on switching behavior is shaped by a trade-off between learning gain and tightened exiting constraint. If early specialization substantially reduces the feasibility of exiting from Engineering—due to a lower likelihood of meeting graduation credit requirements in the new field—then we expect a decline in exits from Engineering, even if the learning gain would otherwise lead to increased switching to revise the initial major choice.

Prediction 3 *(Exits from Engineering) A policy change that reallocates coursework from period $t_0 - t_1$ to period $t_1 - t_2$ reduces exits from Engineering when the tightened exiting constraint outweighs the learning gain.*

Proof. See proof in Appendix D.4. ■

While the framework yields clear predictions for how the policy affects major-switching behavior—depending on whether the intended destination major is within Engineering—the net effect on overall match quality across students is more nuanced. Although match quality is predicted to improve when considering only within-Engineering cases, tightened exiting constraint may hinder students from sorting into better matches when they wish to leave Engineering entirely, thereby reducing overall match quality. For example, consider a student who initially declares aerospace engineering but, due to the additional information gained following the policy change, comes to realize that economics is a better fit. If the student has accumulated too few credits to satisfy the graduation requirements in economics, she may be forced to graduate in aerospace engineering despite the mismatch. In this case, the improved information about relative match quality due to learning gain is insufficient to offset the effect of tightened exiting constraint. Since both types of switching occur simultaneously, the net effect on overall match quality is theoretically ambiguous. I empirically test this prediction in Subsections 4.4 and 5.2.

Prediction 4 *(Overall match quality) The overall effect on match quality of a policy change that reallocates coursework from period t_0–t_1 to period t_1–t_2 depends on the relative strength of the learning gain versus the increased barriers to exiting Engineering.*

Proof. See proof in Appendix D.5. ■

3.3 Discussion

In this subsection, I first provide empirical support for the key assumptions in the framework. I then discuss several alternative modeling choices and compare the framework to those used in the related literature.

Are the framework's key assumptions consistent with empirical facts? While the framework abstracts away from institutional details for modeling simplicity, its key assumptions are consistent with observed empirical patterns. First, there is a significant reduction (increase) in the number of credits accumulated prior to (after) major declaration following the policy change, as shown in panels (a)–(b) of Appendix Figure D.1. Despite this reallocation, the total number of credits accumulated by graduation remains unchanged (see column (3) of Appendix Table A.12). This pattern is consistent with the model's representation of the policy reform, in which students shift approximately 7 credits from the pre-declaration period (baseline mean: 38 credits) to the post-declaration period (baseline mean: 85 credits)—that is, $\overline{N}_1$ decreases and $\overline{N}_2$ increases by similar magnitudes. Second, the effect of learning gain is driven by an increase in the expected number of high-level courses, $p_{XH2} \cdot \overline{N}_2$, which captures the improvement in information about relative match quality acquired after the policy change.[16] This aligns with the empirical finding that the number of high-level (300-level and above), major-specific courses taken by graduation increases significantly—by about 2 credits, a 5 percent rise (panel (c), Appendix Figure D.1). Moreover, this increase is evident as early as the term immediately following declaration, when students take about 0.8 more major-specific credits—a 40 percent rise (panel (d), Figure A.3)—indicating that they receive more informative signals as soon as they declare a major. Finally, the effect of

[16] While it is difficult to empirically test whether high-level courses are more informative, Arteaga (2018) notes that the Colombian central bank favors applicants who have taken advanced monetary economics, suggesting that some employers place a greater value on higher-level field-specific coursework.

the tightened exiting constraint arises from increased specialization following major declaration—an implication of Assumption A1, which is also supported by the data. Formally, this corresponds to $p_{X2} + p_{XH2} > p_{X1}$ for $X \in \{A, B\}$. Restricting the sample to students who do not switch majors—so that pre- and post-declaration major-specific course shares are comparable within the same major—the share of courses whose 4-digit CIP code matches the declared major increases substantially: from 4% to 52% in the College of Engineering, and from 10% to 26% in CALS. Additionally, the data show a reduction of 3.7 non-major credits accumulated by graduation following early major declaration, consistent with a tightened exiting constraint (see column (b) of Appendix Table A.13).

How does the framework compare to alternative modeling choices, term-level interpretations, and frameworks in the literature? One potential modeling concern is whether students, following the policy change, have stronger incentives to explore engineering-specific courses before declaration—potentially reflected in endogenous shifts in p_{A1} and p_{B1}—given the shortened pre-declaration period. In principle, students might also internalize the graduation credit requirements at t_2 and adjust their course selection both before and after t_1 accordingly. The framework abstracts from such endogenous coursetaking behavior in the first period, as empirical variation is limited: Most engineering students allocate the majority of their first-year credits to prerequisites, with only 2% devoted to engineering-specific courses (Appendix Table A.1). Similarly, the model does not incorporate endogenous declaration timing or penalties for late declaration. This simplification is supported by the empirical finding that approximately 80% of students comply with the policy deadline, and most of the remaining students delay by only one term, as discussed in Subsection 2.1.[17] Notably, while grades are one form of signal, other channels—such as newsletters sent to declared students or peer networks—may also convey information about match quality. Since these alternative signals are difficult to measure empirically, it is useful to focus on coursetaking behavior and its observable outcomes—namely, grades and credit accumulation patterns—which can be directly analyzed.

While the model assumes that major switching occurs only at graduation, its logic remains interpretable at the term level.[18] In practice, switching does occur slightly earlier

[17] In a structural estimation context, omitting endogenous timing may overstate the policy effect due to partial noncompliance.

[18] Both interpretations capture students who would prefer not to delay graduation further by accumulating

on average following the policy change—by less than one term—which is beneficial: It allows students to revise their choices sooner and mitigates the potential costs associated with switching later in college—additional costs that the framework abstracts away from. Holding calendar time constant, students accumulate more information about relative match quality earlier due to early declaration, enabling more timely belief updating and more informed major choices. Even if a course outside the declared major is merely delayed—rather than entirely crowded out—due to the start of specialization in the declared major, the earlier accumulation of informative signals about relative major match quality can still meaningfully influence switching behavior.

The framework also aligns with the literature on learning about match quality. Hsu (2018) shows that early course requirements have a greater influence on major choice than preferences or ability when choices span umbrella fields such as natural sciences, social sciences, and humanities. His structural model highlights how path dependence in coursework can limit students' responsiveness to new information when switching is costly—an insight consistent with this framework's implications for exits from Engineering. James (2011) estimates a model of occupational learning with cross-learning across job-specific skills. While his primary focus is on cross-learning, the underlying structure of belief updating based on noisy signals parallels the framework presented here. My framework implicitly allows for cross-learning without explicitly specifying its structure, since courses in both majors A and B inform relative fit.

4 Effects on educational outcomes

In this section, I first outline the empirical strategy used to estimate the effects of incentivizing earlier major declaration on educational outcomes in Subsection 4.1. I then present the first set of results in Subsections 4.2–4.3, focusing on the impact of the reform on a set of proximate educational outcomes. In particular, I analyze how the policy influenced students' post-declaration coursetaking trajectories, and test Predictions 1 and 3 concerning changes in major choice dynamics, as captured by switching behavior (see Subsection 3.2). Finally, in

additional credits to switch into a major with a better fit. Thus, they switch at t_2 only if they have already accumulated enough credits. When interpreted at the term level, the graduation credit requirements can be viewed as a term-level credit requirement to ensure timely graduation.

Subsection 4.4, I present the impact on educational outcomes at graduation and empirically examine the effects of incentivizing early major declaration on overall student–major match quality, which is shown to be theoretically ambiguous in Prediction 4.

4.1 Empirical strategy

I use a difference-in-differences framework to examine how educational outcomes change following the 2011 major declaration reform, comparing engineering students to their CALS peers. Specifically, I estimate the following event study specification:

$$Y_{it} = \sum_{\tau \neq 2010} \alpha_\tau \cdot \mathbb{1}[\tau = t] + \beta \cdot \text{Engineering}_i + \sum_{\tau \neq 2010} \delta_\tau \cdot \mathbb{1}[\tau = t] \cdot \text{Engineering}_i + X_i\gamma + \epsilon_{it}, \quad (1)$$

where Y_{it} denotes the educational outcome of student i in cohort t, with cohort t defined as first-time freshmen enrolled in the fall term of year t. Examples of outcomes include the number of major-specific credits earned in a term, an indicator for switching within Engineering majors, and earning an honors degree. Engineering_i is an indicator for whether student i initially enrolled in the College of Engineering.[19] The α_τ terms are cohort fixed effects that control for overall time trends in the outcomes. I include individual-level controls X_i, including gender, race, and SAT scores, as these characteristics differ between students in the two colleges, as shown in the "Overall" column of the summary statistics in Table A.3.[20] Following Baker et al. (2025), and given that variables such as SAT scores are determined well before students declare a major, these characteristics are treated as predetermined covariates and included as controls. The robustness of the results to specifications with alternative controls is discussed in the relevant parts of Section 4, and the qualitative patterns remain

[19] Internal transfers do not violate the treatment definition based on initial enrollment. University policy permits inter-college transfers no earlier than the start of the second year (99% of cases in the data comply with this rule), and the median transfer between CALS and Engineering occurs in the fourth term. As a result, students are subject to the early declaration incentives based on their initial college of enrollment. Moreover, in both the pre- and post-reform periods, only about 3% of students from CALS transferred into Engineering due to the strict transfer rule at the College of Engineering. While transfers from Engineering to CALS are more likely (10% in the baseline year 2010), these flows account for less than 3% of the overall CALS population, which suggests that spillover effects in peer composition, class sizes, and grading curves from policy-induced changes in internal transfers are likely limited.

[20] In Subsection 4.1.1, I discuss small shifts in certain student characteristics over time across the two colleges (as shown in the last column of Table A.3), and elaborate why these changes are unlikely to be a threat to the identification strategy.

consistent.

The coefficients δ_τ capture the differential changes in outcomes for engineering students relative to their CALS peers in each cohort, with 2010, the year before the policy change, omitted as the reference year. I restrict the sample to Fall 2005–2015 freshmen who began in either CALS or Engineering undeclared, ensuring comparability across cohorts.

To summarize the magnitude of the effects, I report the post-period average of the δ_τ coefficients, calculated as $\frac{1}{5}\sum_{\tau=2011}^{2015}\delta_\tau$. I interpret these estimates as the average treatment effect of *incentivizing early major declaration* induced by the policy.[21] Alternatively, this can be interpreted as an intent-to-treat (ITT) effect, where the treatment is defined as major declaration by the policy deadline (Angrist and Pischke, 2009). Robust standard errors are used throughout the analysis.

4.1.1 Identifying assumptions

To interpret the estimates from the event study specification as causal effects of incentivizing early major declaration on educational outcomes, I rely on two key identifying assumptions: no anticipation of the treatment and parallel trends (Sun and Abraham, 2021; Borusyak, Jaravel and Spiess, 2024). First, the no anticipation assumption requires that students do not alter their behavior in response to the upcoming reform—for example, by adjusting their high school coursework or college application strategy in anticipation of a shortened period for major exploration. In the following, I discuss several reasons why this assumption likely holds. In particular, the event study plots show that the policy effect is already evident in the first treated cohort. Since the first cohort does not have time to adjust before the policy is implemented, this pattern alleviates concerns about a violation of the no anticipation assumption.[22] Second, the parallel trends assumption requires that, in the absence of the reform, the difference in potential outcomes between Engineering and CALS students follows similar trends over time, conditional on observed controls and cohort fixed effects. I present event study plots to visually assess the plausibility of this assumption and report the average

[21] Given that not all students necessarily declared a major earlier in response to the deadline—and that the policy may have been accompanied by broader institutional support, such as advising meetings aimed at guiding students toward declaration—I interpret these estimates as capturing the average effect of exposure to incentives for early major declaration, rather than the effect of early declaration per se.

[22] Formal tests show that differences in the policy impact on main outcomes between the first and later treated cohorts—i.e., testing $\delta_{2011} = \frac{1}{4}\sum_{\tau=2012}^{2015}\delta_\tau$—are statistically insignificant.

of the pre-period δ_τ coefficients, calculated as $\frac{1}{5}\sum_{\tau=2005}^{2009}\delta_\tau$, which is not significantly different from zero across outcomes. I also discuss potential contemporaneous institutional changes and provide evidence that these likely had only a limited influence on the outcomes of interest.

Could the no anticipation assumption fail? It is reasonable to be concerned that later cohorts may have selected into the College of Engineering with knowledge of the declaration requirement, introducing potential self-selection into the treated group. However, several factors suggest this is unlikely to be a significant concern. First, event study figures for the main outcomes, presented across Sections 4–6, show that the results hold for the first cohort affected by the policy change. The significance of results for this cohort—given that the policy was approved after their college application, and implemented after their college enrollment—offers suggestive evidence against anticipatory behavior.[23] Second, students' awareness of the declaration policy is likely limited. The policy is not listed on the college's main website and is only mentioned on the advising center's site, making it difficult for prospective applicants to discover during the application process. Third, it is worth noting that the engineering college in this study is a highly ranked institution with an admission rate of approximately 13%. This places a sharp upper bound on the potential for strategic behavior in admissions, such as applying or enrolling in response to changes in the timing of major declaration.

Could potential contemporaneous institutional changes violate the parallel trends assumption? One potential concern is that the university joined the Common Application in Summer 2010. As a result, freshman applications for 2011 increased by 25%, reaching 39,584. However, enrollment patterns remained stable despite the surge in applications: Total first-year enrollment actually decreased by 3.8%, and Table A.3 further shows that enrollment sizes remained stable across the two colleges. There are some changes in student characteristics upon matriculation before and after 2011, likely reflecting shifts in selection

[23]There was no public announcement about the policy change, and the major declaration requirement was conveyed through a centralized college newsletter post-enrollment. Moreover, there are no significant post-policy differences in student demographics between Engineering and CALS students in the first post-policy cohort (2011). This is based on estimates from Equation (1) without controls, across the demographic variables listed in Table A.3.

due to the expanded applicant pool. Importantly, these changes are largely similar across the two colleges. Even for characteristics such as SAT scores, where changes between the two colleges are statistically different, the magnitudes are economically small, limiting concerns about violations of the parallel trends assumption. Another potential concern is that supply-side changes from the College of Engineering could confound the estimated effects. To assess this possibility, I examine course offerings and enrollments during the first two terms for engineering students, before students were encouraged to declare a major. Specifically, I pool all coursetaking records and track changes in the number of unique courses taken and the average number of students per course. As shown in Panel (a) of Appendix Table A.1, both course availability and class sizes remained stable before and after the policy change, suggesting that changes in the supply of courses—through course access or instructional environment—is unlikely to explain the observed effects.

4.2 Proximate effects on coursetaking

Does incentivizing early major declaration influence students' subsequent coursetaking processes, which may in turn affect how they learn about their relative match quality across majors? I focus on outcomes during the fourth enrolled term—that is, the first term following the declaration deadline—in Table 1, with corresponding event-study figures shown in Figure A.3.

In terms of credits earned during the focus term, the policy has no statistically or economically meaningful effect on total credits earned (–0.2 credits), relative to a baseline average of 14 credits (Column (1)). However, the allocation of those credits changes significantly. Specifically, after categorizing courses based on whether their subject area matches the student's declared major, Column (2) shows an increase of approximately 1 major-specific credit—a statistically significant 17% increase relative to the baseline average of 5.8 credits.[24] Column (3) shows a corresponding decrease of about 1 credit in non-major-specific coursework, suggesting a reallocation away from courses outside the major.

The policy-induced increase in major-specific credits is also accompanied by a shift toward high-level coursework. While there are no significant changes in lower-level 100/200-level

[24]Major-specific credits are defined as courses whose 4-digit CIP code matches the student's declared major's 4-digit CIP code. The 4-digit CIP captures the major definition; for example, Economics is "45.06" and Chemical Engineering is "14.07".

courses (e.g., *IOE 201* is a 200-level course), credits earned from more advanced 300-level and above courses increased by 40% following the policy change, corresponding to an additional 0.8 credits relative to a baseline average of 2 credits (Column (5)).

Table 1: Policy effect on subsequent coursetaking outcomes

	Overall			Major-specific credits		Other credits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total credits	Major-specific credits	Other credits	Major credits (100–200)	Major credits (300+)	General engineering credits	Humanities/BE credits
Years 2005 to 2009	-0.110	-0.206	0.096	-0.097	-0.108	0.045	0.058
	(0.084)	(0.146)	(0.150)	(0.120)	(0.112)	(0.101)	(0.113)
Years 2011 to 2015	-0.205**	0.993***	-1.198***	0.174	0.819***	-0.575***	-0.525***
	(0.083)	(0.144)	(0.149)	(0.119)	(0.113)	(0.100)	(0.111)
Observations	54,056	54,056	54,056	54,056	54,056	54,056	54,056
Baseline mean	14.958	5.780	9.178	3.708	2.071	2.116	2.327
Pre mean	14.884	5.545	9.339	3.583	1.963	2.068	2.529
Post mean	14.830	6.462	8.368	3.924	2.539	1.726	2.025
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports average effects from estimating equation (1) on the impact of the early major declaration policy on coursetaking outcomes during the fourth enrolled term (the first term after the major declaration deadline). There are 54,056 students ever enrolled in the fourth term; students who dropped out earlier are excluded. Columns (1)–(3) report effects on credit allocation: total credits, major-specific credits (courses whose 4-digit CIP code matches the declared major), and other credits. Columns (4)–(5) report effects on course type within the major: the number of 100–200-level major-specific credits (defined by course number) and the number of 300-level or higher major-specific credits. Columns (6)–(7) report effects on non-major-specific coursework: general engineering credits (field-specific engineering courses outside the declared major) and credits earned in humanities and business–economics (BE) courses. The effect for year 2010 is normalized to zero. The coefficient for years 2005–2009 represents the pre-policy average; the coefficient for years 2011–2015 represents the post-policy average. The baseline mean refers to the mean of the outcome variable for the 2010 Engineering cohort. The pre mean refers to the outcome mean for pre-reform Engineering cohorts (2005–2010), and the post mean refers to the outcome mean for post-reform Engineering cohorts (2011–2015). All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, a dummy for female, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

To understand which types of non-major-specific courses are crowded out by the early declaration policy, I further examine the composition of non-major-specific credits and identify two key effects. First, the policy significantly reduces credits earned in general engineering

courses—that is, engineering courses outside a student's declared major—by approximately 0.6 credits, a 27% decline relative to the baseline (Column (6)). This effect likely reflects a shift away from exploratory enrollment across engineering subfields toward earlier specialization following major declaration.[25] Second, the policy decreases enrollment in humanities and business/economics (BE) courses during the post-declaration term (Column (7)): It falls by about 0.5 credits, representing a 23% decline relative to the baseline.

Appendix Table A.4 shows consistent patterns when examining overall course-taking during both the third and fourth enrolled terms, following the timing of the policy urging major declaration by the start of the third term. The results are robust to the exclusion of controls.

In sum, these patterns suggest that incentivizing early major declaration reshapes students' coursetaking behavior, encouraging earlier engagement with high-level, major-specific coursework. This shift may meaningfully influence how students learn about their match quality across majors and shape subsequent major choice dynamics, as reflected in switching behavior.

4.3 Proximate effects on major choice dynamics

Since coursetaking trajectories are significantly affected by the policy change, do students' major choice dynamics also respond, as predicted by the framework in Subsection 3.2? In this subsection, I empirically test Predictions 1 and 3 by examining the policy's impact on within-Engineering switching and exits from Engineering. I first present results on each outcome in Subsections 4.3.1 and 4.3.2, and then discuss heterogeneity in Subsection 4.3.3.

4.3.1 Effects on within-Engineering switching

Recall that Prediction 1 concerns switching within the umbrella field of Engineering, which predicts that, following a policy change of early major declaration, students are more likely to switch majors within Engineering. I examine the policy's impact on the empirical counterpart: the likelihood of switching majors within the initially enrolled college, comparing students in the College of Engineering and CALS. Specifically, within-college switching refers

[25] A similar reduction is observed in the number of distinct engineering fields represented in students' course selections.

to cases where the initially declared major differs from the graduation major, but both majors fall within the same college of initial enrollment. A total of 3,572 students who dropped out are excluded from the analysis.

Column (1) of Table 2 shows the policy's effect on this outcome. Consistent with prediction 1, there is a substantial increase of 5 percentage points, representing a 75% rise relative to the baseline mean of 6.7% (event study shown in in Panel (a) of Appendix Figure A.4). This finding provides suggestive evidence that early major declaration—by requiring students to select a major based on fewer pre-declaration signals and prompting them to engage earlier with more informative high-level coursework—may increase the likelihood that students revise their beliefs and switch majors within Engineering. Appendix Table A.7 presents estimates of the policy's impact under alternative model specifications. The results remain robust whether or not student-level controls are included, and also when fixed effects for the initially declared major are added.

While the results above provide strong evidence supporting Prediction 1 of the framework—highlighting the important role of early major declaration in shaping students' major choice dynamics—one might propose alternative explanations for the observed increase in within-Engineering switching that are unrelated to learning about major fit. Below, I discuss several such possibilities and present evidence suggesting that they are unlikely to account for the observed patterns in this context.

Could the increase be driven only by students switching into easier majors? A potential concern is that the observed increase in within-Engineering switching may only reflect students avoiding academic difficulty by shifting into easier majors. If this were the case, such switching would be less indicative of improved learning about match quality. Appendix Figure A.5 presents the switching matrix across engineering majors before and after the reform. Majors are ranked by the average SAT scores of graduates (as reported in Appendix Table A.2), providing a proxy for relative difficulty or selectivity. For instance, Computer Science Engineering and Biomedical Engineering rank among the most selective majors, while Industrial Engineering and Civil Engineering rank among the least selective. Notably, 54% of switches are into majors ranked as more difficult, suggesting that switching is not systematically biased toward easier options. Columns (1)–(2) of Appendix Table A.6 further disaggregate switching into movements toward easier and harder majors. The policy-

Table 2: Policy effect on within-Engineering switching and exits from Engineering

	(1) Within-Engineering switching	(2) Exits from Engineering
Years 2005 to 2009	0.007	0.017
	(0.010)	(0.012)
Years 2011 to 2015	0.050***	-0.081***
	(0.010)	(0.012)
Observations	52,278	52,278
Baseline mean	0.067	0.100
Pre mean	0.062	0.113
Post mean	0.089	0.068
Controls	Yes	Yes

Notes: This table reports average effects from estimating equation (1) on the impact of the early major declaration policy on switching behavior. A total of 3,572 students who dropped out are excluded from the analysis. Column (1) reports the probability of switching within the umbrella of Engineering majors, and Column (2) reports the probability of exiting from Engineering. The effect for year 2010 is normalized to zero. The coefficient for years 2005–2009 represents the pre-policy average, and the coefficient for years 2011–2015 represents the post-policy average. The baseline mean refers to the mean of the outcome variable for the 2010 Engineering cohort. The pre mean refers to the outcome mean for pre-reform Engineering cohorts (2005–2010), and the post mean refers to the outcome mean for post-reform Engineering cohorts (2011–2015). All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, a dummy for female, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

induced increase in switching is statistically significant and persists across both categories, indicating that the rise in within-Engineering switching is not solely driven by downward academic movement. Finally, Appendix Figure A.6 presents changes in pairwise transition probabilities within Engineering. The non-positive diagonal values in the matrix, which reflect post-policy changes in the probability of remaining in the initially declared major, further suggest a general increase in within-Engineering transitions.

Could the increase be driven only by CS-related majors? Given the growing popularity of computing-related fields, it is natural to ask whether the increase in switching is concentrated in CS-related majors. Columns (3)–(4) of Appendix Table A.6 distinguish switching cases involving CS-related majors—defined as movements into or out of Computer

Science, Computer Engineering, or Electrical Engineering—from all other engineering fields. The increase in switching is significant across both groups. If anything, the effect is slightly larger among non-CS-related majors, suggesting that the policy's impact is not solely driven by shifts involving CS fields.

Could the increase in switching be driven by more time to declare? Another possible explanation is that the observed increase in switching simply reflects students having more time to change majors under the new policy. However, the policy advanced major declaration by less than one term on average, and most students already had ample time to switch under the pre-policy regime—the median switching occurred in the sixth enrolled term. Furthermore, the fraction of students who switched before the declaration deadline (i.e., during the third term) also rises significantly, from 0.5% to 1.5%. If the increase in switching were fully driven by post-reform students declaring earlier and thereby gaining more post-declaration time to switch, we should not observe any increase in switching before the deadline, when pre- and post-reform students have the same amount of time to switch. Since early switchers are unaffected by the change in declaration timing, this pattern indicates that the increase is not driven by simply having more time to switch, but rather by earlier corrections induced by the policy. These patterns suggest that extended time to switch is unlikely to be the primary driver of the observed increase in within-Engineering switching.

Could the increase in within-Engineering switching result from administrative formality rather than meaningful reconsideration? Another potential concern is that the observed switching may partly reflect bureaucratic processes rather than genuine academic re-evaluation—for instance, students might declare a random major simply to satisfy the requirement and then switch later. Several pieces of evidence suggest otherwise. First, despite the policy's clear and strongly worded requirement, approximately 23% of students still miss the declaration deadline. As discussed in Subsection 6.1, these students tend to have weaker academic backgrounds, implying that the declaration threshold represents a substantive hurdle rather than a procedural formality. Second, declaring a major requires scheduling and attending an advising appointment, indicating intentional engagement with the academic planning process. Moreover, engineering majors differ significantly in their

associated labor market outcomes, including earnings and industry placement, as shown in Appendix Table A.2. These differences suggest that the choice of major—even within Engineering—is consequential. Taken together with the observed shifts in coursetaking patterns, these pieces of evidence suggest that the increase in switching reflects meaningful academic decisions rather than mere compliance with administrative requirements, such as declaring a placeholder major to switch later.

4.3.2 Effects on exits from Engineering

To test Prediction 3, whether increased specialization indeed creates barriers to sorting into better-fitting majors due to tightened exiting constraint, I examine the reform's impact on exits from Engineering. The outcome is defined as a case in which a student initially enrolled in one college but ultimately graduated with a different major in another college. Column (2) of Table 2 shows a large and statistically significant effect: a reduction of 8 percentage points, or an 80% decline relative to the baseline rate of exits from Engineering (event study shown in Panel (b) of Figure A.4). This finding is consistent with the framework, which predicts that early specialization in major-specific coursework can lower the likelihood of satisfying graduation credit requirements in other umbrella fields—since such credits are not transferable—thereby reducing the feasibility and probability of exiting into a new field.

To further investigate this mechanism, I examine whether the policy's effect varies by the "distance" between a student's initially declared and final graduation majors. Specifically, I classify exits based on whether the initial and final majors are: (i) across colleges but within the same disciplinary domain (either STEM/BE or non-STEM/BE), which share similar quantitative skill requirements and training; or (ii) across both college and disciplinary domains—such as switching from Engineering to the humanities or social sciences.

Panel (a) of Appendix Figure A.7 shows that the policy effect on exits is much larger when the destination major is further from Engineering. The reduction in exits to other STEM/BE fields is modest—about 1.6 percentage points, a 30% decline—whereas the reduction in exits to non-STEM/BE fields is substantially larger, at 6.5 percentage points. This corresponds to a decline of over 100% relative to the baseline, indicating that the probability of such exits nearly disappears. Furthermore, while the framework simplifies by assuming mutual credit transferability across all engineering majors, improved credit transferability may also facilitate switching between engineering fields. The policy indeed induces a larger

increase in switching rates among engineering majors within the same subgroup, which are more similar in curricular content 3.7 percentage points, a 132% increase, compared to 1.3 percentage points, a 34% increase for other pairs). Panel (b) further plots the changes in pairwise switches following the reform (off-diagonal values in Appendix Figure A.6), showing a negative relationship between changes in pairwise transition probabilities and the distance between majors.[26] This pattern further supports the mechanism in the framework that credit transferability across fields—as proxied by the "distance" between majors—plays a critical role in shaping students' post-declaration behavior.[27]

4.3.3 How do the policy's effects vary across student subgroups?

Given that this study focuses on the timing of major declaration, a natural and important dimension of heterogeneity may arise from differences in students' prior beliefs about their fit across majors. Intuitively, students who are well-informed about their intended major before entering college should be less affected by the timing of declaration.

Although students' priors about match quality are not directly observable, uncertainty may stem from two sources: (1) lower academic preparation in fields relevant to engineering—such as math, physics, or chemistry—and (2) less informational support from family background or minority status, including lower parental education, lower household income, and gender. Students with weaker preparation in STEM-related subjects may lack confidence in their ability to succeed in engineering, while those from less informationally advantaged backgrounds may be less aware of the full range of available college majors. As a result, both groups may be more sensitive to the timing of major declaration. Appendix Table A.8 reports heterogeneous effects of the policy on within-Engineering switching by these dimensions. I focus on within-Engineering switching rather than exits from Engineering because the latter are shaped not only by belief updating about match quality, but also by tightened exiting constraint introduced by the policy reform. Although the framework assumes that all students are unwilling to delay graduation in order to fulfill graduation credit requirements for switching into a major outside Engineering that fits them better, this may not be the case empirically: Students' willingness to bear the cost of delayed graduation likely varies across

[26] Taken together with the low level of content distance across engineering majors, the flat slope further supports the framework's assumption of mutual transferability.

[27] This finding is consistent with Hsu (2018), who shows that path dependence from early coursework can limit the feasibility of switching majors.

groups. For instance, students from lower-income families may be less willing to incur the cost of delayed graduation, making them less likely to exit Engineering—even if their initial major choice was more likely to be a mismatch. As a result, exits from Engineering reflect both differences in prior beliefs and differences in constraints, making it difficult to isolate the role of prior beliefs alone. Across a range of subgroup comparisons, I find suggestive evidence that the policy effect is larger among students with characteristics associated with greater uncertainty about major match. For example, to proxy for academic preparation, students who did not take engineering-related AP tests experienced a 6.6 percentage point increase in within-Engineering switching, compared to a 4.4 percentage point increase among those who did. There is also a stark gender difference: Incentivizing early major declaration leads to a 6.5 percentage point increase in switching among female students, relative to only 3.5 percentage points among male students. However, in most cases, the differences across groups are not statistically significant.

Taken together, the empirical patterns support the framework's predictions: The timing of major declaration shapes not only how students learn about their match quality across majors—affecting how informed they are when making both initial and final major choices—but also how they take courses and accumulate credits, which in turn influences their ability to meet graduation requirements when switching to a different major.

4.4 Effects on educational outcomes at graduation: GPA and honors

Following earlier major declarations, do students, on average, perform better or worse academically by graduation? The answer to this question is important in itself, but it also provides empirical evidence on the impact of overall major match quality across students.[28] As described in the framework, when both within-Engineering switching and exits from Engineering are present, the overall impact on final match quality is theoretically ambiguous (Prediction 4).[29] I assess final match quality using two academic outcomes at graduation:

[28] GPA is widely used as a proxy for major match quality in the literature; see Arcidiacono (2004) and Zafar (2011) for examples.

[29] Prediction 3 provides an unambiguous prediction regarding match quality among engineering graduates when only within-Engineering switching is considered. Empirically, since there is also a significant impact

cumulative GPA and an indicator for receiving an honors degree.[30] Given that there is a small and insignificant policy effect on dropout (-0.5%, p-value = 0.578, shown in Appendix Table A.12), concerns about selection into graduation are alleviated. Out of 55,850 students, 52,278 graduated with an undergraduate degree, forming the analysis sample for graduation outcomes.

Table 3 shows that graduation GPA increases by 0.04 points, approximately 10% of a standard deviation among graduates. Recall that the policy raises the share of students declaring a major before the start of their fourth term — from 61% to 77%, a 16 percentage point increase. Assuming the GPA increase results from the policy's effect of shifting major declaration to before the deadline, a simple back-of-the-envelope calculation (0.04 / 0.16) implies an effect of approximately 0.25 GPA points from declaring earlier — a substantial gain, just under one full letter-grade step (A− to A is approximately 0.30 points). Consistent with these gains, the fraction of students earning an honors degree increases by 7 percentage points — a 14% increase relative to the baseline honors rate. Appendix Figure A.8 plots the event-study coefficients by year for these two outcomes. These improvements in academic performance provide suggestive evidence that earlier major declaration leads to better overall major match quality, and they remain robust to a series of checks discussed below, including adjustments for changes in baseline characteristics and major difficulty.

Moreover, the increase in academic performance persists when restricting to major-specific GPA (0.071 grade points), defined as the average grades in courses that match the student's declared major (column (1), Appendix Table A.9). This further supports the interpretation that incentivizing early major declaration leads to improvements in overall major-specific match quality.

on exits from the College of Engineering, the sample of engineering graduates is affected by the policy. As a result, the observed impact on match quality among engineering graduates reflects a combination of selection and treatment effects. Nevertheless, I also find a similar increase in graduation GPA (0.09 grade points) and in the fraction earning an honors degree (7 percentage points), conditional on graduating with an engineering degree.

[30] Honors degrees are GPA-based. For engineering degrees, the distinctions are *cum laude*, *magna cum laude*, and *summa cum laude*, awarded based on GPA thresholds.

Table 3: Policy effects on GPA and honors degrees at graduation

	(1) GPA	(2) Honors degree
Years 2005 to 2009	-0.010	0.010
	(0.015)	(0.018)
Years 2011 to 2015	0.040***	0.074***
	(0.014)	(0.018)
Observations	52,278	52,278
Baseline mean	3.253	0.531
Pre mean	3.249	0.539
Post mean	3.368	0.657
Controls	Yes	Yes

Notes: This table reports average effects from estimating equation (1) on the impact of the major declaration policy on educational outcomes at graduation. Column (1) reports the effect on cumulative GPA, and Column (2) reports the effect on the probability of earning an honors degree. The effect in year 2010 is normalized to zero. The coefficient for years 2005–2009 is the pre-policy average, and the coefficient for years 2011–2015 is the post-policy average. The baseline mean refers to the mean of the outcome variable for the 2010 Engineering cohort. The pre mean refers to the outcome mean for pre-reform Engineering cohorts (2005–2010), and the post mean refers to the outcome mean for post-reform Engineering cohorts (2011–2015). All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, a dummy for female, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

4.4.1 Discussion

Admittedly, many factors could potentially contribute to an increase in GPA and, consequently, the likelihood of earning an honors degree. In this subsection, I present additional evidence showing that the observed improvement cannot be fully explained by alternative explanations.

Could changes in baseline characteristics explain the increase in academic performance? Appendix Table A.3 shows some relative improvement in pre-college academic performance among engineering students over time (15 points in SAT, and 7 points in SAT Math), raising the possibility that changes in baseline academic ability could explain the GPA increase. Moreover, there is also a relative increase in the fraction of female students in the College of Engineering (4%), which may contribute to the GPA increase, as female

students tend to have higher GPAs. To examine the role of changes in baseline characteristics, I restrict the sample to the pre-reform cohort (2005–2010) and estimate graduation outcomes based on SAT scores and gender. I then use these estimates to predict GPA and honors degree outcomes for post-reform cohorts (2011–2015), based on their SAT scores and gender. The predicted increase is substantially smaller than the observed effect (0.01 grade points in GPA and 0.4% in honors degrees), as shown in Appendix Figure A.9, suggesting that changes in student composition account for only a small portion of the results. I further split the main effect by gender in panels (a) and (b) of Appendix Table A.10, and the significant increase in academic performance persists across both genders. In fact, among engineering students, GPAs of female students are not higher than those of their male counterparts (3.23 vs. 3.26), so changes in gender composition are unlikely to drive the observed increase in GPAs. Finally, I control for SAT scores and gender in the main specifications, as discussed in Section 4.1, holding student characteristics constant when interpreting the results.[31]

Does the increase in GPA persist after accounting for major difficulty or changes in coursetaking? The GPA increase may reflect a shift toward easier majors or majors with looser grading, rather than improved match quality. Although easier majors could be associated with higher GPAs, both upward and downward switches by major difficulty within Engineering account for about 50% of changes, making directional bias unlikely. To address this concern, I demean GPA by each major's average GPA in the first year of the sample period (2005). Column (2) of Appendix Table A.9 shows that the improvement persists. Moreover, I include major fixed effects in Column (4) of Appendix Table A.9, and the significant impact persists at around 0.5 grade points. These results suggest that differences in course difficulty or grading practices across majors are unlikely to be the primary drivers of the observed GPA improvements. Lastly, recall that the policy change results in a longer period after initial declaration, which leads to more high-level major courses being taken post-reform. Since differences in grading across course levels could confound the results, I conduct a course-grade-level analysis in which I control for course fixed effects. The significant positive effect persists as well.

[31] Column (3) of Appendix Table A.9 shows that the estimated increase in GPA remains robust also when no control variables are included.

5 Effects on labor market outcomes

In this section, I examine the effects of incentivizing early major declaration on students' labor market outcomes. I focus on two sets of measures that proxy for student–major match quality in the labor market, providing an additional test of the theoretically ambiguous impact on overall match quality, as discussed in Prediction 4. These results also offer insight into the longer-term consequences of early major declaration incentives, extending the analysis beyond academic performance.

5.1 Empirical strategy

To address concerns that engineering-specific labor market shocks or differences in career characteristics may bias comparisons between engineering students and those in CALS, I adopt an alternative difference-in-differences empirical strategy. I compare students in the engineering college that implemented the 2011 policy change (hereafter, the treated engineering college) with those in a comparable control engineering college that did not change its policy on major declaration, across cohorts before and after the reform. The control engineering college is similar in overall engineering college ranking and is part of another Midwestern flagship university.

The analysis of labor market outcomes relies on job histories sourced from LinkedIn data, which also include self-reported educational records. For example, students usually list college attendance from year X to year Y without specifying the term of enrollment or academic standing at entry. To accurately assign treatment status based on enrollment year and status, among those receiving undergraduate degrees from the treated engineering college, I restrict the treated group to transcript-matched students with observed enrollment records.[32] After excluding students entirely missing publicly available employment information or with incomplete information (such as missing position duration), the final sample

[32] As administrative data from the control engineering colleges are unavailable, the control group consists of students who earned engineering degrees from the control college. The treatment group consists of students enrolled in the engineering college, who may or may not graduate with an engineering degree. The results are robust to defining treatment status using degree information self-reported on LinkedIn—that is, by whether engineering degree earners enrolled before or after 2011. Similar to Footnote 29, restricting the sample to engineering degree earners likewise induces policy-driven selection, so the observed impact on match quality among engineering graduates reflects a combination of selection and treatment effects.

includes 5,468 students in the treated engineering college and 12,677 students in the control engineering college, totaling 90,872 job position records (see Appendix C for data details).

The analysis is based on the following specification:

$$\begin{aligned} Y_{it} = &\sum_{\tau \neq 2010} \alpha_\tau \cdot \mathbb{1}[\tau = t] + \beta \cdot \text{Treated Engineering}_i \\ &+ \sum_{\tau \neq 2010} \delta_\tau \cdot \mathbb{1}[\tau = t] \cdot \text{Treated Engineering}_i + \epsilon_{it}. \end{aligned} \tag{2}$$

In this equation, Y_{it} denotes the outcome of interest for engineering student i from cohort t, such as an indicator of working as an engineer. The variable Treated Engineering$_i$ is an indicator equal to one if student i was enrolled in the engineering college that implemented the reform to incentivize early major declaration. The coefficients δ_τ capture cohort-specific differential effects for the treated engineering college relative to the control, using 2010 as the omitted base year. I again report the post-period average of the δ_τ coefficients, calculated as $\frac{1}{5}\sum_{\tau=2011}^{2015} \delta_\tau$, and interpret this estimate as the average treatment effect of incentivizing early major declaration induced by the policy. Note that, since I do not have access to administrative data from the control engineering colleges, I cannot control for baseline ability or demographic characteristics as in analysis for educational outcomes. Instead, Appendix Table B.1 includes controls based on predicted demographics generated by the data vendor—such as gender and race inferred from names—and the inclusion of these controls does not qualitatively affect the results.

As in the previous analysis, I use cohorts enrolled from 2005 to 2015. The LinkedIn profile data are last fully observed in 2024, and the most recent cohort, which enrolled in 2015, primarily graduated in 2019. Accordingly, I construct a balanced panel that tracks students for up to five years after graduation. This empirical design enables comparisons across engineering graduates from different colleges while controlling for time-varying cohort effects and time-invariant college-level differences. The key identifying assumption is that, in the absence of the reform, labor market outcomes for students in the treated and control engineering colleges would have followed parallel trends. Appendix Figure C.3 shows closely aligned trends in LinkedIn match rates between the treated engineering college and CALS, while Appendix Figure C.4 displays parallel patterns of student selection into the engineering sample across Midwestern flagships hosting the treated and control engineering colleges.

Appendix Figures B.1–B.2 further present event study plots that provide supporting evidence for the parallel trends assumption in the labor market outcomes analyzed below.

I conduct several robustness checks in Section 5.3, including: (i) applying the same specification used in the analysis of educational outcomes—comparing students from the College of Engineering and CALS, as detailed in Section 4.1; (ii) adding controls; and (iii) using alternative samples as control groups.

5.2 Outcomes

I focus on two sets of measures that proxy for student–major match quality. First, I examine whether individuals work as engineers and whether they hold positions aligned with their specific engineering major. Second, I analyze early career dynamics—including participation in college internships related to engineering, the duration of the first job, and whether individuals attain a managerial position five years after graduation.

5.2.1 Working as engineers

Although innate student–major match quality is difficult to observe directly, a common approach in the literature is to use proxies based on whether a student's career aligns with their field of study (see, for example, Robst (2007) and McGuinness, Pouliakas and Redmond (2018)). In this context, match quality is proxied by whether students who receive engineering training ultimately work as engineers. If the learning gain outweighs the tightened exit constraint—as suggested by the framework—we would expect to observe improved match quality, reflected in a higher likelihood of working as engineers.

Columns (1)-(2) of Table 4 report the effects of incentivizing early major declaration on engineering career outcomes, measured by indicators of working in an engineering position in the first job and remaining in such a role in the year 5 job. The policy increases the probability of working in an engineering role in the first job by 4 percentage points (a 5% increase), and significantly raises the likelihood of remaining in an engineering position five years after graduation by 7 percentage points. This latter effect represents a 12% increase relative to the 2010 baseline, in which 60% of students remained in engineering roles at year 5—indicating stronger attachment to engineering careers.

Columns (3)-(4) of Table 4 further restrict the outcome to working in major-matched engi-

neering jobs—defined as positions aligned with a student's specific engineering major—and yields similar results. Major-matched engineering jobs are defined according to the CIP-ONET-SOC Crosswalk, in which each major is linked to several occupations based on the content of CIP code and SOC code descriptions (NCES, 2020). For example, students who graduate with a civil engineering major may work as Civil Engineers, Architectural and Engineering Managers, Transportation Engineers, or Water/Wastewater Engineers—all of which are considered occupations that match their major. Similarly, those with a computer science engineering major may work in matched positions ranging from Web Administrators to Video Game Designers. The baseline mean of working in major-matched engineering jobs is much lower; as a result, a 5 percentage point increase represents a substantial 23% gain in working in a field aligned with one's training, suggesting an improvement in major-specific match quality. Appendix Figure B.1 shows the corresponding event study plots for both working as general engineers and as major-matched engineers.

5.2.2 Early career dynamics

The improvement in overall student–major match quality following earlier major declaration may also be reflected in secondary labor market outcomes, such as starting an engineering internship during college or securing better-fitting jobs that lead to stronger early-career attachment and faster advancement. I find a series of results consistent with this hypothesis.

First, with earlier and more intensive exposure to high-level, major-specific engineering coursework due to the policy change, positive effects emerge as early as the college internship stage. Incentivizing early major declaration leads to a 4 percentage point increase in the likelihood of doing an engineering internship during college—a 14% increase relative to the 30% baseline average. Upon entering the labor market after graduation, the policy change has a significant impact on early job stability in engineering: The duration of the first job increases by approximately 140 days. I also examine whether students attain a managerial or senior engineering position five years after graduation and find suggestive evidence of accelerated upward mobility. These patterns, reported in Table 5 and illustrated in Appendix Figure B.2, provide further supporting evidence that incentivizing early major declaration improves overall major–match quality, which in turn contributes to stronger early career outcomes.

Taken together, while the policy's impact on overall match quality is theoretically am-

Table 4: Effects on the likelihood of working as an engineer in the first job and remaining in an engineering role in the year 5 job

	Any engineer		Major-matched engineer	
	First job	Year 5 job	First job	Year 5 job
	(1)	(2)	(3)	(4)
Years 2005 to 2009	0.004	-0.021	0.024	0.014
	(0.024)	(0.028)	(0.028)	(0.025)
Years 2011 to 2015	0.044*	0.073***	0.048*	0.055**
	(0.022)	(0.027)	(0.027)	(0.025)
Observations	18,145	18,145	18,145	18,145
Baseline mean	0.773	0.600	0.344	0.240
Pre mean	0.776	0.583	0.317	0.214
Post mean	0.796	0.656	0.383	0.294

Notes: This table reports average effects from estimating equation (2) on the impact of the major declaration policy on labor market outcomes. Columns (1)-(2) focus on the likelihood of working as an engineer, while Columns (3)-(4) focus on the likelihood of working as a major-matched engineer. A major-matched engineer is defined according to the CIP-SOC Crosswalk developed by NCES and BLS, where matches are based on the alignment between the content of CIP code and SOC code descriptions. Columns (1) and (3) report the effect on the first job, and Columns (2) and (4) report the effect on remaining in an engineering role five years after graduation. The effect in 2010 is normalized to zero. The coefficient for years 2005–2009 represents the pre-policy average, and the coefficient for years 2011–2015 represents the post-policy average. The baseline mean refers to the mean of the outcome variable for the 2010 engineering cohort. All regressions are estimated using OLS and include college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

biguous, the labor market outcomes provide additional evidence that the reform leads to improvements in student–major match quality. These patterns are consistent with earlier findings that the policy improved GPA at graduation and suggest that incentivizing early major declaration may generate lasting benefits beyond college by improving the alignment between students' training and their subsequent career trajectories.

5.3 Robustness

Despite careful cleaning of the labor market data and a rigorous matching process to define treatment status, potential sources of bias remain due to the self-reported nature of LinkedIn data. To assess the robustness of the findings on the primary outcome, I conduct a series

Table 5: Effects on early engineering career dynamics

	(1) College internship	(2) First job duration (days)	(3) Managerial position (Year 5)
Years 2005 to 2009	-0.020	40.646	-0.011
	(0.025)	(61.047)	(0.017)
Years 2011 to 2015	0.043*	140.163**	0.028*
	(0.025)	(54.617)	(0.017)
Observations	18,145	18,145	18,145
Baseline mean	0.265	822	0.094

Notes: This table reports average effects from estimating equation (2) on the impact of the major declaration policy on engineering labor market outcomes. Column (1) reports the impact on participation in college engineering internships; Column (2) reports the impact on the duration of the first engineering job (in days); and Column (3) reports the impact on whether the individual attained a managerial or senior-level engineering position. The effect in 2010 is normalized to zero. The coefficient for years 2005–2009 represents the pre-policy average, and the coefficient for years 2011–2015 represents the post-policy average. The baseline mean refers to the mean of the outcome variable for the 2010 engineering cohort. All regressions are estimated using OLS and include college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

of checks—including alternative empirical specifications, variations in the choice of control group, and alternative sample restrictions. The primary outcome is defined as an indicator for working as an engineer, measured both at the first job and based on persistence in an engineering role five years after graduation, as described in Subsection 5.2.1.

Do the results persist under alternative specifications? I explore two alternative empirical specifications. First, I include additional covariates to control for observable characteristics. Since administrative data are unavailable for students from the control engineering college, I instead use predicted demographic variables provided by the Revelio data source—specifically, gender, race, highest level of education, and the number of LinkedIn connections. Second, I estimate a standard difference-in-differences model using a binary post-reform indicator. This specification replaces the event-study cohort indicators with a

single treatment interaction term:

$$\begin{aligned} Y_{it} = \beta_0 + \beta_1 \,\text{Treated Engineering}_i + \beta_2 \,\text{Post Reform}_t \\ + \beta_3 \,(\text{Treated Engineering}_i \times \text{Post Reform}_t) + \epsilon_{it} \end{aligned} \tag{3}$$

In this equation, Post Reform_t is a binary indicator equal to one for cohorts enrolled after 2010, and $\text{Treated Engineering}_i$ indicates whether student i was enrolled in the engineering college that implemented the policy. The coefficient β_3 captures the average treatment effect of the policy change on the treated group, relative to the control. Appendix Table B.1 presents the results from both approaches. In all cases, the estimates remain directionally consistent with the baseline specification (see Subsection 5.1) and statistically significant, providing additional evidence that the main findings are robust to alternative specifications and the inclusion of demographic controls.

Do the results persist when comparing CALS and engineering students? Despite concerns that field-specific differences may confound labor market comparisons between students in CALS and those in the College of Engineering at the Midwestern flagship university, I replicate the analysis using the same specification from Section 4.1. As shown in Panel (a) of Appendix Table B.2, the results remain qualitatively similar.

Panel (b) of Appendix Table B.2 implements an event-study difference-in-differences-in-differences (DDD) specification, comparing engineering and CALS students across two Midwestern flagship universities before and after the policy reform. The full event-study DDD specification is:

$$\begin{aligned} Y_{ict} = \sum_{\tau \neq 2010} \alpha_\tau \cdot \mathbb{1}[\tau = t] + \sum_{\tau \neq 2010} \delta_\tau \cdot \mathbb{1}[\tau = t] \cdot \text{Treated University}_c \times \text{Engineering}_i \\ + \gamma_1 \text{Treated University}_c + \gamma_2 \text{Engineering}_i + \gamma_3 (\text{Treated University}_c \times \text{Engineering}_i) + \epsilon_{ict}, \end{aligned} \tag{4}$$

where Y_{ict} denotes the labor market outcome for student i in university c and cohort t. The coefficient δ_τ captures cohort-specific treatment effects for engineering students at the treated university, relative to both CALS students and students at the control university, with the 2010 cohort serving as the omitted (reference) group. The estimated effects, based on the average of the post-reform interaction terms $\left(\frac{1}{5}\sum_{\tau=2011}^{2015} \delta_\tau\right)$, remain directionally and quantitatively consistent with the baseline specification, providing additional support for the robustness of the main findings.

Do the results persist when using an alternative control engineering college? To assess the sensitivity of the findings to the choice of control group, I replicate the baseline specification using a second control engineering college. This alternative college is slightly larger in size (18,235 students across 11 cohorts) but shares similar institutional characteristics: It is comparably ranked among engineering colleges and is also part of a public Midwestern flagship university (see Appendix C.2 for details). As shown in Panel (a) of Appendix Table B.3, the estimated positive effects of incentivizing early major declaration on the likelihood of working as an engineer in the first job and remaining in an engineering role five years after graduation remain robust to this alternative comparison.

Do the results persist under alternative treated sample restrictions? The use of publicly available LinkedIn profiles, instead of administrative labor market records, raises two potential concerns: selection bias, due to differential likelihood of having a LinkedIn profile, and matching bias, since I restrict the treatment group to students who can be matched to transcript-based enrollment records. To address these concerns, I conduct two additional robustness checks. First, I expand the sample to include all engineering students identified in the Revelio dataset (8,388 students in total), including those not matched to administrative enrollment records, and re-estimate the main effects. As shown in Panel (b) of Appendix Table B.3, the results remain qualitatively consistent. Second, recognizing that women are less likely to be matched than men in earlier cohorts, I stratify the sample by gender.[33] Appendix Table B.4 shows that the estimated effects are positive and statistically significant for both men and women, suggesting that differential match rates by gender do not drive the main results.

6 Policy discussion

In this section, I discuss several groups of results that are important for assessing the broader impact of the policy but lie beyond the primary focus of the framework's predictions. I first examine patterns of policy compliance in subsection 6.1, focusing on which engineering students declare a specific major before the deadline and which ones fall behind. Subsection 6.2 then considers broader implications from the college's perspective, focusing on institutionally

[33]Matching rates by gender are shown in Panels (b)–(c) of Appendix Figure C.3.

relevant outcomes such as dropout rates and efficiency measures, including credits to degree and terms to degree. Finally, in subsection 6.3, I discuss outcomes related to students' overall college experience, focusing on how the policy affects cumulative coursetaking patterns during college.

6.1 Who follows the policy? Who falls behind?

As mentioned in Section 2.1, approximately 23% of engineering students failed to comply with the new major declaration deadline after the reform—that is, they did not declare an engineering major before their fourth enrolled term. Instead, they were required to meet with an academic advisor and develop an individualized plan. Following the policy change, it is informative to examine who complies with the policy and who falls behind, in order to better understand which students may need more time to choose a major.

Appendix Table A.11 shows that students who miss the deadline tend to have lower academic preparation—reflected in lower SAT scores (1,413 vs. 1,435), fewer AP exams taken (4.71 vs. 5.46), and lower average AP scores (3.87 vs. 4.15). Moreover, they appear to come from more disadvantaged backgrounds: They are more likely to be underrepresented minorities (11% vs. 8%), more likely to come from families with income below $99,000 (31% vs. 26%), and less likely to have parents with a bachelor's degree or higher (81% vs. 84%). These patterns indicate meaningful heterogeneity in major declaration compliance behavior and underscore potential equity concerns brought by the policy across student groups, particularly as it may differentially affect students facing greater uncertainty in major choice and at greater risk of a major–student mismatch.

6.2 How does the policy affect outcomes of interest to the college?

In this subsection, I examine how the policy affects outcomes that are typically of institutional interest, including student dropout rates, degree attainment, and efficiency in completing a degree, measured by credits accumulated and terms enrolled. These outcomes help assess whether the policy is broadly beneficial from the college's perspective, providing evidence of its potential impact on institutional performance and operations.

Appendix Table A.12 reports the relevant results. Following the introduction of the policy, the estimated impact on the overall university dropout rate is a statistically insignificant

decline of 0.5 percentage points, while the policy significantly increases the likelihood of earning an engineering degree by 4 percentage points—a 5% increase compared to baseline. These effects suggest that the policy leads to more students persisting in engineering and successfully completing an engineering degree.

In terms of efficiency measures, the policy reduces the total number of credits earned during college by approximately 2 credits and shortens the number of main academic terms (fall and winter) to degree completion by 0.09 terms. While both effects are statistically significant, their magnitudes are economically small, suggesting that the policy yields only modest improvements in students' academic progression efficiency.[34]

6.3 How does the policy affect students' overall coursetaking experience?

The early major declaration requirement may affect students' overall learning experience in multiple ways. I focus on examining students' cumulative coursetaking patterns at graduation, as shown in Appendix Table A.13.[35]

I find that students complete 1.7 more major-specific credits—defined as courses whose four-digit CIP code matches the student's graduation major—while taking 3.7 fewer non-major credits. Within major-specific coursework, the increase is driven by a shift toward 300-level and higher courses, which are more advanced and plausibly more relevant for labor market applications (see Appendix Figure D.1). In addition, students complete coursework in 0.6 fewer distinct fields overall, where fields are defined based on four-digit CIP codes. This pattern suggests a modest shift toward greater specialization within the major, accompanied by a reduction in non-major coursework. The reduction in non-major study is reflected in humanities and social science courses: The number of distinct disciplines taken falls by 13 and 25 percent, respectively—courses that plausibly contribute to students' broader intellectual development and to the overall utility from the college experience.

[34]The policy does lead to a sharp increase in the fraction of students who spend fewer than the standard eight main academic terms to earn a degree—a 5 percentage point increase, equivalent to a 44% rise compared to the baseline mean.

[35]I restrict the sample to graduates in majors that consistently have graduates throughout the sample period to account for the strong dependence of coursetaking patterns on major and to avoid confounding comparisons arising from shifts in major composition over time.

7 Conclusion

This paper examines the consequences of incentivizing early major declaration on students' educational and labor market outcomes. I first develop a dynamic framework in which students learn about their match quality across majors through course grades while accumulating credits toward graduation. The framework highlights how the timing of major declaration shapes both the information students acquire about relative match quality and the feasibility of acting on updated beliefs through major switching. I test the framework's predictions using a difference-in-differences design that exploits a 2011 policy change at a Midwestern flagship university, which incentivized early major declaration among engineering students.

The results show that incentivizing early declaration meaningfully reshapes students' academic trajectories. Students shift toward earlier and more intensive engagement with major-specific coursework—especially high-level courses—are more likely to switch majors within Engineering, and are less likely to exit from Engineering altogether. These findings align with the framework's predicted trade-off between learning gain from more high-level coursework and the tightened exit constraint created by increased specialization. I also find improvements in academic performance—higher GPAs and increased honors graduation rates—as well as positive labor market outcomes, including greater persistence in engineering careers, longer job tenure, and higher rates of managerial attainment.

Taken together, the findings suggest that the timing of field specialization plays an important role in shaping both educational and career outcomes. Conditional on preserving students' ability to adjust their choices—particularly when early specialization reduces the feasibility of exiting the initial field—policies that encourage earlier declaration may support more informed major choices and improve student–major match quality at graduation and beyond.

These findings raise several important questions for future research. First, this study focuses on major choices among students enrolled in the College of Engineering, which provides a case of students choosing among a set of related majors while uncertain which to pursue. While the learning framework is broadly applicable, this setting may differ meaningfully from other academic disciplines. In particular, high-level coursework may play a less critical role in informing major fit in non-engineering fields. Future work should examine how the

structure of disciplinary knowledge mediates the effects of early field specialization across different domains. Second, while the results suggest improved student-major match quality, they also indicate a potential trade-off in the form of reduced exposure to a broader set of fields as seen in Subsection 6.3. This raises the question of how the depth versus breadth of knowledge contributes to long-term outcomes, given that both are important (Han, Lee and Yoon, 2025). Studying the relative returns to specialization and generalization could help inform the design of optimal human capital development strategies and the timing of specialization decisions. Third, it is important to understand how students experience the incentives for early major declaration. While it may improve match quality, early declaration can generate unintended psychological costs, such as increased stress or pressure to commit early. Future research could explore the perceived consequences of early specialization from the student's point of view and investigate its implications for well-being and college experience.

In sum, the timing of field specialization involves trade-offs. Encouraging earlier, but not rigidly fixed, specialization—along with institutional support for informed decision-making and credit transferability—may promote better student-major matches and stronger long-term outcomes.

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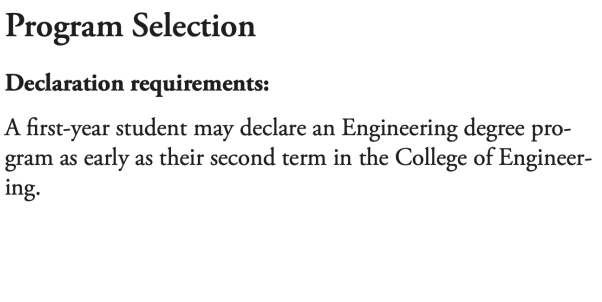
ONLINE APPENDIX

A Additional results on educational outcomes

A.1 Appendix figures

Figure A.1: Major declaration policy in the College of Engineering: before vs. after reform

(a) Policy in 2010 and earlier

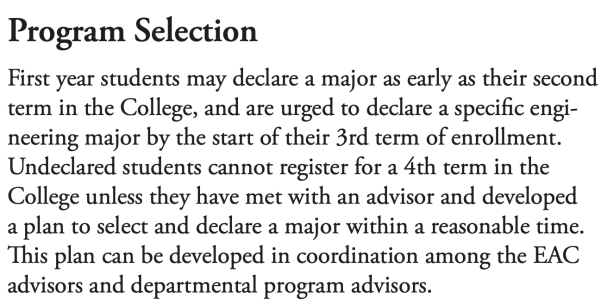


Program Selection

Declaration requirements:

A first-year student may declare an Engineering degree program as early as their second term in the College of Engineering.

(b) Policy in 2011 and later

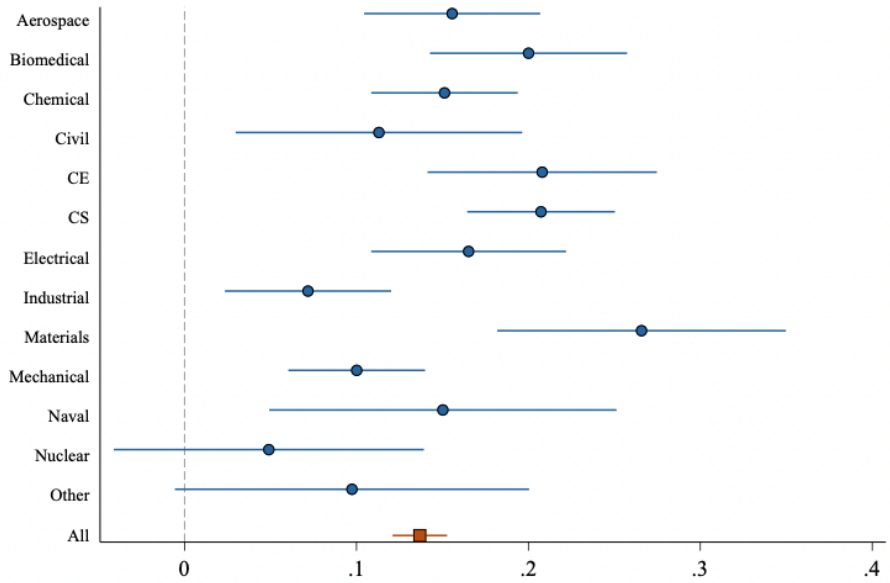


Program Selection

First year students may declare a major as early as their second term in the College, and are urged to declare a specific engineering major by the start of their 3rd term of enrollment. Undeclared students cannot register for a 4th term in the College unless they have met with an advisor and developed a plan to select and declare a major within a reasonable time. This plan can be developed in coordination among the EAC advisors and departmental program advisors.

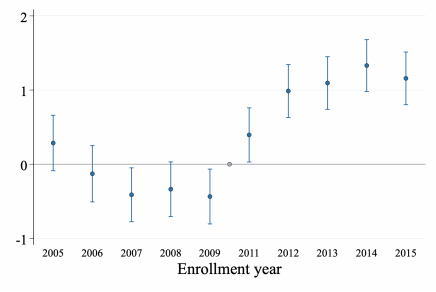
Notes: This figure illustrates the major declaration policy in the College of Engineering before and after the 2011 reform, highlighting the introduction of formal deadlines. Policies are sourced from archived academic bulletins.

Figure A.2: Impact of the policy on the fraction of students declaring before the fourth term across engineering majors

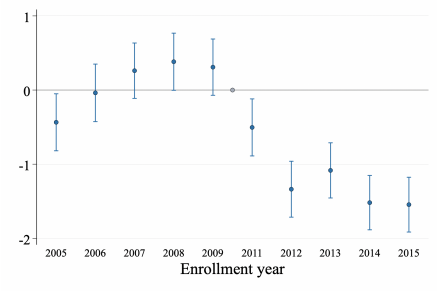


Notes: This figure shows the increase in the fraction of students declaring before the fourth term across engineering majors, comparing cohorts before (2005–2010) and after (2011–2015) the policy change. Each point represents the change in the fraction of students declaring before the fourth term in the post-policy period relative to the pre-policy period, shown separately by major, with horizontal lines indicating 95% confidence intervals. "Aerospace" represents aerospace engineering, "Biomedical" represents biomedical engineering, "Chemical" represents chemical engineering, "Civil" represents civil engineering, "CE" represents computer engineering, "CS" represents computer science, "Electrical" represents electrical and electronic engineering, "Industrial" represents industrial and operations engineering, "Materials" represents materials engineering, "Mechanical" represents mechanical engineering, "Naval" represents naval engineering, and "Nuclear" represents nuclear engineering. "Other" groups all majors with fewer than 100 observations across the 11 cohorts, such as engineering physics and space science and engineering. "All" aggregates all engineering majors together.

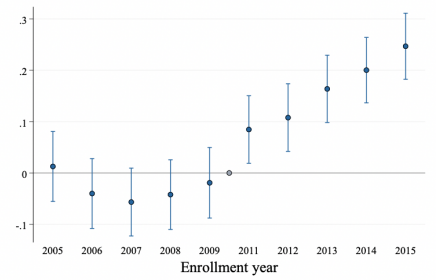
Figure A.3: Proximate effects on coursetaking in the fourth term



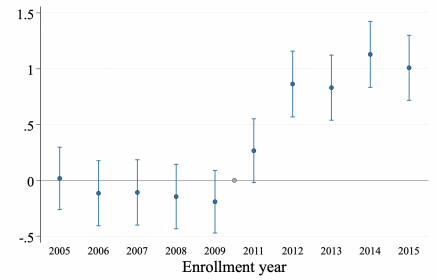
(a) Major-specific credits



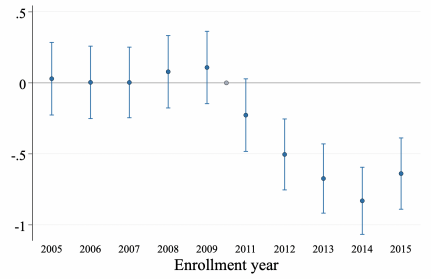
(b) Non-major-specific credits



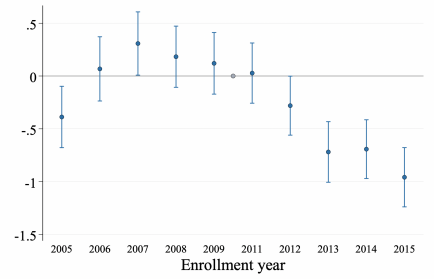
(c) Major-specific course level



(d) 300+ major-specific credits



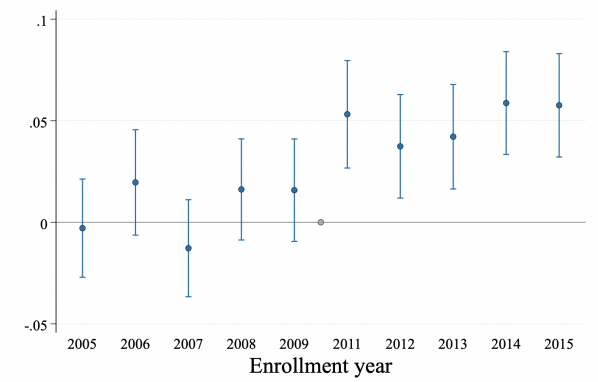
(e) General engineering credits



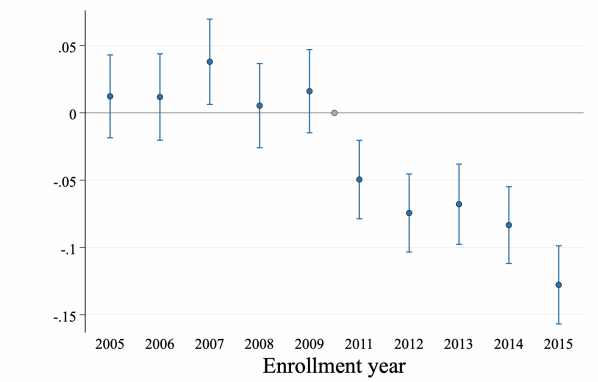
(f) Humanities/BE credits

Notes: This figure plots the estimated year-by-year effects of the major declaration policy on coursetaking patterns during the fourth term (the first term after the declaration deadline), based on equation (1). Panel (a) shows results for the number of major-specific credits, where a course is defined as major-specific if its 4-digit CIP code matches that of the student's declared major. Panel (b) shows results for the number of non-major-specific credits, defined as all credits not classified as major-specific. Panel (c) shows results for the number of 100–200-level major-specific credits defined by course number (e.g., IOE 201 is a 200-level course). Panel (d) shows results for the number of major-specific credits from courses at the 300 level or above. Panel (e) shows results for credits earned in engineering courses that do not belong to the student's declared engineering major. Panel (f) shows results for credits earned in humanities or business and economics (BE) courses. Year 2010 is normalized to zero and serves as the omitted (reference) year. All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, a dummy for female, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects. Robust standard errors are used to construct 95% confidence intervals.

Figure A.4: Policy effect on within-Engineering switching and exits from Engineering



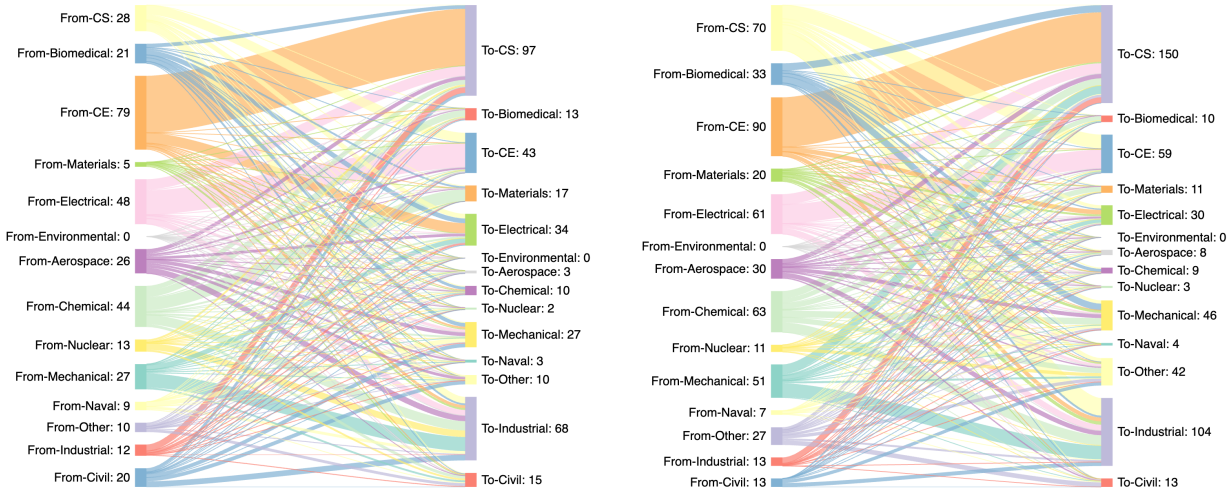
(a) Within-Engineering switching



(b) Exits from Engineering

Notes: This figure plots the estimated year-by-year effects of the early major declaration policy on the probability of switching within the umbrella of Engineering majors (Panel (a)) and of exits from Engineering (Panel (b)), based on equation (1). A total of 3,572 students who dropped out are excluded from the analysis. Year 2010 is normalized to zero and serves as the omitted (reference) year. All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, a dummy for female, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects. Robust standard errors are used to construct 95% confidence intervals.

Figure A.5: Within-Engineering switching matrix before and after the reform

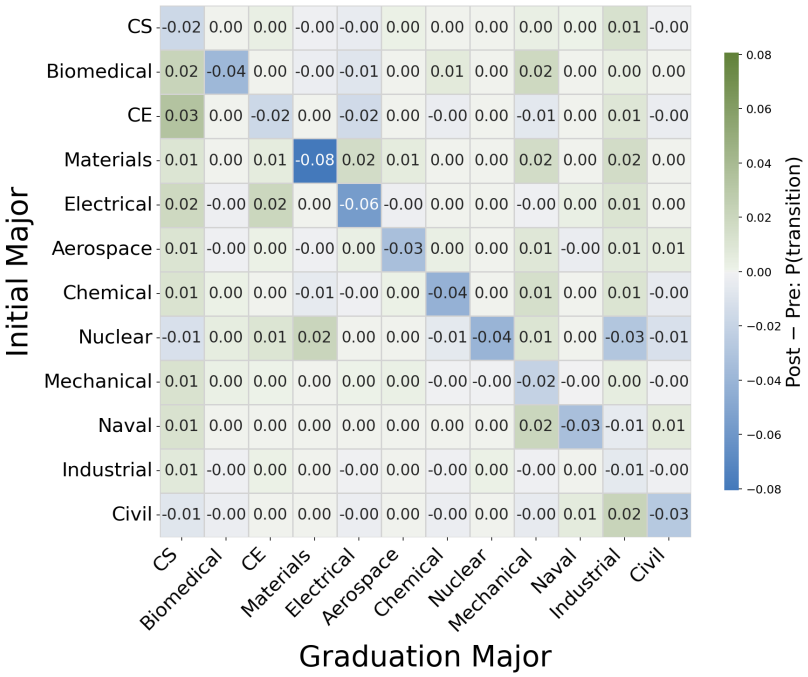


(a) Pre-reform (Cohorts 2005–2010)

(b) Post-reform (Cohorts 2011–2015)

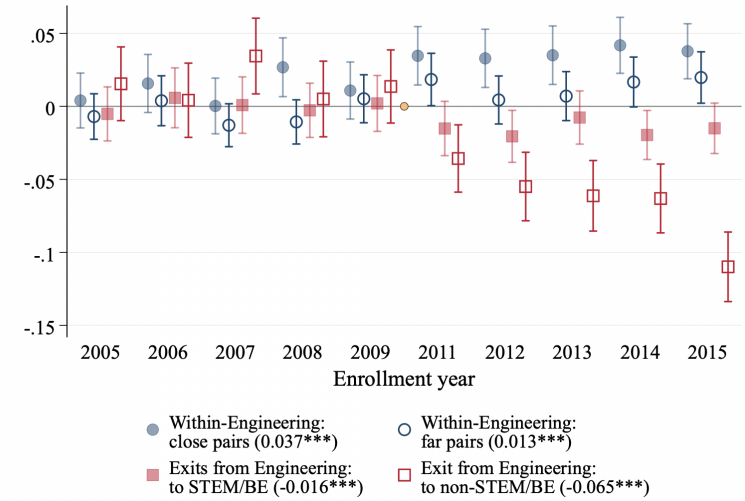
Notes: This figure presents the within-Engineering switching matrix based on 831 observed switching cases among students initially enrolled in the College of Engineering. Panel (a) shows patterns for pre-reform cohorts (2005–2010), and Panel (b) for post-reform cohorts (2011–2015). The left-hand side lists engineering majors initially declared, and the right-hand side lists engineering majors at graduation. Majors are ordered by average SAT score (from high to low) based on Appendix Table A.2. The number next to each major label indicates the total number of switches associated with that major. For example, "From-CS: 28" means that 28 students switched from Computer Science to another engineering major. The width of each flow line reflects the relative frequency of switches. For full major names associated with each label, see Appendix Figure A.2.

Figure A.6: Changes in pairwise transition probabilities within Engineering

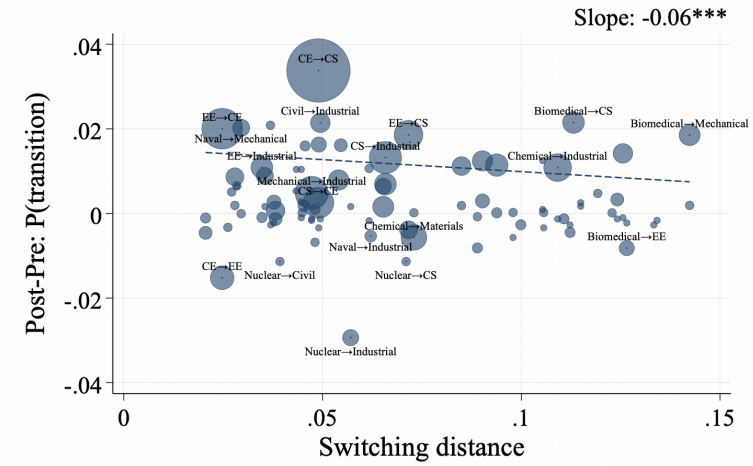


Notes: This figure shows the change in pairwise transition probabilities (Post - Pre) based on switching cases among students initially enrolled in the College of Engineering. Less common majors are excluded from the analysis (the "Other" category, defined in Appendix Figure A.2, includes majors such as Engineering Physics and Space Science and Engineering). Darker shades indicate larger magnitudes, with negative changes shown in blue and positive changes in green. The y-axis lists engineering majors initially declared, and the x-axis lists engineering majors at graduation. Majors are ordered by average SAT score (from high to low) based on Appendix Table A.2. For full major names associated with each label, see Appendix Figure A.2.

Figure A.7: Effect of early major declaration on switching by distance



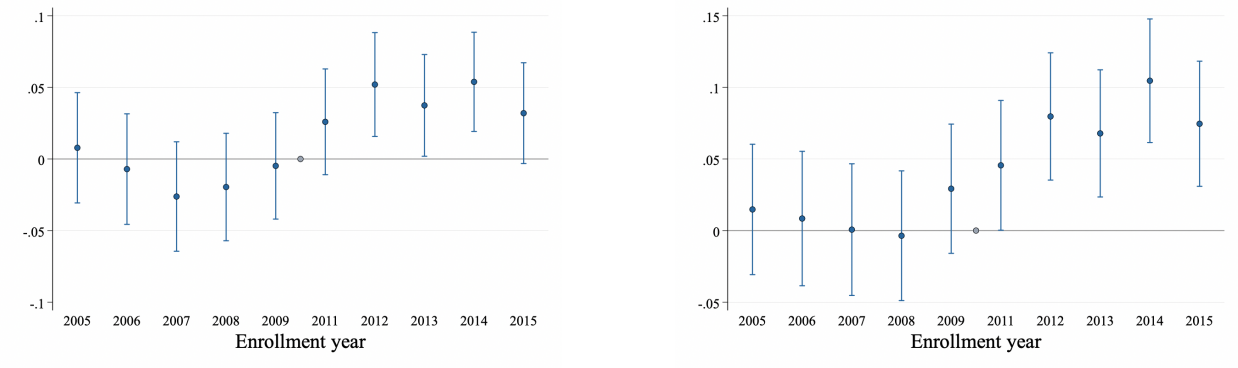
(a) Overall switching by distance



(b) Within Engineering switching by distance

Notes: This figure presents the relationship between switching and switching distance. Panel (a) reports year-by-year effects of the early major declaration policy on switching behavior by switching distance, based on equation (1). Outcomes are defined as follows: Within-Engineering: close pairs denotes switches within the same engineering subgroup (i.e., small pairwise distance), where engineering is split into four groups: Electrical/CE/CS; Mechanical, Aerospace, and Naval; Chemical, Biological, and Materials; and other Engineering fields. Within-Engineering: far pairs denotes switches across these subgroups. Exits from Engineering are further classified as switching to another STEM/BE field or to a non-STEM/BE field. Year 2010 is normalized to zero and serves as the omitted (reference) year. All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, a dummy for female, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects. Robust standard errors are used to construct 95% confidence intervals. The post-policy average of the coefficient for years 2011–2015 is shown in parentheses. Panel (b) plots the change in transition probabilities between the pre- and post-policy periods for pairs of engineering majors (off-diagonal values in Appendix Figure A.6); only pairs with changes greater than 0.01 are labeled for clarity. Switching distance is the Euclidean distance between course-content vectors, where a major's course-content vector is the distribution of course load across academic departments. The average course-content distance from Economics to engineering majors is 0.62, versus 0.07 among engineering majors. Labels indicate the switching pair (e.g., "CE→CS" denotes a switch from CE to CS). The linear fit is estimated with frequency weights equal to the total pairwise switching frequency. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Figure A.8: Policy effect on GPA and honors degrees at graduation

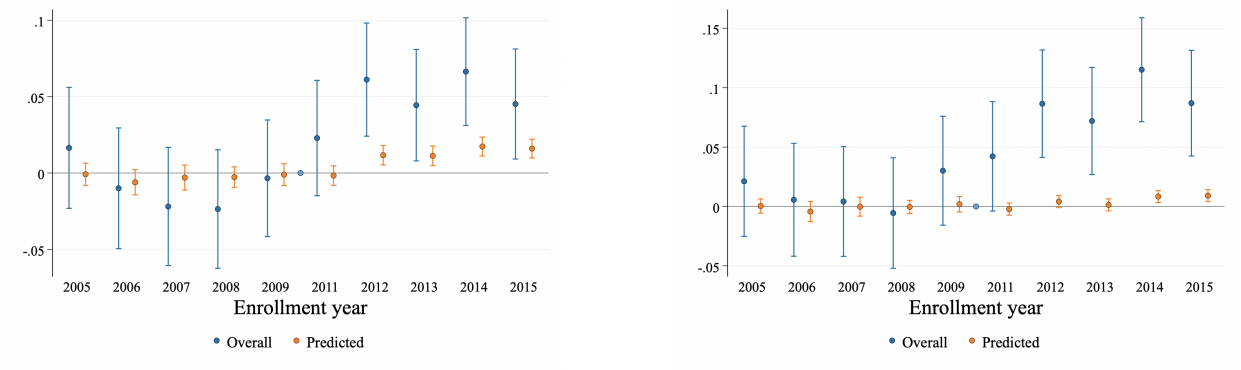


(a) GPA

(b) Earning honors degree

Notes: This figure plots the estimated year-by-year effects of the major declaration policy on graduation outcomes, based on equation (1). Panel (a) shows the effect on cumulative GPA, and panel (b) shows the effect on the probability of earning an honors degree. Year 2010 is normalized to zero and serves as the omitted (reference) year. All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, a dummy for female, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects. Robust standard errors are used to construct 95% confidence intervals.

Figure A.9: Policy effect on GPA and earning honors degrees at graduation



(a) GPA

(b) Earning honors degree

Notes: This figure plots the estimated year-by-year effects of the early major declaration policy on academic outcomes. Panel (a) reports effects on GPA; Panel (b) reports effects on the probability of earning an honors degree. Year 2010 is normalized to zero and serves as the omitted (reference) year. "Overall" estimates are obtained using OLS with equation (1) and include student-level controls (SAT composite and math scores, a female indicator, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects. "Predicted" estimates are based on a model estimated using pre-reform cohorts (2005–2010), where graduation outcomes are regressed on SAT scores and gender, and then used to predict post-reform outcomes based on students' characteristics. Robust standard errors are used to construct 95% confidence intervals.

A.2 Appendix tables

Table A.1: First-year engineering course offerings and coursetaking patterns before and after the policy change

	Pre	Post	P-value
Panel (a): Aggregate course-level metrics			
Number of courses	411	428	0.20
Average course size	172	170	0.05
Panel (b): Term-level student coursetaking patterns			
Total credits taken	15	15	0.01
Total credits earned	15	15	0.01
Share of general engineering courses	0.24	0.24	0.68
Share of math course	0.19	0.19	0.53
Share of physics courses	0.15	0.17	0.00
Share of chemistry courses	0.25	0.22	0.00
Share of field-specific engineering courses	0.02	0.03	0.00
Share of humanities courses	0.10	0.10	0.12
Share of social science courses	0.03	0.02	0.00
Observations	7,221	6,221	

Notes: This table compares aggregate course-level metrics (Panel (a)) and term-level coursetaking patterns (Panel (b)) among students enrolled in the College of Engineering before (cohorts 2005–2010) and after (cohorts 2011–2015) the 2011 major declaration policy change, focusing specifically on first-year courses. "Pre" refers to the period prior to the policy implementation; "Post" refers to the period following it. Panel (a) reports the total number of courses taken by engineering students and their average enrollment size. Panel (b) reports the average number of credits taken and earned per term, as well as the share of courses taken by subject area: general engineering, math, physics, chemistry, field-specific engineering, humanities, and social science. Field-specific engineering refers to courses tied to particular engineering majors, while general engineering courses are not field-specific. P-values test for differences between the "Pre" and "Post" periods.

Table A.2: Student characteristics and labor market outcomes by engineering major

	Student body			At graduation		5 years after graduation
	Size	**SAT**	**SAT Math**	**Median earnings**	**Top industry**	**Median earnings**
Aerospace	1,010	1,401	733	$82,500	Aerospace and Defense	$113,025
Biomedical	751	1,429	739	$75,000	Healthcare	$85,471
Chemical	1,327	1,400	733	$80,000	Chemicals	$100,000
Civil	589	1,357	704	$69,000	Civil and Construction	$88,870
CE	595	1,416	737	$92,000	Software and Computer Services	$123,120
CS	2,065	1,431	742	$120,000	Software and Computer Services	$153,297
Electrical	838	1,412	745	$82,000	Technology Hardware and Equipment	$109,107
Environmental	102	1,412	715	$65,500	Civil and Construction	$88,990
Industrial	1,785	1,377	724	$83,000	Consulting	$109,239
Materials	426	1,413	739	$75,500	Technology Hardware and Equipment	$95,471
Mechanical	2,186	1,395	730	$79,000	Automobiles and Parts	$101,514
Naval	252	1,391	740	$70,500	Industrial Engineering and Transportation	$92,852
Nuclear	274	1,398	726	N/A	Utilities	$99,564
Other	235	1,387	715	N/A	N/A	N/A
All	12,435	1,402	732	$90,000	N/A	N/A

Notes: This table presents information on the student body and labor market outcomes by engineering major, based on the major students graduated with. "Size" refers to the number of students who graduated in each major across 11 cohorts. "SAT" and "SAT Math" represent the average SAT scores of students in each major. Labor market outcomes at graduation are drawn from the First Destination Survey conducted by the College of Engineering, based on the most recent three graduating cohorts (2022–2024). "Top industry" refers to the industry that employs the largest share of graduates in each major. Median annual earnings five years after graduation are from the College Scorecard. Majors with fewer than 100 graduates across the sample period are grouped under "Other," which includes less common majors such as engineering physics and space science and engineering. "N/A" indicates missing information from the data source.

Table A.3: Summary statistics of student demographics and academic profiles before and after the policy change

	Overall			Engineering			CALS			DiD
	Engineering	CALS	P-value	Pre	Post	P-value	Pre	Post	P-value	Post
Age	18.26	18.23	0.00	18.28	18.24	0.00	18.23	18.23	0.78	-0.02
Female	0.25	0.56	0.00	0.24	0.27	0.00	0.56	0.55	0.00	0.04**
White	0.69	0.75	0.00	0.71	0.67	0.00	0.75	0.74	0.00	-0.01
URM	0.08	0.09	0.06	0.07	0.09	0.01	0.10	0.07	0.00	0.02***
SAT	1,393.97	1,356.81	0.00	1,363.95	1,428.78	0.00	1,335.98	1,381.17	0.00	15.22***
SAT math	723.73	678.67	0.00	712.06	737.26	0.00	670.81	687.86	0.00	7.31***
Number of AP test	4.37	3.66	0.00	3.62	5.23	0.00	3.19	4.21	0.00	0.53***
Mean AP score	3.92	3.66	0.00	3.78	4.07	0.00	3.58	3.74	0.00	0.08***
Household income $<$ \$99,000	0.30	0.27	0.00	0.32	0.27	0.00	0.28	0.25	0.00	-0.01
Household income $\geq$ \$100,000	0.45	0.47	0.00	0.37	0.54	0.00	0.40	0.55	0.00	-0.00
Parent with bachelor(+) degree	0.84	0.83	0.67	0.84	0.83	0.71	0.83	0.83	0.49	0.01
Enrollment size	1,222	3,855	0.00	1,204	1,244	0.30	3,812	3,908	0.47	-57
Observations	13,442	42,408		7,221	6,221		22,866	19,543		

Notes: This table presents summary statistics for key student characteristics by college, comparing Engineering and CALS students, and by time period before and after the reform. The table is divided into four main sections: Overall, Engineering, CALS, and DiD. The "Overall" columns compare all Engineering and CALS students. The "Engineering" and "CALS" columns report pre- and post-policy values for each group, along with p-values testing for differences across periods. The final "DiD" column reports the average post-policy difference between Engineering and CALS students, based on the coefficient—averaged over the 2011–2015 period—estimated from equation (1) without controls. "Age" refers to age at enrollment. "Female" is a binary indicator equal to one if the student self-identifies as female and zero if male. "White" is a binary indicator based on administrative race data. "URM" indicates underrepresented minority status and includes U.S. citizens or permanent residents who self-identify as Hispanic, Native American, Black or African American, Native Hawaiian, or Other Pacific Islander. "SAT" scores (overall and math) include either reported SAT scores or ACT scores converted to SAT-equivalent values. If both are available, the average of the SAT and converted ACT scores is used. "Number of AP tests" is the total number of AP exams taken, and "Mean AP score" is the average score across all AP exams taken. "Household income $<$ \$99,000" and "Household income $\geq$ \$100,000" are indicators based on administrative household income brackets. "Parent with bachelor(+) degree" is an indicator equal to one if at least one parent has a bachelor's degree or higher. "Enrollment size" is the number of students in each entering cohort. "Observations" refer to the number of students in each group. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.4: Effects of early major declaration on subsequent coursetaking outcomes during the third and fourth enrolled terms

	Overall			Major-specific credits		Other credits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total credits	Major-specific credits	Other credits	Major credits (100–200)	Major credits (300+)	General engineering credits	Humanities/BE credits
Years 2005 to 2009	0.026	0.059	-0.033	-0.043	0.102	0.154	-0.226
	(0.179)	(0.232)	(0.245)	(0.180)	(0.128)	(0.156)	(0.186)
Years 2011 to 2015	-0.378**	2.029***	-2.407***	0.946***	1.084***	-1.085***	-0.914***
	(0.179)	(0.230)	(0.243)	(0.179)	(0.129)	(0.156)	(0.182)
Observations	54,056	54,056	54,056	54,056	54,056	54,056	54,056
Baseline mean	28.842	8.643	20.199	6.417	2.226	4.093	4.642
Pre mean	28.945	8.422	20.523	6.264	2.158	4.073	4.938
Post mean	28.486	10.341	18.145	7.529	2.811	3.455	3.821
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports average effects from estimating equation (1) on the impact of the early major declaration policy on coursetaking outcomes during the third and fourth enrolled terms. There are 54,056 students ever enrolled in the fourth term; students who dropped out earlier are excluded. Columns (1)–(3) report effects on credit allocation: total credits, major-specific credits (courses whose 4-digit CIP code matches the declared major), and other credits. Columns (4)–(5) report effects on course type within the major: the number of 100–200-level major-specific credits (defined by course number) and the number of 300-level or higher major-specific credits. Columns (6)–(7) report effects on non-major-specific coursework: general engineering credits (field-specific engineering courses outside the declared major) and credits earned in humanities and business–economics (BE) courses. The effect for year 2010 is normalized to zero. The coefficient for years 2005–2009 represents the pre-policy average; the coefficient for years 2011–2015 represents the post-policy average. The baseline mean refers to the mean of the outcome variable for the 2010 Engineering cohort. The pre mean refers to the outcome mean for pre-reform Engineering cohorts (2005–2010), and the post mean refers to the outcome mean for post-reform Engineering cohorts (2011–2015). All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, a dummy for female, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.6: Policy effect on within-Engineering switching by type

	(1) Easier	(2) Harder	(3) CS-related	(4) Non CS-related
Years 2005 to 2009	0.011*	-0.004	-0.007	0.014*
	(0.007)	(0.007)	(0.006)	(0.008)
Years 2011 to 2015	0.034***	0.015**	0.012*	0.038***
	(0.007)	(0.007)	(0.007)	(0.008)
Observations	52,278	52,278	52,278	52,278
Baseline mean	0.022	0.044	0.040	0.026
Controls	Yes	Yes	Yes	Yes

Notes: This table reports average effects from estimating equation (1) on the impact of the early major declaration policy on within-Engineering switching behavior, disaggregated by type of switch. Column (1) reports the effect on switching to an easier major, defined as switching to a major with a lower average SAT score among graduates. Column (2) reports the effect on switching to a harder major, defined as switching to a major with a higher average SAT score. Column (3) shows the effect on switching into CS-related fields (Computer Science, Computer Engineering, and Electrical Engineering). Column (4) reports the effect on switching across other engineering fields not classified as CS-related. Year 2010 is normalized to zero and serves as the omitted (reference) year. The coefficient for years 2005–2009 represents the pre-policy average; the coefficient for years 2011–2015 represents the post-policy average. The baseline mean refers to the mean of the outcome variable for the 2010 Engineering cohort. All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, a dummy for female, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.7: Robustness of policy effects on within-Engineering switching and exits from Engineering

	(1)	(2)	(3)	(4)
	Within-Engineering	Within-Engineering	Exits from Engineering	Exits from Engineering
Years 2005 to 2009	0.007	-0.000	0.018	0.019*
	(0.010)	(0.009)	(0.012)	(0.011)
Years 2011 to 2015	0.051***	0.036***	-0.083***	-0.030***
	(0.010)	(0.009)	(0.012)	(0.011)
Observations	52,278	52,276	52,278	52,276
Baseline mean	0.067	0.067	0.100	0.100
Controls	No	Yes	No	Yes
Major FEs	No	Yes	No	Yes

Notes: This table reports average effects from estimating equation (1) on the impact of the early major declaration policy on switching behavior. Columns (1)–(2) report the effect on the probability of switching majors within Engineering; Columns (3)–(4) report the effect on the probability of exiting from Engineering. In all columns, year 2010 is normalized to zero and serves as the omitted (reference) year. The coefficient for years 2005–2009 represents the pre-policy average; the coefficient for years 2011–2015 represents the post-policy average. The baseline mean refers to the mean of the outcome variable for the 2010 Engineering cohort. Columns (1) and (3) report results from OLS models estimated without controls. Columns (2) and (4) add student-level controls (SAT composite and math scores, a dummy for female, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects and fixed effects for the initially declared major. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.8: Heterogeneous policy effect on within-Engineering switching

	SAT Math score		Took AP test		AP test score		Parental education		Household income		Gender	
	Low	High	No	Yes	Low	High	Low	High	Low	High	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Years 2005 to 2009	0.017	0.007	0.017	0.001	0.005	0.000	0.028	0.006	0.016	0.016	-0.007	0.007
	(0.016)	(0.012)	(0.018)	(0.012)	(0.019)	(0.015)	(0.040)	(0.010)	(0.029)	(0.013)	(0.018)	(0.012)
Years 2011 to 2015	0.064***	0.044***	0.066***	0.044***	0.052***	0.041***	0.088**	0.048***	0.060**	0.053***	0.065***	0.035***
	(0.018)	(0.012)	(0.020)	(0.012)	(0.020)	(0.015)	(0.042)	(0.010)	(0.030)	(0.012)	(0.019)	(0.012)
Observations	23,670	28,608	20,170	32,108	12,768	19,340	2,504	47,367	4,934	33,658	25,539	26,739
Baseline mean	0.049	0.072	0.065	0.067	0.060	0.070	0.036	0.068	0.045	0.062	0.072	0.065
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
P-value	0.367		0.355		0.665		0.349		0.851		0.177	

Notes: This table reports average effects on within-Engineering switching from estimating equation (1) separately by subgroup.Columns (1)–(2) split the sample by whether a student's SAT Math score is below or above the median; columns (3)–(4) by whether the student took an engineering-related AP test (Physics, Chemistry, or Calculus); columns (5)–(6) by whether the AP score is below or above the median; Columns (7)–(8) split by parental education (below a college degree vs. some college or above); columns (9)–(10) by whether annual household income is below $75,000; and columns (11)–(12) by gender (women vs. men). Reported p-values test for differences in the policy effect across groups. In all columns, the year 2010 is normalized to zero and serves as the omitted (reference) year. The coefficient for years 2005–2009 represents the pre-policy average, and the coefficient for years 2011–2015 represents the post-policy average. The baseline mean refers to the mean of the outcome variable for the 2010 Engineering cohort. All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, a female dummy, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.9: Robustness checks for the policy effect on GPA at graduation

	(1) Major GPA	(2) Demeaned GPA	(3) GPA	(4) GPA	(5) Grades
Years 2005 to 2009	0.001	-0.006	-0.009	-0.009	0.002
	(0.015)	(0.014)	(0.015)	(0.014)	(0.007)
Years 2011 to 2015	0.051***	0.042***	0.048***	0.047***	0.047***
	(0.015)	(0.014)	(0.015)	(0.014)	(0.007)
Observations	41,561	49,763	52,278	52,274	394,575
Baseline mean	3.287	-0.009	3.253	3.253	3.279
Controls	Yes	Yes	No	Yes	Yes
Major FEs	No	No	No	Yes	No
Course FEs	No	No	No	No	Yes

Notes: This table reports average effects from estimating equation (1) on the impact of the major declaration policy on GPA at graduation across various robustness specifications. Column (1) reports the effect on major-specific GPA, defined as the average GPA in courses that match the student's declared major by four-digit CIP code. Column (2) reports the effect on GPA after adjusting for major difficulty by demeaning GPA using each major's average GPA in the baseline year (2005). Column (3) does not include any control, and Column (4) includes major fixed effects. Column (5) presents results from a course-grade-level analysis with course fixed effects. The effect in year 2010 is normalized to zero. The coefficient for years 2005–2009 is the pre-policy average, and the coefficient for years 2011–2015 is the post-policy average. The baseline mean refers to the mean of the outcome variable for the 2010 Engineering cohort. All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, a dummy for female, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.10: Policy effect on GPA at graduation by gender

	Panel (a): Male students	
	(1)	(2)
	GPA	Earning honors degree
Years 2005 to 2009	-0.025	0.004
	(0.019)	(0.022)
Years 2011 to 2015	0.038**	0.071***
	(0.018)	(0.022)
Observations	26,739	26,739
Baseline mean	3.260	0.546
Controls	Yes	Yes
	Panel (b): Female students	
	(1)	(2)
	GPA	Earning honors degree
Years 2005 to 2009	0.024	0.030
	(0.029)	(0.035)
Years 2011 to 2015	0.072**	0.116***
	(0.028)	(0.034)
Observations	25,539	25,539
Baseline mean	3.232	0.484
Controls	Yes	Yes

Notes: This table reports average effects from estimating equation 1 on the impact of the major declaration policy on GPA at graduation, separately by gender. Panel (a) restricts the sample to male students, and panel (b) to female students. Column (1) reports the effect on cumulative GPA, and column (2) reports the effect on earning an honors degree. The effect in year 2010 is normalized to zero. The coefficient for years 2005–2009 is the pre-policy average, and the coefficient for years 2011–2015 is the post-policy average. The baseline mean refers to the mean of the outcome variable for the 2010 Engineering cohort. All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, a dummy for female, race dummies, and a dummy for missing demographic information), as well as college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.11: Demographic characteristics by declaration deadline compliance

	Declaration term		
	Before 4th term (Meet deadline)	During/after 4th term (Miss deadline)	P-value
Age	18.23	18.26	0.07
Female	0.28	0.26	0.18
White	0.67	0.65	0.09
URM	0.08	0.11	0.00
SAT	1,435	1,412	0.00
SAT math	741	729	0.00
Number of AP tests	5.46	4.71	0.00
Average of AP scores	4.15	3.87	0.00
Household income $<$ \$99,000	0.26	0.31	0.00
Household income $\geq$ \$100,000	0.55	0.52	0.05
Parent with bachelor(+) degree	0.84	0.81	0.00
Observations	4,624	1,395	

Notes: This table compares demographic characteristics between engineering students who meet the major declaration deadline (i.e., declare before the fourth term) and those who miss the deadline (i.e., declare during or after the fourth term). The comparison is restricted to post-policy students who enter the College of Engineering as freshmen in cohorts 2011–2015. A total of 615 students who drop out without declaring a major are excluded from the comparison. Reported P-values test for differences between the two groups. See Appendix Table A.3 for detailed variable definitions.

Table A.12: Policy effect on educational outcomes of institutional interest

	(1) Dropout	(2) Engineering degree	(3) Credits to degree	(4) Terms to degree
Years 2005 to 2009	0.007	-0.015	0.060	0.013
	(0.009)	(0.012)	(0.529)	(0.033)
Years 2011 to 2015	-0.005	0.040***	-1.865***	-0.088***
	(0.009)	(0.012)	(0.519)	(0.032)
Observations	55,850	55,850	52,278	52,278
Baseline mean	0.079	0.828	123.370	8.178
Pre mean	0.089	0.808	123.766	8.197
Post mean	0.061	0.873	120.691	8.068
Controls	Yes	Yes	Yes	Yes

Notes: This table reports average effects estimated from equation (1), examining the impact of the major declaration policy on educational outcomes of institutional interest. Column (1) shows the effect on the dropout rate, defined as not earning any undergraduate degree. Column (2) reports the effect on earning an engineering degree. Column (3) presents the effect on total credits earned during college, conditional on degree completion. Column (4) shows the effect on the total number of academic terms (fall and winter) enrolled until graduation, also conditional on earning a degree. The year 2010 is normalized to zero. Coefficients for 2005–2009 represent the pre-policy average; those for 2011–2015 represent the post-policy average. The baseline mean refers to the outcome mean for the 2010 engineering cohort. The pre mean refers to the outcome mean for pre-reform Engineering cohorts (2005–2010), and the post mean refers to the outcome mean for post-reform Engineering cohorts (2011–2015). All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, gender, race/ethnicity indicators, and a missing-demographics dummy), along with college and cohort fixed effects. Robust standard errors are shown in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.13: Effects on coursetaking outcomes at graduation

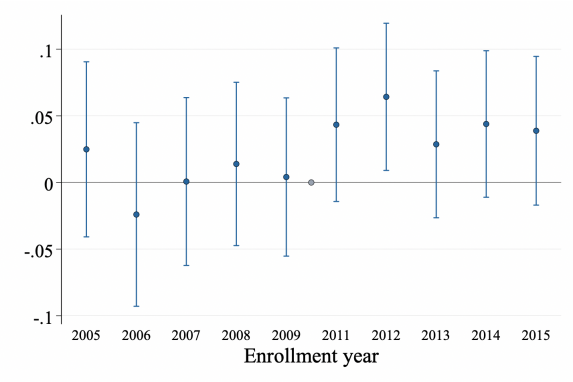
	(1) Major credits	(2) Non-major credits	(3) Fields taken	(4) Humanities	(5) Social sci./Bus.
Years 2005 to 2009	-0.744*	0.757	0.141	-0.067	-0.004
	(0.432)	(0.680)	(0.097)	(0.045)	(0.056)
Years 2011 to 2015	1.743***	-3.708***	-0.594***	-0.149***	-0.393***
	(0.427)	(0.672)	(0.098)	(0.044)	(0.058)
Observations	49,950	49,950	49,950	49,950	49,950
Baseline mean	45.380	73.680	11.781	1.600	1.519
Pre mean	44.462	74.342	11.594	1.596	1.468
Post mean	46.983	70.375	11.816	1.351	1.605
Controls	Yes	Yes	Yes	Yes	Yes

Notes: This table reports average effects estimated from equation (1), examining the impact of the major declaration policy on overall coursetaking patterns at graduation. The sample is restricted to students who complete a degree and further limited to common majors that appear across all 11 cohorts. Column (1) shows the effect on the total number of major credits, defined as the sum of course credits sharing the same four-digit CIP code as the graduated major. Column (2) reports the effect on non-major credits. Column (3) presents the effect on the number of distinct fields in which courses were taken, where each field is identified by the four-digit CIP code of the course. Columns (4) and (5) show the effect on the number of distinct non-major humanities and social science fields in which students took courses. The year 2010 is normalized to zero. Coefficients for 2005–2009 represent the pre-policy average; those for 2011–2015 represent the post-policy average. The baseline mean refers to the outcome mean for the 2010 engineering cohort. The pre mean refers to the outcome mean for pre-reform Engineering cohorts (2005–2010), and the post mean refers to the outcome mean for post-reform Engineering cohorts (2011–2015). All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, gender, race/ethnicity indicators, and a dummy for missing demographic information), as well as college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

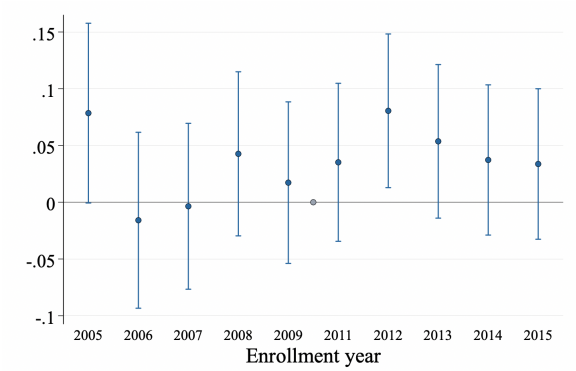
B Additional results on labor market outcomes

B.1 Additional figures

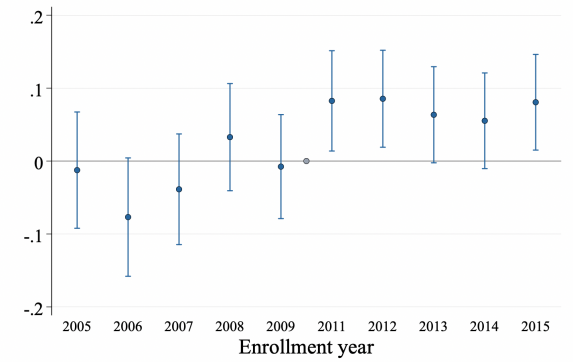
Figure B.1: Effects on the likelihood of working as an engineer in the first job and remaining in an engineering role five years after graduation



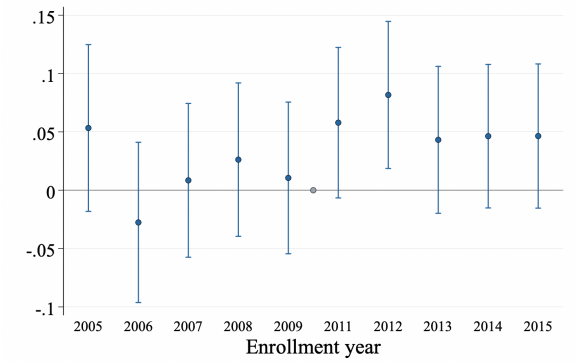
(a) Working as an engineer in the first job



(b) Working in a major-matched engineering role in the first job



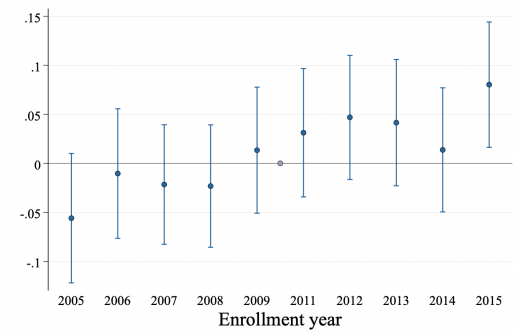
(c) Remaining in an engineering role five years after graduation



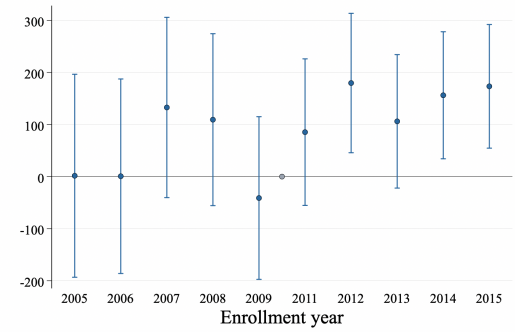
(d) Remaining in a major-matched engineering role five years after graduation

Notes: This figure plots the estimated cohort-by-cohort effects of the early major declaration policy on the likelihood of working in an engineering role in Panels (a) and (c), and in a major-matched engineering role in Panels (b) and (d), based on equation (2). The "year 5 job" refers to the job held five years after graduation. Panels (c) and (d) measure whether individuals remain in an engineering role or a major-matched engineering role at that time. Year 2010 is normalized to zero and serves as the omitted (reference) year. All regressions are estimated using OLS and include college and cohort fixed effects. Robust standard errors are used to construct 95% confidence intervals.

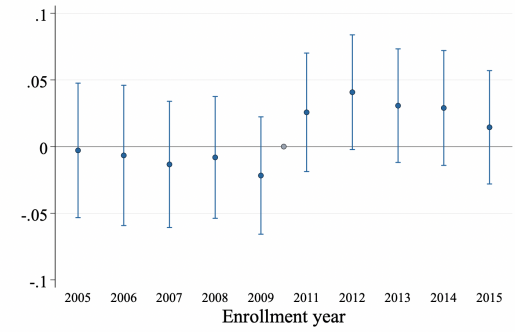
Figure B.2: Effects on early engineering career outcomes



(a) College engineering internship



(b) First job duration (in days)



(c) Managerial or senior role by Year 5

Notes: This figure plots the estimated cohort-by-cohort effects of the early major declaration policy on early engineering career outcomes, based on equation (2). Panel (a) reports the impact on participation in college engineering internships; Panel (b) reports the impact on the duration of the first engineering job (in days); and Panel (c) reports the impact on whether the individual attained a managerial or senior-level engineering position by Year 5. Year 2010 is normalized to zero and serves as the omitted (reference) year. All regressions are estimated using OLS and include college and cohort fixed effects. Robust standard errors are used to construct 95% confidence intervals.

B.2 Additional tables

Table B.1: Alternative specifications for the likelihood of working as an engineer in the first job and remaining in an engineering role five years after graduation

	Panel (a): Additional controls	
	(1)	(2)
	First job	Year 5 job
Years 2005 to 2009	0.005	-0.015
	(0.024)	(0.028)
Years 2011 to 2015	0.043*	0.068**
	(0.022)	(0.027)
Observations	18,145	18,145
Baseline mean	0.773	0.600
	Panel (b): Binary difference-in-differences	
	(1)	(2)
	First job	Year 5 job
Treated Engineering	0.040***	0.086***
× Post reform	(0.013)	(0.016)
Observations	18,145	18,145
Baseline mean	0.773	0.600

Notes: This table reports robustness checks on the primary outcome—working as an engineer—under two alternative specifications. Panel (a) reports estimates from Equation (2), including predicted demographic controls from the Revelio data source: gender, race, highest education level, and number of LinkedIn connections. Panel (b) estimates a binary difference-in-differences model using Equation (3), where "Post reform" is defined as cohorts after 2010. Column (1) reports the effect on working as an engineer in the first job; Column (2) reports the effect on remaining in an engineering role five years after graduation. The 2010 cohort is normalized to zero. Robust standard errors are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table B.2: CALS as an alternative control group: Effects on the likelihood of working as an engineer

	Panel (a): Difference-in-differences	
	(1)	(2)
	First job	Year 5 job
Years 2005 to 2009	-0.007	-0.009
	(0.024)	(0.026)
Years 2011 to 2015	0.021	0.050**
	(0.023)	(0.024)
Observations	23,917	23,917
Baseline mean	0.773	0.600
	Panel (b): Difference-in-differences-in-differences	
	(1)	(2)
	First job	Year 5 job
Years 2005 to 2009	-0.021	-0.021
	(0.028)	(0.031)
Years 2011 to 2015	0.048*	0.065**
	(0.027)	(0.029)
Observations	74,662	74,662
Baseline mean	0.773	0.600

Notes: This table reports average treatment effects of the major declaration policy on the likelihood of working as an engineer, using CALS students as the alternative control group. Panel (a) reports estimates from a difference-in-differences event-study specification adjusted from Equation (1). Panel (b) reports estimates from a difference-in-differences-in-differences (DDD) specification based on Equation (4), which compares engineering and CALS students across treated and control universities, before and after the reform. Column (1) shows effects on working as an engineer in the first job; Column (2) shows effects on remaining in an engineering role five years after graduation. The coefficients for 2005–2009 represent the average pre-policy effect, and those for 2011–2015 represent the average post-policy effect. The 2010 cohort is omitted and normalized to zero. The baseline mean refers to the 2010 engineering cohort. All regressions are estimated using OLS and include college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table B.3: Alternative control engineering college and treated sample restrictions: Effects on the likelihood of working as an engineer

	Panel (a): Alternative control engineering college	
	(1)	(2)
	First job	Year 5 job
Years 2005 to 2009	0.006	-0.017
	(0.023)	(0.027)
Years 2011 to 2015	0.031	0.075***
	(0.022)	(0.026)
Observations	23,350	23,350
Baseline mean	0.773	0.600
	Panel (b): Alternative treated sample restriction	
	(1)	(2)
	First job	Year 5 job
Years 2005 to 2009	0.014	0.000
	(0.020)	(0.024)
Years 2011 to 2015	0.027	0.040*
	(0.019)	(0.023)
Observations	21,065	21,065
Baseline mean	0.794	0.639

Notes: This table reports average treatment effects of the major declaration policy on the likelihood of working as an engineer. Panel (a) presents estimates from a difference-in-differences event-study specification (based on Equation (2)) using an alternative control engineering college that is similar in institutional characteristics but slightly larger in size. Panel (b) uses an alternative treated sample that includes all engineering students identified in the Revelio dataset, including those not matched to enrollment records. Column (1) reports the effect on working as an engineer in the first job; column (2) reports the effect on remaining in an engineering role five years after graduation. The 2010 cohort is normalized to zero. Coefficients for years 2005–2009 represent the pre-policy average; coefficients for years 2011–2015 represent the post-policy average. The baseline mean refers to the mean of the outcome variable for the 2010 engineering cohort. All regressions are estimated using OLS and include college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table B.4: Effects of the major declaration policy on the likelihood of working as an engineer, by gender

	Panel (a): Male students	
	(1)	(2)
	First job	Year 5 job
Years 2005 to 2009	0.002	-0.022
	(0.026)	(0.031)
Years 2011 to 2015	0.038	0.074**
	(0.025)	(0.030)
Observations	14,490	14,490
Baseline mean	0.786	0.621
	Panel (b): Female students	
	(1)	(2)
	First job	Year 5 job
Years 2005 to 2009	0.010	-0.025
	(0.055)	(0.064)
Years 2011 to 2015	0.087*	0.102*
	(0.051)	(0.060)
Observations	3,655	3,655
Baseline mean	0.728	0.528

Notes: This table reports average treatment effects of the major declaration policy on the likelihood of working as an engineer, separately for male and female students. Estimates are based on Equation (2). Column (1) reports the effect on working as an engineer in the first job; Column (2) reports the effect on remaining in an engineering role five years after graduation. The 2010 cohort is normalized to zero. Coefficients for years 2005–2009 represent the pre-policy average, and coefficients for years 2011–2015 represent the post-policy average. The baseline mean refers to the mean of the outcome variable for the 2010 engineering cohort. All regressions are estimated using OLS and include college and cohort fixed effects. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

C Data appendix

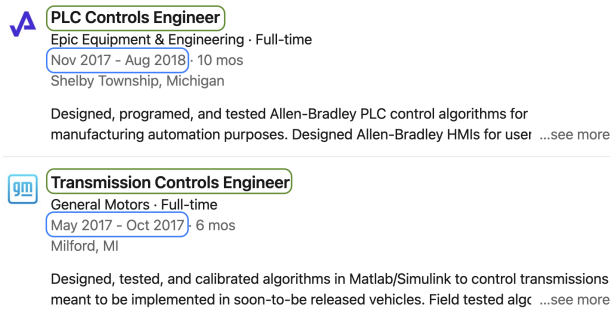
This section provides additional details on the construction of the labor market dataset. I first describe the data source and then outline the process used to match students to publicly available LinkedIn profiles.

C.1 LinkedIn data

The LinkedIn data are obtained via Revelio Labs, a data vendor commonly used in academic research. The version of the dataset I use is a one-time snapshot taken in early 2025. It contains all publicly available LinkedIn profiles at that time, offering CV-style information such as employer names, job titles, employment start and end dates, job locations, educational institutions attended, degrees obtained, and graduation years. Figure C.1 presents an example LinkedIn profile, which includes detailed employment and education histories.

Figure C.1: Sample LinkedIn profile screenshots

(a) Employment section



(b) Education section



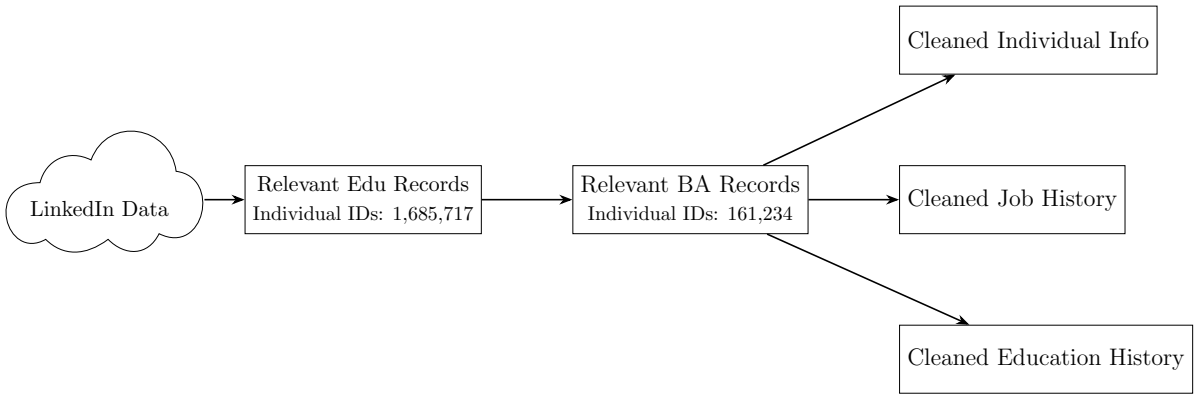
Notes: Panel (a) shows the employment section, and panel (b) shows the education section of a public LinkedIn profile (with the specific college masked for privacy). Key information such as job titles, position durations, majors, and education durations is highlighted with circles by the author.

To identify relevant students, I begin with all LinkedIn profiles listing some education from either (i) the Midwestern flagship university or (ii) the institutions hosting the control engineering colleges. This yields 1,685,717 unique profiles. I apply several filtering steps to retain relevant records:

- Restrict to profiles that report earning any *undergraduate* degree from one of the relevant institutions.
- Drop duplicate entries and profiles with inconsistencies (e.g., multiple undergraduate degrees from different institutions; dual-degree entries from the same institution are retained).
- Remove inactive or incomplete profiles (e.g., missing start or end dates for degree completion).
- Restrict to degree completion years within a 3–6 year window following college entry, for students entering college between 2005 and 2015.

Using the cleaned list of profile IDs, totaling 161,234, I then extract the full education and employment histories. The filtering process is summarized in Appendix Figure C.2.

Figure C.2: Filtering and processing LinkedIn records



Notes: This figure illustrates the filtering pipeline used to construct the final LinkedIn sample. Raw LinkedIn education data are screened for relevant education records, further cleaned to obtain the final list of user IDs, and then used to generate cleaned education histories, job histories, and individual-level information.

C.2 Matching and forming the analysis sample

I match the main sample—freshmen who enrolled in the College of Engineering or CALS at the Midwestern flagship university between Fall 2005 and Fall 2015, as recorded in administrative transcript data—to cleaned LinkedIn profiles. Of the 55,850 anonymized students, 55,657 (99.6%) have name information from the registrar available for matching.[36] The remaining students are excluded due to missing registrar-provided crosswalks. I further exclude students with duplicate names within the same cohort to avoid potential matching errors, resulting in a final sample of 55,346 students. Using first name, last name, and graduation year, I successfully match 25,319 students to their LinkedIn profiles, yielding a match rate of about 60% among engineering students in the main analysis sample.

To cleanly define treatment status by enrollment year and enrollment status for students earning degrees from the College of Engineering at the Midwestern flagship university, I include only transcript-matched students from this college in the analysis sample. I further use three different control groups across the main specification and robustness checks:

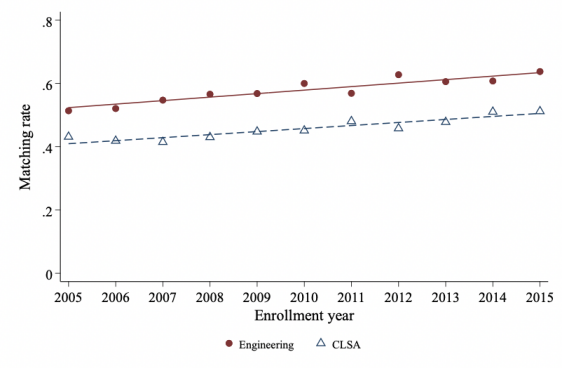
1. Transcript-matched students enrolled in CALS at the Midwestern flagship university,
2. Students graduated from Control Engineering College 1, and
3. Students graduated from Control Engineering College 2.

Match rates over time between the College of Engineering and CALS are shown in Appendix Figure C.3. The time trends are largely parallel, with no divergence around the policy change, suggesting that the matching process for the College of Engineering does not introduce differential selection relative to CALS. Appendix Figure C.3 also plots the fraction of engineering students by enrollment cohort across different student samples. Since the main specification for labor market outcomes compares engineering students only, the parallel trends across samples alleviate concerns about differential selection into the analysis sample. Appendix Table C.1 further confirms that there is no statistically significant change in selection into the analysis sample between the treated Engineering college and the various control groups following the reform.

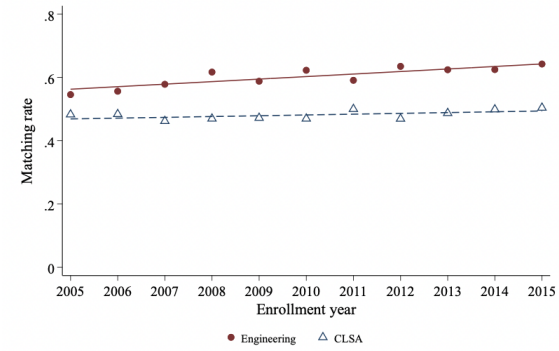
[36] This research was approved by the University's Institutional Review Board (IRB), which authorized the use of identified student data to link datasets. Access was granted through a Memorandum of Understanding (MOU), which outlined data security protocols. All identifiers were destroyed after linkage, in accordance with IRB and MOU requirements.

Selection into LinkedIn profiles Admittedly, having a public LinkedIn profile may already be a selective outcome. To assess the extent of selection, I compare students matched to LinkedIn profiles and unmatched students in the main sample at the Midwestern flagship university in Appendix Table C.2. Overall, the degree of selection into LinkedIn appears limited—for example, the SAT score difference is less than 15 points—and does not differ systematically between the College of Engineering and CALS.

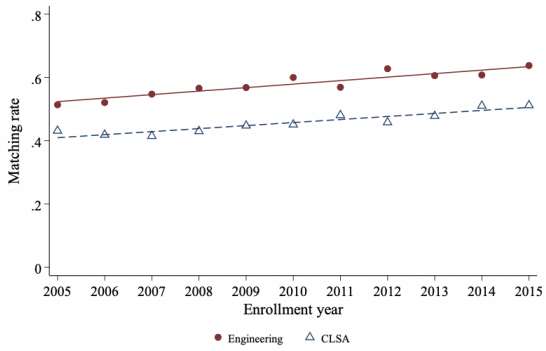
Figure C.3: Matching trends between College of Engineering and CALS



(a) Overall



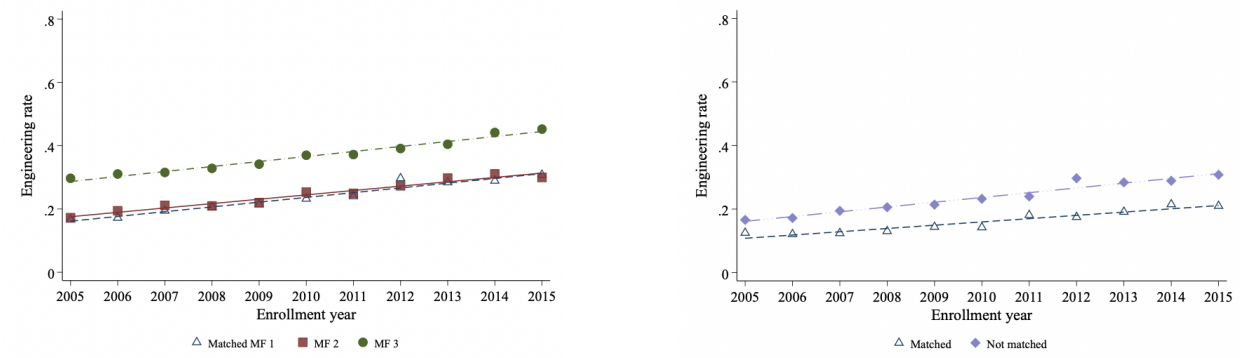
(b) Male students



(c) Female students

Notes: These figures report LinkedIn match rates by enrollment cohort. Panel (a) plots the overall trend, panel (b) shows male students, and panel (c) shows female students. The match rate by cohort is calculated as the ratio of the number of students matched between transcript records and LinkedIn profiles in a given enrollment year to the total number of students in that cohort.

Figure C.4: Fraction of engineering students across student samples



(a) Across three Midwestern flagships

(b) By matched and unmatched students

Notes: These figures report the fraction of engineering students by enrollment cohort across different student samples. Panel (a) compares trends among transcript-matched students at the Midwestern flagship used in the main analysis and among all undergraduate degree earners from two additional Midwestern flagships that host control engineering colleges (denoted MF2 and MF3). MF2 is used in the main analysis; MF3 is used as a robustness check for control engineering college selection. Panel (b) compares the fraction of engineering students among transcript-matched and unmatched students at the Midwestern flagship undergoing the policy change.

Table C.1: Selection rate comparison: Transcript-matched engineering students vs. control groups across analysis samples

	Selection on matching	**Selection on Engineering**		
	(1) vs. CALS	(2) vs. Control 1	(3) vs. Control 2	(4) vs. Unmatched
Years 2005 to 2009	-0.034	-0.004	-0.006	-0.028
	(0.020)	(0.011)	(0.011)	(0.012)
Years 2011 to 2015	-0.027	0.009	-0.000	-0.001
	(0.019)	(0.011)	(0.011)	(0.012)
Observations	52,289	108,298	105,225	52,289
Baseline mean	0.600	0.232	0.232	0.232

Notes: This table reports average pre-policy and post-policy differences in selection rates between transcript-matched engineering students and various control groups. Estimates are based on equation (1) without controls and are averaged over the 2005–2009 (pre-policy) and 2011–2015 (post-policy) periods. Column (1) compares selection into the matched sample between Engineering and CALS students at the Midwestern flagship university, where selection is measured by the fraction of students matched to LinkedIn profiles. Columns (2) and (3) compare selection into Engineering degrees at the treated university versus students at the two Midwestern flagships hosting the control engineering colleges, measured by the fraction of engineering students relative to the total number of students. Column (4) compares the fraction of engineering students between matched and unmatched samples at the Midwestern flagship undergoing the policy change. Robust standard errors are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table C.2: Comparison of characteristics between transcript–LinkedIn matched and unmatched students

	Engineering			CALS			DiD
	Matched	Unmatched	P-value	Matched	Unmatched	P-value	
Age	18.25	18.26	0.17	18.22	18.25	0.00	0.015
Female	0.23	0.26	0.00	0.54	0.57	0.00	0.005
White	0.69	0.69	0.76	0.76	0.74	0.00	-0.016*
URM	0.08	0.09	0.07	0.08	0.09	0.04	-0.004
SAT	1,400.82	1,388.87	0.00	1,361.89	1,352.45	0.00	2.519
SAT math	727.87	720.65	0.00	680.74	676.89	0.00	3.372***
Number of AP tests	4.53	4.24	0.00	3.82	3.52	0.00	-0.007
Average of AP scores	3.97	3.89	0.00	3.68	3.63	0.00	0.023
Household income $<$ \$99,000	0.28	0.31	0.00	0.26	0.28	0.00	-0.016*
Household income $\geq$ \$100,000	0.48	0.43	0.00	0.48	0.45	0.00	0.022**
Parent with bachelor(+) degree	0.84	0.83	0.17	0.84	0.82	0.00	-0.011
Observations	5,737	7,705	0.43	19,582	22,826	0.46	

Notes: This table compares students matched to LinkedIn profiles and unmatched students in the main sample at the Midwestern flagship university, based on initial enrollment in the College of Engineering or CALS. See Appendix Table A.3 for detailed variable definitions. The DiD column reports the difference-in-differences between matched and unmatched students across Engineering and CALS. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

D Theory appendix

In this section, I first present several propositions used in the proofs of the framework's predictions, and then derive the formal proofs for the predictions discussed in Section 3.2.

D.1 Derivations of Propositions

Proposition 1 *The posterior distribution of the relative match quality M_i at t_1, after receiving grades in courses from majors A and B with counts $n_{i1A}, n_{i1B}, n_{i2A}, n_{i2B}, n_{i2AH}, n_{i2BH}$, is given by:*

$$M_i \mid G_{i1} \sim \mathcal{N}(\mu_1, \theta_1),$$

$$\mu_1 = \frac{\frac{\tau_A n_{i1A}}{\eta_A^2}\bar{G}_{i1A} + \frac{\tau_B n_{i1B}}{\eta_B^2}\bar{G}_{i1B}}{\frac{1}{\sigma_i^2} + \frac{\tau_A^2 n_{i1A}}{\eta_A^2} + \frac{\tau_B^2 n_{i1B}}{\eta_B^2}}, \quad \theta_1 = \left(\frac{1}{\sigma_i^2} + \frac{\tau_A^2 n_{i1A}}{\eta_A^2} + \frac{\tau_B^2 n_{i1B}}{\eta_B^2}\right)^{-1};$$

Similarly, the posterior distribution of the relative match quality M_i at t_2 is given by:

$$M_i \mid G_{i1}, G_{i2} \sim \mathcal{N}(\mu_2, \theta_2),$$

$$\mu_2 = \frac{\frac{\tau_A n_{i1A}}{\eta_A^2}\bar{G}_{i1A} + \frac{\tau_B n_{i1B}}{\eta_B^2}\bar{G}_{i1B} + \frac{\tau_A n_{i2A}}{\eta_A^2}\bar{G}_{i2A} + \frac{\tau_A n_{i2AH}}{\eta_{AH}^2}\bar{G}_{i2AH} + \frac{\tau_B n_{i2B}}{\eta_B^2}\bar{G}_{i2B} + \frac{\tau_B n_{i2BH}}{\eta_{BH}^2}\bar{G}_{i2BH}}{\frac{1}{\sigma_i^2} + \frac{\tau_A^2 n_{i1A}}{\eta_A^2} + \frac{\tau_B^2 n_{i1B}}{\eta_B^2} + \frac{\tau_A^2 n_{i2A}}{\eta_A^2} + \frac{\tau_A^2 n_{i2AH}}{\eta_{AH}^2} + \frac{\tau_B^2 n_{i2B}}{\eta_B^2} + \frac{\tau_B^2 n_{i2BH}}{\eta_{BH}^2}},$$

$$\theta_2 = \left(\frac{1}{\sigma_i^2} + \frac{\tau_A^2 n_{i1A}}{\eta_A^2} + \frac{\tau_B^2 n_{i1B}}{\eta_B^2} + \frac{\tau_A^2 n_{i2A}}{\eta_A^2} + \frac{\tau_A^2 n_{i2AH}}{\eta_{AH}^2} + \frac{\tau_B^2 n_{i2B}}{\eta_B^2} + \frac{\tau_B^2 n_{i2BH}}{\eta_{BH}^2}\right)^{-1}.$$

Proof: First, I derive the joint distribution of M_i and G_i. Assume latent match quality

$$M_i \sim \mathcal{N}(0, \sigma_i^2), \qquad G_i = \Lambda M_i + \epsilon_i, \quad \epsilon_i \sim \mathcal{N}(0, \Sigma_\epsilon),$$

where the stacking matrix $\Lambda \in \mathbb{R}^{d\times 1}$ (with $d = n_{1A} + n_{1B} + n_{2A} + n_{2AH} + n_{2B} + n_{2BH}$) is

$$\Lambda = \begin{bmatrix} \tau_A \, \mathbf{1}_{n_{1A}} \\ \tau_B \, \mathbf{1}_{n_{1B}} \\ \tau_A \, \mathbf{1}_{n_{2A}} \\ \tau_A \, \mathbf{1}_{n_{2AH}} \\ \tau_B \, \mathbf{1}_{n_{2B}} \\ \tau_B \, \mathbf{1}_{n_{2BH}} \end{bmatrix},$$

and the (block-diagonal) noise covariance matrix is

$$\Sigma_\epsilon = \operatorname{diag}\big(\eta_A^2 \mathbf{1}_{n_{1A}},\, \eta_B^2 \mathbf{1}_{n_{1B}},\, \eta_A^2 \mathbf{1}_{n_{2A}},\, \eta_{AH}^2 \mathbf{1}_{n_{2AH}},\, \eta_B^2 \mathbf{1}_{n_{2B}},\, \eta_{BH}^2 \mathbf{1}_{n_{2BH}}\big).$$

Because G_i is an affine transformation of the Gaussian M_i plus independent Gaussian noise, the vector $\big(M_i, G_i^\top\big)^\top$ is jointly Gaussian:

$$\begin{pmatrix} M_i \\ G_i \end{pmatrix} \sim \mathcal{N}\Big(\mathbf{0},\, \begin{bmatrix} \sigma_i^2 & \sigma_i^2 \Lambda^\top \\ \sigma_i^2 \Lambda & \sigma_i^2 \Lambda\Lambda^\top + \Sigma_\epsilon \end{bmatrix}\Big),$$

since

$$\operatorname{Cov}(M_i, G_i) = \operatorname{Cov}\big(M_i, \Lambda M_i\big) = \sigma_i^2 \Lambda, \qquad \operatorname{Var}(G_i) = \Lambda\Lambda^\top \sigma_i^2 + \Sigma_\epsilon,$$

and $\operatorname{Cov}(M_i, \epsilon_i) = 0$.
Then, by the standard multivariate-normal conditioning formula,

$$M_i \,\big|\, G_{i1} \sim \mathcal{N}\big(\mu_1,\, \theta_1\big), \quad \mu_1 = \Sigma_{MG_1} \Sigma_{G_1}^{-1} G_{i1}, \quad \theta_1 = \Sigma_M - \Sigma_{MG_1} \Sigma_{G_1}^{-1} \Sigma_{G_1 M}.$$

Because M_i is one-dimensional,

$$\Sigma_M = \sigma_i^2, \qquad \Sigma_{MG_1} = \sigma_i^2 \Lambda_1^\top, \qquad \Sigma_{G_1} = \sigma_i^2 \Lambda_1 \Lambda_1^\top + \Sigma_{\epsilon 1},$$

where

$$\Lambda_1 = \begin{bmatrix} \tau_A \, \mathbf{1}_{n_{1A}} \\ \tau_B \, \mathbf{1}_{n_{1B}} \end{bmatrix}, \quad \Sigma_{\epsilon 1} = \operatorname{diag}\big(\eta_A^2 \mathbf{1}_{n_{1A}},\, \eta_B^2 \mathbf{1}_{n_{1B}}\big).$$

To invert $\Sigma_{G_1} = \sigma_i^2 \Lambda_1 \Lambda_1^\top + \Sigma_{\epsilon 1}$, I apply the *Woodbury (Sherman–Morrison–Woodbury) identity*

$$\big(A + UCV\big)^{-1} = A^{-1} - A^{-1} U \big(C^{-1} + V A^{-1} U\big)^{-1} V A^{-1}.$$

Take $A = \Sigma_{\epsilon 1}$, $U = \Lambda_1$, $C = \sigma_i^2 \equiv \frac{1}{\lambda_0}$, $V = \Lambda_1^\top$. With $D_1 = A^{-1} = \Sigma_{\epsilon 1}^{-1}$ we obtain

$$\Sigma_{G_1}^{-1} = D_1 - D_1\Lambda_1\big(\sigma_i^{-2} + \Lambda_1^\top D_1 \Lambda_1\big)^{-1}\Lambda_1^\top D_1.$$

Because $\Lambda_1 = \begin{bmatrix} \tau_A \mathbf{1}_{n_{1A}} \\ \tau_B \mathbf{1}_{n_{1B}} \end{bmatrix}$ and $D_1 = \operatorname{diag}\big(\eta_A^{-2}\mathbf{1}_{n_{1A}},\ \eta_B^{-2}\mathbf{1}_{n_{1B}}\big)$,

$$\Lambda_1^\top D_1 \Lambda_1 = \tau_A^2 \frac{n_{1A}}{\eta_A^2} + \tau_B^2 \frac{n_{1B}}{\eta_B^2} \equiv \lambda_1.$$

$$\Sigma_{G_1}^{-1} = D_1 - \frac{1}{\lambda_0 + \lambda_1} D_1 \Lambda_1 \Lambda_1^\top D_1.$$

$$\Lambda_1^\top \Sigma_{G_1}^{-1} \Lambda_1 = \underbrace{\Lambda_1^\top D_1 \Lambda_1}_{=\lambda_1} - \frac{1}{\lambda_0 + \lambda_1} \underbrace{\Lambda_1^\top D_1 \Lambda_1}_{=\lambda_1} \underbrace{\Lambda_1^\top D_1 \Lambda_1}_{=\lambda_1} = \lambda_1 - \frac{\lambda_1^2}{\lambda_0 + \lambda_1} = \frac{\lambda_0\,\lambda_1}{\lambda_0 + \lambda_1}$$

$$\theta_1 = \sigma_i^2 - \sigma_i^2 \Lambda_1^\top \Sigma_{G_1}^{-1} \Lambda_1 \sigma_i^2 = \sigma_i^2 - \sigma_i^2 \left(\frac{\lambda_0 \lambda_1}{\lambda_0 + \lambda_1}\right) \sigma_i^2 = \sigma_i^2 \left(\frac{\lambda_0}{\lambda_0 + \lambda_1}\right) = \frac{1}{\lambda_0 + \lambda_1}.$$

$$\begin{aligned}
\mu_1 &= \sigma_i^2 \Lambda_1^\top \Sigma_{G_1}^{-1} G_{i1} \\
&= \sigma_i^2 \Lambda_1^\top \Big(D_1 - \frac{1}{\lambda_0 + \lambda_1} D_1 \Lambda_1 \Lambda_1^\top D_1\Big) G_{i1} \\
&= \sigma_i^2 \Big[\underbrace{\Lambda_1^\top D_1 G_{i1}}_{S} - \frac{1}{\lambda_0 + \lambda_1} \underbrace{\Lambda_1^\top D_1 \Lambda_1}_{=\lambda_1} \underbrace{\Lambda_1^\top D_1 G_{i1}}_{S}\Big] \\
&= \sigma_i^2 S \Big(1 - \frac{\lambda_1}{\lambda_0 + \lambda_1}\Big) \\
&= \sigma_i^2 S \frac{\lambda_0}{\lambda_0 + \lambda_1} \\
&= \frac{1}{\lambda_0 + \lambda_1} S.
\end{aligned}$$

Define the sample-mean grades

$$\bar{G}_{1A} := \frac{1}{n_{1A}} \sum_{j=1}^{n_{1A}} G_{i1A}^{(j)}, \qquad \bar{G}_{1B} := \frac{1}{n_{1B}} \sum_{j=1}^{n_{1B}} G_{i1B}^{(j)}.$$

Then

$$S = \Lambda_1^\top D_1 G_{i1} = \tau_A \eta_A^{-2}\, n_{1A}\, \bar{G}_{1A} + \tau_B \eta_B^{-2}\, n_{1B}\, \bar{G}_{1B}.$$

Hence

$$M_i \,\big|\, G_{i1} \;\sim\; \mathcal{N}\big(\,\mu_1,\,\theta_1\big),$$

with

$$\mu_1 = \frac{\tau_A \,\dfrac{n_{1A}}{\eta_A^2}\,\bar{G}_{1A} \;+\; \tau_B \,\dfrac{n_{1B}}{\eta_B^2}\,\bar{G}_{1B}}{\sigma_i^{-2} \;+\; \tau_A^2 \,\dfrac{n_{1A}}{\eta_A^2} \;+\; \tau_B^2 \,\dfrac{n_{1B}}{\eta_B^2}}, \qquad \theta_1 = \Big(\sigma_i^{-2} + \tau_A^2 \,\frac{n_{1A}}{\eta_A^2} + \tau_B^2 \,\frac{n_{1B}}{\eta_B^2}\Big)^{-1}.$$

Following the same method, I derive

$$M_i \mid G_{i1}, G_{i2} \;\sim\; \mathcal{N}\big(\,\mu_2,\,\theta_2\big), \qquad \mu_2 = \sigma_i^2\,\Lambda^\top \Sigma_G^{-1} G_i, \qquad \theta_2 = \sigma_i^2 - \sigma_i^2\,\Lambda^\top \Sigma_G^{-1} \Lambda.$$

$$\Sigma_G = \sigma_i^2 \Lambda\Lambda^\top + \Sigma_\epsilon, \qquad D = \Sigma_\epsilon^{-1}, \qquad \lambda_0 = \sigma_i^{-2}.$$

Applying the Woodbury identity,

$$\Sigma_G^{-1} = D - \frac{1}{\lambda_0 + \lambda_1 + \lambda_2}\, D\Lambda\Lambda^\top D,$$

where

$$\lambda_1 = \tau_A^2 \frac{n_{1A}}{\eta_A^2} + \tau_B^2 \frac{n_{1B}}{\eta_B^2}, \qquad \lambda_2 = \tau_A^2\Big(\frac{n_{2A}}{\eta_A^2} + \frac{n_{2AH}}{\eta_{AH}^2}\Big) + \tau_B^2\Big(\frac{n_{2B}}{\eta_B^2} + \frac{n_{2BH}}{\eta_{BH}^2}\Big).$$

Because

$$\Lambda^\top D \Lambda = \lambda_1 + \lambda_2,$$

we have

$$\theta_2 = \big(\lambda_0 + \lambda_1 + \lambda_2\big)^{-1}.$$

Setting $S = \Lambda^\top D G_i$ and repeating the algebra used for μ_1 gives

$$\mu_2 = \frac{1}{\lambda_0 + \lambda_1 + \lambda_2}\, S.$$

Define the block-specific sample means

$$\bar{G}_{1A} = \frac{1}{n_{1A}} \sum_{j=1}^{n_{1A}} G_{i1A}^{(j)}, \quad \bar{G}_{1B} = \frac{1}{n_{1B}} \sum_{j=1}^{n_{1B}} G_{i1B}^{(j)}, \quad \bar{G}_{2A} = \frac{1}{n_{2A}} \sum_{j=1}^{n_{2A}} G_{i2A}^{(j)},$$

$$\bar{G}_{2AH} = \frac{1}{n_{2AH}} \sum_{j=1}^{n_{2AH}} G_{i2AH}^{(j)}, \quad \bar{G}_{2B} = \frac{1}{n_{2B}} \sum_{j=1}^{n_{2B}} G_{i2B}^{(j)}, \quad \bar{G}_{2BH} = \frac{1}{n_{2BH}} \sum_{j=1}^{n_{2BH}} G_{i2BH}^{(j)}.$$

Then

$$S = \tau_A \eta_A^{-2}\, n_{1A}\, \bar{G}_{1A} + \tau_B \eta_B^{-2}\, n_{1B}\, \bar{G}_{1B} + \tau_A \eta_A^{-2}\, n_{2A}\, \bar{G}_{2A} + \tau_A \eta_{AH}^{-2}\, n_{2AH}\, \bar{G}_{2AH} + \tau_B \eta_B^{-2}\, n_{2B}\, \bar{G}_{2B} + \tau_B \eta_{BH}^{-2}\, n_{2BH}\, \bar{G}_{2BH}.$$

Hence

$$M_i \mid G_{i1}, G_{i2} \sim \mathcal{N}\big(\mu_2,\ \theta_2\big), \qquad \theta_2 = \big(\sigma_i^{-2} + \lambda_1 + \lambda_2\big)^{-1},$$

$$\mu_2 = \frac{\tau_A \frac{n_{1A}}{\eta_A^2}\, \bar{G}_{1A} + \tau_B \frac{n_{1B}}{\eta_B^2}\, \bar{G}_{1B} + \tau_A \frac{n_{2A}}{\eta_A^2}\, \bar{G}_{2A} + \tau_A \frac{n_{2AH}}{\eta_{AH}^2}\, \bar{G}_{2AH} + \tau_B \frac{n_{2B}}{\eta_B^2}\, \bar{G}_{2B} + \tau_B \frac{n_{2BH}}{\eta_{BH}^2}\, \bar{G}_{2BH}}{\sigma_i^{-2} + \tau_A^2\Big(\frac{n_{1A}}{\eta_A^2} + \frac{n_{2A}}{\eta_A^2} + \frac{n_{2AH}}{\eta_{AH}^2}\Big) + \tau_B^2\Big(\frac{n_{1B}}{\eta_B^2} + \frac{n_{2B}}{\eta_B^2} + \frac{n_{2BH}}{\eta_{BH}^2}\Big)}.$$

□

Proposition 2 *Let the posterior beliefs about relative match quality at t_1 and t_2 be denoted as follows:*

$$E_1 = \mathbb{E}[M_i \mid G_{i1A}, G_{i1B}], \qquad E_2 = \mathbb{E}[M_i \mid G_{i1A}, G_{i1B}, G_{i2A}, G_{i2AH}, G_{i2B}, G_{i2BH}].$$

Define

$$\lambda_0 := \sigma_i^{-2}, \quad \lambda_1 := \tau_A^2 \frac{n_{1A}}{\eta_A^2} + \tau_B^2 \frac{n_{1B}}{\eta_B^2}, \quad \lambda_2 := \tau_A^2\Big(\tfrac{n_{2A}}{\eta_A^2} + \tfrac{n_{2AH}}{\eta_{AH}^2}\Big) + \tau_B^2\Big(\tfrac{n_{2B}}{\eta_B^2} + \tfrac{n_{2BH}}{\eta_{BH}^2}\Big),$$

and set

$$\theta_1 := (\lambda_0 + \lambda_1)^{-1}, \qquad \theta_2 := (\lambda_0 + \lambda_1 + \lambda_2)^{-1}.$$

Then the distribution of posterior beliefs is written as:

$$(E_1, E_2)^\top \sim \mathcal{N}\Big(\mathbf{0},\ \begin{bmatrix} \sigma_i^2 - \theta_1 & \sigma_i^2 - \theta_1 \\ \sigma_i^2 - \theta_1 & \sigma_i^2 - \theta_2 \end{bmatrix}\Big).$$

Proof: The vector $(M_i, G_i^\top)^\top$ is multivariate normal. Both E_1 and E_2 are linear projections of M_i onto the information contained in G_{i1} and (G_{i1}, G_{i2}), respectively; hence (E_1, E_2) is bivariate normal. Because each grade has mean 0, these projections are mean 0.
For any random variable X,

$$Var(\mathbb{E}[M_i \mid X]) = Var(M_i) - \mathbb{E}[Var(M_i \mid X)].$$

With $Var(M_i) = \sigma_i^2$ and posterior variances θ_1, θ_2 obtained earlier,

$$Var(E_1) = \sigma_i^2 - \theta_1, \qquad Var(E_2) = \sigma_i^2 - \theta_2.$$

Because the period 2 grade vector $G_{i2} := (G_{i2A}, G_{i2AH}, G_{i2B}, G_{i2BH})$ augments the period 1 vector $G_{i1} := (G_{i1A}, G_{i1B})$, we have

$$E_1 = \mathbb{E}[M_i \mid G_{i1}], \qquad E_2 = \mathbb{E}[M_i \mid G_{i1}, G_{i2}] = \mathbb{E}[E_1 \mid G_{i1}, G_{i2}].$$

The covariance is derived as:

$$\begin{aligned}
\mathrm{Cov}(E_1, E_2) &= \mathbb{E}[E_1 E_2] \\
&= \mathbb{E}\big[E_1 \mathbb{E}[M_i \mid G_{i1}, G_{i2}]\big] \\
&= \mathbb{E}\big[\mathbb{E}[E_1 M_i \mid G_{i1}, G_{i2}]\big] \quad \text{(since } E_1 \text{ is } G_{i1}\text{-measurable)} \\
&= \mathbb{E}[E_1 M_i] \\
&= \mathbb{E}[\mathbb{E}[M_i \mid G_{i1}] M_i] \\
&= \mathbb{E}[E_1^2] \quad \text{(by projection properties)} \\
&= Var(E_1) \\
&= \sigma_i^2 - \theta_1.
\end{aligned}$$

This covariance, together with the variances above, yields the stated covariance matrix. □

Proposition 3 (Major switching probability) *Let*

$$\lambda_0 = \frac{1}{\sigma_i^2}, \qquad \lambda_1 = \frac{\tau_A^2 n_{1A}}{\eta_A^2} + \frac{\tau_B^2 n_{1B}}{\eta_B^2}, \qquad \lambda_2 = \tau_A^2 \left(\frac{n_{2A}}{\eta_A^2} + \frac{n_{2AH}}{\eta_{AH}^2} \right) + \tau_B^2 \left(\frac{n_{2B}}{\eta_B^2} + \frac{n_{2BH}}{\eta_{BH}^2} \right),$$

and define the posterior beliefs at t_1 and t_2 as

$$E_1 = \mathbb{E}[M_i \mid G_{1A}, G_{1B}], \qquad E_2 = \mathbb{E}[M_i \mid G_{1A}, G_{1B}, G_{2A}, G_{2AH}, G_{2B}, G_{2BH}].$$

(i) Switching* within *the umbrella major.

When both majors fall under the same umbrella (e.g., within engineering), switching is unconstrained by graduation credit requirements. The probability of switching is given by:

$$P_{switch,\, within} = \frac{1}{2} - \frac{1}{\pi} \arcsin\left(\sqrt{\frac{\lambda_1(\lambda_0 + \lambda_1 + \lambda_2)}{(\lambda_0 + \lambda_1)(\lambda_1 + \lambda_2)}} \right).$$

(ii) Switching* outside *the umbrella major.

When the undeclared major $X \in \{A, B\}$ lies outside the umbrella field, the student must accumulate sufficient credits from both X-specific and Z-type courses. Because first-period courses are drawn jointly from a pool of size $\overline{N}_1$, define:

$$n_{1XZ} := n_{1X} + n_{1Z} \sim \text{Binomial}\big(\overline{N_1},\ q_{XZ1}\big), \qquad q_{XZ1} := p_{X1} + p_{Z1}.$$

Similarly, second-period courses are drawn from a pool of size $\overline{N}_2$, where:

$$n_{2XZ} := n_{2X} + n_{2Z} \sim \text{Binomial}\big(\overline{N_2},\ q_{XZ2}\big), \qquad q_{XZ2} := p_{X2} + p_{Z2}.$$

Let the total number of credits toward major X be:

$$S_X = n_{1XZ} + n_{2XZ}.$$

Switching to major X is feasible only if $S_X \geq Q$, where Q is the graduation credit requirements. Thus, the probability of satisfying the credit constraint is:

$$P_{credits,X} = 1 - \sum_{i=0}^{Q-1} \sum_{a=max(0,i-\overline{N_2})}^{\min(i,\overline{N_1})} \binom{\overline{N_1}}{a} q_{XZ1}^{a} (1 - q_{XZ1})^{\overline{N_1}-a} \binom{\overline{N_2}}{i-a} q_{XZ2}^{i-a} (1 - q_{XZ2})^{\overline{N_2}-i+a}.$$

The overall switching probability when the target major is outside the umbrella field is:

$$P_{switch,\ outside} = P_{switch,\ within} \times P_{credits,X}.$$

Proof: From Proposition 2,

$$Var(E_1) = \sigma_i^2 - \frac{1}{\lambda_0 + \lambda_1}, \qquad Var(E_2) = \sigma_i^2 - \frac{1}{\lambda_0 + \lambda_1 + \lambda_2}, \qquad Cov(E_1, E_2) = Var(E_2).$$

Hence

$$\rho \ = \ \frac{Cov(E_1, E_2)}{\sqrt{Var(E_1)\,Var(E_2)}} = \frac{Var(E_1)}{\sqrt{Var(E_1)\,Var(E_2)}} = \sqrt{\frac{Var(E_1)}{Var(E_2)}} = \sqrt{\frac{\lambda_1(\lambda_0 + \lambda_1 + \lambda_2)}{(\lambda_1 + \lambda_2)(\lambda_0 + \lambda_1)}}.$$

For a centered bivariate normal pair (E_1, E_2) with correlation ρ, the probability that E_1 and E_2 lie

on opposite sides of zero is given by:

$$P(E_1 \geq 0, E_2 < 0) = \frac{1}{4} - \frac{1}{2\pi} \arcsin \rho.$$

By symmetry, the probability $P(E_1 < 0, E_2 \geq 0)$ is identical. Thus, the total probability of a "switch" between periods is:

$$P_{\text{switch, within}} = P(E_1 < 0, E_2 \geq 0) + P(E_1 \geq 0, E_2 < 0) = 2\left(\frac{1}{4} - \frac{1}{2\pi} \arcsin \rho\right).$$

To switch *outside* the umbrella major the student must both reverse the sign of her posterior mean and accumulate enough credits. Because the sign change event and the course–count event are independent, their probabilities multiply, yielding $P_{\text{switch, outside}} = P_{\text{switch, within}} \times P_{\text{credits},X}$. □

Proposition 4 (Probability of a mismatch) *Let*

$$\lambda_0 = \frac{1}{\sigma_i^2}, \qquad \lambda_1 = \frac{\tau_A^2 n_{1A}}{\eta_A^2} + \frac{\tau_B^2 n_{1B}}{\eta_B^2}, \qquad \lambda_2 = \tau_A^2\left(\tfrac{n_{2A}}{\eta_A^2} + \tfrac{n_{2AH}}{\eta_{AH}^2}\right) + \tau_B^2\left(\tfrac{n_{2B}}{\eta_B^2} + \tfrac{n_{2BH}}{\eta_{BH}^2}\right),$$

and let

$$E_2 = \mathbb{E}\big[M_i \mid G_{1A}, G_{1B}, G_{2A}, G_{2AH}, G_{2B}, G_{2BH}\big]$$

denote the final posterior belief of relative match quality at graduation. Then the probability that a mismatch occurs—that is, the posterior belief and the true match quality M_i disagree in sign—is:

$$P(\textit{Mismatch}) = P(E_2 \geq 0,\ M_i < 0) + P(E_2 < 0,\ M_i \geq 0) = \frac{1}{2} - \frac{1}{\pi} \arcsin\left(\sqrt{\frac{\lambda_1 + \lambda_2}{\lambda_0 + \lambda_1 + \lambda_2}}\right).$$

Proof: Because $\left(M_i, G_1^\top, G_2^\top\right)^\top$ is multivariate normal, the pair $\left(E_2, M_i\right)$ is centered bivariate normal with

$$\operatorname{Var}(M_i) = \frac{1}{\lambda_0}, \qquad \operatorname{Var}(E_2) = \sigma_i^2 - \frac{1}{\lambda_0 + \lambda_1 + \lambda_2} = \frac{\lambda_1 + \lambda_2}{\lambda_0(\lambda_0 + \lambda_1 + \lambda_2)},$$

and $\operatorname{Cov}(E_2, M_i) = \operatorname{Var}(E_2)$. Hence

$$\rho = \frac{\operatorname{Cov}(E_2, M_i)}{\sqrt{\operatorname{Var}(E_2)\ \operatorname{Var}(M_i)}} = \sqrt{\frac{\lambda_1 + \lambda_2}{\lambda_0 + \lambda_1 + \lambda_2}}.$$

For any zero-mean bivariate normal (X, Y) with correlation ρ, $P(X \geq 0, Y < 0) = \frac{1}{4} - \frac{1}{2\pi} \arcsin \rho$; symmetry implies $P(X < 0, Y \geq 0) = P(X \geq 0, Y < 0)$. Substituting $(X, Y) = (E_2, M_i)$ and summing the two disjoint events yields the stated mismatch probability. □

D.2 Derivations of Prediction 1

Prediction 1 (Within-Engineering switching) *When both majors A and B fall within Engineering, a policy change that reallocates coursework from period $t_0 - t_1$ to period $t_1 - t_2$ increases the probability of switching between majors.*

Proof: Recall Proposition 3

$$P_{\text{switch, within}} = \frac{1}{2} - \frac{1}{\pi} \arcsin\left(\sqrt{\frac{\lambda_1 (\lambda_0 + \lambda_1 + \lambda_2)}{(\lambda_0 + \lambda_1)(\lambda_1 + \lambda_2)}} \right), \qquad \rho = \sqrt{\frac{\lambda_1 (\lambda_0 + \lambda_1 + \lambda_2)}{(\lambda_0 + \lambda_1)(\lambda_1 + \lambda_2)}}.$$

Differentiating ρ with respect to λ_1 and λ_2 yields

$$\frac{\partial \rho}{\partial \lambda_1} = \frac{\lambda_0 \lambda_2 (\lambda_0 + 2\lambda_1 + \lambda_2)}{2\rho (\lambda_0 + \lambda_1)^2 (\lambda_1 + \lambda_2)^2} > 0, \qquad \frac{\partial \rho}{\partial \lambda_2} = -\frac{\lambda_0 \lambda_1}{2\rho (\lambda_0 + \lambda_1)(\lambda_1 + \lambda_2)^2} < 0.$$

Because

$$\frac{dP_{\text{switch, within}}}{d\rho} = -\frac{1}{\pi \sqrt{1 - \rho^2}} < 0,$$

we obtain

$$\frac{\partial P}{\partial \lambda_1} < 0, \qquad \frac{\partial P}{\partial \lambda_2} > 0.$$

Given

$$\lambda_1 = \tau_A^2 \frac{n_{1A}}{\eta_A^2} + \tau_B^2 \frac{n_{1B}}{\eta_B^2}, \qquad \lambda_2 = \tau_A^2 \Big(\frac{n_{2A}}{\eta_A^2} + \frac{n_{2AH}}{\eta_{AH}^2} \Big) + \tau_B^2 \Big(\frac{n_{2B}}{\eta_B^2} + \frac{n_{2BH}}{\eta_{BH}^2} \Big),$$

reducing any first-period course count (n_{1A}, n_{1B}) lowers λ_1, and since $\partial P / \partial \lambda_1 < 0$, P increases. Note that period-1 mismatch similarly decreases as

$$\phi_1 := \frac{\lambda_1}{\lambda_0 + \lambda_1}$$

increases, and a smaller ϕ_1 means less information and thus a higher probability that the interim major choice mismatches true ability. Therefore, dropping any first-period course lowers n_{1A} or n_{1B}, reduces λ_1, and therefore raises period-1 mismatch.

Increasing any second-period count $(n_{2A}, n_{2B}, n_{2AH}, n_{2BH})$ raises λ_2, and because $\partial P / \partial \lambda_2 > 0$, P again increases. High-level courses have larger marginal impact because $\eta_{AH}^2 < \eta_A^2$ and $\eta_{BH}^2 < \eta_B^2$.

Hence a policy that shifts coursework from t_1 to t_2 unambiguously raises the switching probability, establishing Prediction 1. □

D.3 Derivations of Prediction 2

Prediction 2 (Match quality among engineering graduates) *When both majors A and B fall within Engineering, a policy change that reallocates coursework from period $t_0 - t_1$ to period $t_1 - t_2$ reduces the probability of mismatch at graduation.*

Proof. Proposition 4 gives

$$P(\text{Mismatch}) = \frac{1}{2} - \frac{1}{\pi}\arcsin\Big(\sqrt{\tfrac{\lambda_1+\lambda_2}{\lambda_0+\lambda_1+\lambda_2}}\Big).$$

The policy change moves n first-period courses to the second period. Let p_{A1}, p_{B1} be the probabilities that a first-period course is in A or B; let $p_{A2}, p_{B2}, p_{AH2}, p_{BH2}$ be the corresponding second-period probabilities (regular and high-level). The information parameters become

$$\lambda_1' = \lambda_1 - n\Big(\tau_A^2 \tfrac{p_{A1}}{\eta_A^2} + \tau_B^2 \tfrac{p_{B1}}{\eta_B^2}\Big), \qquad \lambda_2' = \lambda_2 + n\Big(\tau_A^2\Big(\tfrac{p_{A2}}{\eta_A^2} + \tfrac{p_{AH2}}{\eta_{AH}^2}\Big) + \tau_B^2\Big(\tfrac{p_{B2}}{\eta_B^2} + \tfrac{p_{BH2}}{\eta_{BH}^2}\Big)\Big).$$

Let $S = \lambda_1 + \lambda_2$. After the policy change,

$$\begin{aligned} S' &= \lambda_1' + \lambda_2' \\ &= S + n\Big[\tau_A^2\Big(\tfrac{p_{A2}}{\eta_A^2} + \tfrac{p_{AH2}}{\eta_{AH}^2} - \tfrac{p_{A1}}{\eta_A^2}\Big) + \tau_B^2\Big(\tfrac{p_{B2}}{\eta_B^2} + \tfrac{p_{BH2}}{\eta_{BH}^2} - \tfrac{p_{B1}}{\eta_B^2}\Big)\Big] = S + \Delta. \end{aligned}$$

Because the following condition holds as well as $\eta_{AH} < \eta_A, \eta_{BH} < \eta_B$:

- If A is declared at t_1, then $p_{AH2} > 0$, so $p_{A2} + p_{AH2} > p_{A1}$; $p_{BH2} = 0$ with $p_{B2} = p_{B1}$, and $p_{Z2} < p_{Z1}$;
- If B is declared at t_1, then $p_{BH2} > 0$, so $p_{B2} + p_{BH2} > p_{B1}$; $p_{AH2} = 0$ with $p_{A2} = p_{A1}$, and $p_{Z2} < p_{Z1}$;

We have

$$\Delta > n\Big[\tau_A^2\Big(\tfrac{p_{A2}}{\eta_A^2} + \tfrac{p_{AH2}}{\eta_A^2} - \tfrac{p_{A1}}{\eta_A^2}\Big) + \tau_B^2\Big(\tfrac{p_{B2}}{\eta_B^2} + \tfrac{p_{BH2}}{\eta_B^2} - \tfrac{p_{B1}}{\eta_B^2}\Big)\Big] > 0.$$

Hence $S' = S + \Delta > S$. Let $T = \lambda_0 + \lambda_1 + \lambda_2$; then $T' = T + \Delta$. Since

$$\frac{S+\Delta}{T+\Delta} > \frac{S}{T},$$

and $\arcsin(\sqrt{\cdot})$ is increasing on $(0,1)$,

$$P' = \frac{1}{2} - \frac{1}{\pi}\arcsin\Big(\sqrt{\tfrac{S+\Delta}{T+\Delta}}\Big) < \frac{1}{2} - \frac{1}{\pi}\arcsin\Big(\sqrt{\tfrac{S}{T}}\Big) = P.$$

Thus moving n courses from $t=1$ to $t=2$ decreases the mismatch probability P. □

D.4 Derivations of Prediction 3

Prediction 3 (Exits from Engineering) *A policy change that reallocates coursework from period $t_0 - t_1$ to period $t_1 - t_2$ reduces exits from Engineering when the tightened exiting constraint outweighs the learning gain.*

Proof. From Proposition 3, the probability of exiting to a non-engineering field $X \in \{A, B\}$ is given by:

$$P_{\text{exit}} = P_{\text{switch, within}} \times P_{\text{credits},X}.$$

Prediction 1 has already established that $P_{\text{switch, within}}$ increases following the policy change.
We now analyze how the policy affects the credit constraint component $P_{\text{credits},X}$. In each period, students draw courses from a single pool. Let:

$$n_{1XZ} := n_{1X} + n_{1Z} \sim \text{Binomial}(\overline{N}_1,\, q_{XZ1}), \qquad q_{XZ1} := p_{X1} + p_{Z1},$$

$$n_{2XZ} := n_{2X} + n_{2Z} \sim \text{Binomial}(\overline{N}_2,\, q_{XZ2}), \qquad q_{XZ2} := p_{X2} + p_{Z2}.$$

Let $S_X = n_{1XZ} + n_{2XZ}$ denote the total number of credits accumulated toward major X. The expected number of such credits is:

$$\mathbb{E}[S_X] = \overline{N}_1 q_{XZ1} + \overline{N}_2 q_{XZ2}.$$

The policy change shifts n courses from period 1 to period 2, holding $\overline{N}_1 + \overline{N}_2$ fixed. That is:

$$\overline{N}_1^{\text{new}} = \overline{N}_1 - n, \qquad \overline{N}_2^{\text{new}} = \overline{N}_2 + n.$$

Then the change in the expected credit total is:

$$\Delta_{\text{mean}} := \mathbb{E}[S_X]^{\text{new}} - \mathbb{E}[S_X]^{\text{old}} = -n(q_{XZ1} - q_{XZ2}).$$

Since exiting requires that $S_X \geq Q$, and the binomial sum S_X stochastically decreases in its mean, the probability of satisfying the credit requirement also decreases:

$$\Delta_{\text{credit}} := P^{\text{new}}_{\text{credits},X} - P^{\text{old}}_{\text{credits},X} < 0, \quad \text{if } q_{XZ1} > q_{XZ2}.$$

Therefore, when the decline in credit feasibility dominates the increase in information (reflected in $P_{\text{switch, within}}$), the overall probability of exiting engineering decreases after the policy—establishing Prediction 3. □

D.5 Derivations of Prediction 4

Prediction 4 (Overall match quality) *The overall effect on match quality of a policy change that reallocates coursework from period t_0–t_1 to period t_1–t_2 depends on the relative strength of the learning gain versus the increased barriers to exiting Engineering.*

Proof. Define the zero-mean correlations used in posterior mismatch calculations:

$$\rho_{ME_1} = \sqrt{\frac{\lambda_1}{\lambda_0 + \lambda_1}}, \qquad \rho_{ME_2} = \sqrt{\frac{\lambda_1 + \lambda_2}{\lambda_0 + \lambda_1 + \lambda_2}}, \qquad \rho_{E_1E_2} = \sqrt{\frac{(\lambda_1 + \lambda_2)(\lambda_0 + \lambda_1)}{\lambda_1(\lambda_0 + \lambda_1 + \lambda_2)}}.$$

Let $p_{\text{able}} := \Pr(S_X \geq Q)$ denote the probability that a student satisfies the credit requirement to switch to the undeclared major $X \in \{A, B\}$, where

$$S_X = n_{1X} + n_{2X} + n_{1Z} + n_{2Z}$$

represents the total number of accumulated credits that count toward graduation in major X.
If the credit threshold is met (able = 1), the student chooses their final major according to the sign of E_2, yielding a mismatch probability of:

$$P_{\text{mis|able}} = \frac{1}{2} - \frac{1}{\pi} \arcsin(\rho_{ME_2}).$$

If the threshold is not met (able = 0), the student cannot switch and is constrained to keep their initial major (which depends on the sign of E_1). The mismatch probability in this case is:

$$P_{\text{mis|}\neg\text{able}} = \frac{1}{2} - \frac{1}{\pi} \arcsin(\rho_{ME_1}).$$

Averaging over the credit feasibility states yields the total mismatch probability when A and B lie outside a common umbrella:

$$P(\text{Mismatch}) = p_{\text{able}} \left(\frac{1}{2} - \frac{1}{\pi} \arcsin(\rho_{ME_2}) \right) + (1 - p_{\text{able}}) \left(\frac{1}{2} - \frac{1}{\pi} \arcsin(\rho_{ME_1}) \right).$$

This expression can be simplified to highlight the two competing effects:

$$P(\text{Mismatch}) = \frac{1}{2} - \frac{1}{\pi} \left(p_{\text{able}} \arcsin(\rho_{ME_2}) + (1 - p_{\text{able}}) \arcsin(\rho_{ME_1}) \right).$$

Now consider how the policy change affects each component:

- Prediction 3 implies that p_{able} decreases (as less likely to meet the credit threshold post-policy).
- Prediction 2 implies that ρ_{ME_1} decreases (due to reduced first-period information), while ρ_{ME_2} increases (due to enhanced second-period information).

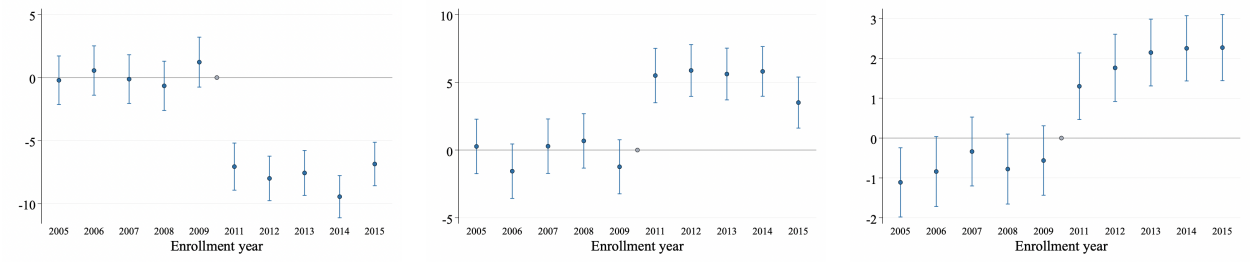
In the mismatch expression above, these changes operate in opposite directions:

- The weight on $\arcsin(\rho_{ME_2})$ declines, but its value increases;
- The weight on $\arcsin(\rho_{ME_1})$ increases, but its value decreases.

Thus, the overall effect on mismatch is ambiguous. If p_{able} is initially high, the decline in its weight on ρ_{ME_2} dominates, causing mismatch to increase. If p_{able} is low, the increased weight on ρ_{ME_1} dominates, potentially reducing mismatch. Ultimately, the direction of change depends on the magnitude of p_{able}, which itself is shaped by parameters like Q_X, p_{Z1}, and p_{Z2} that are not directly altered by the policy. □

D.6 Empirical support for key assumptions in the framework

Figure D.1: Empirical evidence supporting key framework assumptions



(a) Credits before declaration (b) Credits after declaration (c) 300+ major-specific credits

Notes: This figure plots the estimated year-by-year effects of the early major declaration policy on students' coursetaking behavior, based on equation (1). Panel (a) shows the average number of credits accumulated prior to major declaration across cohorts. Panel (b) shows the average number accumulated after declaration. Panel (c) shows the total number of high-level (300-level and above) credits taken in the graduation major. Year 2010 is normalized to zero and serves as the omitted (reference) year. All regressions are estimated using OLS and include student-level controls (SAT composite and math scores, a female indicator, race indicators, and a missing-demographics indicator), as well as college and cohort fixed effects. Robust standard errors are used to construct 95% confidence intervals.