Does Teaching Entrepreneurship Produce Entrepreneurs?*

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I examine whether undergraduate entrepreneurship education increases business formation by exploiting the staggered introduction of entrepreneurship programs across more than 150 U.S. universities. I construct a novel hand-collected dataset combining historical university catalogs with LinkedIn career histories for over 300,000 graduates and implement a within-university, adjacent-cohort difference-in-differences design. Exposure to entrepreneurship education reduces immediate business formation by 10.7% compared to the prior cohort, driven entirely by fewer small businesses, with no effect on growth-oriented ventures, long-run entrepreneurial entry, or longer-run business continuation. The decline is stronger at public universities and among earlier program adopters, consistent with a screening mechanism. The findings suggest entrepreneurship education has limited effectiveness as a policy tool for stimulating new business creation.

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1. Introduction

"Most of what you hear about entrepreneurship is all wrong. It's not magic; it's not mysterious; and it has nothing to do with genes. It's a discipline and, like any discipline, it can be learned."

– Peter Drucker
Father of Modern Management

"The paradox of teaching entrepreneurship is that such a formula necessarily cannot exist; because every innovation is new and unique, no authority can prescribe in concrete terms how to be innovative."

– Peter Thiel
Co-Founder of Paypal

[1] [2]

The creation of new businesses, particularly innovative ventures, is seen as one of the key drivers of economic growth (Schumpeter (1942), King and Levine (1993), Decker et al. (2014)). Therefore, scholars and policymakers invest considerable effort in exploring ways to foster entrepreneurship within the economy. Whether teaching entrepreneurship produces entrepreneurs remains an open question with important implications for education policy, business dynamism, and long-run macroeconomic outcomes. In this paper, I study how formal undergraduate entrepreneurship education affects business formation and entrepreneurial performance, and how these effects vary with local economic conditions and institutional characteristics.

In the United States alone, more than 300 colleges now offer bachelor's degrees in entrepreneurship.[3] These programs are designed to teach students to "act as an entrepreneur within companies, startups, or self-employment", "prepare them to launch new ventures," "not only teach them about entrepreneurship but make them entrepreneurs," and "provide the business training needed to launch and grow a successful enterprise."[4] Despite their growing prevalence and stated objectives, there is limited causal evidence on whether these degree programs increase entrepreneurial activity or improve entrepreneurial outcomes.

To address this question, I exploit the staggered introduction of undergraduate en-

[1] Drucker, P. F. (1985). Innovation and entrepreneurship: Practice and principles. Harper & Row.
[2] Thiel, P. (2014). Zero to one: Notes on startups, or how to build the future. Crown Business.
[3] According to online sources (e.g. Bachelors Portal) that list college degrees in the U.S.
[4] From websites of Drexel University, Indiana University, Ball State University, and the University of Illinois Chicago, in the same order.

trepreneurship degree programs across U.S. universities. I construct a novel dataset that combines the timing of program introductions with individual-level LinkedIn career histories of more than 300,000 graduates from over 150 universities. This setting allows me to estimate the causal effect of formal entrepreneurship education on entry into entrepreneurship, the types of businesses graduates found, the persistence of those businesses, and the mechanisms underlying the decision to become an entrepreneur.

While raw correlations suggest that students who choose entrepreneurship majors are more likely to start businesses, these estimates include selection effects and cannot be interpreted causally. To identify the causal effect of entrepreneurship education, I exploit quasi-exogenous variation in the timing of entrepreneurship program introductions using a difference-in-differences (DiD) design that compares adjacent cohorts within the same university. This design mitigates concerns that the estimates are driven by endogenous college or major selection. In contrast to the raw correlations, the estimated causal effect is negative: exposure to entrepreneurship education reduces immediate post-graduation entrepreneurial entry. This decline is driven entirely by a reduction in small-business formation, with no effect on entry into growth-oriented ventures.[5] Moreover, the short-run decline is not offset over time. Exposure has no effect on cumulative business formation within three, five, or seven years after graduation, indicating that these programs do not increase the long-run supply of entrepreneurs.

I further examine whether entrepreneurship education improves outcomes among those who start a business by measuring business continuation, defined as whether founders continue to report their businesses as active on LinkedIn one, three, or five years after founding. Conditional on becoming an entrepreneur, exposure to entrepreneurship education has no effect on continuation for the full sample or for growth-oriented ventures at any horizon. Among small businesses, sophomores are modestly more likely than seniors to still report their business as active after one year, but this difference does not persist: the three- and five-year estimates are negative and statistically indistinguishable from zero. These findings suggest

[5]This distinction follows the characterization in Hurst and Pugsley (2011), who show that the majority of U.S. "small businesses" are not growth-oriented ventures but rather provide standardized goods and services in existing markets, with limited intentions to innovate or expand. In their framework, small business formation (e.g., self-employment and very small ventures, typically 1–19 employees) is conceptually distinct from growth-oriented entrepreneurship, which involves scalable ventures, innovation, and job creation.

that entrepreneurship education does not durably improve post-entry business continuation and that its primary effect is to influence entrepreneurial entry rather than the persistence of businesses once founded.[6]

A large body of literature studies entrepreneurship education, often focusing on specific courses or programs and relying on survey-based pretest–posttest designs (e.g., Vesper and Gartner (1997), Souitaris et al. (2007), Oosterbeek et al. (2010), Von Graevenitz et al. (2010)). While informative, much of this work faces two key limitations. First, students endogenously select into entrepreneurial education, raising concerns that estimated effects reflect selection rather than treatment. Second, prior studies typically rely on relatively small samples, often ranging from a few hundred to a few thousand individuals.[7] These limitations make it difficult to establish whether entrepreneurship education causally affects entrepreneurial behavior at scale.

This paper addresses these challenges by introducing an empirical strategy that compares adjacent cohorts within the same institution: sophomores who are exposed to the entrepreneurship major option are compared to seniors who were not offered the degree when they chose their majors. As illustrated in Figure 1, because the entrepreneurship program did not exist at the time earlier cohorts enrolled, this design mitigates endogeneity in students' college and major choices. The identifying variation comes from within-university differences across cohorts rather than cross-sectional differences across schools.[8]

To clarify the experimental design, in a perfect world, the treatment group would be those who went through entrepreneurship education, and the control group would be those who wanted to get this education but could not. In this setting, the treated group is all

[6]Additional analyses reported in Appendix D examine a subsample of growth-oriented ventures that can be matched to Crunchbase, a commercial database that tracks startup financing, acquisitions, and other startup-level outcomes. I find no statistically significant effects of entrepreneurship education on the likelihood of receiving external financing, being acquired, or growing to more than ten employees. These results should be interpreted with caution because the match rate to Crunchbase is relatively low, only about 18% of growth-oriented ventures can be matched, with substantially lower coverage among earlier cohorts, and the database does not comprehensively capture all entrepreneurial businesses or growth-oriented ventures.

[7]For example, Galloway and Brown (2002) study 1,933, Oosterbeek et al. (2010) 409, Von Graevenitz et al. (2010) 196, Iglesias-Sánchez et al. (2016) 392, Maresch et al. (2016) 3,581, Yousaf et al. (2021) 380, and Smolka et al. (2024) 450 individuals.

[8]While seniors in the control cohort may still take individual entrepreneurship courses or participate in select program components, they cannot formally major in entrepreneurship. Any exposure received by seniors would make the treatment and control groups more similar, biasing estimated effects toward zero and providing a conservative assessment of full program exposure.

sophomores of the university who have entrepreneurship majors as a degree option, and the control group is all seniors within the same university who were not offered entrepreneurship as a major degree at the time they were sophomores. For example, the University of San Francisco (USF) introduced its "Entrepreneurship" major degree in 2005. Sophomore students at USF had no knowledge of the upcoming entrepreneurship program when making their college enrollment decisions. One year after their entrance, sophomores have the option to choose entrepreneurship as a major, while all seniors have already chosen their majors. Therefore, students who entered USF in 2004 will be the treated group, sophomores, while students who entered in 2002, seniors, will be the control group. The DiD empirical design compares entrepreneurial outcomes across these two groups to assess their contribution to business formation. At the time of university entrance, neither the treatment nor the control group knows about the entrepreneurship program. Therefore, these results are robust to endogeneity concerns. One can claim these degrees may be created because of the demand coming from students. Since creating a major degree is not a one-day task, this possibility only underestimates the results. If juniors or seniors who are not able to choose an entrepreneurship major request these degrees from the university, this demand aligns the control group closer to the perfect setting.

One potential concern with this approach is that the introduction of an entrepreneurship major may not represent a sharp shock if programs evolve gradually over time. Universities may first hire entrepreneurship faculty, expand course offerings, introduce minors or concentrations, and only later formalize a major degree. To address this concern, I distinguish between institutions that introduce entrepreneurship as a standalone major and those that adopt minors, concentrations, and majors in a staggered sequence. Focusing on universities where the entrepreneurship major represents a discrete institutional change, and estimating specifications based on both the earliest degree offering and standalone major introductions, I show that the results are driven by the introduction of the major itself rather than by earlier incremental program components.

The richness of the LinkedIn data enables a detailed analysis of entrepreneurial outcomes. Career histories are traced from graduation to 2023, allowing for the study of immediate entry,

delayed entry, and business continuation.[9] For individuals who start businesses, information from job titles, company names, job descriptions, and position start dates is combined with a large language model (LLaMA-4 Scout) to classify ventures into risky, high-technology startups and more traditional small ventures. The classification follows the distinction in Hurst and Pugsley (2011) between growth-oriented entrepreneurial ventures and the large share of small businesses that primarily provide standardized goods and services in existing markets. This distinction allows the analysis to separate effects on marginal small-business formation from effects on high-growth entrepreneurship, which are not affected.

Robustness analyses further support the causal interpretation of the results. The findings are not driven by peer-learning mechanisms documented by Lerner and Malmendier (2013) nor by broader trends in U.S. business dynamism (Decker et al., 2016; Akcigit and Ates, 2021). The negative effects persist in environments with both high and low pre-existing entrepreneurial peer exposure and during periods of rising aggregate business entry.

To better understand what drives this discouragement effect, I further decompose the average treatment effect across several dimensions of the program and student heterogeneity. The effect is stronger among students at larger-cohort size and public, lower-cost institutions, providing suggestive evidence for a liquidity-constraint channel (Evans and Jovanovic, 1989; Kerr and Nanda, 2009). When schools are split at the median introduction year (introduced by 2008), the decline in entrepreneurship is concentrated among earlier adopters and is absent among programs introduced later. Using a more conservative cutoff that isolates the earliest pioneer programs (introduced by 2000) shows a similar pattern: the decline remains statistically significant among later adopters, but it is substantially larger among the earliest programs. This pattern also addresses a potential concern with the research design. Because identification is based on the first cohort exposed to a newly introduced program, the estimated effects could reflect temporary adjustment costs associated with launching a new curriculum rather than the effect of entrepreneurship education itself. The effect is strongest among the earliest adopters but remains negative for programs introduced after 2000, indicating that the findings are unlikely to be driven solely by temporary start-up effects.

[9]Entrepreneurship major programs were introduced at different points in time for different schools, with introduction years ranging from 1985 to 2018 in the sample.

Although the estimates for post-2008 introductions are no longer statistically different from zero, they also provide no evidence that later implementations increased entrepreneurial entry. While the earlier negative effects appear to have attenuated as programs matured, there is no evidence that later implementations generated positive short-run effects on business formation. Under additional checks, the estimated effects are not driven by differences in local economic conditions measured by the business formation rate or the SAT scores of the universities at the time programs were introduced.

This paper contributes to several strands of literature. Closely related literature studies entrepreneurship education and business training programs. Existing evidence primarily comes from randomized business training interventions, often implemented in developing-country settings. These studies show that training improves business knowledge and practices, but find mixed evidence on business creation, profits, growth, and longer-run firm outcomes (Karlan and Valdivia, 2011; de Mel et al., 2014; Lyons and Zhang, 2018; Ashraf and Lyons, 2026). Closest to this paper within this literature is Eesley and Lee (2021), who study the introduction of entrepreneurship programs at Stanford University using alumni surveys and a school-level instrumental-variables design, and find limited effects on entrepreneurship rates but gains in entrepreneurial quality. I build on this literature along three dimensions. First, rather than a small-scale training program or a single elite institution, I study the staggered introduction of entrepreneurship degree programs at scale, across more than 150 U.S. universities. Second, rather than relying on survey self-reports, I use large-scale professional data to track entrepreneurial entry and business type directly for over 300,000 graduates. Third, my identification strategy compares adjacent cohorts within the same university rather than alumni before and after program introduction or students across different schools, which limits confounding from compositional shifts, cross-school selection, and declining business dynamism. Together, this design allows me to assess the aggregate, economy-wide effects of institutionalized entrepreneurship education, complementing previous work.

A second literature studies knowledge transmission as a driver of entrepreneurial entry. Gofman and Jin (2024) show that losing access to AI faculty reduces students' likelihood of founding AI companies, and Babina and Howell (2024) show that R&D know-how spills over as employees move between firms. Both papers treat knowledge transmission as an

input to entrepreneurship that arises informally, through mentorship or job mobility. I complement this work by examining an institutionalized channel of knowledge transmission, the entrepreneurship degree, and by identifying its effect using variation in program timing rather than in individual exposure to a specific mentor or employer.

A third literature is on labor market conditions and social interactions as important drivers of entrepreneurial entry. Graduating into a recession can induce skilled workers to found more successful firms (Hacamo and Kleiner, 2022); early exposure to entrepreneurial peers increases entrepreneurial entry and success among women (Mertz et al., 2024); and entrepreneurial coworkers encourage both entry and entrepreneurial performance (Wallskog, 2025). These studies emphasize informal learning and exogenous labor market conditions as mechanisms shaping entrepreneurial selection. This paper asks whether formal entrepreneurship education provides a structured institutional mechanism that similarly influences who enters entrepreneurship.

From a policy perspective, the findings challenge the view that expanding entrepreneurship degree programs increases business creation. Such programs may improve decision-making and reduce inefficient entry, but they do not appear to raise aggregate entrepreneurial activity, and reduce small-business formation, raising questions about how universities and policymakers should define and evaluate the goals of formal entrepreneurship education.

2. Conceptual Framework

Entrepreneurial entry is a decision made under uncertainty, in which individuals compare the expected returns from founding a business with those from alternative career paths. This comparison depends not only on entrepreneurial ability but also on subjective beliefs about success probabilities, costs, and risk. A large literature shows that these beliefs are often optimistic, causing individuals to enter entrepreneurship despite expected returns that, ex post, do not justify entry (Camerer and Lovallo, 1999; Koellinger et al., 2007).

Formal entrepreneurship education enters this decision problem by changing two things: what students believe about the returns to founding a business and their capacity to act on those beliefs. These effects can operate through two economically distinct channels, and the net effect depends on their relative strength. This framing matches the paper's empirical

design: the DiD estimates in Section 5 recover the net effect of the two forces, while the heterogeneity tests in Section 7 are designed to trace out which force is doing the work.

2.1. The Encouragement Channel

The first channel operates by increasing entrepreneurial human capital. Coursework in entrepreneurship can develop skills relevant to founding a business, including opportunity identification, financial literacy, and basic managerial competence, thereby increasing expected productivity conditional on entry. At the same time, entrepreneurship education can boost students' confidence in their own entrepreneurial ability, which prior work shows is a key predictor of entering entrepreneurship (Souitaris et al., 2007). Field experiments in developing-country settings similarly show that business training programs often accelerate start-up activity shortly after participation, particularly among new or marginal entrepreneurs (de Mel et al., 2014; Lyons and Zhang, 2018).

Since undergraduate programs coincide with the period in which students choose among post-graduation career paths, reductions in these frictions should shift entrepreneurial entry forward in time. Students who would otherwise postpone entrepreneurship because of limited information, confidence, or access to relevant resources may instead enter immediately after graduation. The economics of this channel is therefore one of reduced entry frictions rather than changes in underlying entrepreneurial ability or preferences. By lowering the perceived and actual costs of starting a business through improved access to financing, mentorship, and university-affiliated networks, it increases the attractiveness of entrepreneurship at the point of career choice.

2.2. The Screening Channel

Standard models of occupational choice imply that individuals make career decisions under imperfect information about both their own ability and the distribution of returns. If prospective entrepreneurs are systematically overoptimistic, then education that provides more accurate information about entrepreneurship can improve decision-making by correcting those beliefs. Exposure to realistic information about failure rates, capital requirements, and the day-to-day demands of running a business may lead some students to revise their expected

returns from entrepreneurship downward and instead pursue alternative careers. In this case, lower entrepreneurial entry reflects better selection rather than reduced entrepreneurial ability.

Consistent with this mechanism, Von Graevenitz et al. (2010) find that entrepreneurship education can reduce entrepreneurial intentions for some students while simultaneously increasing perceived entrepreneurial skills, consistent with a learning environment in which individuals receive informative signals and update their beliefs about entrepreneurial aptitude. Relatedly, Lerner and Malmendier (2013) show that exposure to entrepreneurial peers discourages unsuccessful ventures while leaving successful ventures unaffected, consistent with selection effects rather than uniform changes in entrepreneurial propensity.

Under this channel, entrepreneurship education should reduce entry primarily among individuals who would have founded businesses with relatively low expected returns. Rather than encouraging all students equally, improved information reallocates individuals away from entrepreneurship when it is not their highest-return career option.

2.3. Net Predictions

The encouragement and screening channels are not competing theories of entrepreneurship education but complementary mechanisms that operate through different economic margins. Because they have opposite implications for entrepreneurial entry, the net effect of entrepreneurship education depends on which channel dominates. The predictions below summarize the empirical implications that distinguish these mechanisms.

Prediction 1 (Immediate Entrepreneurial Entry). If the encouragement channel dominates, entrepreneurship education should increase entry. If the screening channel dominates, it should reduce entry by improving selection. Moreover, because screening primarily affects marginal entrepreneurs, any decline in entry should be concentrated among small-scale businesses rather than growth-oriented ventures.

Prediction 2 (Persistence over Time). If entrepreneurship education primarily accelerates entry by reducing frictions, any initial difference between exposed and unexposed cohorts should diminish over time as delayed entrepreneurs eventually enter. If, instead, entrepreneurship education screens out marginal entrepreneurs, the initial difference should

persist because those individuals permanently select into alternative careers rather than merely postponing entrepreneurship.

Prediction 3 (Heterogeneity). If screening is the dominant mechanism, its strength should vary systematically with the environment in which students make entrepreneurial decisions. Screening based on individualized feedback should be stronger in smaller, more resource-intensive programs, whereas screening based on general information should be largely independent of cohort size. The screening effect should also be stronger where financing constraints make entrepreneurial mistakes more costly, weaker where attractive outside employment opportunities raise the opportunity cost of entrepreneurship, and may differ across program vintages if entrepreneurship curricula evolve as programs mature and later adopters benefit from the experience of earlier adopters. Finally, if screening operates by correcting overly optimistic beliefs, its effects should be strongest for cohorts exposed during periods of unusually favorable local business conditions.

Prediction 4 (Outcomes Conditional on Entry). If entrepreneurship education primarily operates through screening, exposed entrepreneurs should be positively selected, implying unchanged or modestly higher rates of business continuation. If entrepreneurship education primarily encourages entry by lowering barriers without improving selection, exposed entrepreneurs should be no more likely and potentially less likely to continue operating their businesses.

3. Setting and Data

The LinkedIn dataset used in this paper is provided through Nubela from data collected in 2023. This dataset contains self-reported information on individuals' educational backgrounds and professional experiences in the United States. From this dataset, I identify profiles that list entrepreneurship as an undergraduate degree (i.e., major, concentration, or minor). Using these profiles, a list of universities that offer undergraduate entrepreneurship degree programs is constructed.

To determine when each university began offering a formal entrepreneurship degree, I collect university course catalogs dating back to the 1970s. These catalogs are typically publicly archived and provide detailed information on degree offerings, curricula, faculty, and

related programs such as accelerators or business plan competitions. For each university, catalogs around the earliest appearance of entrepreneurship majors in the LinkedIn data are manually reviewed to record the year in which the entrepreneurship degree first appears. An example course catalog can be found in the Appendix, Figure A1. These program introduction years are then merged with individuals' education and employment histories from LinkedIn. This setting enables a quasi-exogenous research design in which the staggered introduction of entrepreneurship degree programs across universities generates plausibly exogenous variation in exposure to entrepreneurship education.

By linking individuals' outcomes to the timing of program adoption at their university, I construct a treatment and control framework suitable for causal inference. The resulting dataset, which contains freshmen, sophomores, juniors, seniors, and the two prior cohort around the introduction, forms the basis of my baseline ordinary least squares (OLS) regressions that suffer from selection bias and the DiD specifications to establish causality. A key advantage of LinkedIn data is its broad coverage of U.S. professionals and the granularity with which it records both educational and career histories. These features make it possible to systematically track individuals' academic majors, degree completion, and subsequent entrepreneurial activity at scale.

At the same time, I acknowledge three limitations. First, the data are self-reported and may contain measurement error in educational or occupational histories. Second, LinkedIn users are not a random sample of the population, which raises potential concerns about representativeness. Third, and more fundamentally, the identification strategy is anchored at the moment a school first introduces its entrepreneurship major: sophomores are, by construction, the first cohort exposed to the newly created program. As a result, the treatment effect I estimate is a treatment-on-the-newly-treated effect, identified from each program's earliest implementation years. I do not observe how the same program, at the same school, affects entry many years later, once curricula have been revised, faculty have accumulated teaching experience, and complementary infrastructure such as accelerators or alumni mentor networks has been built out. I reconfirm the first two issues with statistical analyses to demonstrate that the findings are not driven by systematic reporting errors or sample selection, and return to the third limitation when interpreting the program-vintage results in Section 7.

To ensure the representativeness of the sample, I restrict the analysis to institutions whose observed graduation counts are consistent with their expected institutional size. As expected, the sample is dominated by large public universities, such as Arizona State University, Texas A&M University, and the University of California, Los Angeles, which account for the highest graduation volumes and reflect their central role in the U.S. higher education system. Other large public flagships (including the University of Washington, Indiana University Bloomington, the University of Wisconsin–Madison, Florida State University, and the University of Minnesota–Twin Cities) also appear frequently, with graduation counts in line with their scale and enrollment capacity.

Mid-sized public universities (e.g., the University of Houston, the University of Illinois Chicago, Central Michigan University, and Appalachian State University) and private institutions (e.g., Syracuse University, Brigham Young University, Boston College, and Notre Dame University) are represented in proportions that align closely with their relative size and selectivity.

A small number of institutions (e.g., Thomas Edison State College, Troy State University, California State University, Fresno, and DePaul University) exhibit implausibly low graduation counts relative to their known enrollment, likely reflecting data inconsistencies rather than true institutional scale. Observations with graduation counts that are inconsistent with plausible institutional scale are excluded from the sample. The final sample, therefore, exhibits plausible and expected variation across large public universities, mid-sized institutions, and smaller specialized colleges, supporting the external validity of the analysis.

3.1. Classification of New Businesses

LinkedIn employment histories are used to identify and classify new business creation at scale. The underlying data include job titles, company names, company websites, and free-text job descriptions, which together provide rich information on entrepreneurial activity and business characteristics. This approach allows moving beyond self-reported entrepreneurship and systematically distinguishing between qualitatively different forms of new business formation.

The classification, detailed in Appendix C, proceeds in three steps. First, each experience is

evaluated to determine whether it corresponds to an entrepreneurial role, defined as founding, co-founding, or operating a newly created business. Second, entrepreneurial experiences are classified into small businesses and growth-oriented businesses. Small businesses include local or non-scalable activities such as retail shops, personal services, and consulting. Growth-oriented businesses are defined as startups with scalable business models, often technology-enabled, operating in markets with high growth potential. Third, each entrepreneurial experience is assigned to a broad industry category, including Software, Healthcare, Consulting, Consumer Goods, Finance, Real Estate, Education, Manufacturing, and Other.

The classification is implemented using a LLaMA-4 Scout large language model, which processes both structured and unstructured inputs and returns standardized outputs in a structured format indicating entrepreneurial status, business type, and industry. The model is prompted with explicit definitions for each category to ensure consistency across observations and cohorts. To validate the AI-based classification, I conduct manual checks on a randomly selected subset of observations, independently reviewing the underlying LinkedIn profiles and job descriptions. These checks confirm that the model accurately identifies entrepreneurial roles and meaningfully distinguishes between small, non-scalable businesses and growth-oriented, scalable ventures. Misclassifications are rare for clearly defined cases and are primarily confined to borderline situations, such as early-stage consulting businesses that later transition into scalable ventures.

Any remaining classification error is unlikely to bias the main results. If anything, residual noise would attenuate estimated effects rather than mechanically generate them. Because the same classification procedure is applied uniformly across cohorts and institutions, any remaining error is orthogonal to program introduction timing and does not threaten the identification strategy. Overall, this approach provides a scalable and transparent method for measuring entrepreneurial entry and business type using large professional datasets.

4. Empirical Strategy

The effects of formal entrepreneurship education on business formation are analyzed in two steps. First, the entrepreneurship students are characterized under OLS, which provides a benchmark for the magnitude of pre-existing differences between students who choose

entrepreneurship degrees and their peers, is documented. Then, I estimate causal effects using the variation arising from the staggered introduction of entrepreneurship programs across universities.

4.1. Selection into Entrepreneurship Majors: OLS Evidence

The analysis begins by estimating OLS regressions relating majoring in entrepreneurship to subsequent business formation outcomes. While these estimates are subject to selection bias, they help characterize the population of students who select into entrepreneurship programs and provide context for interpreting the causal estimates.

Specifically, I estimate the following specification:

$$Y_{ist} = \alpha + \beta \cdot EntrepreneurshipMajor_{ist} + \mu_s + \tau_t + \varepsilon_{ist}, \tag{1}$$

where Y_{ist} denotes a binary indicator for entrepreneurial outcomes of individual i at school s in the time the degree introduced, t. I consider a range of outcomes, including immediate entrepreneurial entry (first job after college), immediate entry into small and growth-oriented businesses, indicators for ever founding a business, and entrepreneurship within fixed post-graduation windows.

$EntrepreneurshipMajor_{ist}$ is an indicator equal to one if the individual majored in entrepreneurship in school s, at the time t. All specifications include school fixed effects μ_s, which absorb time-invariant differences across institutions, and degree-introduction-year fixed effects τ_t, which control for aggregate conditions affecting entrepreneurial activity at the time of entry. Standard errors are clustered at the school level throughout. Because treatment timing, the introduction of an entrepreneurship program, is assigned at the school level, the regression error ε_{ist} is likely correlated across students at the same institution. Unobserved shocks to a school's entrepreneurial culture, career-services quality, or local labor market conditions plausibly affect all of that school's graduates in a given cohort simultaneously. Treating individual observations as independent when the underlying treatment varies only at the school level would understate the true standard errors and overstate statistical significance (Bertrand et al., 2004). Clustering at the school level allows for arbitrary within-school

correlation in ε_{ist} across students and cohorts, addressing this concern.[10]

Equation 1 is estimated separately for freshman and sophomore cohorts. These estimates are descriptive and contain positive selection into entrepreneurship majors; accordingly, β should not be interpreted as a causal effect of entrepreneurship education.

4.2. Causal Effects of Entrepreneurship Program Introduction

To estimate the causal effect of entrepreneurship education, I exploit the staggered introduction of entrepreneurship degree programs across universities and implement a DiD design that compares adjacent cohorts within the same institution.[11] The core idea is that sophomores at the time of program introduction are plausibly exposed to the entrepreneurship curriculum, while seniors who have largely completed their coursework are not. I therefore compare outcomes for sophomores (treatment group) to seniors (control group) within the same school, estimating variants of

$$Y_{ist} = \alpha + \beta \cdot Sophomore_{ist} + \mu_s + \tau_t + \varepsilon_{ist}, \tag{2}$$

where $Sophomore_{ist}$ equals one if individual i from school s was a sophomore at the time t the entrepreneurship program was introduced, and zero if a senior. The fixed effects μ_s capture time-invariant school characteristics, τ_t denotes major-introduction-year fixed effects, and standard errors are clustered at the school level throughout. Across all specifications, β captures the causal effect of exposure to entrepreneurship education on the outcome Y_{ist}, and together they test the hypotheses introduced above.

I estimate Equation 2 for four sets of outcomes. First, Y_{ist} is $ImmediateEntrepreneur_{ist}$, an indicator for founding a business as one's first post-graduation job.

Second, conditional on entering entrepreneurship, I replace Y_{ist} with $SmallBusiness_{ist}$ and $GrowthBusiness_{ist}$ to isolate changes in the composition of entry.

Third, I set $Y_{ist} = PostGradEntrepreneur_{ist}^{(k)}$, an indicator for founding a business within

[10] With 159 schools in the earliest-shock sample, the number of clusters (including both the major- and minor/concentration-based subsamples) is above the range where few-cluster inference is a concern (Cameron and Miller, 2015).

[11] The empirical approach is related in spirit to Chakrabarti et al. (2023), but differs in that identification here relies on plausibly exogenous staggered program adoption rather than an instrumental variables strategy.

$k \in \{3, 5, 7\}$ years of graduation, to assess whether any short-run decline in entry is offset over longer horizons.

Fourth, restricting the sample to individuals who become entrepreneurs at universities introducing a standalone major, I set $Y_{ist} = Continuation_{ist}^{(k)}$, an indicator for whether the founder still reports the business as active on LinkedIn at least $k \in \{1, 3, 5\}$ years after founding, estimated separately for all businesses, small businesses, and growth-oriented ventures.

4.3. Entrepreneurship Education: Major, Minor, and Concentration

A central challenge in estimating the causal effects of entrepreneurship education is defining exposure in a way that is both empirically credible and economically meaningful. Universities introduce entrepreneurship education through multiple channels, including full majors, minors, and concentrations, often at different points in time. As a result, program availability could evolve gradually rather than arriving as a single, sharp shock.

To address this issue, I distinguish between three measures of exposure that capture different margins of entrepreneurship education. The earliest introduction of any entrepreneurship program, the introduction of a full entrepreneurship major, and the introduction of minors or concentrations. Figure 3 illustrates the relationship among these exposure definitions and clarifies their overlap.

The **earliest shock** captures the first instance in which any formal entrepreneurship curriculum becomes available at a university. This measure provides the broadest definition of exposure and maximizes statistical power. It reflects the point at which students can first access entrepreneurship coursework, regardless of depth or degree intensity. Because this definition encompasses majors, minors, and concentrations, it serves as a natural baseline for assessing the overall effect of program availability.

The **major** specification isolates the introduction of a full entrepreneurship degree. Compared to the earliest shock, this measure captures a more intensive and structured form of entrepreneurship education, typically involving a dedicated curriculum, required coursework, and sustained exposure. By focusing on majors, I test whether deeper engagement with entrepreneurship education produces different effects than general program availability.

The **minor and concentration** specification captures lower-intensity exposure to entrepreneurship education. These programs generally involve fewer required courses and are more easily combined with other fields of study.

Using these three exposure definitions serves two purposes. First, it allows testing whether estimated effects scale with program intensity. If entrepreneurship education operates through skill acquisition, effects should be strongest for majors and weaker for minors or concentrations. Second, comparing across exposure definitions provides a built-in falsification test. If estimated effects were driven by cohort-specific trends, peer effects, or unobserved institutional changes coinciding with program rollout, similar effects would appear across all exposure measures.

Throughout the analysis, identification relies on within-school comparisons of adjacent cohorts. By comparing sophomores to seniors within the same institution and controlling for school fixed effects and major-introduction-year fixed effects, the design absorbs time-invariant institutional characteristics and common shocks affecting entrepreneurial activity. This approach mitigates concerns that differences across universities, rather than program exposure, drive the results.

5. Results

The analysis begins by documenting the relationship between entrepreneurship education and business formation using baseline OLS results, which provide a benchmark for the magnitude of selection into entrepreneurship majors. It then turns to causal estimates that exploit quasi-exogenous variation in exposure timing to entrepreneurship programs, distinguishing between immediate entry and longer-run business formation.

5.1. OLS Evidence and Selection into Entrepreneurship Majors

Table 2 reports OLS estimates relating to majoring in entrepreneurship to a range of business formation outcomes. Panel A focuses on the sophomore cohort, while Panel B presents results for freshmen. Across all specifications, the coefficient on the entrepreneurship major indicator is positive and statistically significant at the one percent level.

For immediate outcomes, students who major in entrepreneurship are 5.0–5.3 percentage points more likely to enter entrepreneurship as their first post-college job. This increase

reflects higher entry into both small businesses and growth-oriented ventures, with slightly larger effects for small business formation. The positive association persists over longer horizons. Entrepreneurship majors are substantially more likely to ever found a business and to do so within three to five years of graduation. The magnitudes are economically large; entrepreneurship majors are more than 10 percentage points more likely to become entrepreneurs at some point in their careers.

Nonetheless, these estimates should be interpreted as descriptive. Students who select into entrepreneurship majors differ systematically from their peers in ways that are correlated with entrepreneurial entry. As a result, the OLS coefficients reflect substantial positive selection and cannot be interpreted as causal effects of entrepreneurship education.

Rather than providing causal evidence, Table 2 serves two purposes. First, it establishes that students who choose entrepreneurship majors have a strong revealed preference for entrepreneurial careers. Second, it shows the sharp contrast between selection-based estimates and the causal effects estimated below.

5.2. Immediate Business Formation

Table 3 reports DiD estimates of immediate post-graduation business formation, exploiting variation in exposure timing to entrepreneurship programs. I compare sophomores, who are exogenously exposed to the program, to seniors within the same institution, who serve as the control group. All specifications include school fixed effects and major-introduction-year fixed effects (years put into 3-year and 5-year bins bundled together).

Panel A shows that exposure to entrepreneurship education reduces immediate entrepreneurial entry. Across the earliest-shock specification and the major-based specification, sophomores are 0.14–0.22 percentage points less likely to enter entrepreneurship as their first job relative to seniors. These effects are statistically significant at the one percent level. In contrast, exposure through minors or concentrations has no statistically significant effect on immediate entry.

Panels B and C decompose this effect by venture type, conditioning on being an entrepreneur. The decline in immediate entry is entirely driven by small business formation. Sophomores exposed to entrepreneurship education are significantly less likely to found small

businesses upon graduation, while there is no detectable effect on entry into growth-oriented ventures. The absence of effects on growth businesses suggests that the decline in entry does not reflect a general discouragement of entrepreneurship, but rather a selective decline in small-scale business formation.

The concentration of statistically significant effects in the earliest-shock and entrepreneurship-major specifications, together with the absence of effects for entrepreneurship minors and concentrations, also serves as a built-in falsification test. If the estimated effects reflected cohort-specific trends, peer effects, or broader institutional changes accompanying program introduction, similar effects would be expected across all exposure measures. The observed pattern is instead more consistent with the effects arising from formal, intensive entrepreneurship education.

To assess the validity of the identification strategy, I examine the parallel trends assumption using event-study estimates of immediate business entry across cohorts relative to the introduction of entrepreneurship education at a given institution. Cohorts are indexed by academic timing relative to program introduction, with the cohort immediately graduating (seniors) omitted as the reference group. The absence of systematic differences across pre-treatment cohorts provides formal evidence against differential pre-trends, supporting the identifying assumption (see Figure 4).

These findings directly contrast with the positive OLS estimates and are inconsistent with the hypothesis that entrepreneurship education increases immediate entrepreneurial entry. Instead, they support a screening interpretation. Formal entrepreneurship education appears to discourage marginal entry into small businesses at the point of labor market entry.

5.3. Long-Run Business Formation

Table 4 examines whether the short-run decline in entrepreneurial entry is offset by increased business formation over longer horizons. I estimate the effect of exposure to entrepreneurship education on the probability of founding a business within three, five, and seven years after graduation.

Across all time windows and specifications, the estimated coefficients are small and statistically indistinguishable from zero. There is no evidence that students exposed to

entrepreneurship programs are more likely to become entrepreneurs in the medium or long run. This pattern holds regardless of whether exposure is defined using the earliest program introduction, major-based exposure, or minor and concentration-based exposure.

The results indicate that entrepreneurship education does not increase cumulative business formation over time. The negative effects observed at labor market entry are not reversed in subsequent years, nor are they compensated by delayed entrepreneurial entry. Combined with the immediate-entry results, these findings imply that entrepreneurship education reduces small-business formation without generating offsetting increases in growth-oriented or delayed entrepreneurship.

The evidence rejects the view that entrepreneurship education expands the supply of entrepreneurs. While students who select into entrepreneurship majors are substantially more likely to start businesses, quasi-exogenous exposure to these programs reduces immediate entry and has no effect on longer-run business formation. These patterns are consistent with entrepreneurship education shaping selection into entrepreneurship rather than increasing aggregate entrepreneurial activity.

5.4. Business Continuation

In this section, I examine whether entrepreneurship education influences business survival by using LinkedIn employment records to track the continuation of entrepreneurial ventures. Results are in Table 5.

For the full sample of businesses and for growth ventures, the coefficients at one, three, and five years are small, switch sign across horizons, and are not statistically different from zero. Entrepreneurship education does not measurably help or hurt whether these businesses stay open. The one result that reaches significance is for small businesses at the one-year mark: sophomores are 0.95 percentage points more likely than seniors to still have their business running after one year, significant at the 10% level. This effect does not last. At three and five years, the small business coefficients turn negative and are not significant.

These results suggest that entrepreneurship education gives immediate entrants some short-term help getting a small business through its first year, but it does not change whether the business continues longer term, and it has no effect on growth ventures at any horizon.

6. Robustness

This section examines alternative explanations for the main findings and assesses the robustness of the estimated effects of entrepreneurship education.

6.1. Peer Effects versus Entrepreneurship Education

A natural concern in interpreting the estimated effects of entrepreneurship education is that they may reflect peer influences rather than the causal impact of formal instruction. A large literature shows that peers affect educational investments (Bursztyn and Jensen, 2015), financial decisions (Bursztyn et al., 2014; Ouimet and Tate, 2020), risk-taking behavior, and career choices through social learning, information transmission, and norm formation. Peer effects have been documented in financial decision making, educational effort, and occupational choice, and are therefore relevant for interpreting causal estimates in educational settings.

Peer effects are also important in entrepreneurship. Using random assignment of MBA students into sections, Lerner and Malmendier (2013) identify the causal effect of entrepreneurial peers on post-business-school entrepreneurship. They find that a higher share of entrepreneurial peers reduces overall entrepreneurial entry by discouraging unsuccessful ventures, while the effect on successful ventures is positive. This evidence suggests that peer exposure can affect selection into entrepreneurship rather than simply increasing entry.

Relatedly, Nanda and Sørensen (2010) show that exposure to entrepreneurial coworkers increases the likelihood of entrepreneurial entry, consistent with informal learning and reduced perceived entry costs. These findings indicate that peer effects in entrepreneurship operate through multiple channels, including information transmission and belief updating, and may discourage marginal entry that could explain the main results of the paper.

These mechanisms raise the possibility that changes in entrepreneurial outcomes following the introduction of entrepreneurship programs reflect peer composition rather than curricular content. To assess this possibility, I exploit cross-school variation in pre-existing entrepreneurial exposure among students. To do so, schools are classified based on the number of sophomores with entrepreneurial experience prior to or during their second year, where

prior experience is defined as participation in small ventures, entrepreneurship-related class projects significant enough to report on LinkedIn, or family businesses. Schools with more than five such sophomores are classified as having more prior peer entrepreneurial experience, while schools with five or fewer are classified as having fewer prior peer entrepreneurial experiences. I then estimate linear probability models of immediate entrepreneurial entry as in Equation 2, defined as having an entrepreneurial first job after college, separately for these two subsamples following the main specifications. This design allows for a comparison between environments with greater and more limited scope for peer-based learning about entrepreneurship.

Across both subsamples in Table 6, exposure to entrepreneurship education during the sophomore year is associated with a lower probability of subsequent entrepreneurial entry. While the estimated effect is smaller in schools with greater prior peer entrepreneurial exposure, it remains negative and statistically significant. This pattern is consistent with peer effects playing some role in shaping entrepreneurial decisions, as students in environments with more entrepreneurial peers may already benefit from informal entrepreneurial exposure. However, the persistence of a negative treatment effect in both subsamples indicates that differences in peer composition cannot fully account for the main results. Instead, the evidence suggests that peer effects may contribute to, but do not solely explain, the estimated impact of entrepreneurship education.

This finding is informative for interpretation. Consistent with Lerner and Malmendier (2013), formal entrepreneurship education may lead students to update beliefs about the feasibility and expected returns of entrepreneurship and to optimally select out of entry. Unlike informal peer interactions, coursework emphasizes structured evaluation of ideas and exposes students to failure rates and opportunity costs. These features can discourage low-quality entry without reducing high-quality entrepreneurial activity.

This analysis does not imply that peer effects are unimportant. Rather, it shows that the estimated effects of entrepreneurship education persist even after accounting for meaningful variation in peer entrepreneurial exposure. As such, the results support an interpretation in which formal entrepreneurship education has an independent influence on entrepreneurial outcomes, separate from social learning within cohorts. This distinction is important for

understanding the mechanisms through which educational institutions shape entrepreneurial behavior and for interpreting the DiD results presented in the paper.

6.2. Business Dynamism versus Entrepreneurship Education

A related concern is that the estimated negative effect of entrepreneurship education reflects broader declines in business dynamism rather than the impact of cohort exposure to entrepreneurship programs. A growing literature documents a long-run decline in U.S. business formation and reallocation, raising the possibility that differences across graduating cohorts capture aggregate trends rather than educational effects. However, existing evidence also shows that business entry increased during the early 2000s. Using Census Bureau Longitudinal Business Database data, Decker et al. (2016) document a rise in business entry and job reallocation in the period leading up to the mid-2000s. Similarly, using Business Dynamics Statistics data, Akcigit and Ates (2021) show that aggregate entry rates increase in the early 2000s while declining before and later in the decade.

To assess whether the main results are driven by changes in aggregate business dynamism, I conduct two complementary tests, reported in Table 7. Both analyses restrict the sample to entrepreneurship-major schools with program introductions between 2000 and 2004, ensuring that the graduation years of the treated and control cohorts fall within the period of rising aggregate business entry documented in prior work.

Panel A presents a within-school event-study analysis that examines entrepreneurial entry across cohorts during this period, providing support for the parallel trends assumption. The specification mirrors the baseline regression and includes school fixed effects and cohort-year fixed effects, with standard errors clustered at the school level. Seniors serve as the control group. The estimates for cohorts graduating one and two years earlier are statistically indistinguishable from zero, indicating no differential pre-trends in entrepreneurial outcomes across institutions during this period.

Panel B uses the same restricted sample to estimate the baseline DiD specification. For each program introduction year, I compare seniors graduating in the introduction year to sophomores graduating two years later. The dependent variables indicate whether an individual's first job after graduation is founding any business, a small business, or a growth-

oriented business.

Across all specifications, exposure to entrepreneurship education during the sophomore year is associated with a lower probability of immediate business formation. The effect is present for overall entrepreneurship as well as for small and growth-oriented businesses. Because these estimates are obtained during a period of increasing aggregate business entry, they are unlikely to be driven by declining business dynamism. Therefore, these results confirm that the main results reflect cohort exposure to entrepreneurship education rather than contemporaneous shifts in aggregate entry conditions.

6.3. LinkedIn Data Checks

A natural concern when using LinkedIn data is that platform coverage may vary systematically across cohorts, as older cohorts are less likely to maintain active profiles than more recent graduates. If seniors are differentially underrepresented relative to sophomores, estimates of entrepreneurial entry could in principle be mechanically biased. This concern is, however, structurally limited in the present setting. The empirical design compares cohorts separated by only one to two academic years, rather than by long graduation windows: sophomores and seniors exposed around program introduction typically graduate within a narrow temporal band and are observed in the same LinkedIn snapshot. As a result, any differential adoption or attrition in platform usage that varies with years-since-graduation is likely to be minimal within adjacent cohorts.

A second data concern arises from the incomplete reporting of educational histories on LinkedIn. Some individuals report only the start year of their undergraduate degree, omitting the completion year. For these cases, I impute degree completion by assuming a standard four-year duration, consistent with typical U.S. undergraduate programs. This assumption allows these observations to be retained in the baseline sample and preserves statistical power. To assess sensitivity, I re-estimate all main specifications after dropping individuals who do not report an explicit degree end date. The resulting coefficients change slightly in magnitude but remain stable in sign and statistical significance.[12]

[12]Restricting the major-program sample to individuals with an explicit degree end date (116,548 of 116,810 observations, a loss of 0.2%) leaves all three coefficients close to their baseline values in Table 3.

A further concern is the selective reporting of entrepreneurial experiences. Because LinkedIn profiles are self-reported, individuals may choose to omit short-lived or unsuccessful ventures, potentially leading to undercounting of failed entrepreneurship. To address this issue, I first examine the timing between individuals' graduation dates and their first recorded professional experience. I verify that, for the vast majority of entrepreneurs, the first reported experience begins within one year of graduation, suggesting that early entrepreneurial activity is systematically captured rather than selectively omitted. Second, even if failed ventures were disproportionately deleted from profiles, this bias would likely be stronger for seniors than for sophomores, since seniors have been in the labor market longer and have had more opportunities both to experiment and to retrospectively curate their profiles. Such differential deletion would mechanically inflate observed entrepreneurial entry among seniors relative to sophomores, biasing estimates toward zero or positive effects. The fact that the paper finds negative effects of exposure to entrepreneurship education, therefore, runs counter to this concern and strengthens the interpretation of the results.

7. Mechanisms

The results in Section 5 point toward a screening interpretation: exposure to entrepreneurship education reduces immediate entry into small-scale ventures without generating any offsetting increase in growth-oriented entry. A screening story, however, is not a single mechanism but rather a label for several economically distinct channels that could produce the same average effect. In this section, I unpack the average treatment effect along five dimensions that the existing literature suggests should matter for the strength of the screening channel: cohort size, as a proxy for the intensity of instructional resources per student; SAT scores and university status, as proxies for both the opportunity cost of entrepreneurial entry facing the marginal student and access to outside financing; the vintage of the program itself; and the state of the local economy at the time a cohort is exposed to the program. Throughout, I maintain the baseline specification of Equation 2, re-estimating it separately within sub-samples defined by the dimension of interest, with the same school and major-year fixed effects.

7.1. Cohort Size and the Scale of Program Delivery

A natural question is whether the effects of entrepreneurship education vary with the scale at which programs are delivered. Cohort size is an imperfect proxy for program scale, capturing not only the number of students exposed to entrepreneurship education but also broader differences in how programs are organized. Larger cohorts may coincide with greater instructional capacity, more developed entrepreneurship ecosystems, and more standardized curricula, while smaller cohorts may offer more individualized instruction and closer student-faculty interaction. Because these dimensions are not separately observed, cohort size should be interpreted as a reduced-form measure of program scale rather than a proxy for any single organizational feature.

If the screening mechanism depends on intensive mentoring, repeated feedback, or close interactions with faculty, its effectiveness may vary with program size. Alternatively, larger programs may rely more heavily on standardized curricula and institutionalized program structures, producing more uniform screening effects across students. Peer environments may also differ across program size, affecting the informational content of entrepreneurial exposure, consistent with the emphasis on peer-based learning and selection in Lerner and Malmendier (2013) and Nanda and Sørensen (2010). In practice, cohort size likely reflects a combination of these channels.

To examine whether treatment effects differ with program scale, I split schools at the across-school mean number of sophomore-plus-senior observations (1,854) and re-estimate Equation 2 separately in each subsample, restricting attention to the major-based exposure definition. Table 8 reports the results. The estimated effect is negative for both large- and small-cohort schools, although statistical significance is attained only among the larger-cohort institutions. The estimated magnitudes are similar, with coefficients of -0.23 and -0.18 percentage points, respectively, while the smaller-cohort sample contains fewer than half as many observations and correspondingly larger standard errors.

These results should therefore be interpreted with caution. The concentration of statistical significance among larger programs is consistent with the possibility that larger entrepreneurship majors deliver more standardized or consistently implemented educational experiences.

Because the data do not include faculty size, instructional resources, or other measures of program capacity, the analysis cannot distinguish between these competing explanations. Accordingly, the cohort-size analysis provides suggestive evidence that the estimated reduction in immediate entrepreneurial entry is at least as pronounced in larger programs, while leaving open the organizational mechanisms through which program scale influences implementation.

7.2. Institutional Selectivity and the Opportunity-Cost Channel

The screening channel may vary with the academic selectivity of the institution. The opportunity-cost channel predicts that the same informational correction should produce a larger behavioral response when the student's next-best alternative is more attractive. Students at higher-scoring institutions tend to face better outside offers. Therefore, even a modest downward revision in perceived entrepreneurial returns should be sufficient to push the marginal student toward the more appealing wage-employment alternative, implying a larger screening effect at higher-scoring schools.

I proxy selectivity with the school-level average SAT score from the U.S. Department of Education's College Scorecard, classify schools as "high-SAT" if their average exceeds the across-school mean (1,099.7), and re-estimate Equation 2 within each group.[13]

Table 9 reports the results. The estimates are similar across subsamples, with no statistically meaningful difference between high- and low-SAT schools. If anything, the point estimates are slightly larger in low-SAT institutions, though differences are small relative to sampling uncertainty. Overall, the evidence does not indicate that screening effects are concentrated in more selective institutions. This suggests that the mechanism is not primarily driven by differential sensitivity to outside wage opportunities at the top of the ability distribution, and is instead broadly stable across the selectivity margin.

7.3. Institution Type and Financing Constraints

A related channel concerns the financial position of the marginal entrant. Evans and Jovanovic (1989) document a positive association between personal wealth and entrepreneurial entry, consistent with the presence of financing frictions that make external capital costly

[13]College Scorecard data, U.S. Department of Education, https://collegescorecard.ed.gov/data/.

or limited for potential entrepreneurs. To examine this channel, I partition the sample into public and private institutions.[14]

In this setting, if the marginal small-business entrant discouraged by entrepreneurship education is disproportionately liquidity constrained and therefore more sensitive to downside risk, then the screening effect should be concentrated among students facing tighter budget constraints. This mechanism implies stronger effects at lower-cost public institutions, where a greater fraction of the marginal financing burden is borne by students' own constrained resources, and attenuated effects at higher-cost private institutions, where selection on stronger household balance sheets is more pronounced.

Table 10 reports estimates separately for public and private institutions. The effect is negative and statistically significant at public institutions, while it is smaller in magnitude and statistically indistinguishable from zero at private institutions, though it remains negative in sign. Taken together with the selectivity patterns documented in Table 9, and noting that lower-SAT institutions in the sample are disproportionately public, these results point to a consistent interpretation: the discouragement effect of entrepreneurship education is concentrated among students who are more likely, based on institutional context, to be financially constrained, rather than among students with stronger outside options or greater capacity to self-insure against entrepreneurial failure.

7.4. Program Vintage and Scale-Up Attenuation

The fourth channel exploits variation in the timing of entrepreneurship major adoption, which occurred over a period spanning more than three decades, with early programs appearing in the mid-1980s and the bulk of adoptions concentrated between 2001 and 2007 (Figure A1). Program vintage may capture systematic differences in the scale and organization of entrepreneurship education across stages of diffusion. Early programs were typically introduced on a smaller scale and were often more experimental in structure,

[14]Private institutions exhibit higher sticker prices than public institutions, and this gap persists even after accounting for financial aid. NCES data indicate that low-income students enrolled in private, not-for-profit four-year institutions face average net prices that are approximately $2,800 higher than comparable students at public four-year institutions, despite receiving more generous aid packages (Choy, 2000). These differences imply that, at private institutions, observed enrollment is more strongly selected on household resources and the ability to self-finance educational and associated opportunity costs, whereas at public institutions a larger share of students is likely closer to the margin of financial constraint.

whereas later adopters reflect a period in which entrepreneurship education had become more institutionalized, with more standardized curricula and larger departmental infrastructures.

This evolution implies potential heterogeneity in the estimated effects. If the screening channel depends on intensive interaction, small cohorts, or course experimentation, effects may be stronger among early adopters, where programs are more concentrated and less standardized. By contrast, more scaled and standardized delivery among later adopters could attenuate treatment intensity and thus weaken estimated effects.

Later adoption may coincide with improvements in curriculum design, faculty experience, and complementary entrepreneurship infrastructure (e.g., accelerators, pitch competitions, and alumni networks). Accordingly, program vintage provides a reduced-form test of whether estimated effects vary with the maturity and scale of program implementation.

I implement two complementary comparisons. First, I split the sample at the median introduction year in the major-school sample (2008), comparing schools that introduced their major on or before the median to those that introduced it afterward (Table 11). Second, I use a fixed cutoff of 2000 to isolate early adopters, well before the main wave of program introductions (Table 12). The median split reveals a strong contrast: the estimated effect is larger in magnitude and more precisely estimated among pre-2008 programs. The 2000 cutoff further isolates a small set of early adopters and yields an even larger point estimate, while post-2000 programs continue to exhibit a smaller but statistically significant negative effect.

Importantly, this vintage variation should be interpreted in conjunction with the identifying variation in Equation 2. The empirical design identifies effects at the point of program introduction, and thus primarily reflects outcomes for early cohorts exposed to a newly implemented major. To the extent that program quality, infrastructure, and complementary resources evolve after adoption, the estimates can be interpreted as capturing effects during early implementation stages rather than steady-state program impacts.

The concentration of the estimated effects among earlier adopters is informative for interpreting the evolution of entrepreneurship programs over time. One possible interpretation is that institutions introducing majors during the initial diffusion phase operated under greater uncertainty regarding program design and implementation, whereas later adopters could draw on accumulated experience, emerging best practices, and increasingly standardized curricular

models. Such cross-institutional learning may have mitigated the reduction in immediate entrepreneurial entry observed among earlier cohorts. However, the post-2008 estimates are statistically indistinguishable from zero, providing no evidence that later implementations increased entrepreneurial entry beyond the baseline. Thus, while organizational learning and program refinement may have attenuated the earlier negative effects, the results do not suggest that subsequent generations of entrepreneurship majors generated positive short-run effects on business formation.

These findings indicate that the consequences of entrepreneurship education evolved as programs diffused across institutions, but they do not support the view that later, more mature implementations substantially increased immediate entrepreneurial activity, either.

7.5. Local Business Dynamics and Entry

The final channel considers the state of the local economy at the time a cohort is first exposed to the program. Cohorts entering college during periods of regional expansion may form more optimistic beliefs about entrepreneurial prospects, while cohorts entering during downturns may have more pessimistic priors. If the primary role of the curriculum is to update or discipline these priors, then the magnitude of adjustment should be larger when initial beliefs are further from fundamentals. This perspective is consistent with Oyer (2008) and Schoar and Zuo (2017), who document persistent effects of labor market conditions at career entry on occupational sorting and managerial behavior.

An alternative hypothesis, following Hacamo and Kleiner (2022), is that weak local labor markets may induce selection into entrepreneurship out of necessity rather than opportunity. Under this mechanism, entrants in downturns are less marginal ex ante, which would imply weaker or potentially reversed screening effects in low-growth environments.

I measure local economic conditions using a forward-looking three-year average of the state-level establishment entry rate from the Census Bureau's Business Dynamics Statistics,[15] evaluated in the calendar year of each school's program introduction. I then split the sample at the across-school mean (10.57). Unlike the aggregate time-series robustness checks in

[15]Business Dynamics Statistics, U.S. Census Bureau: https://www2.census.gov/programs-surveys/bds/tables/time-series/2023/bds2023_st.csv.

Section 7.2, this test exploits cross-state variation in local business dynamism at the time of program rollout.

Table 13 shows that the estimated effect is negative and similar in magnitude across both high- and low-growth states, with statistically indistinguishable coefficients. The absence of meaningful heterogeneity provides little support for either competing mechanism: the effect is not concentrated in high-growth environments, as would be predicted by a belief-correction channel, nor does it attenuate or reverse in low-growth environments, as implied by a necessity-driven entrepreneurship channel.

Overall, the screening effect appears robust to local business cycle conditions at program introduction. This reinforces the national-level evidence in Section 7.2 and suggests that the baseline results are not driven by contemporaneous variation in business dynamism (Decker et al., 2016; Akcigit and Ates, 2021).

8. Conclusion

This paper examines whether undergraduate entrepreneurship education increases business formation. Exploiting the staggered introduction of entrepreneurship majors across U.S. universities and comparing adjacent cohorts within institutions, I provide causal evidence on how formal entrepreneurship education affects entrepreneurial entry. The results show that entrepreneurship education reduces immediate business formation, with the decline driven entirely by fewer small-business ventures. I find no offsetting increase in growth-oriented entrepreneurship or delayed business formation over longer horizons. Conditional on entry, entrepreneurship education also does not improve business continuation in the longer horizons. These findings suggest that entrepreneurship education primarily affects who enters entrepreneurship rather than increasing either the quantity or subsequent performance of new businesses.

The evidence is most consistent with entrepreneurship education operating as a screening mechanism. By providing students with more complete information about entrepreneurial opportunities and risks, these programs appear to discourage marginal entry into small businesses without affecting growth-oriented ventures. More broadly, the results contribute to the literature on entrepreneurial entry by showing that formal entrepreneurship education

influences occupational selection, but has limited effectiveness as a policy tool for stimulating new business creation.

One important caveat is that the estimates identify the effects of newly introduced entrepreneurship programs. Because the empirical design compares the first cohorts exposed to a new major with adjacent unexposed cohorts, the results need not reflect the long-run effects of mature programs after curricula, faculty experience, and complementary infrastructure have evolved. Future research with longer post-introduction panels could examine whether these effects persist as programs mature.

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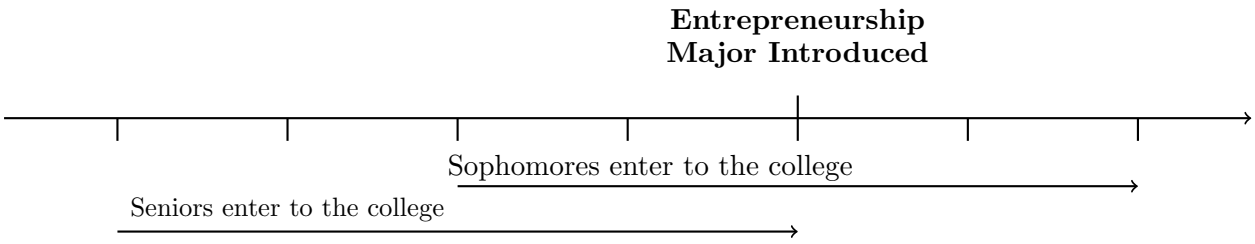
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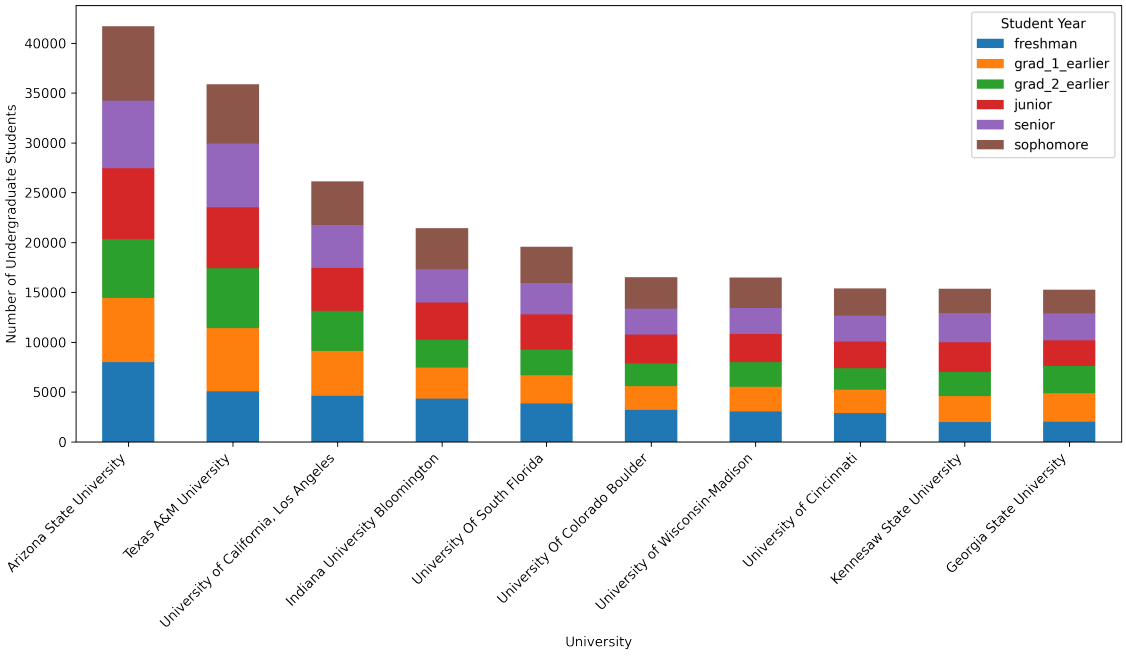
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Figure 1. Empirical Setting Timeline



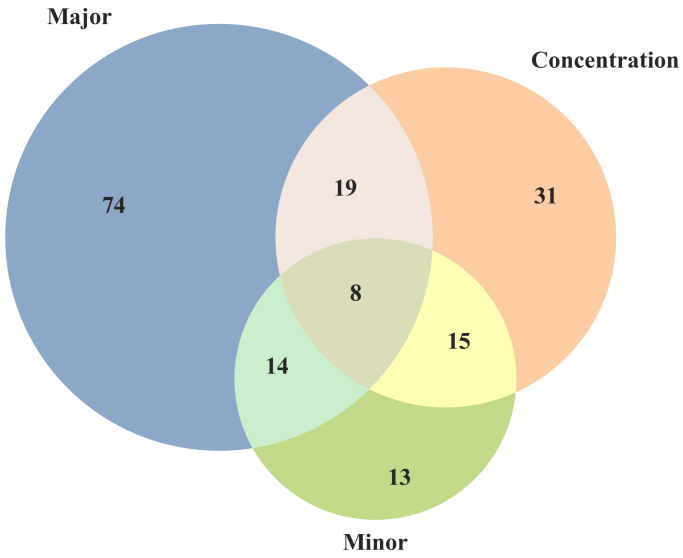
This figure illustrates the empirical timing of the introduction of entrepreneurship bachelor's degrees and the resulting treatment and control cohorts within a university. The horizontal axis represents academic time relative to the program's introduction. Sophomores constitute the treated cohort, as they are exposed to the entrepreneurship major upon entry into their second year, while seniors serve as the control cohort, having already finalized their majors prior to the program's availability. The figure illustrates the within-college, adjacent-cohort comparison picturing the empirical design.

Figure 2. Distribution of Students (Top 10 Schools in the Sample)



This figure displays the distribution of undergraduate students across academic cohorts (freshman, sophomore, junior, and senior) for the ten institutions with the largest number of observations in the final sample. Information on the top ten schools is informative because these institutions account for a substantial share of the overall sample and therefore have the greatest potential to influence the empirical results. Within each institution, students are represented across all cohorts, with no single cohort disproportionately dominating the sample. This balanced cohort composition mitigates concerns that the estimated effects are driven by cohort-specific representation or by mechanical differences in the academic stage of students. Moreover, the similarity in cohort distributions across these large institutions suggests that the identifying variation in the analysis is not driven by a small subset of schools or cohorts, supporting the representativeness of the sample among its largest contributors.

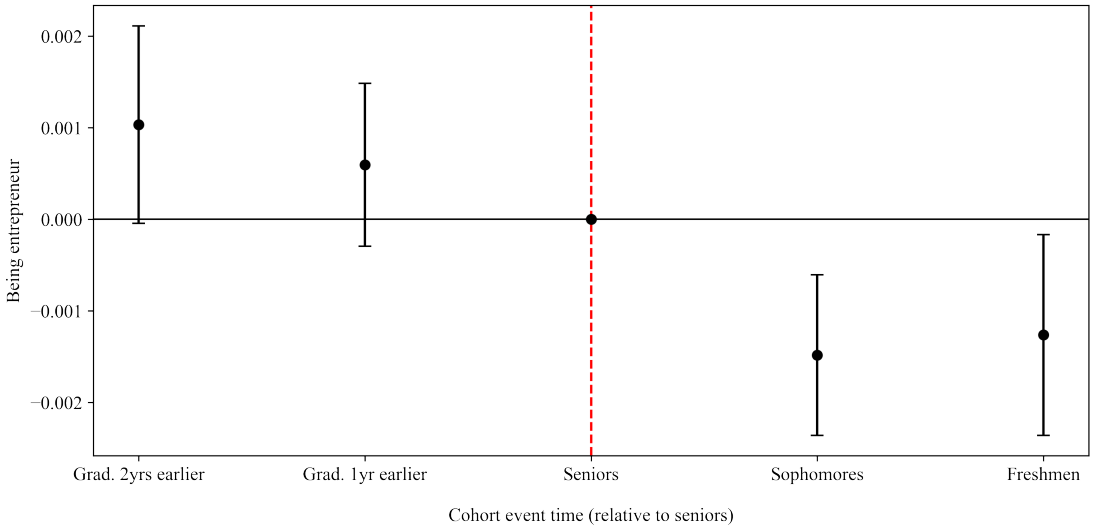
Figure 3. Overlap in Entrepreneurship Degree Offerings Across Schools



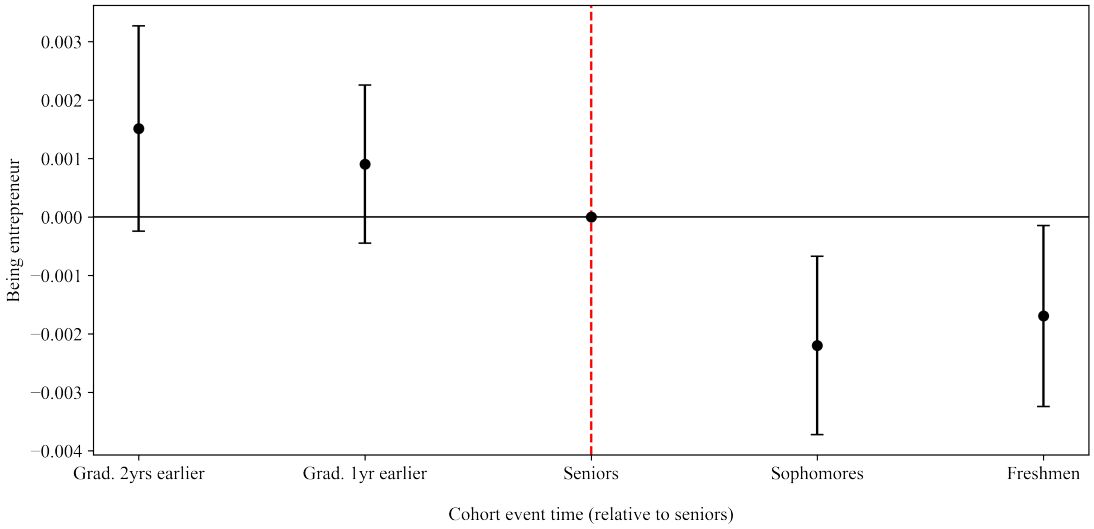
This figure illustrates the overlap across institutions in the sample that offer entrepreneurship education in the form of a bachelor's major, a minor, or a concentration. Each circle represents schools that introduce a given degree type, and intersections capture institutions offering multiple forms of entrepreneurship education. The diagram clarifies the structure of program availability used in the empirical analysis. In the earliest shock specifications, all schools are included, provided they are sufficiently represented in the data. In the baseline OLS analysis, entrepreneurship major schools were included. In analyses that distinguish the intensity of entrepreneurship education, the sample is restricted to institutions that introduce a single, clearly defined degree type (i.e., major, minor, or concentration), excluding schools with overlapping introductions. This restriction ensures that treatment timing is unambiguous and avoids conflating staggered or bundled program expansions with the introduction of a specific degree.

Figure 4. Event-Study Evidence on Parallel Trends: Immediate Business Entry

(a) Earliest Shock



(b) Major Programs



Panel (a) reports event-study estimates of immediate business entry across cohorts relative to the earliest introduction of entrepreneurship education at a given institution. Panel (b) presents corresponding estimates for institutions that introduce entrepreneurship as a standalone major. In both panels, cohorts are indexed by academic timing relative to program introduction, with the cohort immediately graduating (seniors) omitted as the reference group. The absence of systematic differences across pre-treatment cohorts provides formal evidence against differential pre-trends, supporting the parallel trends assumption underlying the DiD design. Restricting attention to major programs in Panel (b) isolates the effect of intensive entrepreneurship education and avoids confounding treatment timing arising from overlapping or staggered introductions of minors or concentrations.

Table 1. Summary Statistics: Entrepreneurship Outcomes Across Samples

	Earliest Shock	**Major**	**Minor & Concentration**
Panel A: Sample Coverage			
Number of schools	159	63	43
Panel B: Immediate Entrepreneurship (Entrepreneur at Graduation)			
Average entrepreneurs per school (Seniors)	19.58	17.69	21.38
Maximum per school (Seniors)	105	105	68
Minimum per school (Seniors)	1	1	3
Average entrepreneurs per school (Sophomores)	19.38	16.22	21.81
Maximum per school (Sophomores)	125	125	87
Minimum per school (Sophomores)	1	2	2
% entrepreneurs per school (Seniors)	2.20%	2.06%	2.23%
% entrepreneurs per school (Sophomores)	2.07%	1.96%	2.23%
% Small Business (Seniors, conditional)	71%	80%	69%
% Growth Business (Seniors, conditional)	29%	20%	31%
% Small Business (Sophomores, conditional)	71%	78%	68%
% Growth Business (Sophomores, conditional)	29%	22%	32%
Panel C: Ever Entrepreneurship (At Any Point in Time)			
Average entrepreneurs per school (Seniors)	19.58	73.24	84.60
Maximum per school (Seniors)	105	394	223
Minimum per school (Seniors)	1	7	6
Average entrepreneurs per school (Sophomores)	19.38	70.19	82
Maximum per school (Sophomores)	125	337	251
Minimum per school (Sophomores)	1	4	3
% ever entrepreneurs per school (Seniors)	9.80%	9.08%	9.41%
% ever entrepreneurs per school (Sophomores)	8.66%	8.22%	8.42%
% Small Business (Seniors, conditional)	71%	62%	65%
% Growth Business (Seniors, conditional)	29%	38%	35%
% Small Business (Sophomores, conditional)	71%	70%	62%
% Growth Business (Sophomores, conditional)	29%	30%	38%

This table reports school-level summary statistics for entrepreneurship outcomes across samples. Immediate entrepreneurship measures whether an individual operates a business at the time of graduation, while ever entrepreneurship captures whether the individual becomes an entrepreneur at any point after graduation. Entrepreneurship rates are computed at the school-class level and then averaged across schools to avoid mechanically overweighting large institutions. Business-type shares are calculated conditional on entrepreneurship within each school. The substantial dispersion across schools motivates the identification strategy exploiting within-school cohort variation.

Table 2. Entrepreneurship Major and Business Formation: OLS Evidence

	Immediate Entry	Immediate Small	Immediate Growth	Ever Entry	Within 3 Years	Within 5 Years	Ever Small	Ever Growth
Panel A: Sophomore Cohort								
Entrepreneurship Major	0.050***	0.026***	0.023***	0.116***	0.107***	0.106***	0.040***	0.076***
	(0.010)	(0.006)	(0.007)	(0.020)	(0.016)	(0.016)	(0.010)	(0.014)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.007	0.007	0.003	0.051	0.048	0.054	0.044	0.015
Observations	59,994	59,994	59,994	59,994	59,745	59,744	59,994	59,994
Panel B: Freshman Cohort								
Entrepreneurship Major	0.053***	0.029***	0.024***	0.118***	0.104***	0.106***	0.065***	0.054***
	(0.010)	(0.007)	(0.006)	(0.017)	(0.015)	(0.015)	(0.013)	(0.008)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.009	0.008	0.007	0.049	0.049	0.054	0.044	0.012
Observations	61,030	61,030	61,030	61,030	60,899	60,900	61,030	61,030

This table reports OLS estimates based on Equation 1, relating entrepreneurship education to business formation outcomes. The key independent variable is an indicator equal to one if the individual majored in entrepreneurship. The specification compares individuals within the same institution who selected into an entrepreneurship major to all other students at that institution. Outcomes include immediate post-college entrepreneurship entry, immediate entry into small and growth businesses, and measures of entrepreneurship at later stages of the life cycle. All regressions include school fixed effects and year fixed effects, where the year corresponds to the individual's venture start year, thus absorbing time-varying aggregate conditions affecting entrepreneurial activity. Standard errors are clustered at the school level and reported in parentheses. These estimates are descriptive in nature and reflect substantial positive selection into entrepreneurship majors; accordingly, the coefficients should not be interpreted as causal. Subsequent sections address identification using quasi-experimental variation arising from the plausibly exogenous introduction of entrepreneurship degree programs. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3. Immediate Business Formation by Exposure Timing and Academic Track

	Earliest Shock	**Major**	**Minor & Concentration**
Panel A: Immediate Entrepreneurship (First Job)			
Sophomore vs. Senior	-0.0014***	-0.0022***	-0.0005
	(0.0005)	(0.0008)	(0.0009)
R^2	0.0041	0.0032	0.0040
Observations	324,882	116,810	97,898
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Panel B: Small Business (Conditional on Being an Entrepreneur)			
Sophomore vs. Senior	-0.0015***	-0.0017***	0.0010
	(0.0004)	(0.0007)	(0.0008)
R^2	0.0046	0.0035	0.0069
Observations	324,882	116,810	97,898
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Panel C: Growth Business (Conditional on Being Entrepreneur)			
Sophomore vs. Senior	-0.0003	-0.0004	0.0001
	(0.0003)	(0.0003)	(0.0005)
R^2	0.0024	0.0010	0.0033
Observations	324,882	116,810	97,898
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes

This table reports linear probability model estimates of immediate post-graduation business formation outcomes, equation 2. Panel A uses an indicator for whether an individual's first job after college is founding their own business. Panels B and C condition on entrepreneurship and distinguish between small business formation and growth-oriented ventures, respectively. The main explanatory variable is an indicator for being a sophomore, with seniors serving as the control group. All specifications include school fixed effects and major-introduction-year fixed effects, where experience years are grouped into three-year bins (results are robust to grouping experience start years into five-year bins). Standard errors are heteroskedasticity-robust and clustered at the school level, and are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table 4. Business Formation Within Fixed Post-Graduation Windows

	Earliest Shock	**Major**	**Minor & Concentration**
Panel A: Entrepreneur Within 3 Years of Graduation			
Sophomore vs. Senior	0.0010	0.0006	0.0020
	(0.0008)	(0.0015)	(0.0018)
R^2	0.0448	0.0443	0.0509
Observations	324,882	116,810	97,898
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Panel B: Entrepreneur Within 5 Years of Graduation			
Sophomore vs. Senior	0.0009	0.0005	0.0020
	(0.0009)	(0.0016)	(0.0018)
R^2	0.0505	0.0506	0.0544
Observations	324,882	116,810	97,898
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Panel C: Entrepreneur Within 7 Years of Graduation			
Sophomore vs. Senior	0.0005	0.0003	0.0011
	(0.0010)	(0.0018)	(0.0016)
R^2	0.0533	0.0541	0.0549
Observations	324,882	116,810	97,898
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes

This table reports linear probability model estimates of business formation within fixed post-graduation windows, equation 2. The dependent variable is an indicator equal to one if the individual founds a business within 3, 5, or 7 years of college completion, respectively. The main explanatory variable is an indicator for being a sophomore, with seniors serving as the control group. All specifications include school fixed effects and major-introduction-year fixed effects, where experience years are grouped into three-year bins (results are robust to grouping experience start years into five-year bins). Standard errors are heteroskedasticity-robust and clustered at the school level, and are reported in parentheses.

Table 5. Business Continuation: Major Shock
(Immediate Entrepreneurs)

	1 Year	3 Years	5 Years
Panel A: Any Business (Conditional on Being an Entrepreneur)			
Sophomore vs. Senior	0.0061	-0.0029	-0.0241
	(0.0049)	(0.0132)	(0.0188)
R^2	0.2914	0.2927	0.4174
Observations	2,105	2,105	2,105
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Panel B: Small Business (Conditional on Being Entrepreneur)			
Sophomore vs. Senior	0.0095*	-0.0114	-0.0244
	(0.0054)	(0.0151)	(0.0205)
R^2	0.2915	0.3043	0.4246
Observations	1,609	1,609	1,609
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Panel C: Growth Business (Conditional on Being Entrepreneur)			
Sophomore vs. Senior	-0.0110	0.0257	-0.0319
	(0.0095)	(0.0371)	(0.0607)
R^2	0.3846	0.3264	0.4212
Observations	496	496	496
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes

This table reports linear probability model estimates of business continuation at 1-, 3-, and 5-year horizons following founding a business, restricted to the sample of immediate college entrepreneurs (the "major degree introduction shock" sample). The sample is restricted to individuals who become entrepreneurs right after graduation and attended a school with a major degree offering. The dependent variable is an indicator equal to one if the venture continues at least the specified number of years. The main explanatory variable is an indicator for being a sophomore, with seniors serving as the control group. Panel A considers all businesses, while Panels B and C distinguish between small businesses and growth-oriented ventures, respectively. All specifications include school fixed effects and experience start-year fixed effects. Standard errors are heteroskedasticity-robust and clustered at the school level, and are reported in parentheses. Statistical significance at the 10% level is denoted by *.

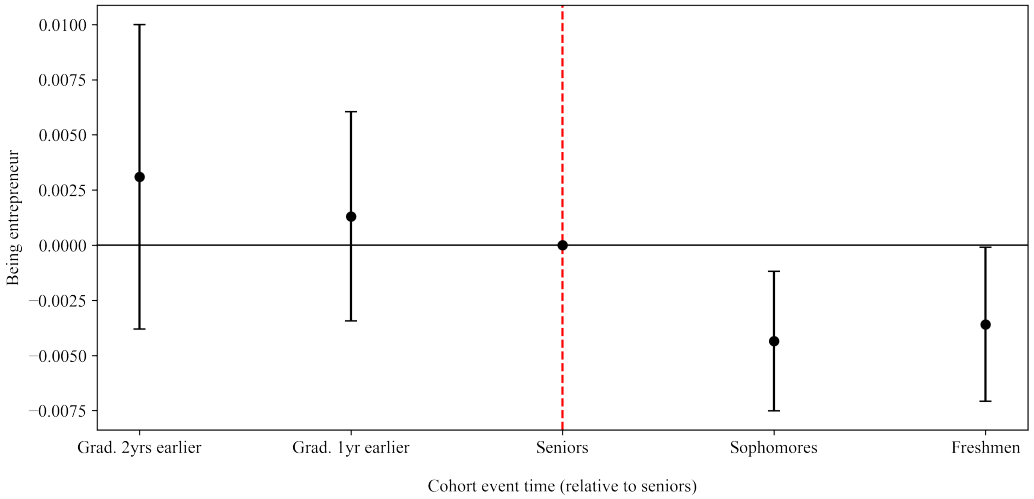
Table 6. Peer Effects versus Entrepreneurship Education

	Full Sample	**Many Prior Peer Experience**	**Few Prior Peer Experience**
Entrepreneurial Sophomore	-0.0024***	-0.0017*	-0.0043***
	(0.0008)	(0.0009)	(0.0012)
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Number of Students	121,470	88,561	32,909
Number of Schools	64	33	31

This table reports linear probability model estimates of immediate entrepreneurship following the introduction of an entrepreneurship major degree. The dependent variable is an indicator equal to one if the individual's first job after college is entrepreneurial. The key independent variable is an indicator for being a sophomore at the time of program exposure, with seniors serving as the omitted category. Schools are classified based on the number of sophomores with prior entrepreneurial experience (defined as participation in small ventures, class projects, or family businesses) prior to or during their second year. "Many prior peer experience" schools are defined as having more than five such students, while "few prior peer experience" schools have five or fewer. All specifications include school fixed effects and major-introduction-year fixed effects grouped into three-year bins. Standard errors are heteroskedasticity-robust and clustered at the school level, and are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively. Results are robust to using five-year experience-year fixed effects, with coefficients remaining significant at the 1% level.

Table 7. Business Dynamism Tests: Pre-Trends and Immediate Business Formation

Panel A: Event-Study Test for Business Dynamism Across Cohorts



Panel B: Immediate Business Formation During Periods of Rising Aggregate Entry

	Any Entrepreneurship	**Small Business**	**Growth Business**
Sophomore vs. Senior	-0.0060***	-0.0048***	-0.0012*
	(0.0016)	(0.0013)	(0.0006)
R^2	0.0129	0.0115	0.0054
Observations	19,811	19,811	19,811
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes

This table examines whether the estimated effects are driven by broader shifts in business dynamism rather than cohort exposure to entrepreneurship education. Panel A presents within-school event-study estimates of the likelihood of becoming an entrepreneur across cohorts during the increase in the business entrance era. The specification mirrors the baseline regression and includes school fixed effects and cohort-year fixed effects, with standard errors clustered at the school level. Seniors (event time 0) serve as the control group. The estimates for cohorts graduating one and two years earlier are statistically indistinguishable from zero, indicating no differential pre-trends in entrepreneurial outcomes across institutions during this period. Panel B reports linear probability model estimates of immediate post-graduation business formation in a restricted sample of entrepreneurship-major schools and program introduction years between 2000 and 2004, a period during which aggregate U.S. business entry rates are rising. For each program introduction year, the comparison is made between seniors graduating in the introduction year and sophomores graduating two years later, over a window in which aggregate entry increases based on prior evidence. The dependent variables indicate whether an individual's first job after graduation is founding any business, a small business, or a growth-oriented business. All specifications include school fixed effects and major=introduction-year fixed effects, where experience years are grouped into three-year bins (results are robust to grouping experience start years into five-year bins). Standard errors are heteroskedasticity-robust and clustered at the school level, and are reported in parentheses. Statistical significance at the 10%, 5%, and 1% levels is denoted by *, **, and ***, respectively.

Table 8. Immediate Entry by Cohort Size

	Large Cohort Schools (> Mean)	**Small Cohort Schools (≤ Mean)**
Sophomore vs. Senior	-0.0023**	-0.0018
	(0.0009)	(0.0015)
R^2	0.0023	0.0042
Observations	79,381	37,429
Number of Schools	22	41
Year FE	Yes	Yes
School FE	Yes	Yes

This table reports linear probability model estimates of immediate entrepreneurial entry, splitting major-program schools by sophomore-plus-senior cohort size. The dependent variable is an indicator equal to one if the individual's first job after college is founding a business. The sample is restricted to sophomores and seniors at schools offering an entrepreneurship major. Large-cohort schools have a sophomore-plus-senior sample size above the across-school mean (1,854); small-cohort schools fall at or below this mean. All specifications include school fixed effects and major-introduction-year fixed effects, with introduction years grouped into three-year bins. Standard errors are heteroskedasticity-robust and reported in parentheses. Statistical significance at the 5% level is denoted by **.

Table 9. Immediate Entry by Institutional Selectivity

	High-SAT Schools (> Mean)	**Low-SAT Schools (≤ Mean)**
Sophomore vs. Senior	-0.0020*	-0.0025**
	(0.0011)	(0.0012)
R^2	0.0014	0.0047
Observations	57,934	51,788
Number of Schools	21	37
Year FE	Yes	Yes
School FE	Yes	Yes

This table reports linear probability model estimates of immediate entrepreneurial entry, splitting major-program schools by institutional selectivity. Selectivity is proxied by the school-level average SAT score, obtained from the U.S. Department of Education's College Scorecard. High-SAT schools have an average score above the across-school mean (1,099.7); low-SAT schools fall at or below this mean. All specifications include school fixed effects and major-introduction-year fixed effects, with introduction years grouped into three-year bins. Standard errors are heteroskedasticity-robust and reported in parentheses. Statistical significance at the 10% and 5% levels is denoted by * and **, respectively.

Table 10. Immediate Entry by Institution Type

	Public Schools	**Private Schools**
Sophomore vs. Senior	-0.0022**	-0.0016
	(0.0009)	(0.0018)
R^2	0.0031	0.0029
Observations	97,215	19,595
Number of Schools	43	20
Year FE	Yes	Yes
School FE	Yes	Yes

This table reports linear probability model estimates of immediate entrepreneurial entry, splitting major-program schools by public versus private institution type. All specifications include school fixed effects and major-introduction-year fixed effects. Standard errors are heteroskedasticity-robust, and reported in parentheses. School counts are not separately tabulated for this split; the sample is restricted to schools with a non-missing public/private classification. Statistical significance at the 5% level is denoted by **.

Table 11. Immediate Entry by Program Vintage: Median Split

	Introduced $\leq$ 2008 (Median Split)	**Introduced $>$ 2008 (Median Split)**
Sophomore vs. Senior	-0.0041***	0.0003
	(0.0011)	(0.0010)
R^2	0.0032	0.0014
Observations	64,365	52,445
Number of Schools	45	18
Year FE	Yes	Yes
School FE	Yes	Yes

This table reports linear probability model estimates of immediate entrepreneurial entry, splitting major-program schools at the median major-introduction year in the sample (2008). All specifications include school fixed effects and major-introduction-year fixed effects, with introduction years grouped into three-year bins. Standard errors are heteroskedasticity-robust, and reported in parentheses. Statistical significance at the 1% level is denoted by ***.

Table 12. Immediate Entry by Program Vintage: Early-Adopter Cutoff

	Introduced $\leq$ 2000	**Introduced $>$ 2000**
Sophomore vs. Senior	-0.0045*	-0.0017**
	(0.0026)	(0.0008)
R^2	0.0030	0.0021
Observations	15,928	100,882
Number of Schools	14	49
Year FE	Yes	Yes
School FE	Yes	Yes

This table reports linear probability model estimates of immediate entrepreneurial entry, splitting major-program schools at a fixed cutoff year (2000) that isolates the earliest-adopting pioneer institutions from the larger cohort of schools that introduced majors during the main wave of adoption. All specifications include school fixed effects and major-introduction-year fixed effects, with introduction years grouped into three-year bins. Standard errors are heteroskedasticity-robust, and reported in parentheses. Statistical significance at the 10% and 5% levels is denoted by * and **, respectively.

Table 13. Immediate Entry by State Establishment Entry Rate

	High Entry Rate ($>$ Mean)	**Low Entry Rate ($\leq$ Mean)**
Sophomore vs. Senior	-0.0023*	-0.0020**
	(0.0012)	(0.0010)
R^2	0.0027	0.0020
Observations	57,645	59,165
Number of Schools	31	32
Year FE	Yes	Yes
School FE	Yes	Yes

This table reports linear probability model estimates of immediate entrepreneurial entry, splitting major-program schools by state-level business dynamism at the time of program introduction. The local-conditions measure is a forward-looking three-year average of the state-level establishment entry rate, beginning in the calendar year of each school's earliest program shock, obtained from the Census Bureau's Business Dynamics Statistics. High-entry-rate states are above the across-school mean (10.57); low-entry-rate states are at or below this mean. All specifications include school fixed effects and major-introduction-year fixed effects, with introduction years grouped into three-year bins. Standard errors are heteroskedasticity-robust and reported in parentheses. Statistical significance at the 10% and 5% levels is denoted by * and **, respectively.

A. Identifying the Introduction of Entrepreneurship Majors

To determine the introduction year of undergraduate entrepreneurship degree programs, historical course catalogs from university archives and official university websites were manually collected. The first year in which an entrepreneurship major explicitly appeared as a degree-granting program was then identified.

Figure A1 illustrates this procedure using Suffolk University as an example. The 1998 undergraduate catalog does not list entrepreneurship as a standalone major, while the 1999 catalog explicitly introduces entrepreneurship as a new major. I therefore code 1999 as the introduction year of the entrepreneurship major at Suffolk University. This approach is applied consistently across all schools in the sample.

Figure A1. Example of Identifying the Introduction of an Entrepreneurship Major



SUFFOLK UNIVERSITY

History Founded to overcome barriers of income and discrimination, Suffolk University has a proud history of enabling its students to become honored members of the academic community, the business world, the professions of accounting, public service, health, law and the judiciary. In 1906, Gleason L. Archer founded Suffolk Law School to make knowledge of the law available to those denied access to a legal education by virtue of social class, religion or income. Archer had come to Boston from rural Maine to study law. He subsequently sought to provide an opportunity for other working students to study law. The response was enthusiastic, eventually prompting him to initiate other programs.

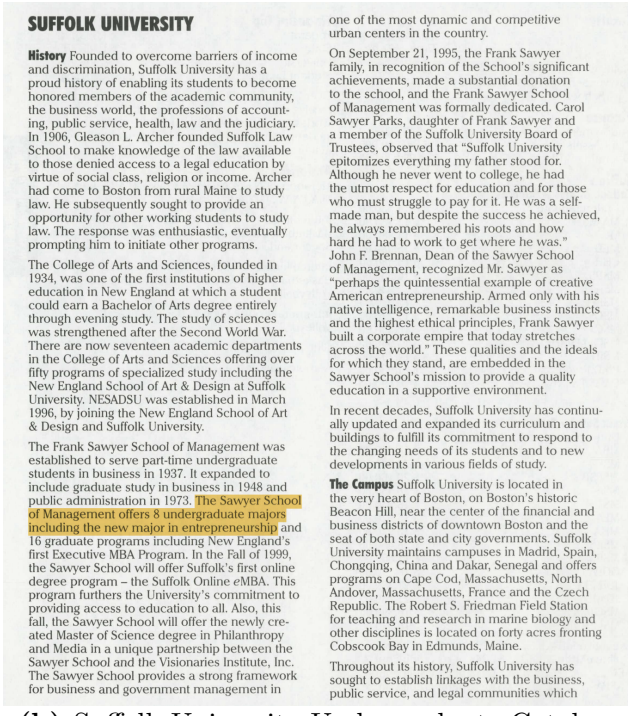
The College of Arts and Sciences, founded in 1934, was one of the first institutions of higher education in New England at which a student could earn a Bachelor of Arts degree entirely through evening study. The study of sciences was strengthened after the Second World War. There are now sixteen academic departments in the College of Arts and Sciences offering over fifty programs of specialized study.

The Frank Sawyer School of Management was established to serve part-time undergraduate students in business in 1937. It expanded to include graduate study in business in 1948 and public administration in 1973. The Sawyer School offered New England's first Executive MBA program in 1978. An undergraduate major in computer information systems was developed in 1981. The Sawyer School provides a strong framework for business and government management in one of the most dynamic and competitive urban centers in the country.

On September 21, 1995, the Frank Sawyer family, in recognition of the School's significant achievements, made a substantial donation to the school, and the Frank Sawyer School of Management was formally dedicated. Carol Sawyer Parks, daughter of Frank Sawyer and a member of the Suffolk University Board of Trustees, observed that "Suffolk University epitomizes everything my father stood for. Although he never went to college, he had the utmost respect for education and for those who must struggle to pay for it. He was a self-made man, but despite the success he achieved, he always remembered his roots and how hard he had to work to get where he was." John F. Brennan, Dean of the Sawyer School of Management, recognized Mr. Sawyer as "perhaps the quintessential example of creative American entrepreneurship. Armed only with his native intelligence, remarkable business instincts and the highest ethical principles, Frank Sawyer built a corporate empire that today stretches across the world." These qualities and the ideals for which they stand, are embedded in the Sawyer School's mission to provide a quality education in a supportive environment.

In recent decades, Suffolk University has continually updated and expanded its curriculum and buildings to fulfill its commitment to respond to the changing needs of its students and to new developments in various fields of study. The Robert S. Friedman Field Station for teaching and research in marine biology and other disciplines was developed on forty acres fronting Cobscook Bay in Edmunds, Maine.

(a) Suffolk University Undergraduate Catalog (1998)



SUFFOLK UNIVERSITY

History Founded to overcome barriers of income and discrimination, Suffolk University has a proud history of enabling its students to become honored members of the academic community, the business world, the professions of accounting, public service, health, law and the judiciary. In 1906, Gleason L. Archer founded Suffolk Law School to make knowledge of the law available to those denied access to a legal education by virtue of social class, religion or income. Archer had come to Boston from rural Maine to study law. He subsequently sought to provide an opportunity for other working students to study law. The response was enthusiastic, eventually prompting him to initiate other programs.

The College of Arts and Sciences, founded in 1934, was one of the first institutions of higher education in New England at which a student could earn a Bachelor of Arts degree entirely through evening study. The study of sciences was strengthened after the Second World War. There are now seventeen academic departments in the College of Arts and Sciences offering over fifty programs of specialized study including the New England School of Art & Design at Suffolk University. NESADSU was established in March 1996, by joining the New England School of Art & Design and Suffolk University.

The Frank Sawyer School of Management was established to serve part-time undergraduate students in business in 1937. It expanded to include graduate study in business in 1948 and public administration in 1973. The Sawyer School of Management offers 8 undergraduate majors including the new major in entrepreneurship and 16 graduate programs including New England's first Executive MBA Program. In the Fall of 1999, the Sawyer School will offer Suffolk's first online degree program – the Suffolk Online *e*MBA. This program furthers the University's commitment to providing access to education to all. Also, this fall, the Sawyer School will offer the newly created Master of Science degree in Philanthropy and Media in a unique partnership between the Sawyer School and the Visionaries Institute, Inc. The Sawyer School provides a strong framework for business and government management in one of the most dynamic and competitive urban centers in the country.

On September 21, 1995, the Frank Sawyer family, in recognition of the School's significant achievements, made a substantial donation to the school, and the Frank Sawyer School of Management was formally dedicated. Carol Sawyer Parks, daughter of Frank Sawyer and a member of the Suffolk University Board of Trustees, observed that "Suffolk University epitomizes everything my father stood for. Although he never went to college, he had the utmost respect for education and for those who must struggle to pay for it. He was a self-made man, but despite the success he achieved, he always remembered his roots and how hard he had to work to get where he was." John F. Brennan, Dean of the Sawyer School of Management, recognized Mr. Sawyer as "perhaps the quintessential example of creative American entrepreneurship. Armed only with his native intelligence, remarkable business instincts and the highest ethical principles, Frank Sawyer built a corporate empire that today stretches across the world." These qualities and the ideals for which they stand, are embedded in the Sawyer School's mission to provide a quality education in a supportive environment.

In recent decades, Suffolk University has continually updated and expanded its curriculum and buildings to fulfill its commitment to respond to the changing needs of its students and to new developments in various fields of study.

The Campus Suffolk University is located in the very heart of Boston, on Boston's historic Beacon Hill, near the center of the financial and business districts of downtown Boston and the seat of both state and city governments. Suffolk University maintains campuses in Madrid, Spain, Chongqing, China and Dakar, Senegal and offers programs on Cape Cod, Massachusetts, North Andover, Massachusetts, France and the Czech Republic. The Robert S. Friedman Field Station for teaching and research in marine biology and other disciplines is located on forty acres fronting Cobscook Bay in Edmunds, Maine.

Throughout its history, Suffolk University has sought to establish linkages with the business, public service, and legal communities which

(b) Suffolk University Undergraduate Catalog (1999)

B. LinkedIn School Matching

B.1. Data Sources

The analysis relies on LinkedIn educational data combined with a curated list of universities offering formal entrepreneurship majors. Specifically:

- **LinkedIn Education Data:** The primary dataset containing user-level education information such as school name, school profile URL, degree name, and field of study.
- **Entrepreneurship Major Schools:** A manually curated Excel file lists 174 universities offering entrepreneurship majors (After matching, the ones with inconsistent and unexpected cohort sizes dropped. Therefore, the earliest shock that contains all schools in the sample has 159 universities in total, which is less than 174 schools.)

B.2. Preprocessing and Data Overview

The first goal is to understand how individuals report their educational institutions on LinkedIn and the resulting variation in school names. To this end, I preprocess the education data as follows:

- Focus on key columns: `school`, `school_profile_url`, `degree_name`, and `field_of_study`.
- Standardize text entries to improve consistency. Given the heterogeneity in how users enter school names on LinkedIn, I implemented the following text cleaning procedure:
  - Convert all text to lowercase.
  - Replace common separators with spaces.
  - Remove punctuation, emojis, numbers, and extra whitespace.
  - Collapse multiple consecutive spaces.

This process was applied to both LinkedIn school names and the curated entrepreneurship school list, generating a `clean_school` field.

B.3. Fuzzy Matching Procedure

To map LinkedIn school entries to the curated university list, I applied fuzzy string matching:

1. Used `RapidFuzz`'s `partial_ratio` scorer for matching university names. This method was chosen because it effectively identifies high similarity even when one string is a substring of another, which is common in LinkedIn data due to abbreviations, missing words, or variations in how schools are reported (e.g., "University of California, Berkeley" vs. "UC Berkeley"). Compared to full-string matching, `partial_ratio` increases recall while maintaining high precision in identifying correct school matches.

2. Batch-matched schools with a threshold score of 85, storing the best match and corresponding score. Applied rules for final selection:

   - Keep all matches with score ≥ 97.7.
   - For $97.5 \leq \text{score} < 97.7$, drop matches containing generic words (e.g., "business school", "state university").
   - Always retain manually validated matches, regardless of score.

B.4. Manual Adjustments and Filtering

Despite high-quality fuzzy matching, several manual corrections were necessary to ensure accurate mapping for large universities with multiple campuses or similarly named institutions. Key adjustments include:

- Corrected ambiguous matches for *University of Pennsylvania*, *Texas A&M*, and *University of Maryland*.
- Excluded non-relevant entries such as high schools, executive programs, and extension schools.
- Applied school-specific filters to remove irrelevant locations or program variants (e.g., *University of Washington* campus extensions, *Florida Atlantic University* non-Atlantic entries).

B.5. Final Dataset Construction

After matching and filtering:

1. Each LinkedIn school entry is mapped to a unique `school_id`.
2. Student counts are aggregated per university to compute sample sizes.
3. A final `match_flag` ensures that the cleaned matched name is consistent with the cleaned original LinkedIn name.

The resulting mapping includes all curated entrepreneurship schools, with each university associated with the number of LinkedIn profiles in the sample. For instance, the top universities by LinkedIn student counts are presented in Figure 2.

B.6. Reproducibility Notes

- All scripts and data cleaning steps are implemented in Python using `pandas`, `pyarrow`, and `rapidfuzz`.
- Intermediate data frames (i.e., `school_value_counts.pkl`, `linkedin_matched.pkl`) are saved for reproducibility and verification.
- Manual acceptance and school-specific rules are documented and applied consistently across the different samples in the dataset (i.e., major, minor, and concentration schools).

This procedure ensures a high-quality mapping between LinkedIn profiles and formal entrepreneurship programs, supporting the subsequent empirical analysis in the paper.

C. LLM-Based Classification of Entrepreneurial Experiences

C.1. Motivation

Section 3.1 classifies each LinkedIn work experience into an entrepreneurial status (entrepreneur vs. non-entrepreneur), a business type (small business vs. growth-oriented business) conditional on entrepreneurship, and an industry category. Rule-based keyword matching on job titles is poorly suited to this task: titles such as "Partner," "Owner," or "Founder" are used inconsistently across contexts, and distinguishing a founder from, for example, a "Sales Partner" at an established public firm requires interpreting the free-text job description rather than the title alone. I therefore use a large language model (LLaMA-4 Scout) to jointly classify entrepreneurial status, business type, and industry from the combination of company name, job title, and job description associated with each LinkedIn experience. This appendix documents the exact prompt, the implementation of the classification pipeline, and the manual validation exercise used to assess its reliability.

C.2. Classification Prompt

Each work experience is passed to the model as a single structured prompt. The prompt fixes the definitions of entrepreneurial status and business type ex ante, so that classification criteria are held constant across the more than 300,000 individuals and multiple cohorts in the sample. The exact prompt template is reproduced below:

LLM Prompt Template

```
PROMPT_TEMPLATE = """
You are a strict classifier. You will be given the work experience of a person.
Experience:
- Company: {company}
- Title: {title}
- Description: {description}
Your tasks:
1. Determine whether this experience indicates TRUE ENTREPRENEURSHIP.
   Definition of entrepreneur:
   - Someone who founded, co-founded, owns, co-owns, or independently operates a
    business.
   - Someone doing "Business Partner" or "Sales Partner" at a large company (e.g.,
    Boston Scientific) is NOT an entrepreneur.
   - Someone who is a partner in a company they *founded* IS an entrepreneur.
2. If the person IS an entrepreneur:
   Classify the BUSINESS TYPE as:
   - "Small Business" -> local shop, services, consulting, freelancing, non-tech small
    company, consulting practice
   - "Growth Business" -> startup, tech-enabled product, high-scale growth potential
3. Classify the INDUSTRY:
   - Software
   - Healthcare
   - Consulting
   - Consumer Goods
   - Finance
   - Real Estate
   - Education
   - Manufacturing
   - Other
Return ONLY a JSON object:
{
  "is_entrepreneur": "Yes" or "No",
  "business_type": "Small Business" or "Growth Business" or null,
  "industry": "Industry Name" or null
}
"""
```

First, the definition of entrepreneurship explicitly excludes titles that superficially resemble founder roles but reflect conventional employment, such as "Business Partner" or "Sales Partner" designations at large, established firms; this guards against overcounting entrepreneurial entry based on title inflation, a known concern with resume- and profile-based data. Second, the small-business/growth-business distinction operationalizes the Hurst and Pugsley (2011) taxonomy used in the main text: the prompt directs the model to classify local, service-based, or self-employed activity as "Small Business" and scalable, technology-enabled ventures as "Growth Business," rather than leaving this distinction to be inferred implicitly from industry codes. Third, the schema is constrained to return a single, machine-parseable JSON object with a fixed set of fields, which allows the output to be validated and merged back into the underlying panel without manual parsing of free-text model output.

C.3. Implementation

- **Model:** LLaMA-4 Scout, queried via API by JetStream, at a temperature of 0 to encourage deterministic outputs across repeated queries on the same input.
- **Inputs:** Company name, job title, and job description exactly as reported on LinkedIn, with no additional preprocessing beyond standard whitespace and encoding cleanup.
- **Output parsing:** The returned JSON object is parsed and validated against the fixed schema; malformed responses are re-queried and flagged for manual review.
- **Scale:** The classifier is applied uniformly to all experiences with possible entrepreneurial keywords in their title or description in the sample, ensuring that classification criteria do not vary across cohorts, schools, or program-introduction timing, which is important for identification since any residual classification error is then orthogonal to the treatment variable (Sophomore vs. Senior status) by construction.

C.4. Manual Validation

To assess the reliability of the LLM classification, I manually review a sample of 15,310 LinkedIn work experiences drawn randomly from the full sample. For each experience in the validation sample, I independently read the company name, title, and description and

assign a manual label for entrepreneurial status, business type, and industry, following the same definitions given to the model in the prompt above. Manual labels are assigned without reference to the model's output to avoid anchoring. Disagreements between the manual labels and the model's classification are concentrated in a small number of similar cases rather than being distributed uniformly across the sample. The most common source of disagreement is early-stage consulting or freelance roles that later evolve into scalable ventures, family-business succession cases where the respondent did not found the firm but assign herself as principal, or dual-role descriptions combining entrepreneurial and wage-employment language in a single entry. Similarly, since the content producers in social media platforms define themselves as the founder of a particular page, these cases were cleaned with manual coding.

C.5. Implications for the Main Results

Two features of the validation exercise are relevant for interpreting the paper's main estimates. First, because the same prompt and model are applied to every possibly entrepreneurial experience regardless of cohort or school, any residual classification error is unrelated to the Sophomore/Senior treatment indicator used in Equation 2, so misclassification should attenuate rather than generate the estimated treatment effects, consistent with the argument in Section C. Second, the match between Crunchbase and the ventures identified within LinkedIn as growth businesses aligned with this classification. More than 90% of the matched businesses were identified as growth businesses. Small businesses that were not included in the tests

Classification scripts, the prompt template, and parsed outputs are implemented in Python, with raw model responses and parsed JSON stored for reproducibility. The manually reviewed validation sample, including reviewer labels and notes for disagreement cases, is retained separately from the model output to allow independent auditing of the accuracy statistics. Manual changes in the data after reviewing this sample are saved in a separate Python notebook.

D. Crunchbase Growth Business Results

Table A1. Crunchbase-Matched Growth-Venture Outcomes: Major Shock

	Immediate	**Within 3 Years**	**Within 5 Years**
Panel A: Received External Funding			
Sophomore vs. Senior	-0.0451	-0.0225	-0.0185
	(0.1816)	(0.0482)	(0.0398)
R^2	0.2751	0.0985	0.0951
Observations	95	444	482
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Panel B: Acquired			
Sophomore vs. Senior	-0.0424	-0.0071	-0.0036
	(0.0606)	(0.0196)	(0.0205)
R^2	0.1860	0.1621	0.1262
Observations	95	444	482
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Panel C: More Than 10 Employees			
Sophomore vs. Senior	0.0087	0.0198	-0.0004
	(0.1153)	(0.0693)	(0.0625)
R^2	0.3935	0.1711	0.1748
Observations	84	409	442
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes

This table reports linear probability model estimates of Crunchbase-recorded outcomes for growth-oriented ventures founded immediately after graduation, within 3 years, and within 5 years of graduation, restricted to the "major degree introduction shock" sample (schools introducing a standalone entrepreneurship major). The sample is restricted to growth-oriented entrepreneurs who are successfully matched to Crunchbase. Panel A reports whether the venture is recorded as having received any external funding round. Panel B reports whether the venture is recorded as having been acquired. Panel C reports whether the venture is recorded as having grown to more than 10 employees. The main explanatory variable is an indicator for being a sophomore, with seniors serving as the control group. All specifications include school fixed effects and major-introduction-year fixed effects, with introduction years grouped into three-year bins. Standard errors are heteroskedasticity-robust and clustered at the school level, and are reported in parentheses. None of the estimated coefficients are statistically distinguishable from zero at conventional significance levels.

Table A2. Crunchbase-Matched Growth-Venture Outcomes: Earliest Shock (Majors, Minors, and Concentrations)

	Immediate	**Within 3 Years**	**Within 5 Years**
Panel A: Received External Funding			
Sophomore vs. Senior	0.0192	0.0151	0.0099
	(0.0757)	(0.0257)	(0.0243)
R^2	0.3136	0.1237	0.1162
Observations	331	1,559	1,683
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Panel B: Acquired			
Sophomore vs. Senior	0.0241	0.0011	-0.0025
	(0.0486)	(0.0140)	(0.0143)
R^2	0.2784	0.1026	0.0913
Observations	331	1,559	1,683
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Panel C: More Than 10 Employees			
Sophomore vs. Senior	-0.0696	-0.0209	-0.0134
	(0.0825)	(0.0296)	(0.0272)
R^2	0.3586	0.1085	0.1088
Observations	295	1,432	1,543
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes

This table reports linear probability model estimates of Crunchbase-recorded outcomes for growth-oriented ventures founded immediately after graduation, within 3 years, and within 5 years of graduation, using the "earliest shock" sample, which includes schools introducing entrepreneurship education through a major, minor, or concentration. The sample is restricted to growth-oriented entrepreneurs who are successfully matched to Crunchbase. Panel A reports whether the venture is recorded as having received any external funding round. Panel B reports whether the venture is recorded as having been acquired. Panel C reports whether the venture is recorded as having grown to more than 10 employees. The main explanatory variable is an indicator for being a sophomore, with seniors serving as the control group. All specifications include school fixed effects and major-introduction-year fixed effects, with introduction years grouped into three-year bins. Standard errors are heteroskedasticity-robust and clustered at the school level, and are reported in parentheses. None of the estimated coefficients are statistically distinguishable from zero at conventional significance levels.

E. Stylized Facts: Entrepreneurship Majors

In this section, I provide a summary of the career trajectories and entrepreneurial activities of graduates who majored in entrepreneurship across over 150 U.S. universities. Starting from the introduction of official entrepreneurship degrees in universities, most U.S. colleges and universities introduced their first formal entrepreneurship programs (i.e., majors, minors, or certificates) between 2001 and 2007, according to the data collected through archival yearly university catalogs. This aligns with evidence from the 2004–2005 OECD Survey of Entrepreneurship Education, which found entrepreneurship curricula were widely established by the mid-2000s and often housed in traditional departments or business schools. The Kauffman Foundation timeline documents the rapid growth of entrepreneurship offerings, from roughly 100 programs in 1975 to over 500 by 2006, alongside a more than twenty-fold increase in course offerings. In addition, a 2005 W. P. Carey School of Business study found that three-quarters of small-business owners held university degrees, and growing ventures were twice as likely to be led by executives with MBAs, implying the demand for higher education in supporting entrepreneurial activity (OECD, 2008; Morelix, 2015; W. P. Carey School of Business, Arizona State University, 2005). These sources collectively support the correct timing of program introductions in the early 2000s in the sample reported in Figure A1, and also with the number of graduates through the years in Figure A2.

Figures A3 through A6 document systematic differences in post-graduation career paths between graduates of undergraduate entrepreneurship programs and other college graduates. While these patterns are descriptive, they closely mirror empirical regularities documented in the finance and economics literature on entrepreneurial selection, experimentation, and persistence.

E.1. Entrepreneurial Entry

Figure A3 shows that entrepreneurship graduates are substantially more likely to have at least one entrepreneurial experience after graduation compared to the overall population of college graduates. This pattern is consistent with a large literature showing that individuals with entrepreneurial preferences and lower aversion to risk are more likely to sort into

entrepreneurship-related occupations (Levine and Rubinstein, 2017; Hvide and Panos, 2014).

This difference likely reflects both selection and treatment effects. On the selection side, individuals with stronger entrepreneurial preferences or lower risk aversion may self-select into entrepreneurship majors (Hvide and Panos, 2014; Levine and Rubinstein, 2017). At the same time, coursework, peer exposure, and access to entrepreneurial networks may reduce informational frictions and psychological barriers to entry, particularly for marginal entrepreneurs who would not otherwise found a business (Astebro et al., 2014). Importantly, Figure A3 captures realized entrepreneurial activity rather than intentions, echoing prior evidence that many latent entrepreneurs never translate interest into action in the absence of institutional support.

E.2. Serial Entrepreneurship

Figure A4 compares the distribution of the number of entrepreneurial experiences across groups. Entrepreneurship graduates are not only more likely to ever enter entrepreneurship, but are also more likely to accumulate multiple entrepreneurial experiences over their careers. In contrast, entrepreneurial activity among other graduates is more concentrated in a single venture.

This pattern aligns with evidence that entrepreneurial skills are learned through experience and are at least partially transferable across ventures (Lafontaine and Shaw, 2016). Learning-by-doing models emphasize that early entrepreneurial experiences generate information about ability and improve managerial skills, increasing persistence even after failure (Manso, 2016). The higher incidence of repeat entrepreneurship among entrepreneurship graduates suggests that these individuals may face lower re-entry costs or perceive lower stigma from failure, both of which can contribute to serial entrepreneurial behavior.

E.3. Timing of Entrepreneurial Entry

Figure A5 documents that entrepreneurship graduates tend to enter entrepreneurship earlier after graduation. A larger share of entrepreneurship graduates found their first venture within a few years of completing college, whereas entrepreneurial entry among other graduates is more dispersed over the life cycle.

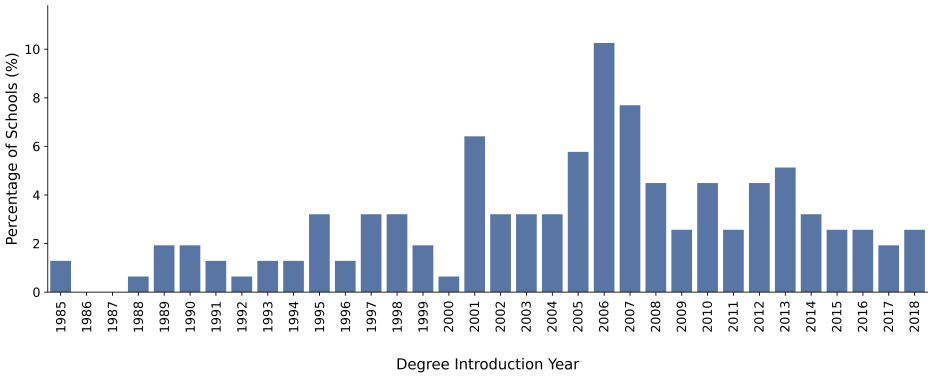
This timing difference is consistent with evidence that entrepreneurial entry decisions are shaped by opportunity costs, uncertainty, and learning dynamics. Early exposure to entrepreneurship may reduce uncertainty about entrepreneurial ability and lower the perceived cost of experimentation (Astebro et al., 2014). In addition, prior work shows that entrepreneurial outcomes vary systematically with age and career stage, reflecting trade-offs between learning, risk, and outside options (Azoulay et al., 2020; Hurst and Pugsley, 2011). Earlier entry among entrepreneurship graduates may also reflect the importance of university-based networks and peer effects, which are most salient immediately after graduation.

E.4. Industry Sorting

Figure A6 compares the industry composition of entrepreneurial experiences across groups. While there is substantial overlap, entrepreneurship graduates are relatively more concentrated in business services, technology, and consumer-oriented sectors.

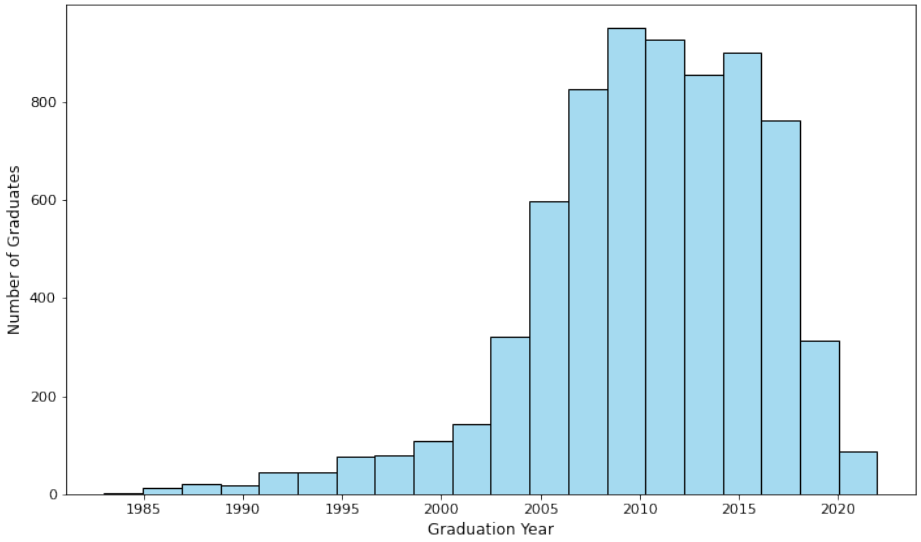
This pattern is consistent with prior work showing that opportunity-driven entrepreneurship is more prevalent in sectors with lower fixed costs of experimentation and a greater reliance on human capital (Hurst and Pugsley, 2011). These industries may be particularly attractive to entrepreneurship graduates whose training emphasizes opportunity recognition, business planning, and scalable venture models. More broadly, the observed industry sorting suggests that entrepreneurship education may influence not only whether individuals become entrepreneurs, but also the types of ventures they pursue.

Figure A1. Distribution of Earliest Shock Years Across Schools



This figure shows the distribution of the earliest year in which a university in the sample introduces any formal undergraduate entrepreneurship program (major, minor, or concentration). Each observation corresponds to a university, and the shock year is identified using archival undergraduate course catalogs. The distribution highlights the clustering of program introductions in the early 2000s, consistent with the rapid expansion of entrepreneurship education documented in prior anecdotal evidence.

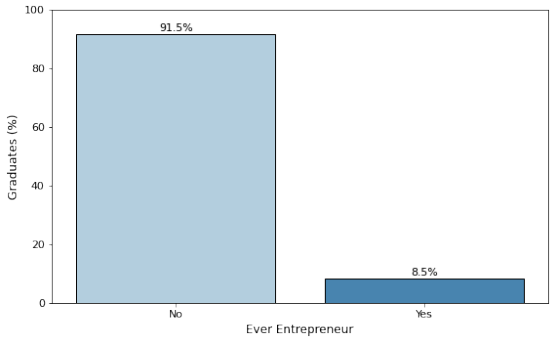
Figure A2. Entrepreneurship Students Graduation Year Distribution



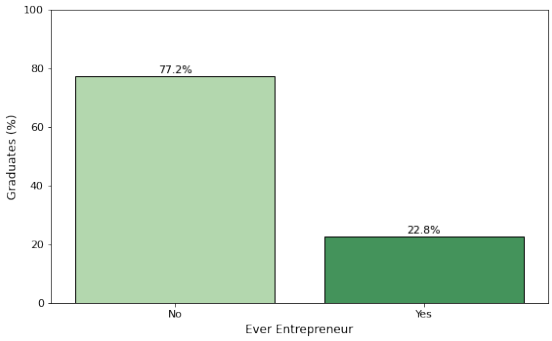
This figure plots the distribution of graduation years for students who complete an undergraduate entrepreneurship degree. The figure illustrates that the bulk of entrepreneurship graduates emerge after the early 2000s, reflecting both the timing of program introductions shown in Figure A1 and the subsequent growth in cohort sizes as programs mature.

Figure A3. Comparison of Having at Least One Entrepreneurial Experience After Graduation

(a) All Graduates with at Least One Entrepreneurial Experience



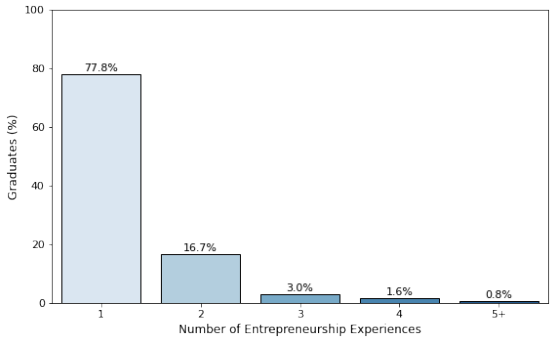
(b) Entrepreneurship Graduates with at Least One Entrepreneurial Experience



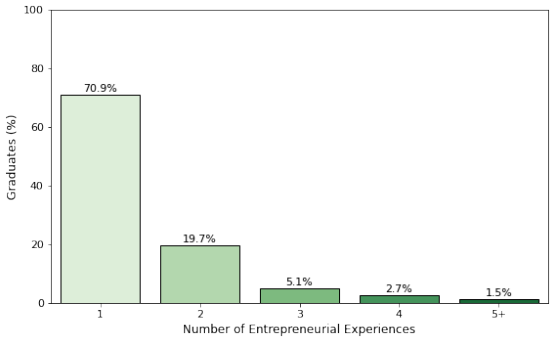
Panel (a) shows the share of all graduates who have at least one entrepreneurial experience at any point after graduation, while Panel (b) reports the same measure for graduates who completed an entrepreneurship degree. An entrepreneurial experience is defined as holding a founder or co-founder role in a business, as identified from LinkedIn career histories. Entrepreneurship graduates are substantially more likely to ever engage in entrepreneurship relative to the overall graduate population, confirming selection bias.

Figure A4. Comparison of the Number of Entrepreneurship Experience Distribution

(a) Distribution of Entrepreneurship Experiences of All Graduates



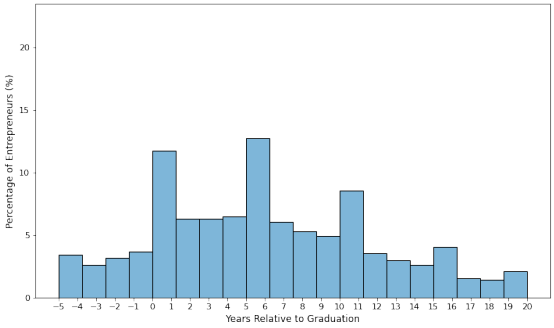
(b) Distribution of Entrepreneurship Experiences of Entrepreneurship Graduates



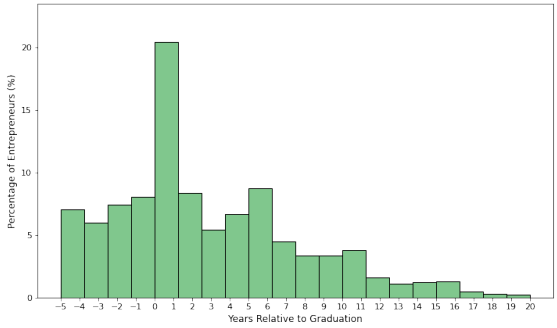
Panel (a) presents the distribution of the total number of entrepreneurial experiences among all graduates who have ever entered entrepreneurship, while Panel (b) reports the corresponding distribution for entrepreneurship graduates. The figure shows that entrepreneurship graduates are more likely to accumulate multiple entrepreneurial experiences over their careers, whereas most entrepreneurs in the broader graduate population have only a single entrepreneurial spell.

Figure A5. Comparison of Time to First Entrepreneurial Experience

(a) Time to First Entrepreneurial Experience of All Graduates



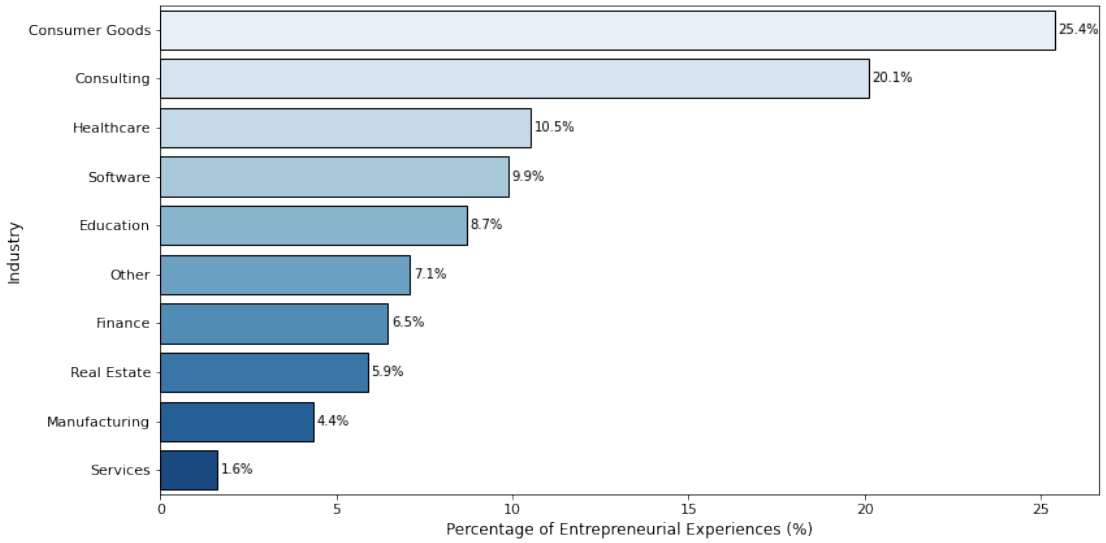
(b) Time to First Entrepreneurial Experience of Entrepreneurship Graduates



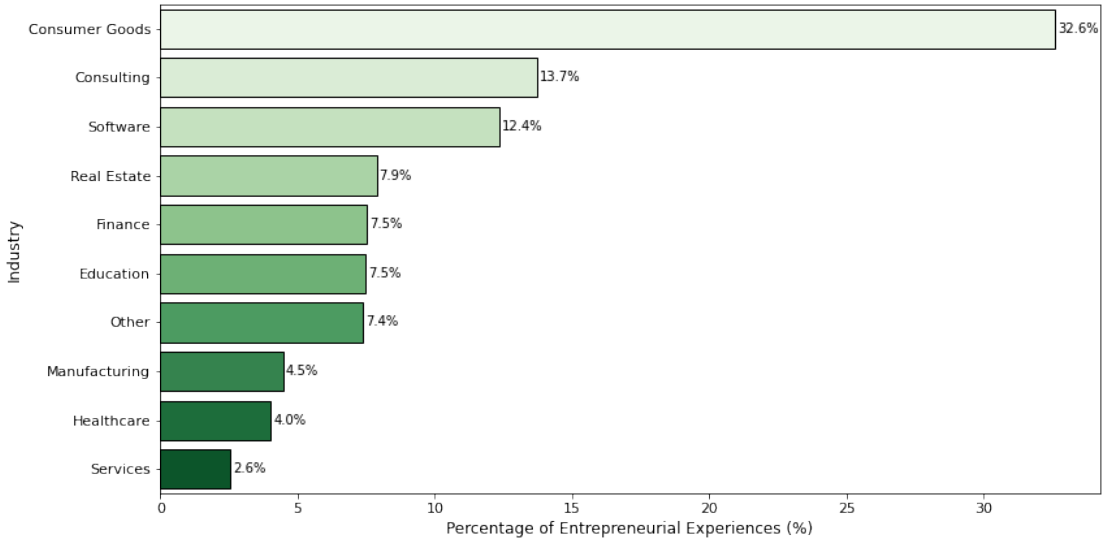
Panel (a) shows the distribution of the number of years between graduation and first entrepreneurial experience for all graduates who have ever become entrepreneurs, while Panel (b) reports the same distribution for entrepreneurship graduates. Entrepreneurship graduates tend to enter entrepreneurship earlier in their careers, with a larger share founding their first venture shortly after graduation.

Figure A6. Comparison of Top 10 Industries for Entrepreneurship Experiences

(a) Top 10 Industries of All Graduates



(b) Top 10 Industries of Entrepreneurship Graduates



This figure reports the top ten industries in which entrepreneurial experiences occur for all graduates (Panel A) and entrepreneurship graduates (Panel B). Industries are classified based on LinkedIn business categories. While there is substantial overlap across groups, entrepreneurship graduates are relatively more concentrated in business services, technology, and consumer-oriented industries, indicating systematic differences in the types of ventures pursued.