Does Teaching Entrepreneurship Produce Entrepreneurs?*

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I examine whether undergraduate entrepreneurship education increases business formation, exploiting the staggered introduction of entrepreneurship programs at over 150 U.S. universities. Using newly assembled data linking historical university catalogs to LinkedIn career histories for more than 300,000 graduates, I implement a within-university, adjacent-cohort difference-in-differences design. Program introductions reduce immediate business formation by 7.5 percent, with no effect on long-run entry or firm survival. Text-based classification via a large language model indicates the decline is driven by small-business entry, with no offsetting increase in growth-oriented entrepreneurship. These findings suggest university entrepreneurship education has limited effectiveness as a policy tool for stimulating new business creation or survival.

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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

The creation of new businesses, especially innovative ones, is seen as one of the most important factors in 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. A central question in this effort is whether entrepreneurship can be taught. The answer has direct implications for education policy, business dynamism, and long-run macroeconomic outcomes.

Universities have responded to this interest by rapidly expanding formal entrepreneurship education. 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," and "not only teach them about entrepreneurship but make them entrepreneurs," "provide the business training needed to launch and grow a successful enterprise."[4] Despite their growing prevalence and strong claims, there is limited causal evidence on whether these degree programs actually increase entrepreneurial activity or improve entrepreneurial outcomes.

This paper studies whether formal undergraduate entrepreneurship education affects business formation and entrepreneurial performance. Using the staggered introduction of entrepreneurship major degree programs across U.S. universities, I examine how exposure

[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, University of Illinois Chicago, in the same order.

to entrepreneurship education influences entry into entrepreneurship, the types of firms founded, and firm survival. The analysis leverages a novel dataset that links the timing of entrepreneurship degree introductions to individual-level LinkedIn career histories for over 300,000 graduates from more than 150 universities.

The main results are stark. While raw correlations suggest that students who choose entrepreneurship majors are more likely to start businesses, these associations reflect selection and cannot be interpreted causally. Exploiting quasi-experimental variation in exposure timing through a difference-in-differences design, which compares adjacent cohorts within the same university, yields sharply different conclusions. Exposure to entrepreneurship education reduces immediate post-graduation entrepreneurial entry, an effect driven entirely by a decline in small-business formation, with no impact on entry into growth-oriented ventures. Moreover, this short-run decline is not offset over time: exposure has no effect on cumulative firm formation within three, five, or seven years after graduation, indicating that these programs do not increase the long-run supply of entrepreneurs. Because the DiD compares cohorts within the same university, endogeneity in students' college or major choices is mitigated, providing a clean assessment of program effects.

I further examine whether entrepreneurship education improves outcomes among those who do start businesses. Conditional on becoming an entrepreneur, exposure to entrepreneurship education does not increase firm survival at one-, three-, or five-year horizons. If anything, survival probabilities are modestly lower at longer horizons, particularly for small businesses, while survival among growth-oriented ventures is unaffected. These findings indicate that entrepreneurship education does not improve post-entry firm performance and instead primarily discourages marginal entrepreneurial entry.

These results contribute to the long-standing debate over whether entrepreneurship stems from nature or nurture. A large 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.[5] These limitations make it difficult to establish whether entrepreneurship education causally affects entrepreneurial behavior at scale.

This paper addresses these challenges by exploiting the timing of entrepreneurship degree introductions within universities as a quasi-experimental design. The empirical strategy 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.[6]

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 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. On the one hand, 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 on the other hand 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 2001, seniors, will be the control group. The differences-in-differences 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 know about the entrepreneurship

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

[6]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.

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 or exogenous 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 2022, allowing for the study of immediate entry, delayed entry, and firm survival.[7] 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 firms into risky, high-technology startups and more traditional small ventures. This distinction allows the analysis to separate discouragement of marginal entrepreneurship from potential shifts toward higher-quality or higher-growth ventures.

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. In

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

addition, a series of checks addressing potential measurement and selection concerns inherent to LinkedIn data, including differential cohort coverage, incomplete education histories, and selective reporting of failed ventures, leave the results qualitatively unchanged. These exercises confirm the conclusion that the estimated effects reflect the impact of exposure to entrepreneurship education rather than artifacts of data construction or sample composition.

Lastly, I document several stylized facts about the career outcomes of students who participated in the entrepreneurship program. Relative to other college graduates, entrepreneurship students are substantially more likely to engage in entrepreneurial activity after graduation, enter entrepreneurship earlier in their careers, and accumulate multiple entrepreneurial experiences over time. They also exhibit more diverse early career paths, consistent with greater occupational experimentation, and sort disproportionately into business services, technology, and consumer-oriented industries. While these patterns are descriptive and reflect both selection and treatment effects, they closely mirror well-established regularities in the entrepreneurship literature and motivate the empirical strategy of the paper to identify the causal impact of entrepreneurship education on early-career entrepreneurial outcomes.

This paper contributes to several strands of the literature. A closely related literature studies entrepreneurship education and business training programs, primarily through targeted interventions. Karlan and Valdivia (2011) and de Mel et al. (2014) evaluate randomized business training programs in developing-country settings and document improvements in business practices and knowledge, with more limited and heterogeneous effects on profits, firm growth, and long-run outcomes. More recent work explores complementarities between training and access to finance (Ashraf and Lyons, 2026). While these studies provide valuable insights into mechanisms, they often focus on short-term outcomes, small-scale programs, or highly specific populations (Lyons and Zhang, 2018).

It also adds to the growing debate on the determinants of entrepreneurship: nature versus nurture. While prior research has examined the role of knowledge spillovers, labor market shocks, and early exposure in shaping entrepreneurial outcomes, the literature remains inconclusive on whether formal education in entrepreneurship itself can influence entrepreneurial performance. For example, Gofman and Jin (2024) look at how losing AI professors shapes whether students go on to start AI companies, showing how much

entrepreneurial success depends on knowledge transmission. Similarly, Babina and Howell (2024) focus on how corporate R&D spills over when employees move between firms, again implying the importance of skills and know-how for new startups. This paper complements these findings by causally assessing the role of an institutionalized version of knowledge transmission (entrepreneurship degree programs) in fostering entrepreneurial skills.

Other factors, such as labor market conditions and early exposure, have also been shown to affect entrepreneurial entry. Hacamo and Kleiner (2022) demonstrate that graduating during recessions can push highly skilled individuals into entrepreneurship, producing more successful firms than voluntary entrepreneurs. Mertz et al. (2024) show that having entrepreneurial peers early on makes women more likely to go into entrepreneurship and build successful firms. Also, Wallskog (2025) finds that workplace ties to entrepreneurial coworkers boost both entry and firm outcomes, especially within demographic groups. Building on these findings, this paper asks whether formal degree programs can play a similar, or even stronger, role than these more organic forms of entrepreneurial networks in shaping who starts firms.

This paper also complements recent work that examines the effects of university entrepreneurship programs, most closely Eesley and Lee (2021). They study the introduction of entrepreneurship programs at the business and engineering schools at Stanford University using alumni survey data and a school-level instrumental variables strategy, finding limited effects on entrepreneurship rates but improvements in entrepreneurial quality. The present setting differs along several important dimensions. First, it examines the introduction of formal entrepreneurship degree programs at scale, across many universities, rather than a small number of initiatives at a single elite institution. Second, while Eesley and Lee (2021) rely on retrospective alumni surveys, large-scale professional data are used here, enabling entrepreneurial entry to be tracked in a comprehensive and systematic way. Third, the identification strategy is more localized by comparing adjacent cohorts within the same university rather than pooling all graduates before and after program introduction. This design sharply limits compositional and time-trend concerns, such as declining business dynamism. Together, these differences allow for assessment of the aggregate, economy-wide implications of institutionalized entrepreneurship education, rather than its effects in a single, highly selected environment.

The results in this paper suggest that entrepreneurship education primarily operates as a screening mechanism rather than a catalyst for entry. By providing structured information about the risks, costs, and low average returns of small-scale entrepreneurship, these programs discourage marginal and likely unsuccessful entry without stimulating higher-growth entrepreneurial activity. From a policy perspective, this evidence challenges the view that expanding entrepreneurship degree programs will increase firm creation. While such programs may improve decision-making and reduce inefficient entry, they do not raise aggregate entrepreneurial activity and may reduce small-business formation, raising important questions about how universities and policymakers should define the objectives and evaluate the success of formal entrepreneurship education.

The remainder of the paper is organized as follows. Section 2 develops the hypotheses. Section 3 describes the setting and the construction of a novel dataset on entrepreneurship degree introductions and individual entrepreneurial outcomes. Section 4 outlines the empirical strategy. Section 5 presents the main results, Section 6 reports a series of robustness analyses that distinguish the effects of formal entrepreneurship education from peer influences, broader trends in business dynamism, and any data artifacts, and Section 7 provides stylized facts for entrepreneurship major graduates. Section 7 concludes with a discussion of implications for education policy and firm formation.

2. Hypothesis Development

Entrepreneurial entry is a decision made under uncertainty, in which individuals compare the expected returns to founding a firm with alternative career options. These expected returns depend not only on entrepreneurial ability but also on beliefs about success probabilities, costs, and risk. A large literature documents that prospective entrepreneurs tend to hold overly optimistic beliefs about their likelihood of success, which can lead to excessive entry relative to ex post outcomes (Camerer and Lovallo, 1999; Koellinger et al., 2007). Formal entrepreneurship education can affect this decision problem through multiple channels, and these channels need not operate in the same direction.

A useful distinction for understanding these effects is between short-run entry decisions, which reflect initial experimentation, and longer-run outcomes, which reflect learning, selection,

and persistence. The hypotheses below are organized around this distinction and are closely aligned with the empirical tests in the paper.

2.1. Entrepreneurship Education and Short-Run Entry

One channel through which entrepreneurship education may affect entry is human capital accumulation. Coursework in entrepreneurship can build skills relevant for starting a business, including opportunity identification, managerial ability, and financial literacy, which increase expected productivity conditional on entry. Empirical evidence shows that exposure to entrepreneurship education is associated with higher entrepreneurial self-efficacy and perceived behavioral control, outcomes commonly viewed as important antecedents to entrepreneurial entry (Souitaris et al., 2007). Experimental evidence from developing countries further suggests that training programs often accelerate business start-up shortly after participation, particularly among new or marginal entrepreneurs (de Mel et al., 2014; Lyons and Zhang, 2018).

Entrepreneurship programs that are introduced during undergraduate years intersect with the time when students are making early career choices and face substantial uncertainty about their post-graduation paths. By reducing informational frictions and psychological barriers to entry, such programs may induce individuals who would otherwise delay entrepreneurship to start businesses immediately after graduation. Importantly, this mechanism operates through changes in perceived costs and uncertainty rather than permanent changes in ability or preferences.

At the same time, entrepreneurship education can also discourage immediate entry. Exposure to realistic information about failure rates, capital constraints, and the demands of entrepreneurship may correct overly optimistic beliefs, leading some individuals to opt out of starting a business. Which effect dominates is ultimately an empirical question. However, existing evidence suggests that training programs often shift the timing of entry, accelerating experimentation even when long-run ownership rates do not increase.

H_0**:** Exposure to university entrepreneurship education increases business formation in the short run.

2.2. Learning, Selection, and Long-Run Entry

While entrepreneurship education may stimulate early experimentation, its implications for long-run firm ownership are theoretically ambiguous. In standard learning and selection models, individuals enter entrepreneurship with imperfect information about their own ability and the distribution of returns. As they accumulate information through experience, lower-ability or poorly matched entrepreneurs are more likely to exit, generating substantial turnover in ownership over time. In this framework, education and early exposure can accelerate learning by improving beliefs about entrepreneurial fit and expected payoffs, leading to better initial decision rather than higher entry.

Recent empirical evidence supports this interpretation. Von Graevenitz et al. (2010) show that entrepreneurship education can reduce entrepreneurial intentions for a subset of students by correcting overly optimistic beliefs, even as it raises entrepreneurial skills and knowledge. Using survey data from Stanford University alumni, Eesley and Lee (2021) find that entrepreneurship programs do not increase overall entrepreneurship rates, but are associated with higher firm survival and revenues among entrants for business school graduates, consistent with improved selection rather than extensive-margin effects. Experimental evidence also points in this direction. de Mel et al. (2014) show that business training accelerates entry among potential entrepreneurs, but does not lead to higher long-run ownership rates, as many entrants subsequently exit.

Taken together, this evidence suggests that entrepreneurship education may encourage early experimentation without increasing the total number of individuals who remain entrepreneurs in the long run. Short-run increases in business formation may therefore disappear over time as learning and selection occur.

H_1**:** The positive effect of university entrepreneurship education on business formation diminishes over longer horizons.

2.3. Peer Effects and Entrepreneurial Learning

Entrepreneurial learning may occur not only through formal coursework, but also through peer interactions. Students are exposed to information about entrepreneurship through class-

mates' experiences, informal discussions, and observation of peers who are actively experimenting with venture creation. Such peer-based learning can shape beliefs about entrepreneurial feasibility, risk, and expected returns independently of structured entrepreneurship education.

If entrepreneurship education primarily operates through belief updating or screening mechanisms, these effects may interact with the surrounding peer environment. In settings with greater exposure to entrepreneurial peers, students may already observe the challenges and risks of entrepreneurship through close social contacts. As a result, the informational content of formal entrepreneurship education may be reinforced, potentially amplifying screening effects and discouraging marginal entry (Lerner and Malmendier, 2013).

To examine this mechanism, one can exploit variation in entrepreneurial peer exposure across universities. By identifying sophomores with entrepreneurial experience prior to or during their second year (including participation in small ventures, entrepreneurship-related class projects, or involvement in family businesses), schools with higher entrepreneurship interaction could be determined. Following the prior literature, these students are treated as the peer exposure group. There is substantial variation across institutions in the prevalence of such students, allowing for a comparison between environments with relatively high and low levels of peer-based entrepreneurial learning.

H_2**:** The effect of university entrepreneurship education on business formation differs by the level of entrepreneurial peer exposure, with stronger screening effects in environments with greater peer exposure.

2.4. Implications for Extensive and Intensive Margins

The mechanisms described above have distinct implications for extensive- and intensive-margin outcomes. If entrepreneurship education and peer exposure jointly screen out poorly matched individuals while encouraging experimentation among more capable ones, entry on the extensive margin may decline or remain unchanged, even as outcomes among surviving firms improve. In this case, negative or null effects on entry may coexist with positive effects on firm quality, survival, or scale.

Conversely, if education primarily lowers entry barriers without meaningful screening, one

would expect positive effects on both extensive-margin entry and intensive-margin outcomes. In the first part of the paper focuses on extensive-margin business formation consequences by separately examining short-run entry, long-run entry, and differences across peer environments; then, test for survival rates to explore intensive margin effects of these programs.

H_3**:** Exposure to undergraduate entrepreneurship programs increases firm survival among new ventures. If these programs primarily act as a screening mechanism, we expect survival rates to be unchanged or modestly lower, particularly for small-scale businesses, reflecting the discouragement of marginal entry rather than improvements in intensive-margin outcomes.

3. Setting and Data

In this paper, LinkedIn dataset collected by Nubela data company in 2021 that contains self-reported information on individuals' educational backgrounds and professional experiences in the United States used as main data source. 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 are constructed.

To determine when each university began offering a formal entrepreneurship degree, university course catalogs dating back to the 1950s are collected. 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 internet appendix, figure 12. These program introduction years are then merged with individuals' education and employment histories from LinkedIn. This setting creates a quasi-experimental 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 that contains sophomores, juniors, and seniors around the introduction forms the basis of my baseline ordinary least squares (OLS) regressions that suffer from selection bias and the difference-in-differences (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 two 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. I reconfirm these issues with statistical analyses to demonstrate that the findings are not driven by systematic reporting errors or sample selection.

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 research universities, such as Arizona State University, Purdue 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, 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) are represented in proportions that align closely with their relative size and selectivity. The sample also includes smaller, specialized institutions with strong academic or entrepreneurial reputations, such as Babson College, whose lower absolute graduation counts are nonetheless consistent with their focused missions.

A small number of institutions (e.g., Thomas Edison State College, Troy State University, Cal State 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 firm characteristics. This approach allows moving beyond self-reported entrepreneurship and systematically distinguishing between qualitatively different forms of new business formation.

The classification 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, consulting, and freelancing. 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 JSON 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 firm descriptions. These checks confirm that the model accurately identifies entrepreneurial roles and meaningfully distinguishes between small, non-scalable businesses and growth-oriented,

scalable ventures. Misclassification is rare for clearly defined cases and is primarily confined to borderline situations, such as early-stage consulting firms that later transition into scalable startups.

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 firm type using large professional datasets.

4. Empirical Strategy

The effects of formal entrepreneurship education on business formation are analyzed in two steps. I first document selection into entrepreneurship majors using OLS, which provides a benchmark for the magnitude of pre-existing differences between students who choose entrepreneurship degrees and their peers. I then estimate causal effects using quasi-experimental 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 to 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:

$$Y_i = \alpha + \beta \cdot \text{Entrepreneurship Major}_i + \mu_s + \tau_t + \varepsilon_i, \tag{1}$$

where Y_i denotes a binary indicator for entrepreneurial outcomes of individual i. 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.

Entrepreneurship Major$_i$ is an indicator equal to one if the individual majored in entrepreneurship. All specifications include school fixed effects μ_s, absorbing time-invariant differences across institutions, and venture-start-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.

Equation 1 is estimated separately for freshman and sophomore cohorts. These estimates are descriptive and reflect substantial 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.[8]

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.

The baseline estimating equation for immediate entry is:

$$ImmediateEntrepreneur_{is} = \alpha + \beta \cdot \text{Sophomore}_{is} + \mu_s + \tau_t + \varepsilon_{ist}, \tag{2}$$

where $ImmediateEntrepreneur_{is}$ is an indicator equal to one if individual i from school s . The variable Sophomore$_{is}$ equals one if the individual was a sophomore at the time the entrepreneurship program was introduced and zero if the individual was a senior. The fixed effects μ_s capture time-invariant school characteristics, while τ_t denotes experience-start-year fixed effects. Standard errors are clustered at the school level.

[8]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.

4.3. Firm Type and Conditional Specifications

To distinguish between different forms of entrepreneurship, I extend Equation 2 by conditioning on entrepreneurial entry and estimating separate effects for small business and growth-oriented firm formation. Specifically, I estimate:

$$SmallBusiness_{is} = \alpha + \beta \cdot \text{Sophomore}_{is} + \mu_s + \tau_t + \varepsilon_{ist}, \tag{3}$$

$$GrowthBusiness_{is} = \alpha + \beta \cdot \text{Sophomore}_{is} + \mu_s + \tau_t + \varepsilon_{ist}, \tag{4}$$

where $SmallBusiness_{is}$ and $GrowthBusiness_{is}$ are indicators equal to one if entrepreneur i starts a small or growth-oriented business, respectively. These regressions are estimated conditional on entering entrepreneurship, allowing to isolate changes in the composition of entrepreneurial entry.

4.4. Long-Run Business Formation

Finally, whether exposure to entrepreneurship education affects business formation over longer horizons is investigated. The following linear probability models of the form:

$$PostGradEntrepreneur_{is}^{(k)} = \alpha + \beta \cdot \text{Sophomore}_{is} + \mu_s + \tau_t + \varepsilon_{ist}, \tag{5}$$

where $PostGradEntrepreneur_{is}^{(k)}$ is an indicator equal to one if individual i founds a business within $k \in \{3, 5, 7\}$ years of graduation. The specification mirrors Equation 2, with the same set of fixed effects and clustering structure.

Across all DiD specifications, the coefficient β captures the causal effect of exposure to entrepreneurship education on business formation outcomes. By examining immediate entry, firm type, and longer-run entrepreneurship, this framework provides a test for hypotheses that were introduced earlier.

4.5. Business Survival by Exposure Timing

To assess whether entrepreneurship education affects outcomes beyond initial entry, I examine firm survival among individuals who become entrepreneurs. The analysis focuses on whether the timing of exposure to entrepreneurship education influences the likelihood that a newly founded business survives for a given number of years.

For each entrepreneur i, I estimate the following linear probability model:

$$Survive_{i,k} = \alpha + \beta \cdot Sophomore_i + \mu_s + \tau_t + \varepsilon_i, \tag{6}$$

where $Survive_{i,k}$ is an indicator equal to one if the business survives at least $k \in \{1, 3, 5\}$ years following founding. $Sophomore_i$ is an indicator for belonging to the cohort exposed to entrepreneurship education at the time of program introduction, with seniors serving as the control group.

The sample is restricted to individuals who become entrepreneurs and attend universities that introduce a standalone entrepreneurship major. I estimate the model separately for all businesses and by business type, distinguishing between small businesses and growth-oriented ventures.

4.6. 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 course-based 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 to test 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.

The empirical results show that this is not the case. Effects are concentrated in the earliest shock and major specifications, while minors and concentrations exhibit no statistically significant impact on entrepreneurial entry or firm formation. This pattern is inconsistent with explanations based on peer composition or gradual institutional changes and instead points to the role of formal, intensive entrepreneurship education.

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 experience-start-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 provides a benchmark for the magnitude of selection into entrepreneurship majors. It then turns to causal estimates that exploit quasi-experimental variation in exposure timing to entrepreneurship programs, distinguishing between immediate entry and longer-run firm formation.

5.1. OLS Evidence and Selection into Entrepreneurship Majors

Table 2 reports OLS estimates relating 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 and 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, which exploit plausibly exogenous variation in program availability.

5.2. Immediate Business Formation

Before presenting the main DiD estimates, it is important to validate the parallel trends assumption. Event-study estimates of immediate business entry across cohorts relative to the introduction of entrepreneurship education at a given institution show that 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 underlying assumption of the setting. Focusing on institutions that introduce entrepreneurship as a standalone major further isolates the effect of intensive entrepreneurship education and avoids potential confounding from overlapping or staggered introductions of minors or concentrations (see Figure 4).

Table 3 reports DiD estimates of immediate post-graduation business formation, exploiting variation in exposure timing to entrepreneurship programs. I compare sophomores, who are plausibly exposed to the program, to seniors within the same institution, who serve as the control group. All specifications include school fixed effects and experience-start-year fixed effects (solely experience start year, as well as, years put into 3 years and 5 years 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 firm 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.

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 Firm Formation

Table 4 examines whether the short-run decline in entrepreneurial entry is offset by increased firm 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 firm 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-experimental exposure to these programs reduces immediate entry and has no effect on longer-run firm formation. These patterns are consistent with entrepreneurship education shaping selection into entrepreneurship rather than increasing aggregate entrepreneurial activity.

5.4. Firm Survival

This section examines whether exposure to entrepreneurship education improves firm survival among individuals who become entrepreneurs by discussing results provided in Table 5. If entrepreneurship education enhances entrepreneurial skill or leads to selection into higher-quality ventures, one would expect higher survival rates among businesses founded by exposed cohorts.

The results provide little support for this hypothesis. Across all specifications, exposure to entrepreneurship education does not increase firm survival at one-, three-, or five-year horizons. For the full sample of businesses, survival differences between exposed and unexposed cohorts are small and statistically indistinguishable from zero at short horizons. At the five-year horizon, survival is modestly lower for exposed cohorts, with a decline of approximately 1.7 to 1.8 percentage points.

Disaggregating by business type yields a similar pattern. Among small businesses, exposure is associated with slightly lower survival at longer horizons, while short-run survival differences are negligible. In contrast, survival among growth-oriented ventures is unaffected by exposure timing at all horizons, with estimated coefficients close to zero and imprecisely estimated.

Even at the longest horizon, the implied magnitudes are economically modest, indicating that entrepreneurship education does not meaningfully improve post-entry firm durability. Combined with earlier evidence showing reduced immediate entry and no long-run increase in business formation, these results suggest that entrepreneurship education primarily discourages marginal entry without generating offsetting gains in entrepreneurial performance.

6. Robustness

This section examines alternative explanations for the main findings and assesses the robustness of the estimated effects of entrepreneurship education. In particular, I address two concerns. First, the results may reflect peer effects rather than the causal impact of formal entrepreneurship education. Second, the negative estimates may be driven by broader changes in business dynamism that coincide with program introduction. The following subsections evaluate these possibilities using subsample analyses, event-study designs, and periods of

rising aggregate business entry.

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) identifies 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 possibly 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. The magnitude of the estimated effect is similar across schools with high and low peer exposure and does not decrease in settings with more entrepreneurial peers. If peer effects were the primary driver of the main results, one would expect the impact of formal entrepreneurship education to be weaker in environments with greater pre-existing entrepreneurial exposure. The absence of such a pattern suggests that peer composition alone does not account for the estimated effects.

This finding is informative for interpretation. Consistent with the selection mechanism emphasized by 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 firm 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. Panel A presents a within-school event-study analysis that examines entrepreneurial entry across cohorts during the period of rising aggregate business entry, confirming no paralel trend assumption for the schools within the period. 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 restricts the sample to entrepreneurship-major schools and program introduction years between 2000 and 2004, which graduation years of cohorts fall within the period of rising aggregate entry documented in prior work. 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 varies systematically across cohorts. In particular, younger cohorts are more likely to maintain LinkedIn profiles than older cohorts, implying that seniors may be underrepresented relative to sophomores. This feature of the data could mechanically bias estimates of entrepreneurial entry if people who are not on LinkedIn are not taken into account. To address this concern, I exploit the maintained assumption that individuals who become entrepreneurs are disproportionately likely to maintain a LinkedIn presence, given the platform's professional and networking orientation. Under this assumption, missing observations among seniors primarily reflect individuals who did not engage in entrepreneurship. I therefore augment the senior cohort by adding observations assumed to have zero entrepreneurial activity, effectively equalizing cohort sizes. Re-estimating the main specifications under this conservative adjustment yields coefficients that remain negative and economically meaningful. While statistical significance attenuates from the one percent to the ten percent level, the direction and magnitude of the effect are unchanged. This pattern suggests that differential LinkedIn adoption across cohorts cannot explain the main findings and, if anything, balancing sizes by augmenting non-entrepreneurs to seniors biases baseline estimates toward overstating entrepreneurial entry among seniors.

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. This stability indicates that results are not driven by imputation assumptions and that measurement error in education timing does not meaningfully affect the estimated treatment effects.

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.

These checks on the data indicate that the main findings are not an artifact of LinkedIn-specific measurement issues. Differences in platform adoption, incomplete education histories, and selective reporting of entrepreneurial experiences either leave the results unchanged or bias them against finding negative effects.

7. 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 introductions 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 firms 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 5, and also with the number of graduates through the years in Figure 6.

Figures 7 through 11 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.

7.1. Entrepreneurial Entry

Figure 7 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 firm (Astebro et al., 2014). Importantly, Figure 77 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.

7.2. Persistence and Repeat Entrepreneurship

Figure 8 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.

7.3. Career Diversity and Experimentation

Figure 9 shows that entrepreneurship graduates accumulate a larger number of distinct work experiences than other graduates. This greater career diversity is consistent with models of early career experimentation, in which individuals explore multiple occupations before settling into longer-term career paths.

In the context of entrepreneurship, prior work emphasizes the importance of generalist skills and diverse work experience. Lazear's "jack-of-all-trades" theory predicts that individuals with broader skill sets are more likely to become entrepreneurs (Lazear, 2005). The patterns in Figure 9 suggest that entrepreneurship graduates may deliberately pursue more varied early career paths to build general human capital relevant for venture creation, rather than specializing narrowly early in their careers.

7.4. Timing of Entrepreneurial Entry

Figure 10 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.

7.5. Industry Sorting

Figure 11 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.

These stylized facts indicate that entrepreneurship graduates differ systematically from other college graduates in the likelihood, timing, persistence, and structure of entrepreneurial activity. These differences are consistent with a combination of selection and treatment effects emphasized in the existing literature. Importantly, they motivate the empirical strategy used

in this paper, which exploits variation in the timing of entrepreneurship program introductions to identify the causal impact of entrepreneurship education on early-career entrepreneurial outcomes.

8. Conclusion

This paper studies whether undergraduate entrepreneurship education increases firm formation and improves entrepreneurial outcomes. Using the staggered introduction of entrepreneurship majors across U.S. universities and a DiD design comparing adjacent cohorts within the same institution, I provide causal evidence on how formal entrepreneurship education shapes entry into business creation. Contrary to the stated objectives of these programs, I find that the introduction of an entrepreneurship major reduces short-run business formation. This decline is economically meaningful and is entirely driven by a reduction in small-business entry, with no offsetting increase in growth-oriented or high-technology entrepreneurship. Moreover, I find no evidence that these effects reverse over longer horizons.

These findings have direct implications for how entrepreneurship education operates at the extensive margin. Rather than encouraging additional firm formation, formal entrepreneurship training appears to discipline entry by discouraging marginal, small-scale businesses. This pattern is consistent with entrepreneurship education acting as a screening device. By providing structured exposure to the costs, risks, and low average returns of small-business ownership, these programs could reduce overly optimistic entry decisions without deterring higher-growth entrepreneurial activity. Importantly, the absence of effects on growth-oriented firms suggests that entrepreneurship education does not meaningfully increase the supply of high-impact startups, which are typically the focus of innovation and productivity growth in finance and macroeconomic research.

From a firm formation perspective, the results indicate that entrepreneurship education reshapes who enters entrepreneurship rather than expanding the total number of new firms. While prior work emphasizes the role of early exposure, labor market shocks, and peer networks in shaping entrepreneurial entry, this paper shows that institutionalized entrepreneurship education has a distinct effect. It selectively reduces small-business formation while leaving higher-growth activity unchanged. As a result, aggregate firm creation declines.

Importantly, entrepreneurship education also does not improve outcomes among those who do start businesses. Conditional on entry, firms founded by exposed cohorts are no more likely to survive than those founded by unexposed cohorts, and survival is modestly lower at longer horizons for small businesses. Together, these findings indicate that entrepreneurship education primarily affects the extensive margin by discouraging marginal entry rather than improving entrepreneurial quality or firm durability.

These findings carry important policy implications. Over the past two decades, universities have rapidly expanded entrepreneurship degree programs, often at substantial cost to students and their families. These programs are frequently justified by claims that they increase entrepreneurial activity and contribute to economic growth.[9] The evidence in this paper challenges this narrative. While entrepreneurship education may improve decision-making and reduce inefficient entry, it does not increase business formation and, in fact, lowers the creation of small firms. For policymakers and university administrators, this raises questions about whether entrepreneurship degrees should be evaluated based on entry counts, firm growth, or broader labor market outcomes rather than on stated intentions alone.

More broadly, the results suggest caution in treating entrepreneurship education as a direct policy lever for stimulating firm creation. If the policy objective is to increase the number of new businesses, particularly small firms that account for a large share of employment, formal entrepreneurship education may be an ineffective or even counterproductive tool. At the same time, if the objective is to reduce inefficient entry and improve selection into entrepreneurship, these programs may still play a valuable role. Distinguishing between these goals is critical for designing effective education and innovation policies.

[9] University entrepreneurship degree programs explicitly state objectives such as training students to (i) act as entrepreneurs within companies, startups, or self-employment; (ii) prepare students to launch new ventures; (iii) not only teach entrepreneurship but produce entrepreneurs; and (iv) provide the business training needed to launch and grow successful enterprises. These descriptions are drawn from official program materials at Drexel University, Indiana University, Ball State University, and the University of Illinois Chicago.

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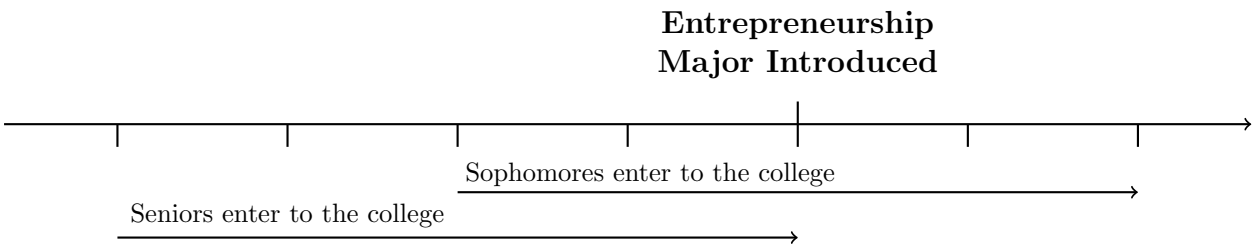
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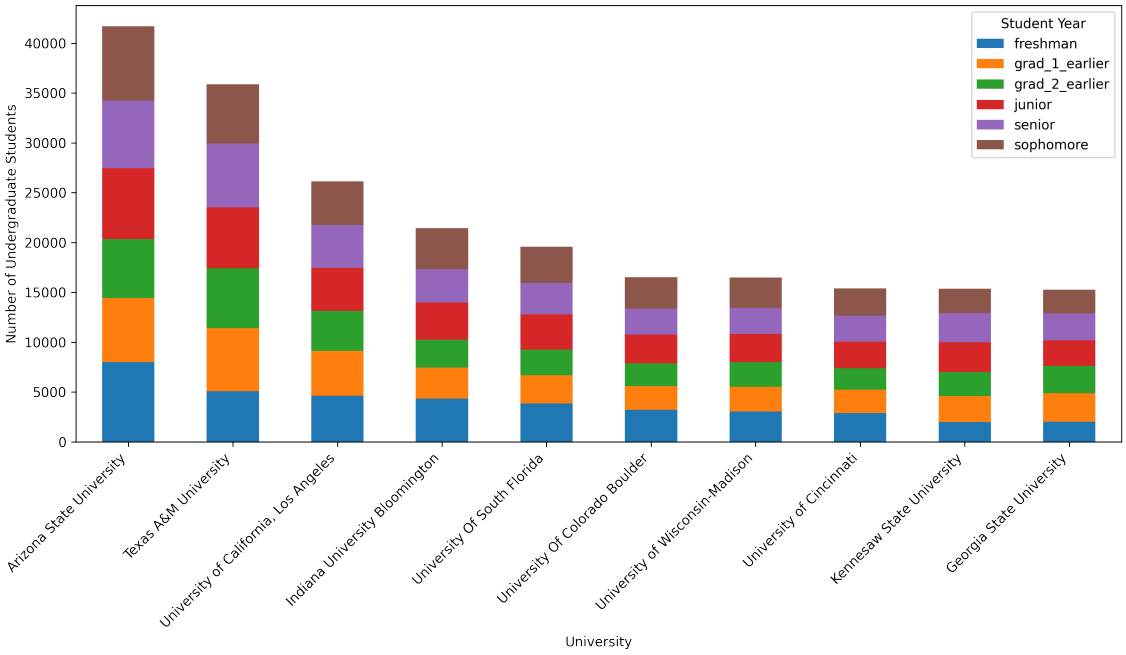
Yousaf, U., Ali, S. A., Ahmed, M., Usman, B., and Sameer, I. (2021). From entrepreneurial education to entrepreneurial intention: a sequential mediation of self-efficacy and entrepreneurial attitude. *International Journal of Innovation Science*, 13(3):364–380.

Figure 1. Empirical Setting Timeline



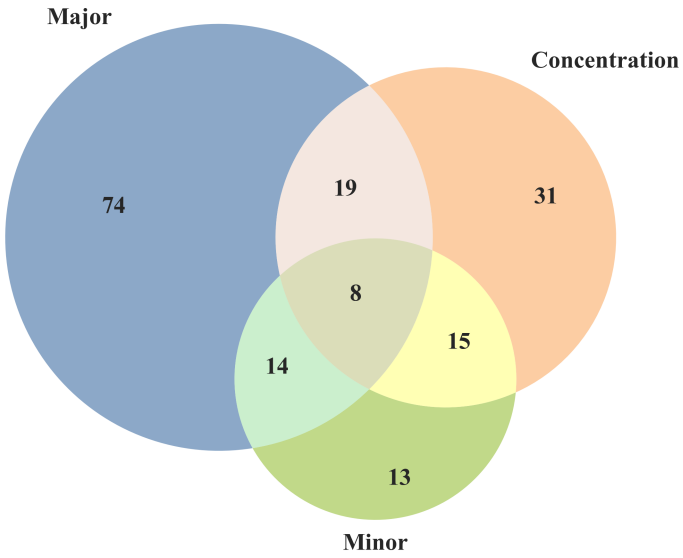
This figure illustrates the empirical timing of the introduction of entrepreneurship bachelor's degrees and the resulting treatment and control cohorts within a university. The horizontal axis represents academic time relative to the program's introduction. Sophomores constitute the treated cohort, as 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.

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.

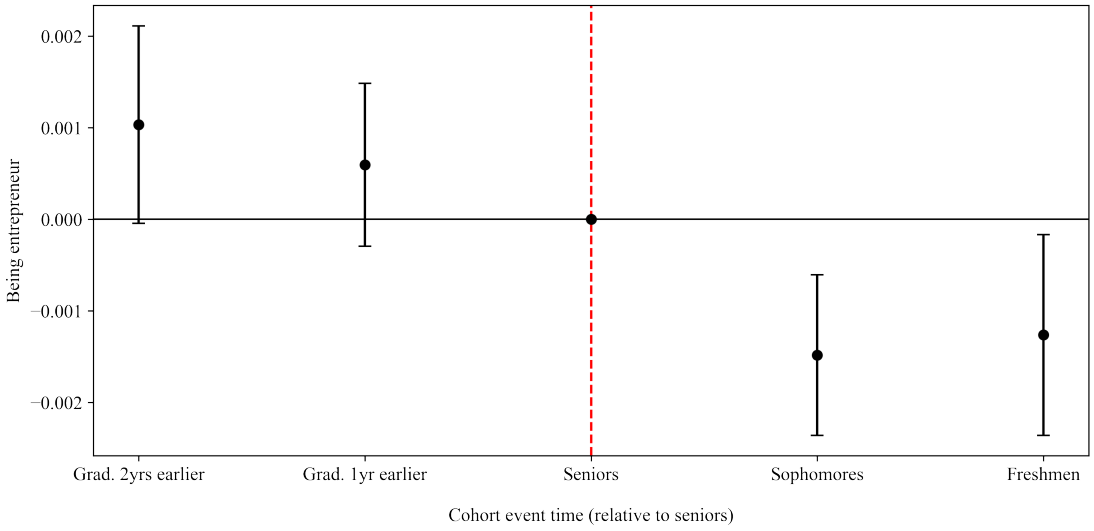
Table 2. Entrepreneurship Major and Business Formation: OLS Evidence

	Immediate Entry	Immediate Small Business	Immediate Growth Business	Ever Entrepreneur	Within 3 Years	Within 5 Years	Ever Small Business	Ever Growth Business
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

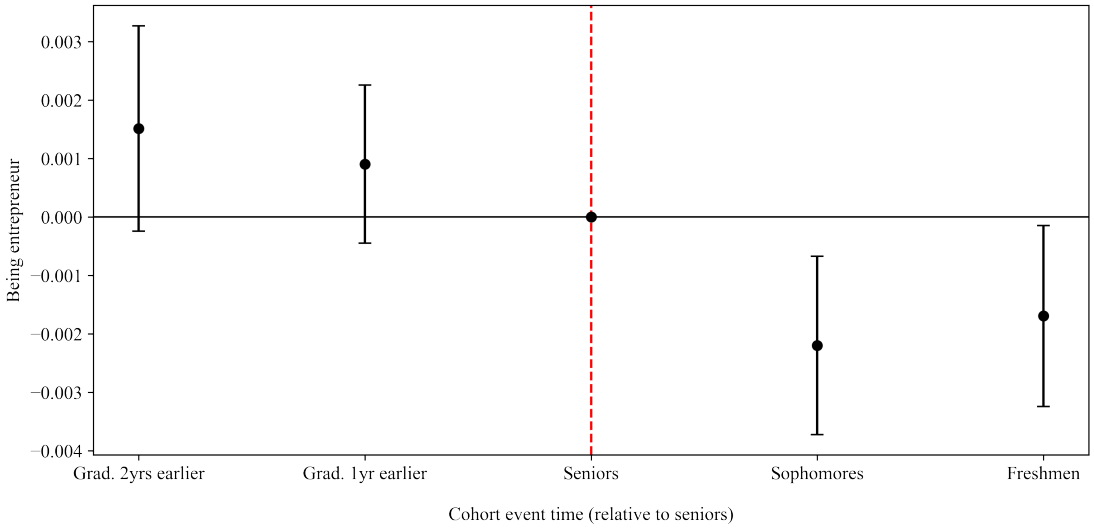
This table reports OLS estimates based on Equation 1, relating entrepreneurship education to business formation outcomes. The key independent variable is an indicator equal to one if the individual majored in entrepreneurship. The specification compares individuals within the same institution who selected 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: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

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 differences-in-differences 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 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 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 experience-start-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	324,937	116,875	97,909
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	324,936	116,875	97,910
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	324,938	116,875	97,909
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 5. 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 experience-start-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. Firm Survival

	1 Year	3 Years	5 Years
Panel A: Any Business (Conditional on Being Entrepreneur)			
Sophomore vs. Senior	-0.0044	-0.0088	-0.0175*
	(0.0039)	(0.0065)	(0.0096)
R^2	0.6789	0.5391	0.4854
Observations	12,462	12,462	12,462
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Panel B: Small Business (Conditional on Being Entrepreneur)			
Sophomore vs. Senior	-0.0038	-0.0091	-0.0182*
	(0.0046)	(0.0066)	(0.0101)
R^2	0.6764	0.5460	0.4999
Observations	8,575	8,575	8,575
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes
Panel C: Growth Business (Conditional on Being Entrepreneur)			
Sophomore vs. Senior	-0.0041	-0.0032	-0.0096
	(0.0057)	(0.0137)	(0.0135)
R^2	0.6950	0.5221	0.4525
Observations	3,881	3,881	3,881
Year FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes

This table reports linear probability model estimates of business survival at 1-, 3-, and 5-year horizons following firm founding. The sample is restricted to individuals who become entrepreneurs and attended a school with a major degree.The dependent variable is an indicator equal to one if the business survives 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 *.

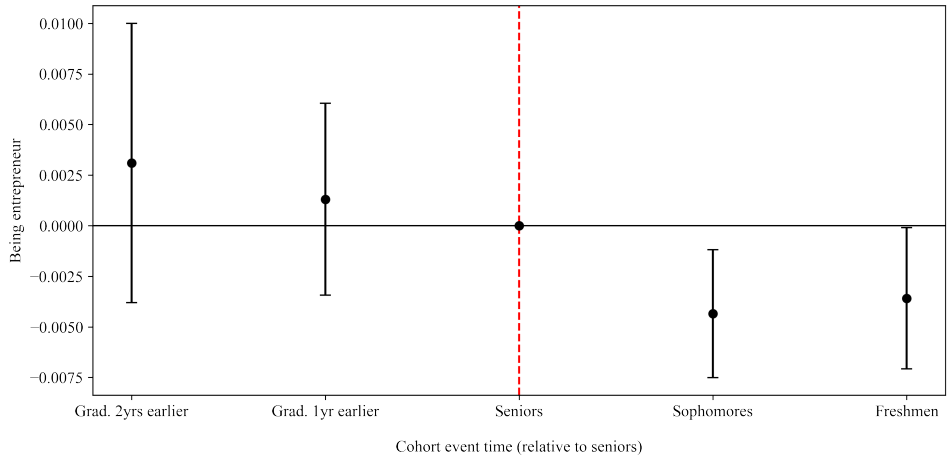
Table 6. Peer Effects versus Entrepreneurship Education

	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 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 experience-start-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

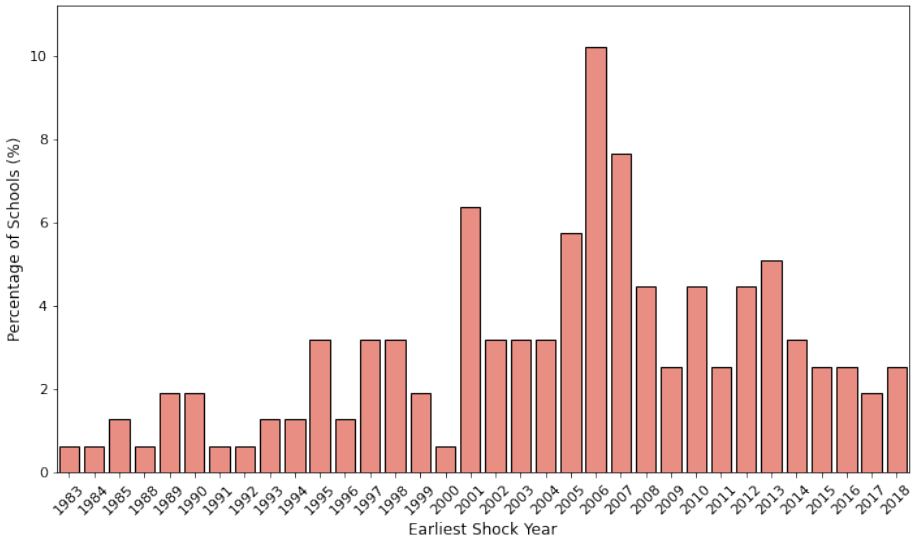


Panel B: Immediate Business Formation During Periods of Rising Aggregate Entry

	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

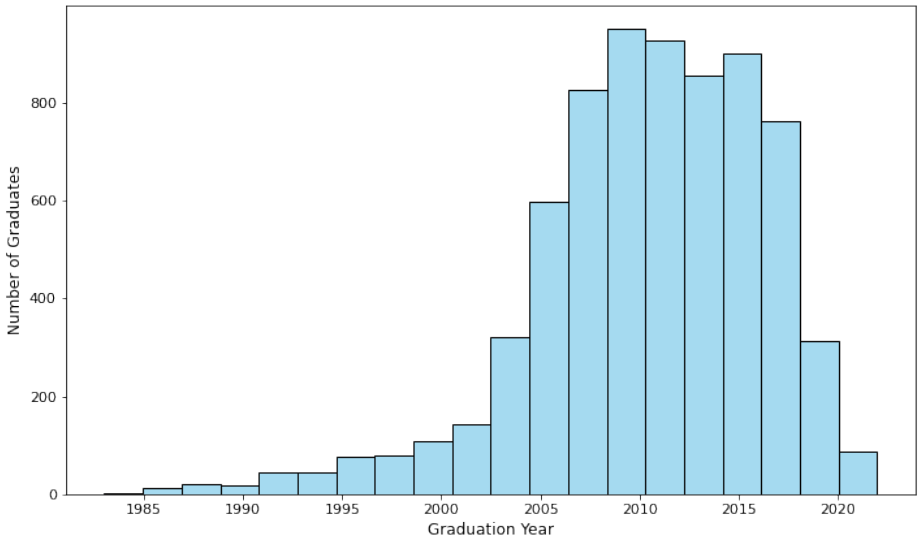
This table examines whether the estimated effects are driven by broader shifts in business dynamism rather than cohort exposure to entrepreneurship education. Panel A presents within-school event-study estimates of the likelihood of becoming an entrepreneur across cohorts during the increase in the business 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 experience-start-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.

Figure 5. 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.

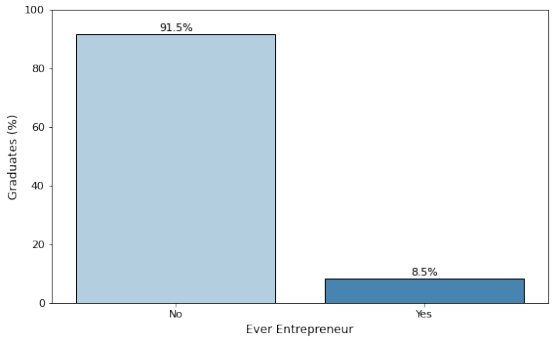
Figure 6. Entrepreneurship Students Graduation Year Distribution



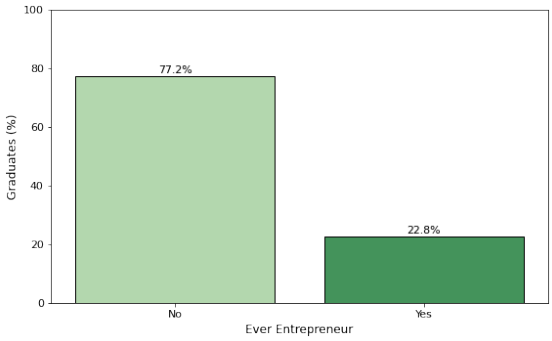
This figure plots the distribution of graduation years for students who complete an undergraduate entrepreneurship degree. The figure illustrates that the bulk of entrepreneurship graduates emerge after the early 2000s, reflecting both the timing of program introductions shown in Figure 5 and the subsequent growth in cohort sizes as programs mature.

Figure 7. Comparison of Having at Least One Entrepreneurial Experience After Graduation

(a) All Graduates with at Least One Entrepreneurial Experience



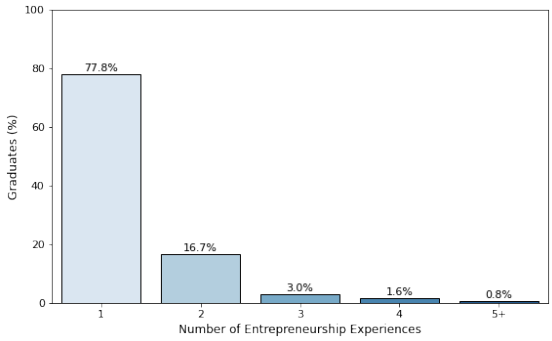
(b) Entrepreneurship Graduates with at Least One Entrepreneurial Experience



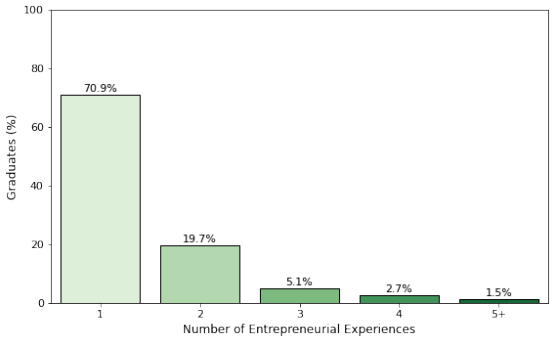
Panel (a) shows the share of all graduates who have at least one entrepreneurial experience at any point after graduation, while Panel (b) reports the same measure for graduates who completed an entrepreneurship degree. An entrepreneurial experience is defined as holding a founder or co-founder role in a firm, 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 8. Comparison of the Number of Entrepreneurship Experience Distribution

(a) Distribution of Entrepreneurship Experiences of All Graduates



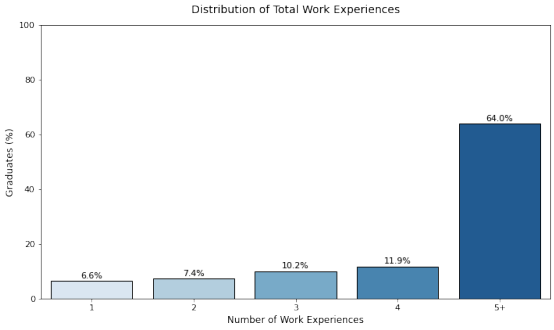
(b) Distribution of Entrepreneurship Experiences of Entrepreneurship Graduates



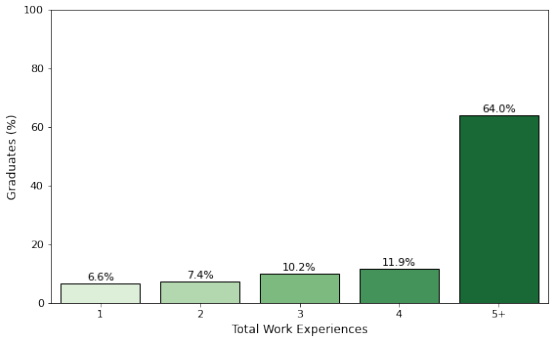
Panel (a) presents the distribution of the total number of entrepreneurial experiences among all graduates who have ever entered entrepreneurship, while Panel (b) reports the corresponding distribution for entrepreneurship graduates. The figure shows that entrepreneurship graduates are more likely to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

Figure 9. Comparison of Total Different Work Experiences

(a) Distribution of Total Work Experiences of All Graduates



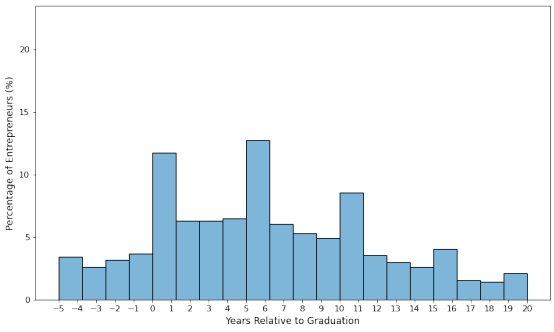
(b) Distribution of Total Work Experiences of Entrepreneurship Graduates



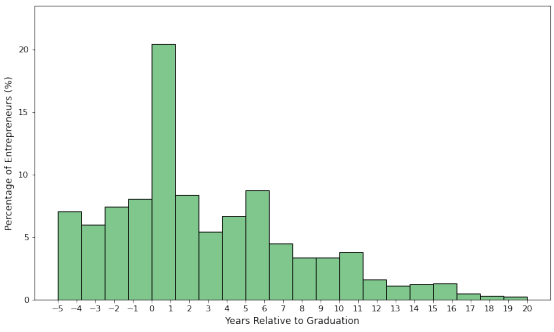
This figure compares the distribution of the total number of distinct work experiences across all graduates (Panel a) and entrepreneurship graduates (Panel b). Work experiences include both entrepreneurial and wage-employment positions recorded in LinkedIn career histories. Entrepreneurship graduates exhibit more diverse career paths, with a higher number of distinct experiences on average, consistent with greater occupational experimentation.

Figure 10. Comparison of Time to First Entrepreneurial Experience

(a) Time to First Entrepreneurial Experience of All Graduates



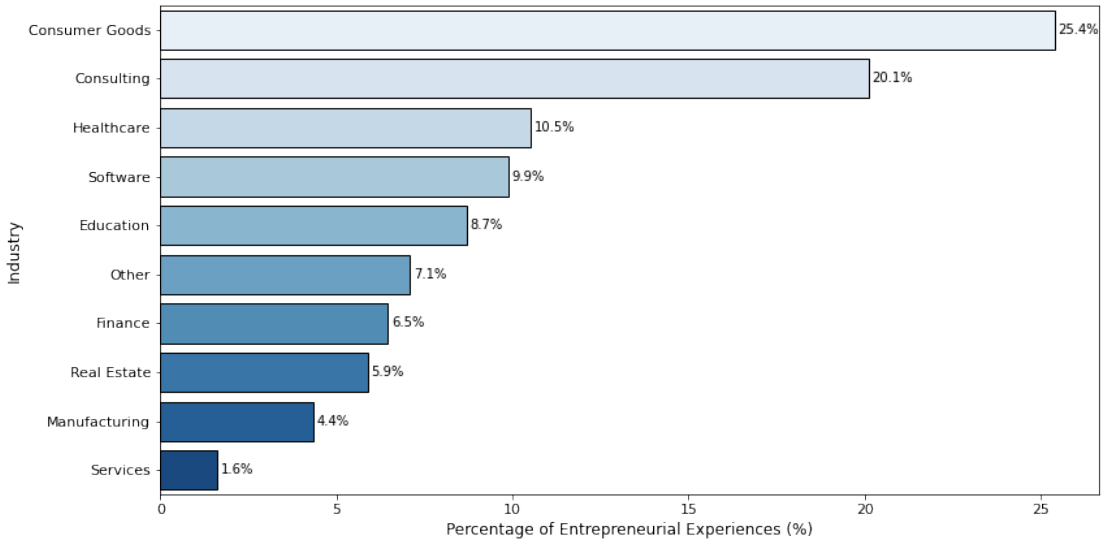
(b) Time to First Entrepreneurial Experience of Entrepreneurship Graduates



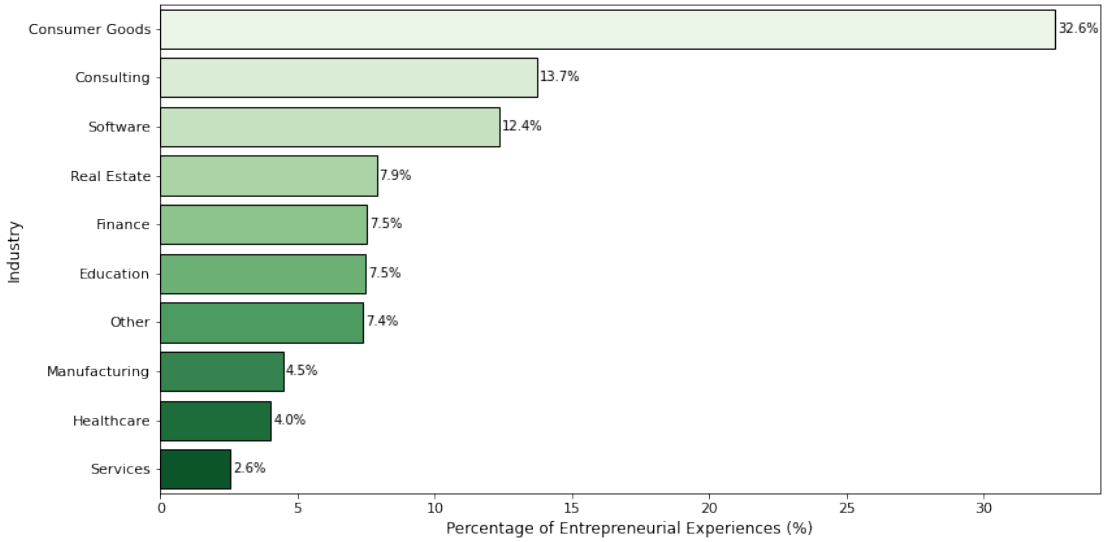
Panel (a) shows the distribution of the number of years between graduation and first entrepreneurial experience for all graduates who have ever become entrepreneurs, while Panel (b) reports the same distribution for entrepreneurship graduates. Entrepreneurship graduates tend to enter entrepreneurship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

Figure 11. 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 firm 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.

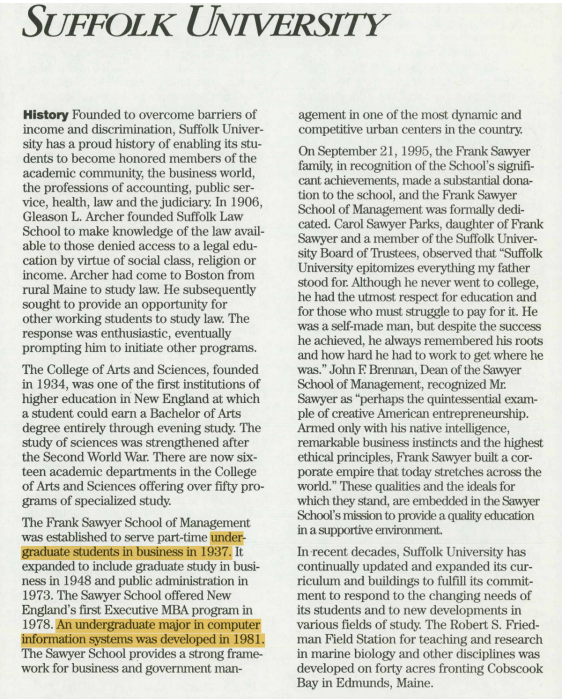
Internet Appendix

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 12 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 12. 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.

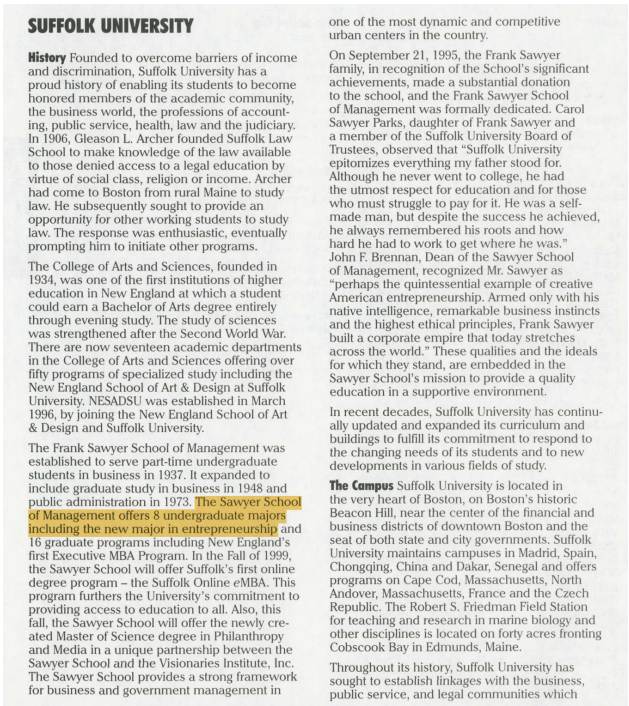
The College of Arts and Sciences, founded in 1934, was one of the first institutions of higher education in New England at which a student could earn a Bachelor of Arts degree entirely through evening study. The study of sciences was strengthened after the Second World War. There are now sixteen academic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



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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.

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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.