Learning from the Band of Brothers: Evidence from Entrepreneurial Spillover*

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Abstract

We study whether and how social groups affect entrepreneurial career choices and performances. Our empirical setting leverages entrepreneurial peer exposure variation in randomly assigned peer groups during compulsory military service, with close to 350 thousand draftees over 15 years. Along the extensive margin, we provide plausibly causal evidence that individuals are more likely to become entrepreneurs if more peers in their social group have had entrepreneurial experiences. The spillover effect is concentrated among individuals without elite university degrees or finance-related majors. Additionally, the spillover effect is stronger if more peers have entrepreneurial potential or entrepreneurial parents. Along the intensive margin, start-ups by individuals with more entrepreneurial peers exhibit better profitability, especially early on in the start-ups' life cycle. Collectively, our results suggest scope for learning entrepreneurial skills through social spillovers and speak to the role of peer effects on economic growth and job creation.

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1. Introduction

It is widely accepted that entrepreneurship is associated with wealth creation and social welfare (Schumpeter 1934, Baumol 1990, Murphy, Shleifer, and Vishny 1991). In particular, young startups play a disproportionally important role in employment growth and innovation (Haltiwanger, Jarmin, and Miranda 2013, Klenow and Li 2021). Accordingly, there has been great interests from policy makers to encourage more entrepreneurship in the economy (e.g., OECD 2010). Correspondingly, academic research focuses on understanding the proximate factors that shape an individual's decision to create a startup.[1] Inspired by successes of local startup ecosystems such as Silicon Valley, prior research has focused on social interactions as a plausible factor; that is, exposure to entrepreneurial peers may plausibly increase an individual's potential to create a startup.[2]

We develop an empirical setting to address several key questions on the social effects of entrepreneurial activities. The traditional methodology in this line of research compares individuals' entrepreneurial outcomes across social groups with variations in entrepreneurial outcomes or experiences. By and large, prior studies show that peer effects lead to more entrepreneurship. However, the answer to this fundamental question remains incomplete. A number of studies call into question inferences with traditional approaches by highlighting endogeneity concerns (Manski 1993, Angrist 2014, Sacerdote 2014, Kuchler and Stroebel 2021). In particular, individuals with entrepreneurial traits may naturally self-select into the same social group, workplace, or location; that is, membership of a social group is endogenous and not random. In this case, a positive correlation between individual entrepreneurship and peer entrepreneurship permits alternative interpretations that are challenging to rule out. Without

[1] For a review of motivations for entrepreneurship, see Segal, Borgia, and Schoenfeld (2005).

[2] Prior evidence suggests peer effects driving entrepreneurship in the contexts of workplace (Gompers, Lerner, and Scharfstein 2005, Nanda and Sørensen 2010, Wallskog 2024), local community (Giannetti and Simonov 2009, Guiso, Pistaferri, and Schivardi 2021), classroom (Falck, Heblich, and Luedemann 2012, Lerner and Malmendier 2013, Kacperczyk 2013, Shue 2013, Hacamo and Kleiner 2021), family members (Djankov, Qian, Roland, and Zhuravskaya 2006, Lindquist, Sol, and Van Praag 2015, Hvide and Oyer 2020), entrepreneurial training (Karlan and Valdivia 2011, Field, Jayachandran, Pande, and Rigol 2016, Eesley and Wang 2017, Chatterji, Delecourt, Hasan, and Koning 2019, Hasan and Koning 2019), or academic research (Marx and Hsu 2022).

random assignment of social groups, it is difficult to isolate peer effects from other confounding factors.[3]

Empirical settings with random assignment of social groups that balance internal and external validity are few and far between. Indeed, studies with clean identification in this literature typically exploit small-scale field experiments in a university context.[4] Notably, Lerner and Malmendier (2013) exploit random assignments of Masters of Business Administration (MBA) students at Harvard Business School (HBS) into sections. In contrast to stylized findings, they show that entrepreneurial peers *reduce* entrepreneurship propensity.[5] Similarly, Eesley and Wang (2017) use randomized student-mentor pairs to show entrepreneur mentors increases students' penchant to become entrepreneurs. While these studies cleverly isolate peer effects, it is unclear the extent to which one could generalize the inferences based on relatively small and specialized samples.

A related issue is that even studies with random social group assignments rely on individuals who choose to enroll in a school, join a firm, or move to a community.[6] These choices are plausibly dependent on what individuals expect to learn from their future entrepreneurial peers. Put differently, empirical estimates of entrepreneurial peer effects do not incorporate the effect along the margin that entrepreneurial individuals decide to join the empirical setting under question. An empirical setting where individuals do not have the liberty to choose to participate will mitigate this concern.

A second key issue concerns the consequences of entrepreneurial peer effects. Prior studies typically document the role of social interactions in individuals' decision to establish start-ups

[3] For example, Nanda and Sørensen (2010) point out that, without a natural experiment, one cannot "completely rule out the possibility of spurious correlation, for example, arising from time-varying individual- or firm-level attributes."
[4] Similarly, Karlan and Valdivia (2011), Field, Jayachandran, Pande, and Rigol (2016), Chatterji, Delecourt, Hasan, and Koning (2019), and Hasan and Koning (2019) exploit small-scale field experiments in entrepreneurial training programs to identify various aspects of peer effects.
[5] With the same HBS MBA student data, Shue (2013) and Hacamo and Kleiner (2021) explore peer effects in terms of corporate policies and confidence, respectively.
[6] One exception is Lindquist, Sol, and Van Praag (2015) where they study entrepreneurial spillovers from parents to their adopted children.

but are constrained from examining subsequent start-up performances due to limited access to startups' detailed financials.[7] Presumably, entrepreneurial spillover may lead to skill spillover (i.e., learning) that lead to better outcomes. Alternatively, exposure to entrepreneurial peers may simply raise an individual's awareness of potential career opportunities. Similarly, social exposure arguably reduces fear and uncertainty associated with entrepreneurship (Kacperczyk 2013, Portyanko et al. 2023), which may induce biased beliefs about own abilities (Cooper, Woo, and Dunkelbert 1988, Camerer and Lovallo 1999, Arabsheibani et al. 2000). These latter mechanisms predict mediocre or inferior startups. Without detailed financial information on follow-up startups, one is limited from examining which mechanisms are borne out of data.

To address these challenges, our paper exploits an empirical setting that balances both internal and external validity. Specifically, we leverage random assignments of Taiwanese male citizens to military units as part of their compulsory military service.[8] All male citizens above eighteen years of age are legally required to serve for twelve months. In addition, we also have detailed personal wealth information that allows us to track entrepreneurial career choices and performances.

Our empirical setting has a few appealing features. First, the assignments to military units are based on public lotteries with little room for manipulation. With randomized social group assignments, we can isolate entrepreneurial peer effects from shared characteristics of endogenously formed peer groups. In the paper, we also provide validity tests that suggest the identified peer groups are indeed random along observable demographic variables. Second, our data covers five consecutive draft years with close to **350,000** draftees and **1,310** peer groups. The scope of our data renders our findings and magnitude estimates to be more generalizable. Third, our ability to track post-military service entrepreneurial performances allows us to

[7] One exception is Wallskog (2024), in which she shows that entrepreneurial spillovers from coworkers lead to worse startups. This implies that, on average, individuals do not pick up entrepreneurial skills from their peers.
[8] With a similar military setting, Lieber and Skimmyhorn (2018) use exogenous assignments of U.S. Army enlisted soldiers to examine peer effects on personal financial decisions.

understand better the nature of spillover and measure the scope of learning therein. In addition, this feature enables us to speak to productivity and labor market implications of spillover effects.

Our first set of findings relate to the existence of entrepreneurial spillover; that is, does exposure to entrepreneurial peers lead to more entrepreneurs? We find that, relative to the sample average, individuals are **16.5** to **20.7** percent more likely to become entrepreneurs in a year if their military unit peers' prior entrepreneurial experience increases by one standard deviation; that is, social interactions lead to entrepreneurial spillovers. In addition, we show that there is no evidence of peer effect prior to social group formation (i.e., before military service). With randomized military unit assignment, our finding is not due to endogenous group selection.

In addition to endogenous group selection, other types of correlated unobservables may also lead to correlated entrepreneurial behavior among members that is not driven by spillover (Manski 1993). For example, a military unit may be more entrepreneurial if it is commanded by senior commanding officers that possess business acumen. To mitigate this concern, our regression specifications include unit (or individual) fixed effects that control for unobservables such as time-invariant personal traits, military unit culture, member composition, or affiliation to specific armed forces.[9] In addition, we also include time fixed effects that capture time-varying aggregate patterns in entrepreneurship activities.

We explore cross-sectional variations to corroborate our main results. First, we expect individuals with better access to entrepreneurial knowledge prior to military service to be less affected by social interactions. Advanced schooling provides training that enhances an individual's managerial ability that promotes entrepreneurship (Lucas 1978, Calvo and Wellisz 1980), leading to a positive relation between education attainment and entrepreneurship (e.g., Borjas and Bronars 1989, Evans and Leighton 1989). In other words, advanced schooling arguably substitutes for social exposure to entrepreneurs, rendering the latter effect weaker at the margin. Consistent with our expectation, we find that entrepreneurial spillover from social

[9] These fixed effects help control for "contextual" effects (in the words of Manski) that help tease out endogenous social effects (Lee 2007, Lee, Liu, and Lin 2010, Hvide and Östberg 2015).

exposure is moderated among individuals with higher education attainment, proxied by elite university degree holders or finance majors. More generally, this finding is consistent with findings that peer effects are substitutes for entrepreneurial exposure in different aspects of life (Nanda and Sørensen 2010).

Next, we expect the baseline spillover effect to be stronger if the focal individual has more peers with entrepreneurial parents.[10] Entrepreneurial parents induce their offsprings to become entrepreneurs with better performance via early exposure to industry knowledge (Hvide and Oyer 2020) or role modeling (Lindquist et al. 2015). Social interactions with these peers arguably lead to better information acquisition that help reduce uncertainty, raise awareness of business opportunities, or acquire better managerial knowledge (Kacperczyk 2013, Guiso et al. 2021, Wallskog 2024). As expected, we find that the effect of entrepreneurial spillover is stronger when there is a higher fraction of peers with entrepreneurial parents.

Our second set of tests attempt to understand the nature of entrepreneurial spillover through the prism of post-military entrepreneurial outcomes. If individuals are simply made aware of the alternative career choice of becoming an entrepreneur by their military peers, they will tend to start inferior or mediocre firms due to overconfident beliefs about their own abilities. In other words, there is limited scope for performance improvement if social interactions primarily only alter ones' preferences. Alternatively, social interactions may entail learning the ropes of managing a start-up, leading to better entrepreneurial outcomes. Similarly, one can plausibly extract private signals about the economy from observing others' decisions to become entrepreneurs. These mechanisms of social interactions entail scope for learning and better entrepreneurial performance.[11]

In general, we find that firms established by individuals with more entrepreneurial peers tend to be smaller at launch, compared to those with less entrepreneurial peers. These start-ups

[10] We define a person to have entrepreneurial parents as having either or both parents have founded firms before his military service.

[11] In the words of Manski (2000), the former type of social interactions is termed "preference interactions." The latter type of social interactions is termed "expectations interactions."

nevertheless show better asset growth, gross profitability, and return on assets in their first year. These findings suggest that entrepreneurial spillover in our setting entails some scope of learning. Interestingly, we also find that the capital structures of these "entrepreneurial spillover firms" consists of significantly more long-term debt (as measured by leverage ratio) while having similar fraction of short-term debt (as measured by current ratio). This finding suggests that social interactions in our sample induce "realist entrepreneurs" that choose to use long-term debt to smooth payoffs across different states of nature (Landier and Thesmar 2009).

Our paper contributes to the literatures in several ways. First and foremost, our paper relates to the literature on the social aspects of entrepreneurial activities, and financial decisions more generally (Kuchler and Stroebel 2021). We exploit a large-scale empirical setting with random assignments to social groups that yields plausibly causal evidence of positive peer effects on an individual's decision to become an entrepreneur. Prior studies have documented entrepreneurial spillovers (with mixed directions) in the contexts of communities, classrooms, workplaces, and families. Our empirical setting improves upon both internal and external validities to help foster a consensus in the literature.

In addition, we contribute to this literature by exploiting the rare opportunity to access detailed financial information of private start-ups. This allows us to speak to what is being transmitted through social interactions. Prior studies typically stop short of testing the extent of entrepreneurial learning due to data limitations (Wallskog 2024). Importantly, the size of our empirical setting and causal nature of our methodology produces estimates and consequences of entrepreneurial peer effects that have clear policy implications.

More broadly, we contribute to the growing literature on the role of social interactions in economic and social outcomes. The majority of studies in this field examine peer effects on in elementary or secondary schools. Compulsory schooling affords rich scenarios in which researchers can exploit randomized classroom assignments, acceptance/reject decisions, roommate assignments, or explicit experiments (Sacerdote 2014). As Sacerdote concludes, however, the size and nature of peer effects are highly context-specific. In other words, peer

effects in test scores or other social behavior among young students cannot be easily generalized to other contexts. In this regard, we differ from prior literature by exploring a (compulsory) non-academic setting that affords causal identification of peer effects in entrepreneurial career choices.

The rest of the paper is organized as follows. Section 2 describes our empirical strategy and data. We report results on entrepreneurial spillover through peer effects in Section 3. In Section 4, we examine the role of peer effects on the performances of start-ups. We conclude the paper in Section 5.

2. Empirical strategy and data

2.1. Institutional background

Testing if social interactions play a role in entrepreneurial activities is challenging primarily because observable social groups are typically formed endogenously; that is, individuals with entrepreneurial preferences are more likely to share similar educational backgrounds, social affiliations, workplaces, or communities. Therefore, a positive correlation between prior-entrepreneurial endeavors and future entrepreneurial activities among group members does not permit a causal interpretation. Without randomized social groups, credible identification of peer effects is difficult (Sacerdote 2014).

To this end, we exploit the random allocation to military units in the conscription system of Taiwan. All male citizens no longer in school will receive conscription notices in the year after their eighteenth birthday (conscription age). Those who pursue further education after the conscription age can defer conscription until graduation. In our sample period, draftees are

required to perform twelve months of active duty military service.[12,13] By law, attempts to avoid military service may be sentenced to up to five years of imprisonment.[14]

All draftees are required to draw lots publicly to determine their assigned armed forces (i.e., Army, Navy, or Air Force) before they formally start their active duty service with five weeks of basic training. Towards the end of their basic training, they are required again to draw lots publicly to decide the specific military unit that they will be assigned to for the remainder of their service. In these lots, the number of positions for each assignment is fixed and pre-announced publicly, leaving little room for manipulation.[15] Effectively, military draftees are assigned to military units with randomized peers by design. Next, we describe peer group identification and group characteristics.

2.2. Sample and data

From the Financial Information Agency (FIA) of the Ministry of Finance, we obtain tax filings and wealth information of all individuals in Taiwan from 2006 to 2021. For our purpose, we can observe and measure an annual panel of individual information including demographics (e.g., age, education, marital status), income, personal wealth (e.g., housing, land, vehicles, liquid assets), pseudonymous employer ID, etc.

Our identification of individuals initiating businesses relies on business tax status information from the FIA database, encompassing information about all registered firms (both public and private, active and inactive) in Taiwan. For this paper, FIA's firm-level information includes owner identification, founding/defunct dates, detailed financial statements, and industry classifications. This information helps us identify each person's entrepreneurial activities and

[12] Starting in 2014, Taiwanese male citizens born after January 1994 receive four months of military training to fulfill the statutory military service obligation.
[13] Male citizens may apply for substitute military service due to health/religious conditions or specialized STEM backgrounds. In 2009, 8% of draftees qualify for substitute military service. Alternatively, college graduates (and above) can obtain reserved officer status upon passing a selective written exam, accounting for 2.2% of all draftees. We exclude these draftees from our sample.
[14] *Punishment Act for Violation to Military Service System*.
[15] Personnel responsible for draftee assignments can be sentenced to no less than three years and up to ten years if found guilty for obstructing the fairness of the process (e.g., accepting a bribe).

performances (if any). We narrow down the sample to include only firms started between 2006 and 2021, with owners who were identified as compulsory military draftees in our sample.

2.2.1. Identifying military units and compulsory military draftees

With each individual's pseudonymous employer ID in our sample, we can precisely identify each individual's workspace peers. While the FIA data does not reveal which employer IDs are military units, we can identify individuals fulfilling their compulsory military service from their income level, along with other demographic information and employee characteristics. Specifically, compulsory military draftees received monthly wages between NT$5,890 to NT$6,630 in **2011**, which is significantly lower than the statutory minimum monthly wage of NT$17,880.[16] Specifically, we first classify an individual in our sample as a candidate potentially fulfilling compulsory military service with the following demographics filters:

(1) birth year < 1994

(2) age between 18 and 25

(3) annual income between NT$5,890 to NT$100,000[17]

Next, we identify military units among salary-paying institutions with the following filters:

(1) public sector

(2) employs more than ten potential candidates

(3) candidate male ratio > 80%

(4) total employee male ratio > 70%

Finally, we identify individuals as serving their military service that satisfy the following criteria:

(1) one must be a candidate from the first set of filters

(2) one must be male

(3) one only serves in the military unit we identify in the second set of filters for no more than two consecutive years.

[16] Equivalent to US$ 615 in 2011.

[17] Military draftees receive a monthly NT$1,000 to NT$2,000 raise each month if they serve in extremely rural areas or outlying islands.

With this approach, we are able to identify **25** military units and **349,561** individuals drafted between 2011 to 2015. Our final sample accounts for approximately **75%** of all Taiwanese citizens eligible for compulsory military service during this period.

To get a sense of the validity of our filters, we compare the distribution of employment duration and employee age between the identified military units (including both identified draftees and other professional military staff) and other institutions in the FIA database. Figure IA.1 in the Internet Appendix shows that distributions of employment duration is distinctly different across these two types of institutions. The majority of employees from the identified military units serve for less than two years (average **2.55** years), while the average employment duration in other institutions is **3.5** years. Figure IA.2 shows that the age distributions are likewise significantly different between the two. The average employee age of the identified military units is **30.09**, while average age of other institutions is a significantly higher **40.53**. Overall, Figures IA.1 and IA.2 provide validity checks for the filters we adopt to identify military units in the FIA database.

2.2.2. K-Means clustering

The average size of military units we are able to identify with the filters in Section 2.2.1 is fairly large, making it difficult to credibly measure and infer peer effects.[18] To address this concern, we employ a machine learning approach to further partition the identified military units into finer peer groups based on within-year draft waves as implied by annual income of draftees in our sample.

As we discussed in Section 2.1, all male citizens will receive conscription notifications in the year after their conscription age or after graduation from school, whichever is later. Instead of reporting to military service in one large wave per year, draftees are required to start their military service in multiple waves in the months after they have eligibility. Accordingly, we exploit the fact that compulsory military draftees receive a pre-determined monthly income; that

[18] 349,561 draftees over five draft years assigned to 25 military units imply that the average size of these military units is close to 2,800 men.

is, the total annual income a military draftee receives can help us determine whether he is drafted earlier or later in his draft year.

Specifically, we partition each military unit-year cohort into K peer groups based on their annual income in the draft year using the K-means clustering method (Hartigan and Wong 1979). To determine the number of peer groups (K), we follow the suggestion of Athey and Imbens (2019) that the choice should be based on the institutional context. Therefore, we restrict potential Ks to be 2, 3, 4, 6, or 12; that is, the choices coincide with annual draft waves in Taiwan that occur every 6 moths, 4 months, 3 months, 2 months, or 1 month. Next, we choose an optimal K of 4 that leads to the most significant reduction in the within cluster sum of squares following Agness et al. (2022). Effectively, we partition each military unit-year cohort into four peer groups.

With this approach, we partition the military units with more than 50 military draftees into four peer groups, yielding a total of **1,310** randomized peer groups over five draft years from 2011 to 2015. The average peer group size is approximately **267**.

2.2.2. Summary statistics

Our final sample consists of **349,561** servicemen drafted between 2011 and 2015 with **1,310** peer groups. For each individual, we focus on a ten-year [−4:+5] window around the year he serves in the military. For instance, if a compulsory military serviceman was drafted in 2011, we look into his entrepreneurial activities from 2007 to 2016. We report detailed definitions of all variables in this paper in Appendix A.

Our main left-hand-side variable is $EntreExperience_{it}$, an indicator variable that equals one if an individual i has had entrepreneurial experience ending in year t, and zero otherwise. Note that, for each individual, once he gains entrepreneurial experience in some year t, the variable will then switch from 0 to 1 for the remainder of the ten-year window regardless of how long the start-up survives.

Table 1 reports the summary statistics for this variable. Across all 3,495,610 individual-years, the mean is 1.15%, suggesting that entrepreneurial experience is overall rare among young

military draftees. We further partition the sample and report summary statistics for this variable before and after military service. Not suprisingly, the average ratio of individuals with entrepreneurial experiences increases from 0.16% before military service and 1.82% after service. Therefore, instead of relying on variations of pre-military service peer group entrepreneurial experience, our empirical model is designed to identify the role of ongoing social interactions in individual's entrepreneurial decisions after military service.[19]

The main righ-hand-side variable is $EntreRatio_{-it-1}$, defined as the ratio of an individual i's military peers that have had entrepreneurial experience ending in year $t-1$. Importantly, the variable is measured excluding individual i to avoid a mechanical relation between the focal individual and the peer group. Similar to $EntreExperience_{it}$, we emphasize on the peers' entrepreneurial experience regardless of whether he is still running the business. Overall, about 0.8% of an individual's peers have had entrepreneurial experience. With an average peer group size of 267 men, this translates to approximately two men that have had prior entrepreneurial experience. This ratio increases from 0.1% before a focal individual's military service to 1.26% after military service.

We also report summary statistics for the demographic variables, measured in each individual's service year. The average age of the military draftees is 22.7, with average annual total income and wealth of NT$68,185 and NT$414,746, respectively. A large majority of the military draftees have obtained college degrees at the time of his service year (85.3%), with 14.4% and 2.8% graduating from public and top 5 universities in Taiwan. In addition, 9% of military draftees majored in finance-related disciplines in college. Given the relatively young age, only 0.5% of individuals in our sample are married upon their service year.

2.3. Random assignment of military draftees

An appealing feature in our study is that the randomness of peer group assignments in the compulsory military service draft can be verified empirically. In particular, we can observe the

[19] Similarly, Shue (2013) examine the effects of ongoing social interactions among MBA students after graduation.

demographic characteristics of the identified draftees before their service. If the assignments to military units are truly randomized, we should observe that pre-service characteristics among the identified draftees in the same peer group to be uncorrelated.

In principle, one can regress each individual's pre-assignment characteristics on his peer-group's leave-one-out average pre-service characteristics. If group assignments are random, then there should be no relationship between the draftee's background and that of his peers (i.e., the regression coefficient should be close to zero). However, Guryan et al. (2009) show that this test tends to give a downward-biased estimation of the coefficient because an individual cannot be his own peer; that is, individuals with high values of a particular characteristic tend to have peers with relatively low values of that particular characteristic, and vice versa. Consequently, positively correlated grouped peers may appear randomly grouped. Therefore, we correct this bias following the methodology of Jochmans (2023), which essentially measures the total within-group variation of the characteristics' values.

We test the randomness of the assignment with the following characteristics: age upon drafted, college degrees, college degrees from public universities, college degrees with a concentration in finance-related fields, marital status, and the fraction of peers with entrepreneurial experience. Panel A of Table 2 reports the distribution of group-average characteristics among the **1,310** peer groups. The mean number of peers in a given group is **266.8**, and the median number is **139.5**. Panel B reports the results of random assignments tests with our peer group identificaiton. The null hypothesis that the assignments are randomly-assigned is not rejected for all characteristics, consistent with the institutional design that peer group assignments are randomly determined by drawing lots.

3. Social groups and entrepreneurial peer effects

3.1. Baseline results

We begin our empical analysis by examining whether an individual's decision to become an entrepreneur is affected by his peers' entrepreneurial experience. Our regression specification is as follows:

$$EntreExperience_{it} = \alpha + \beta_1 EntreRatio_{-it-1} \times Pre_{it} + \beta_2 EntreRatio_{-it-1} \times Post_{it} + Fixed\ Effects + \varepsilon_{it}, \quad (1)$$

where $EntreExperience_{it}$ is an indicator variable that equals one if individual i has gained experience starting a business by year t, as defined previously. The independent variable, $EntreRatio_{-it-1}$, represents the leave-one-out average of $EntreExperience_{it-1}$ for each individual i's military peers in year $t-1$. The one-year lag allows us to mitigate the reflection problem described in Manski (1993); that is, contemporaneous correlation between a focal individual's outcome and peers' outcomes makes it challenging to identify the direction of causality. Our specification mitigates this issue since it is not plausible that an individual's entrepreneurial decision affects his peers' past entrepreneurial activities (Lerner and Malmendier 2013, Wallskog 2024).

We further define the before-window as the years prior to and including the military service year (captured by the indicator variable Pre_{it}), and the post-window as the years after the service year (captured by the indicator variable $Post_{it}$). By interacting $EntreRatio_{-it-1}$ with Pre_{it} and $Post_{it}$, we isolate the correlated entrepreneurial decisions between individuals and their military peers before and after the peer group formation process. While we have already provided evidence for the randomness in the peer group assignment in Section 2.3, the absence of correlated behavior before peer group formation could further speak to the nature of random group assignments.

Our regression results are presented in Table 3, where we account for various levels of fixed effects across specifications. In column (1), we control for window-year fixed effect, controlling for the common likelihood of individuals in our sample gaining entrepreneurial experience in each of the years before and after their compulsory military service, ranging from −4 to +5.

Column (2) includes calendar-year fixed effect, which absorbs the common likelihood starting a business in any year from 2007 (window year: −4 for the 2011 cohort) to 2020 (window year: +5 for the 2015 cohort). Column (3) combines both window-year and calendar-year fixed effects. In column (4), we introduce the interaction between these two types of time fixed effects, controlling for the likelihood of our sample individuals drafted in any of the 5 years and starting a business in any of the calendar years. In column (5) and (6), we further include the group fixed effect or the individual fixed effect. These fixed effects control for an individual's time-invariant unobservable tendency to become an entrepreneur, and other correlated time-invariant unobservables among individuals serving in the same military peer group, larger military unit, or armed force. It is worth reiterating that the only variation in our main variable, $EntreExperience_{it}$, is to change from 0 to 1 once the individual i starts his first business in year t. Therefore, by including the individual fixed effect, the coefficients β_1 and β_2 capture solely the sensitivity of how individual i's decision to start his first business in year t is associated with the variation in entrepreneurial ratio of his peers in year t–1 before or after the peer group formation process. Lastly, the standard errors across all specifications are two-way clustered at the group and year levels.

We find that peers' previous entrepreneurial experience is positively and significantly associated with the focal individual's entrepreneurial decision after the peer group formation process across all specifications. The coefficient estimates for β_1 range from 0.142 to 0.174 from columns (3) to (6) where we control for both time fixed effects. These effects are also economically significant. A one-standard-deviation increase in the $EntreRatio_{-it-1}$ during the post-window is associated with 0.17 percentage points to 0.21 percentage points increases in the probability of individual starting a business, which corresponde to approximately 15 percent to 18 percent more relative to the unconditional sample average likelihood of individuals having entrepreneurial experience (1.15 percentage points). By contrast, Table 3 reports no peer effect before the peer forming year; that is, the coefficient estimates for β_2 are not statistically significant in all specifications, and the economic magnitudes are dramatically smaller.

Figure 1 illustrates the dynamics of these peer effects. To investigate the persistency in the peer effects, we extend the event window to [−4:+10].[20] Instead of interacting $EntreRatio_{-it-1}$ with the Pre and $Post$ indicator variables, we interact it with window-year indicator variables for the focal individual to investigate peer effects in each window year. We plot the the coefficient estimates for each window-year along with the 95% confidence intervals. For window years -4 to +1, the peer effect coefficient estimates are staistically insignificant and fluctuate between positive and negative values. This observation adds credence to the random assignment nature of our empirical setting. The peer effect coefficient estimate turns statistically signficant two years after an individual's service year (i.e., window year +2) and continues to increase. Collectively, Table 3 and Figure 1 document a positive peer effect on entrepreneurial decisions among randomly assigned compulsory military peers in Taiwan.

As discussed earlier, our empirical specification captures entrepreneurial spillover through ongoing social interactions after military service. We do not expect to detect meaningful peer effects from variations in entrepreneurial experience obtained prior to military service. Only 0.3 percent of the draftees have founded firms prior to their military service. In Table B.1 of Appendix B, we test if the fraction of draftees in a group with entrepreneurial experience prior to service ($Pre\ EntreRatio$) leads to more draftees among those without prior experience to become entrepreneurs after military service ($Post\ EntreRatio$).[21] As expected, we do not find a significant effect in column (1).

While few draftees have entrepreneurial experience themselves upon military service, a sizable fraction of them (> 30 percent) have parents with entrepreneurial experience. Given that prior literature has documented within-family entrepreneurial spillover, it is plausible that more social peers with entrepreneurial parents prior to military service leads to more spillovers. Therefore, we alternatively broaden our definition of $Post\ EntreRatio$ to include those with

[20] The window year of +10 is the longest period we could observe since our data ends at 2021, and the first cohort was drafted in 2011.
[21] This specification is similar to Lerner and Malmendier (2013).

parents that have started ($Parents1$) or run ($Parents2$) a business. With these two alternative definitions, we find positive and significant peer effect in columns (2) and (3).

3.2. Cross-sectional variations: education

We take a further step and explore if the strength of peer effects documented in our baseline results vary predictably across individuals. In particular, we expect the role of peers to be weaker among those that have had training to prepare them to be entrepreneurs. We use an individual's education attainment to caputre prior exposure to managerial training (Lucas 1978, Calvo and Wellisz 1980, Borjas and Bronars 1989, Evans and Leighton 1989). The substitution effect should be stronger if an individual went to elite universities or majored in finance-related fields.

We observe detailed personal education information through the FIA dataset.[22] We begin with defining three types of education status: having a bachelor's degree with a finance-related major, having a bachelor's degree from a public university, and having a bachelor's degree from a top-five university in Taiwan.[23,24] Table 1 reports average education attainment of our sample military servicemen: 14.4% graduate from public universities; 2.8% graduate from the top five universities in Taiwan; and 9.0% of college graduates major in finance-related subjects. Accordingly, we define an indicator variable ($Edu.Status$) for these three types of education attainment and interact it with the interaction terms $EntreRatio_{-it-1} \times Post_{it}$ in equation (1).

The cross-sectional results are reported in Table 4. In all columns, we observe that the coefficient estiamtes for $EntreRatio_{-it-1} \times Post_{it}$ stay significantly positive, while the coefficient estimates for $EntreRatio_{-it-1} \times Post_{it} \times Edu.Status_i$ are consistently negative. For example, the coefficient estimates in column (1) suggests that the entreprenerial decision of

[22] Expenses related to children's education can qualify as a tax deduction item. Parents utilizing this deduction are required to provide detailed schooling information for their children, including the names of schools and majors pursued. With access to FIA data from 2003 onwards, we are able to observe detailed college enrollment information (if any) for all compulsory military servicemen drafted between 2011 and 2015 in our sample.
[23] We do not use college degree attainment as a partition since 86.2% of draftees have bachelor's degrees.
[24] In Taiwan, there is a prevailing notion that public (national) universities are academically more competitive than private universities. There is wide consensus that the top five public universities are National Taiwan University, National Tsing Hua University, National Yang Ming Chiao Tung University, National Cheng Kung University, and National Chengchi University.

an individual with finance-related major from colledge is essentially not affected by his peers in the post military service window. This is consistent with our conjecture that business-related trading serves as a substitute for entrepreneurial peer effect.

Interestingly, we observe a net *negative* effect for those with bachelor's degrees from public or top 5 universities. For example, in column (3), a one-standard-deviation increase in the $EntreRatio_{-it-1}$ during the post-window leads a 0.33 [0.0119 × (0.151−0.432) = −0.0033] percentage points *decrease* in the probability of individuals graduating from the top five universities to start their own businesses. This suggests that for these individuals, peers' entrepreneurial experience may be perceived as less valuable, or they may adopt a more cautious approach towards peers' entrepreneurial experiences. These findings corroborate the findings of Lerner and Malmendier (2013), where they find on average negative peer effects in entrepreneurial endeavors among the highly educated and financially-savvy HBS MBA students.

3.3. Cross-sectional variations: peers' entrepreneurial parents

We expect the stregnth of peer effect to be stronger if an individual's military peers have more entrepreneurial parents. The same FIA database identifies parental-offspring connections, thus allowing us to track entrepreneurial activities in each family. As discussed in the above, prior research has consistently documented positive influences of entrepreneurial parents on their offsprings in terms of entrepreneurial career choices and subsequent performance due to better exposure to business knowledge, role modeling, managerial best practices (Lindquist et al. 2015, Hvide and Oyer 2020).

We define an individual to have entrepreneurial parents if either or both of his parents have founded a business prior to his military service year. Accordingly, we define a variable $Peers'Entre.Parents_{-i}$ to be the fraction of an individual i's military group peers with entrepreneurial parents. Alternatively, we define $Peers'Entre.Parents_{-i}$ as an indicator variable that equals to one if the fraction of peers with entrepreneurial parents to be higher than the sample median (**37%**).

In Table 5, we interact $Peers'Entre.Parents_{-i}$ with the interaction term $EntreRatio_{-it-1} \times Post_{it}$ and report the estimation results. In column (1), we adopt the continuous version of $Peers'Entre.Parents_{-i}$. The estimate of the triple-interaction coefficient is a marginally signficant 0.482, which implies that having more peers with entrepreneurial parents strengthens the peer effect. Similarly, the triple-interaction coefficient in column (2) where we use the indicator variable version of $Peers'Entre.Parents_{-i}$ yields a statistically significant estimate of 0.086. From column (2), the coefficients imply that peer effect leads to an 11 (20) percent increase in an individual's probability to start a firm if his peers have low (high) fraction of entrepreneurial peers.

4. Entrepreneurial peer effects and performance

We study the intensive margin of entrepreneurial spillover in this section; that is, if an individual's decision to become an entrepreneur is partly driven by peers that have had entrepreneurial experiences, what is the nature of the spillover? If spillover is simply raising awareness, transmitting institutional knowledge (e.g., the administrative steps to establish a firm), or reduce fear and uncertainty, then spillover arguably leads to mediocre or inferior start-ups because this mechanism merely lowers entry costs of entrepreneurship. By contrast, if spillover involves learning entrepreneurial skills such as industry knowledge or managerial know-hows, then spillover should lead to relatively better outcomes.

To this end, we leverage firm outcomes in our FIA database. We start by looking at firms established by non-repeating compulsory military serviceman (i.e., we only include firms by first-time entrepreneurs). We identify **29,803** such firms from 2006 to 2021 in the FIA dataset that are established by those that served between 2011 to 2015. This suggests that by the end of 2021, approximately 8.53% of the compulsory military servicemen have ever started a business [29,803/349,561=0.0853]. We further drop firms that have never filed corporate income tax or reported financial statements throughout the sample period, leaving us with **16,687** firms. We categorize these firms into three groups: (1) *Before Military Group*: firms initiated before

entrepreneurs complete their service. (2) *Low EntreRatio Group*: firms initiated during or after service years by entrepreneurs from social groups with lower than median $EntreRatio$ in the year before.[25] (3) *High EntreRatio Group*: firms initiated during or after service years by entrepreneurs from social groups with higher than median $EntreRatio$ in the year before. To gauge how entrepreneruial spillover affects entrepreneurial performance, we focus on firms that are initiated after the founder's military service and compare annual firm outcomes between those with low versus high fraction of entrepreneurial peers (i.e., group (2) versus group (3)). Specifically, the empirical specification is as follows:

$$Firm\ Outcome_{it\in[0,5]} = \alpha + \gamma_1 High\ EntreRatio_{-it-1} + Fixed\ Effects + \varepsilon_{it}. \qquad (2)$$

In all our specifications, we include county-industry-year, and draft year-year fixed effects. We also include firm age fixed effect when we group firms of different age together. If a higher fraction of military peers have had entrepreneurial experience leads to better future outcomes for individual i's start-up, then the coefficient estimate of γ_1 is expected to be positive.

We tabulate the number of firms in each category in Panel A of Table 5. Limiting to the **16,687** firms that have matched financial statements, there are 991 in the *Before Military Group*, **XXX** in the *Low EntreRatio Group*, and **YYY** in the *High EntreRatio Group*. The total number of firms without financial statements is considerably less at **13,116**. The ratios of firms in the *High EntreRatio Group* in the matched and non-matched partitions are **XX percent** and **YY percent**, respectively. From this perspective, entrepreneurial peers do not play a significant role in terms of whether a startup commence actual business operations. In Panel B of Table 5, we report industry affiliations of the three groups of firms with matched financial statements. Parallel to our findings in Panel A, industry distributions are highly correlated across the three groups of firms, with the majority falling into the Manufacturing, construction, and wholesale/retail industries.

[25] The median ratio is 4.2%.

In the following sections, we examine three gauges of entrepreneurial performance and outcomes: (1) asset size and growth, (2) profitability, and (3) leverage policy.

4.1. Asset size, growth, and profitability

Table 7 reports the regression results where the dependent variables are log asset (columns 1 to 4) and annual asset growth rate (columns 5 to 7). First, we find that individuals who have more entrepreneurial peers tend to start firms that are smaller in terms of asset size in the entry year ($t = 0$). In column (1), we restrict the sample to firms' entry year and find the coefficient estimate of γ_1 to be a statistically significant -0.082. This implies that the *High EntreRatio* group induces new start-ups that are on average 7.9 percent smaller than those from the *Low EntreRatio* group.

Interestingly, we find that the difference in firm sizes does not exist once we move beyond the entry year. Column (2) shows that the size difference is not significantly different from zero by the end of the first year. Beyond the first year, columns (3) and (4) show that the difference is also not significant once we control for firm age fixed effects. This implies that more exposure to entrepreneurial peers leads to faster growth in assets. In column (5), we find that start-ups' asset growth rates are 14.1 percent higher in their first year if founders have more military peers with entrepreneurial experience. This difference is likewise concentrated in the initial phase of a start-up, with insignificant estimates going beyond the first year. Overall, the results in Table 7 suggest that while peer effects lead to conservatively smaller firms at birth, they nevertheless show better growth in their first year.[26] These findings suggest that entrepreneurial spillover entails some scope of learning.

[26] In the empirical literature, the correlation between firm size and firm growth ranges from no relation (Hart 1962, Haltiwanger et al. 2013), negative (Mansfield 1962, Neumark, Wall, and Zhang 2011), or positive (Singh and Whittington 1975, Bentzen, Madsen, and Smith 2012). Theoretically, Gibrat (1931) argues that firm growth rates are determined by random shocks that are independent of firm size (Gibrat's law). Jovanovic (1982) proposes a model in which start-ups start production as they learn about their managerial abilities with diminishing returns, leading to a negative correlation between firm size and growth. Alternatively, larger firms may have more agency problems where managers pursue growth strategies and not profit-maximizing ones, leading to a positive correlation between firm size and growth.

Higher asset growth can be driven by better profitability. Accordingly, we examine the effect of peer effect on firm profitability in Table 8. The measures of profitability are ROA in columns (1) to (3), and gross profitability in columns (4) to (6). With ROA, the results show that firms in the *High EntreRatio* group enjoy higher profitability in their first year, compared to those in the *Low EntreRatio* group. The coefficient estimate of γ_1 implies that the average difference of ROA in the first year across the two groups is a statistically significant and economically meaningful 5.3 percent. Similar to the results for asset growth, this difference does not persist beyond the first year once we control for firm age fixed effect. In column (4), we find that the difference in first-year gross profitability continues to be statistically significant and economically large at 22.3 percent.

4.3. Leverage

Next, we examine the effect of social interactions on firm fundamentals through the lens of capital structure, measured by leverage ratio and current ratio. Table 9 reports the estimation results. In columns (1) to (4), we do not find social interactions significantly affect current ratio in the entry year or beyond. This implies that social interactions do not lead to aggressive short-term debt policies driven by overconfidence. Interestingly, columns (5) shows that the *High EntreRatio* group has significantly higher long-term debt policy as measured by higher leverage ratio in the entry year. The coefficient estimate of γ_1 implies that the average difference is a statistically and economically significant 7.0 percent. Aside from well-documented tax benefits of debt, this finding also suggests the socially-induced entrepreneurs are "realists" that choose to smooth payoffs across different states of nature (Landier and Thesmar 2009), leading to better initial growth as we document in Table 7 and Table 8.

5. Conclusions

Our paper develops a new empirical setting to study entrepreneurial peer effects that addresses two fundamental questions that are challenging to address in one setting. First, we exploit an institutional feature (i.e., compulsory military drafts) that leads to random social group assignments, which significantly promotes plausible causal interpretations. While our paper is

not the first to study random group assignments, our empirical setting is also much larger, generating estimates that have arguably broader implications. Overall, we find that exposures to entrepreneurial peers increases an individual's propensity to also become an entrepreneur.

That being said, we acknowledge that our results remain context-specific, echoing the nature of peer effect research in social sciences (Sacerdote 2014). Our sample individuals, though large in terms of number, are male citizens who are in their early twenties. Readers are encouraged to interpret our results with these characteristics in mind.

Second, our empirical setting enables us to study consequences of entrepreneurial spillovers; that is, the nature of what is being learned through social interactions. This aspect is often lacking in existing literature due to data limitations. We find that entrepreneurial peer effects increase asset growth rates and profitability, especially during the initial stages of a firm. Interestingly, entrepreneurial spillovers also lead to more reliance on long-term leverage as opposed to short-term leverage, suggesting that socially-induced entrepreneurs do not take excessive risk and utilize the tax benefits of debt.

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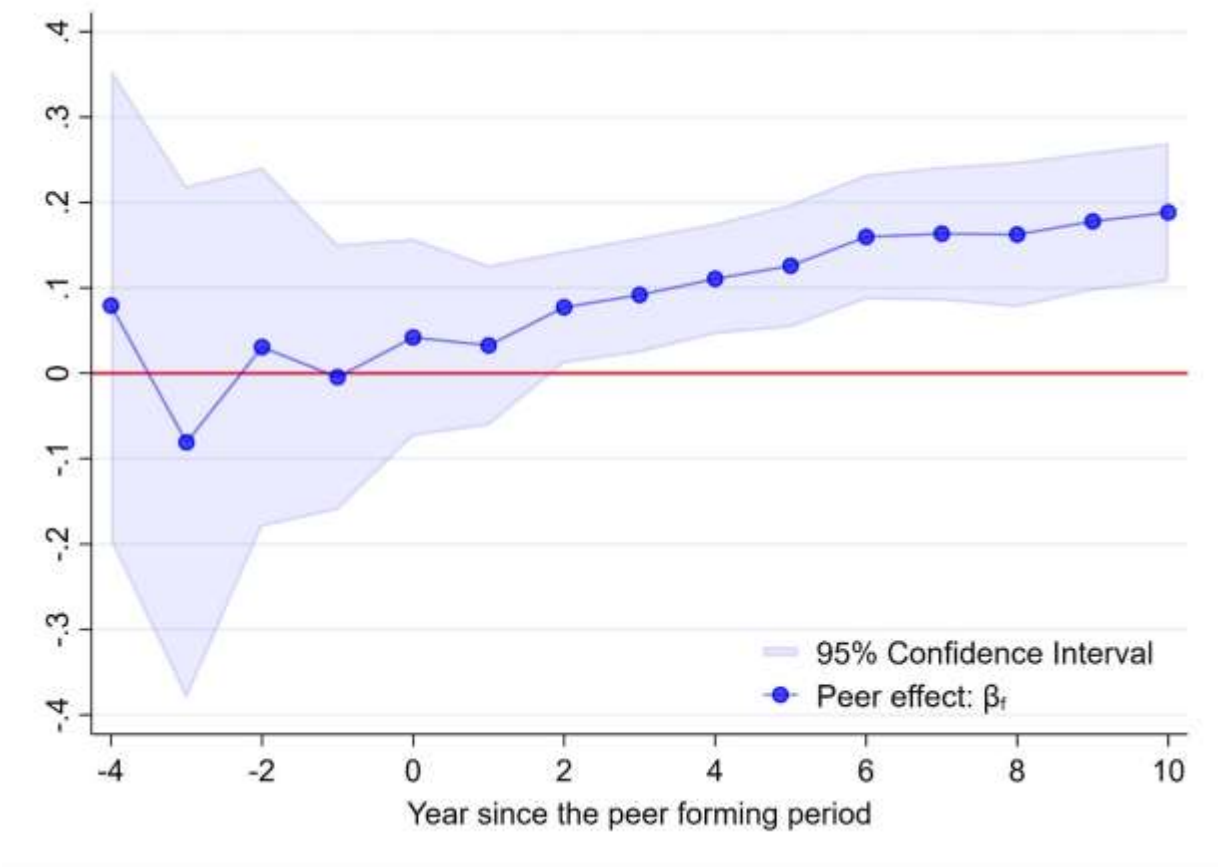

Figure 1. Dynamics of the Peer Effect on Entrepreneurial Decision

The figure plots the year-by-year peer effect coefficients $\widehat{\boldsymbol{\beta}_{\boldsymbol{f}}}$ estimated from the following regression: $\boldsymbol{EntreExperience_{it} = \alpha + \sum_{f=-4}^{10} \beta_f \, EntreRatio_{-it-1} 1_i(t,f) + c_{ft} + c_i + \varepsilon_{i,t}}$, in which $\boldsymbol{EntreRatio_{-i,t-1}}$ is the leave-one-out average ratio of peers with entrepreneurial experience in year $\boldsymbol{t}$. We use $\boldsymbol{f}$ to denote the year since the peer forming year. $\boldsymbol{1_i(t,f)}$ is an indicator variable equal to 1 if year $\boldsymbol{t}$ equals $\boldsymbol{f}$ since $\boldsymbol{i}$'s peer formation year.

Table 1
Summary Statistics

This table provides summary statistics of the main variables in our analysis. The sample consists of 349,561 compulsory military servicemen enrolled between 2011 and 2015. For each individual, we include 10 years' worth of observations: from 4 years before service to 5 years after service. The main dependent variable is $EntreExperience_{it}$, an indicator variable that captures whether individual *i* possesses entrepreneurial experience by the end of year *t*. The main explanatory variable is $EntreRatio_{-it-1}$, defined as the leave-one-out average ratio of *EntreExperience* among individual *i*'s military peers in year *t−1*. We report the summary statistics for the above variables in the full sample ([−4:+5] window years), in the periods before military service ([−4:−1] window years), and in the periods during and after the service ([0:+5] window years). Other demographic variables include individual *i*'s age, income, wealth, and indicator variables that capture if individual *i* holds a bachelor's (and advanced) degree, a bachelor's (and advanced) degree from public universities, a bachelor's (and advanced) degree from the top 5 universities in Taiwan (NTU, NTHU, NYCU, NCKU, and NCCU), a bachelor's (and advanced) degree in areas related to finance, economics, and management, and if one is married. Wealth is the sum of savings, value of vehicles, value of real estate, and stocks in market prices.

	N	Mean	Std.	Min	p25	p50	p75	Max
Dependent/Independent Variables								
EntreExperience	3,495,610	0.0115	0.1068	0	0	0	0	1
EntreExperience (window<0)	1,398,244	0.0016	0.0395	0	0	0	0	1
EntreExperience (window>=0)	2,097,366	0.0182	0.1337	0	0	0	0	1
EntreRatio	3,495,610	0.0079	0.0109	0	0	0.0031	0.0123	0.4
EntreRatio (window<0)	1,398,244	0.0008	0.0020	0	0	0	0.0011	0.0909
EntreRatio (window>=0)	2,097,366	0.0126	0.0119	0	0.0037	0.0093	0.0193	0.4
Demographic Variables								
Age	349,561	22.696	1.898	18	22	23	24	37
Income	349,561	68,184.8	100,556.9	0	0	24,000	107,323	1.5E+07
Wealth	349,561	414,746.2	3,148,188	0	0	0	0	7.4E+08
College	349,561	0.853	0.354	0	1	1	1	1
Public school	349,561	0.144	0.351	0	0	0	0	1
Top5	349,561	0.028	0.164	0	0	0	0	1
Finance	349,561	0.090	0.286	0	0	0	0	1
Marriage	349,561	0.005	0.069	0	0	0	0	1

Table 2
Average Peer Group Characteristics and Test of Randomness

This table provides summary statistics of average group characteristics across military draft groups (Panel A) and the Jochmans (2023) test for random assignment (Panel B). Our sample consists of 25 military units that intake drafted military servants from 2011 to 2015. Except for 2 military units that intake fewer than 50 compulsory military servants during this time period, we conduct a K-means clustering method and subdivide each unit-year peer group into 4 based on the expected month that one is drafted. We then exclude the clustered groups with less than 5 draftees. Our sample comprises a total of 1,310 peer groups. *Group size* is the total number of individuals for the 1,310 peer groups. *Age*, *College* (ratio of servants with college degrees), *Public School* (ratio of servants with public college degrees), *Finance* (ratio of servants with finance-related college degrees), and *Marriage* (ratio of married servants) are the average characteristics of each group by the time that military service begins. *EntreRatio* is the military peers with entrepreneurial experience one-year before the peer-forming year. In Panel B, we report *t*-statistics and corresponding *p*-values of the Jochmans (2023) random assignment tests. The null hypothesis is that individuals are randomly assigned into groups.

	Panel A: Distribution of Peer Group Means							**Panel B**: Tests of Randomness Jochmans (2023)		
	mean	Std.	min	p25	p50	p75	Max	*t*-statistic	*p*-value (two-tail)	*p*-value (right-tail)
Group size	266.84	336.20	5	42	139.5	365	3,191			
Age	22.94	0.99	19.81	22.50	22.90	23.32	28.30	−0.929	0.3530	0.8235
College	0.862	0.172	0.174	0.797	0.927	1.000	1.000	−0.668	0.5039	0.7480
Public School	0.155	0.113	0.000	0.088	0.138	0.194	0.810	0.085	0.9324	0.4662
Finance	0.087	0.055	0.000	0.018	0.054	0.114	0.455	0.757	0.4492	0.2246
Marriage	0.007	0.023	0.000	0.000	0.000	0.006	0.286	0.453	0.6504	0.3252
EntreRatio	0.003	0.007	0.000	0.000	0.000	0.004	0.100	0.701	0.4836	0.2418

Table 3
Peer Effect on Entrepreneurial Decisions

This table reports the panel regression of compulsory military servants' entrepreneurial decisions on the leave-one-out entrepreneur ratio of their military peers along with a set of fixed effects among the 349,651 individuals from an event window of [−4:+5]. The dependent variable, $EntreExperience_{it}$, is an indicator variable that equals 1 if individual i has ever started any business since year t. The independent variable, $EntreRatio_{-it-1}$, is the leave-one-out average ratio of *EntreExperience* for individual i's military peers in year $t-1$. We further interact $EntreRatio_{-it-1}$ with indicator variables, *Post* and *Pre*, which take the value of one when year t is *during and after* or *before* the peer formation year, and zero otherwise, respectively. The control variables are individual fixed effect, group fixed effect, year fixed effect, window year fixed effect, and the interaction of year and window year fixed effect. *t*-statistics (in brackets) are calculated from standard errors two-way clustered at the unit and year level. ***p<0.01, **p<0.05, *p<0.1.

	$EntreExperience_{it}$					
	(1)	(2)	(3)	(4)	(5)	(6)
$EntreRatio_{-it-1} \times Post_{it}$	0.196***	0.489***	0.174***	0.171***	0.144***	0.142***
	4.16	*7.29*	*3.68*	*3.54*	*2.99*	*3.55*
$EntreRatio_{-it-1} \times Pre_{it}$	0.055	−0.143*	0.024	0.021	−0.032	0.071
	1.08	*−1.78*	*0.50*	*0.44*	*−0.70*	*1.24*
Window Year	yes		yes			
Year		yes	yes			
Window Year × Year				yes	yes	yes
Group					yes	
Individual						yes
N	3,146,049	3,146,049	3,146,049	3,146,049	3,146,049	3,146,049
Adj. R2	0.011	0.010	0.012	0.012	0.012	0.435

Table 4
Heterogeneity in Peer Effects on Entrepreneurial Decisions: Education

This table reports the panel regression of compulsory military servants' entrepreneurial decisions on the leave-one-out entrepreneur ratio of their military peers along with a set of fixed effects among the 349,651 individuals from an event window of [−4:+5]. The dependent variable, $EntreExperience_{it}$, is an indicator variable that equals 1 if individual i has ever started any business since year t. The independent variable, $EntreRatio_{-it-1}$, is the leave-one-out average ratio of *EntreExperience* for individual i's military peers in year $t-1$. We interact $EntreRatio_{-it-1}$ with indicator variables, $Post_{it}$ and Pre_{it}, which take the value of one when year t is *during and after* or *before* the peer formation year, and zero otherwise, respectively. We further interact the independent variables with an education indicator variable that takes the value of one if individual i holds a college degree majoring in finance (column 1), holds a public college degree (column 2), or holds a college degree from the top 5 universities (NTU, NTHU, NYCU, NCKU, and NCCU) in Taiwan (column 3). The control variables are individual fixed effect and the interaction of year and window year fixed effect. *t*-statistics (in brackets) are calculated from standard errors two-way clustered at the unit and year level. ***p<0.01, **p<0.05, *p<0.1.

	Finance Degree	Public Schools	Top5 Schools
	(1)	(2)	(3)
$EntreRatio_{-it-1} \times Post_{it}$	0.157***	0.190***	0.151***
	3.95	*4.86*	*3.85*
$EntreRatio_{-it-1} \times Post_{it} \times Edu.\ Status_i$	−0.214***	−0.390***	−0.432***
	−6.44	*−15.90*	*−9.47*
$EntreRatio_{-it-1} \times Pre_{it}$	0.067	0.065	0.070
	1.19	*1.18*	*1.25*
Window Year × Year FE	yes	yes	yes
Individual FE	yes	yes	yes
N	3,146,049	3,146,049	3,146,049
Adj. R2	0.435	0.435	0.435

Table 5
Heterogeneity in Peer Effects on Entrepreneurial Decisions: Peers with Entrepreneurial Parents

This table reports the panel regression of compulsory military servants' entrepreneurial decisions on the leave-one-out entrepreneur ratio of their military peers along with a set of fixed effects among the 349,651 individuals from an event window of [−4:+5]. The dependent variable, $EntreExperience_{it}$, is an indicator variable that equals 1 if individual i has ever started any business since year t. The independent variable, $EntreRatio_{-it-1}$, is the leave-one-out average ratio of *EntreExperience* for individual i's military peers in year $t-1$. We interact $EntreRatio_{-it-1}$ with indicator variables, $Post_{it}$ and Pre_{it}, which take the value of one when year t is *during and after* or *before* the peer formation year, and zero otherwise, respectively. We further interact the independent variables with a variable that captures the extent of entrepreneurship among peers (*Peers' Entre. Parents*$_{-i}$). The variable is defined in the following two ways. In column 1, the variable is defined as the leave-one-out faction of group peers that have entrepreneurial parents. In column 2, the variable is defined as an indicator variable that equals one if the leave-one-out fraction of group peers with entrepreneurial parents is higher than the sample median. The control variables are individual fixed effect and the interaction of year and window year fixed effect. *t*-statistics (in brackets) are calculated from standard errors two-way clustered at the unit and year levels. ***p<0.01, **p<0.05, *p<0.1.

	(1)	(2)
$EntreRatio_{-it-1} \times Post_{it}$	-0.042	0.104***
	-0.38	*2.73*
$EntreRatio_{-it-1} \times Post_{it} \times$ *Peers' Entre. Parents*$_{-i}$	0.482*	0.086***
	1.81	*3.38*
$EntreRatio_{-it-1} \times Pre_{it}$	0.077	0.076
	1.39	*1.39*
Window Year × Year FE	yes	yes
Individual FE	yes	yes
N	3,146,049	3,146,049
Adj. R2	0.435	0.435

Table 6
Industrial distribution of firms started by compulsory military servants

This table reports the number of corporations established by compulsory military servants from 2006 to 2021. We categorize these corporations into three groups: *Before Military Group*, firms initiated by compulsory military servants before their military service; *Control Group*, firms established by compulsory military servants after their service, whose peers have no prior entrepreneurial experience by the year of their service; and *Treatment Group*, firms established by compulsory military servants after their service, with at least one peer who has entrepreneurial experience by the year of their service. Panel A reports the number of firms with matched financial statements, indicating actively operating businesses. Panel B provides the industry distribution of firms across the three categories.

Panel A. Number of firms and financial statement reporting

	Matched	Not Matched	Total
Before Military Group	991	616	1,607
Control Group	2,917	2,454	5,371
Treatment Group	12,779	10,046	22,825
Total	16,687	13,116	29,803

Panel B. Number of firms in each industry

Industry	Before Military Group	Control Group	Treatment Group
Agriculture	8	10	48
Mining	0	0	6
Manufacturing	91	178	818
Electricity and Gas Supply	1	9	49
Water Supply and Remediation	1	16	68
Construction	172	481	2,059
Wholesale and Retail	468	1,208	5,308
Transportation	11	30	132
Hospitality and Catering	35	282	1,175
Publishing	17	113	529
Finance	10	51	238
Real Estate	26	73	432
Professional, Scientific, and Technical Service	52	208	896
Other Industrial and Commercial Services	21	90	350
Education	0	6	36
Medical	1	5	6
Entertainment	19	77	333
Others	24	58	232
No Information	34	22	64
Total	991	2,917	12,779

Table 7
Firm Performance: Asset

This table reports the panel regression of firms' asset size and asset growth in years following their establishment on the treatment status. In columns (1) to (4), the dependent variable is log total assets. In columns (5) to (7), the dependent variable is annual asset growth. The independent variable is an indicator variable that takes the value of one if the leave-one-out ratio of military group peers with entrepreneurial experience by year *t*. Control variables include county-industry-year, draft year-year, and firm age fixe effects (for outcomes two years after establishment). *t*-statistics (in brackets) are calculated from standard errors clustered at county level. ***p<0.01, **p<0.05, *p<0.1.

	Log(Asset)				*Asset Growth*		
	Firm age=0	*Firm age=1*	*Firm age>1*	*Firm age>1*	*Firm age=1*	*Firm age>1*	*Firm age>1*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High EntreRatio	-0.082***	-0.026	-0.144**	-0.044	0.141**	0.124**	0.016
	-2.49	*-0.48*	*-2.29*	*-0.84*	*2.35*	*2.08*	*0.27*
County × Industry × Year FE	yes	yes	yes	yes	yes	yes	yes
Draft Year × Year FE	yes	yes	yes	yes	yes	yes	yes
Firm Age FE				yes			yes
N	11,187	8,592	15,296	15,296	7,950	15,073	15,073
Adj. R2	0.086	0.081	0.072	0.074	0.000	0.002	0.006

Table 8
Firm Performance: Profitability

This table reports the panel regression of firms' profitability in years following their establishment on the treatment status. In columns (1) to (3), the dependent variable is ROA. In columns (4) to (6), the dependent variable is gross profitability (Novy-Marx 2013). The independent variable is an indicator variable that takes the value of one if the leave-one-out ratio of military group peers with entrepreneurial experience by year *t*. Control variables include county-industry-year, draft year-year, and firm age fixe effects (for outcomes two years after establishment). *t*-statistics (in brackets) are calculated from standard errors clustered at county level. ***p<0.01, **p<0.05, *p<0.1.

	ROA			*Gross Profitability*		
	age=1	*age>1*	*age>1*	*age=1*	*age>1*	*age>1*
	(1)	(2)	(3)	(4)	(5)	(6)
High *EntreRatio*	0.053***	0.028***	0.009	0.223**	0.090***	-0.044
	2.40	*2.48*	*0.74*	*2.31*	*2.48*	*-0.85*
County × Industry × Year FE	yes	yes	yes	yes	yes	yes
Draft Year × Year FE	yes	yes	yes	yes	yes	yes
Firm Age FE			yes			yes
N	10,120	20,738	20,738	10,120	20,738	20,738
Adj. R2	0.052	0.054	0.055	0.045	0.043	0.044

Table 9
Firm Performance: Leverage

This table reports the panel regression of firms' leverage policy in years following their establishment on the treatment status. In columns (1) to (3), the dependent variable is current ratio. In columns (4) to (6), the dependent variable is leverage ratio. The independent variable is an indicator variable that takes the value of one if the leave-one-out ratio of military group peers with entrepreneurial experience by year *t*. Control variables include county-industry-year, draft year-year, and firm age fixe effects (for outcomes two years after establishment). *t*-statistics (in brackets) are calculated from standard errors clustered at county level. ***p<0.01, **p<0.05, *p<0.1.

	Current Ratio				*Leverage Ratio*			
	Firm age=0	*Firm age=1*	*Firm age>1*	*Firm age>1*	*Firm age=0*	*Firm age=1*	*Firm age>1*	*Firm age>1*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High *EntreRatio*	5.107	11.28	5.907	2.65	0.070**	0.125***	-0.156	-0.064
	[0.37]	[1.22]	[0.88]	[0.33]	[2.23]	[2.97]	[-1.54]	[-0.73]
County × Industry × Year FE	yes	yes	yes	yes	yes	yes	yes	yes
Draft Year × Year FE	yes	yes	yes	yes	yes	yes	yes	yes
Firm Age FE				yes				yes
N	8,008	7,013	13,084	13,084	11,180	8,586	15,287	15,287
Adj. R2	-0.010	-0.009	0.027	0.027	0.017	-0.009	0.005	0.005

APPENDIX A

Variable Definitions

Dependent variables

Asset growth of a firm in year *t* is the year-to-year growth in a firm's total assets. Negative asset values are replaced with zero.

Current Ratio is a firm's current assets divided by current liabilities.

EntreExperience is an indicator variable that equals one if individual *i* has gained experience starting a business by year *t*.

Gross Profitability of a firm in year *t* is defined as sales minus cost of goods sold, divided by lag total assets. Negative asset values are replaced with zero.

Leverage Ratio of a firm in year *t* is defined as total liabilities divided by total assets. Negative asset values are replaced with zero.

Log(asset) is the natural logorithm of a firm's total assets in year *t*.

ROA of a firm in year *t* is defined as net income divided by lag total assets. Negative asset values are replaced with zero.

Demographic variables

Age is the difference between an individual's birth year and the year of observation.

College is an indicator variable that equals one if individual *i* holds a bachelor's degree by the time military service begins.

Marriage is an indicator variable that equals one if individual *i* is married by the time military service begins.

Public school is an indicator variable that equals one if individual *i* holds a bachelor's degree from a public university by the time military service begins.

Top5 school is an indicator variable that equals one if individual *i* holds a bachelor's degree from one of the top five universities in Taiwan (NTU, NTHU, NYCU, NCKU, NCCU) by the time military service begins.

Other variables

EntreRatio is the leave-one-out ratio of military group peers with entrepreneurial experience by year *t*.

High EntreRatio is an indicator variable if the leave-one-out ratio of peers with entrepreneurial experience (*EntreRatio*) of an individual is higher than the sample median in the year before a firm is founded.

Group size is the number of individuals in a military peer group.

Peers' Entre. Parents (continuous) is the leave-one-out ratio of military group peers with entrepreneurial parents by the time military service begins. An individual has entrepreneurial parents is defined as either or both parents have founded firms.

Peers' Entre. Parents (indicator) is an indicator variable equals to one if individual *i*'s military group peers have higher than median fraction of entrepreneurial parents. An individual has entrepreneurial parents is defined as either or both parents have founded firms.

POST is an indicator variable that equals one if year *t* is during or after military service; that is, window year +1 to year +5.

PRE is an indicator variable that equals one if year *t* is before military service; that is, window year -4 to year 0.

APPENDIX B

Table B.1
Pre-Military Service Entrepreneurial Experience Spillover

This table reports the panel regression of compulsory military servants' entrepreneurial decisions on peers' pre-military entrepreneurial experience. The dependent variable, *Post EntreRatio*, is the ratio of individuals without pre-military extrepreneurial experience and subsequently become entrepreneurs within five years after military service. The independent variable, *Pre EntreRatio*, is the ratio of individuals with pre-military entrepreneurial experience. *Pre EntreRatio (Parents1)* and *Pre EntreRatio (Parents2)* are the ratios of individuals with (1) pre-military entrepreneurial experience or (2) parents who founded firms (*Parents1*) or have run firms (*Parents2*). The control variables are the average age at 2021, group size, and draft year fixed effect. *t*-statistics (in brackets) are calculated from standard errors two-way clustered at the unit and year level. ***p<0.01, **p<0.05, *p<0.1.

	Post EntreRatio		
	(1)	(2)	(3)
Pre EntreRatio	-0.109		
	-0.48		
Pre EntreRatio (Parents1)		0.160***	
		3.47	
Pre EntreRatio (Parents2)			0.188***
			4.54
Age at 2021	-0.002	-0.004	-0.003
	-0.77	*-0.91*	*-0.82*
Group size	0.000	0.000	0.000
	-1.43	*-1.50*	*-1.52*
Draft Year FE	yes	yes	yes
N	1,310	1,310	1,310
Adj. R2	0.055	0.062	0.066

INTERNET APPENDIX

for

Learning from the Band of Brothers: Evidence from Entrepreneurial Spillover

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(NOT INTENDED FOR PUBLICATION)

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Figure IA.1. Distribution of Employment Duration of Identified Military Units.
This figure shows employees' job duration distribution among general public institutions and institutions that we identify as military units. We report the average employee job duration and *p*-values that test the difference between the two groups.

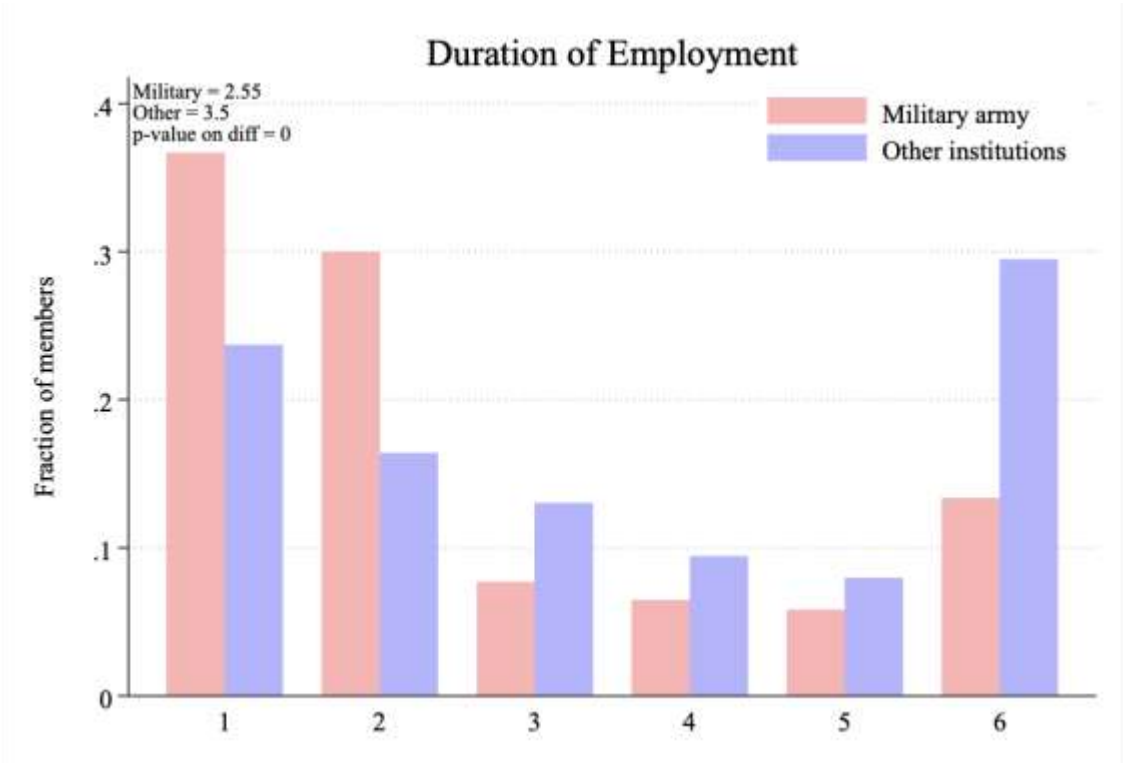

Figure IA.2. Age Distribution of Identified Military Units.
This figure shows employees' age distribution among general public institutions and institutions that we identify as military units. We report average employee ages and *p*-values that test the difference between the two groups.