# Liberalizing Home-Based Business

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## Abstract

Working at home benefits entrepreneurs by lowering fixed costs and allowing them to engage in joint market and household production. We evaluate a large-scale reform in Singapore, the Home Office Scheme, that allowed business creation at one's residential property and study whether home-based entrepreneurship spurs entrepreneurial activities. The difference-in-differences estimate shows that the reform led to a significantly higher level of business creation and that the firms newly created in response to the reform had a higher survival rate. The effect is more pronounced for low-income female individuals and industries with high startup capital, implying that financial constraints and nonpecuniary benefits likely drive the effect. The reform also encourages entrepreneurs to become serial entrepreneurs, and they open a larger business with a similar survival rate for their second firm. Overall, our findings suggest that the program effectively attracted more entry into self-employment without significantly lowering the average quality of the pool.

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# 1. Introduction

Entrepreneurship has long been recognized as a key mechanism for enhancing economic development. The value of supporting entrepreneurship was again confirmed when small businesses acted as one of the most powerful generators of new jobs on the path to global economic recovery from the Great Recession. Therefore, how to design and evaluate policies to remove barriers to entry and foster entrepreneurship is a question that intrigues policy-makers and academics. A large body of studies is devoted to investigating factors that discourage entrepreneurship, which include regulation limits, access to capital, downside risk, and career concerns. At the same time, previous literature has documented substantial nonpecuniary benefits of self-employment, such as enjoyment, control, and flexibility, which explain the significant earning differential between those in self-employment and paid employment (e.g., Hamilton, 2000; Hurst and Pugsley, 2012). This paper echoes this literature by assessing the effects of one large-scale government intervention concerning home-based business creation.

In this paper, we evaluate the Home Office Scheme implemented in Singapore in late 2001 to remove barriers to home-based businesses and allow potential entrepreneurs to set up new businesses in their homes. The reform provided sizable financial and nonfinancial incentives to promote business creation in three aspects. It reduces entrepreneurs' fixed monetary costs, including office space rental and commuting expenditures. Second, workplace flexibility further enhances the nonpecuniary benefits of being an entrepreneur and allows engagement in joint market and household production. Third, prior literature has found that the social status of entrepreneurs (e.g., possible shame from a business failure) is an important driving force behind interest in entrepreneurship (Begley and Tan, 2001). The reform in this study helped obscure the salience of possible business failure and thus helped failed entrepreneurs avoid experiencing the consequent shame and humiliation. In other words, the reform reduced the cost of experimentation and enabled entrepreneurs to work on an idea with fewer concerns about possible negative

consequences. This is supported by anecdotal evidence that entry into entrepreneurship increased following the reform[1].

Identification of the reform's impact requires an estimate of the counterfactual level of entrepreneurial activities to filter out the effect of other potential macroeconomic shocks. Our empirical strategy exploits the variation in exposure to the scheme across industries. To ensure that the newly created businesses would not cause disturbance to residential neighborhoods, the scheme stipulated a negative list of industries prohibited from home-based operations, which constitute our control group. In contrast, the treatment group contains industries allowed under the scheme. Thus, the implemented reform lends itself to examination with a standard difference-in-differences estimation design.

We start our analysis by verifying the validity of our empirical design. We confirm the identification assumption that the treatment and control groups would have displayed similar changes in entrepreneurial activity in the absence of the program. In addition, we include the interaction term of the treatment group dummy with both time-varying industry and macro controls and show that the estimate is robust to consideration of industry-level responses to fluctuations in the overall economy. The difference-in-differences estimate suggests a positive and significant impact of the reform: firm creation grew by 16 percent more following the reform for the treated industries than for the control group.

To investigate the possible channels, we show that the effect is more pronounced for low-income individuals, for whom entry regulation with high fixed costs is most likely to be the greatest barrier to entrepreneurship. The effect is not homogenous across industries and is larger for those that require high starting capital. Altogether, financially constrained entrepreneurs benefit more from the reform, indicating that reducing entry costs is important to encourage firm creation. Moreover, the reform encouraged failed entrepreneurs to start new businesses in the future. We also find evidence that the treatment industries attract relatively more female entrepreneurs, who are supposed to benefit more from engagement in joint market and household production,

[1] According to Skyline Singapore, "more than 3,000 homeowners have jumped on the bandwagon within the first month to run businesses such as computer design, IT accounting, management consultancy and software programming."

consistent with the nonpecuniary benefits channel. However, we do not find evidence supporting the channel of the reform's obscuring of the salience of possible business failure.

An effective policy to promote entrepreneurship should be not only an endeavor of facilitating entry in the short term but also one that creates a healthy entrepreneurial ecosystem. We go beyond the focus on the intensity/magnitude of entrepreneurial activities and investigate the welfare implication of the reform. In particular, we explore whether the Home Office Scheme significantly changed the quality of newly built firms. Survival analysis indicates that the newly created businesses, starting relatively small, are as likely as the control firms to exit in their first two years. More interestingly, they exhibit a higher survival rate in the long horizon. The increase is more pronounced among low-income individuals and industries that require high starting capital, suggesting that financially constrained entrepreneurs benefit more from the reform. Using a difference-in-differences (DiD) design, we show that, in the long run, the Home Office Scheme encouraged entrepreneurs to start a second firm. The second firm is larger, with a survival rate similar to that of the first firm. These results should be interpreted cautiously because we cannot distinguish the selection effect from the treatment effects in this paper. The evidence indicates that the Home Office Scheme effectively spurs entrepreneurial activities and attracts more entry into self-employment without significantly lowering the average quality of the pool.

Our study adds to the growing literature on factors that affect entrepreneurship and policies designed to facilitate entry. Limited access to finance is often viewed as the leading factor that dissuades potential entrepreneurs from creating businesses and hinders startups' growth (Evans and Leighton, 1989; Holtz-Eakin et al., 1994; Hurst and Lusardi, 2004). Several studies document that relaxing financial constraints increases entrepreneurship (Beck, Pamuk, and Uras, 2017; Bertrand et al., 2007; De Mel et al., 2008; William R. Kerr, 2010; Kerr, Lerner, and Schoar, 2011; Andersen and Nielsen 2012; Marquez and Yavuz, 2013; Adelino et al., 2015; Schmalz, Sraer, and Thesmar 2017; Denes et al., 2020). Financial intermediaries play a large role in nurturing entrepreneurial firms (Black and Strahan, 2002; Chemmanur and Fulghieri, 2014). In addition to financial constraints, nonpecuniary benefits and entry barriers in regulation play an important role in business formation (i.e., Hurst and Pugsley, 2012). Djankov et al. (2002) and Klapper et al. (2006) exploit country-level variation in entry regulation to show the impact on entry rates worldwide. Other studies focus on country-level reforms to entry regulation and investigate how

they affect the number of workers drawn into entrepreneurship (Mullainathan and Schnabl, 2010; Bruhn, 2011; Branstetter et al., 2014). The design of policies to remove barriers to entry and foster entrepreneurship also intrigues policy-makers. Governments worldwide spend billions of dollars subsidizing entrepreneurship training programs (Karlan, D. and Valdivia, 2011; Fairlie, R.W., Karlan, D. and Zinman, 2015). In our study, a reform allowing home-based entrepreneurship acts as an efficient form of monetary transfer to entrepreneurs in terms of reducing the operating cost of business creation (e.g., office space rental, commuting expenditure). We thus complement these studies by examining how removing barriers introduces both financial and nonfinancial benefits and encourages marginal entrepreneurs to start new businesses. We corroborate that these types of subsidies indeed foster entrepreneurial activities in the relevant industries.

Our results also contribute to the literature discussing potential inefficiencies arising from zoning (Cutler and Glaeser, 2021). We show that relaxing rigid zoning restrictions can mitigate the inefficiencies that hinder small enterprises from registering and operating businesses from home. We also find that the effect is more pronounced for female individuals. This is consistent with the gendered implications of zoning restrictions: since women are often responsible for children, "the legal separation of home from formal workplaces can make life incredibly difficult" (Criado Perez, 2019). Relaxing zoning restrictions can mitigate gender inequalities by reducing the entry costs of female entrepreneurs.

Finally, our paper relates to the literature that views entrepreneurship as experimental. Entrepreneurship is fundamentally about experimentation because the knowledge and skill set required are unlikely to be known in advance (Kerr, Nanda, and Rhodes-Kropf, 2014). Recent studies (Manso, 2016; Dillon and Stanton, 2017) have taken a theoretical approach to quantifying the option value of experimenting with new business ideas. The experimentation perspective suggests that the foremost hurdle to entrepreneurship is any friction that discourages experimenting with ideas. These frictions include regulation barriers (Klapper, Laeven, and Rajan, 2006), technology (Ewens et al., 2017), financing risk (Nanda and Rhodes-Kropf, 2013), downside career concerns (Gottlieb et al., 2017; Hombert et al., 2020; Di Maggio and Kermani, 2021), health care access (Zandberg, 2021) and uncertainty (Kuhnen and Oyer, 2016). Our results highlight the role of workplace flexibility in facilitating entrepreneurship by enabling entrepreneurs to experiment with ideas without incurring high fixed costs. This finding has important normative

implications for governments to design programs and policies to reduce the cost of experimentation in general.

The remainder of the paper is organized as follows. Section 2 introduces the home-based entrepreneurship reform, and Section 3 presents our data and empirical strategy. Section 4 reports the results of the effects of reform on entrepreneurial activities. Section 5 concludes.

## 2. The Home-Based Entrepreneurship Scheme

Singapore is an island nation with an area of approximately 733 square kilometers. As of 2022, it had a population size of 5.64 million, which included 3.55 million citizens and 0.52 million permanent residents, and the remaining being foreigners. The population is composed of a diverse mix of ethnic groups, including 74% Chinese, 13% Malays, 9% Indians, and 3% of other races. Singapore's land use planning is guided by the Master Plan (MP), a statutory plan reviewed every five years. The MP contains detailed planning parameters on the permissible land use and density for every land parcel in Singapore. The Urban Redevelopment Authority (URA), Singapore's planning authority, uses the zoning and plot ratio and other planning parameters on the MP to guide land developments in the local areas of Singapore. The zoning covers a comprehensive list of land use types, including residential, commercial, industry, hotel mixed-use, and institutional uses. Singapore takes a long-term approach to planning due to its scarce land resources. The MP spells out the development strategies for the next 40 to 50 years to ensure that the amount of land is sufficient to meet long-term population and economic growth needs while balancing environmental quality. This long-term land use plan is reviewed regularly.

Singapore has a dual housing system consisting of a public and a private housing market. Its homeownership rate of 89.3% as of 2022 is one of the highest in the world. The public housing market is highly regulated. The government, via the housing agency, the Housing & Development Board (HDB), builds and sells public housing at concessionary prices to eligible families. New public housing flats, or HDB flats, are sold to only Singaporean citizens whose monthly household income does not exceed S$14,000. For low- and median-income families earning below S$9,000 a month, tiered housing grants ranging from S$5,000 to S$80,000 are given to families buying HDB flats.

The demand for HDB flats is highly competitive, with application rates that can be 10 times the number of available flats launched in some popular housing estates. The HDB requires owners

who successfully applied and bought their flats to fulfill a minimum occupation period (MOP) of 5 years before they are allowed to sell their flats in the secondary market at open market prices[2]. The public housing market serves the housing needs of the bulk of Singaporean residents, constituting 77.9% of the total housing stock in Singapore as of 2022.[3] The balance of the housing stock is provided by the private market, which operates as a *laissez-faire* market. The private market is not subsidized and is expensive to average households. It provides buyers diversity of housing types, which range from apartments and condominiums to landed houses (such as terrace, semidetached and detached houses). Except for landed houses, which can be sold only to Singaporean families, foreigners can buy apartments and condominiums in the private housing market. Private developers buy land from the Urban Redevelopment Authority (URA), the government's land sale agency, and build private housing on the land.

We evaluate the Home Office Scheme (HOS) implemented by the Ministry of National Development of the Singapore government to reduce potential entrepreneurs' costs of starting a new business[4]. Zoning regulations strictly forbade the use of residential addresses for business registrations before the HOS policy. Setting up a business involved incurring monetary and nonmonetary costs, such as finding office space and incurring high rental expenses, furniture and fixture costs, and commuting costs. Such high startup costs could prevent the creation of many small enterprises. The HOS policy, which allowed entrepreneurs in a selected list of industries to register and conduct business activities at residential premises, eased the barriers to entry for many small home business startups and enterprises. These changes were implemented in two stages:

[2] Young Singaporean families follow the typical housing upgrading path by buying their first house from the HDB after they enter the labor market and when their income is still below the ceiling set by the HDB. After fulfilling the MOP, some will sell their HDB flats and move up the housing "ladder" by buying condominiums or apartments in the private market.

[3] These and the following statistics of Singapore's population and housing market are drawn from the *Population Trends 2022*, Department of Statistics, Singapore.

[4] As specified in the official guideline, the purpose of the scheme is to give would-be entrepreneurs the flexibility to conduct business from their homes.

(1) At the end of November 2001, the Urban Redevelopment Authority launched a pilot Home-Based Scheme to allow small-scale businesses to operate from homes in selected mixed-zone areas; and

(2) On June 10, 2003, the Housing & Development Board and the Urban Redevelopment Authority jointly introduced the new Home Office Scheme applicable to all residential units.

The scheme allowed homeowners the flexibility to work from the comfort of their own homes. It reduced startup costs for small businesses by allowing them to operate from the entrepreneur's home rather than from separate, rented office premises. It also saved new entrepreneurs commuting time and expenses and enabled residents to spend more time with their families. The general goal was to incentivize would-be entrepreneurs to create businesses.

Singapore's office rents are relatively expensive in the world. According to market research,[5] in 2000, the monthly average office rent was 4.37 US dollars per square foot. Assuming that a small firm requires at least 100 square feet, the monthly rent would be approximately 437 US dollars. Additionally, the scheme helped to reduce commuting time and transportation costs, lowering operating expenditure by a significant amount for a typical small firm.

Under the Home Office Scheme, registration for home office authorization is easily done online, and entrepreneurs can commence their businesses immediately upon successful registration. Hence, this scheme is particularly suitable for startups that wish to minimize time and costs in setting up an office. In addition, business owners do not have to worry about their home office authorization expiring, since the permit is valid as long as the business remains in operation. Two important notes about the reform are worth mentioning. First, business registration at residential addresses was strictly forbidden before the scheme. The new policy gave entrepreneurs the flexibility to conduct business from their homes. Second, to ensure that the newly created home businesses would not cause disturbance to the residential neighborhood, the scheme stipulated a negative list of industries prohibited from home-based operations. In other words, entrepreneurs in all industries except those in Panel A of Table A1 can use their residential home address to

[5] https://www.straitstimes.com/business/property/soaring-hong-kong-office-rents-are-now-triple-those-of-singapore

register businesses. The negative list includes mainly industries that might disturb neighbors due to heavy foot traffic or the nature of the manufacturing process. For example, a) maid agencies, d) commercial schools, e) sales/marketing offices, and p) beauty/hair-dressing/massage therapy services are industries that often draw heavy foot traffic. g) The manufacture/preparation/processing of products and goods is on the negative list due to noise during manufacturing process. The list of non-permissible businesses enables us to estimate the counterfactual level of entrepreneurial activities in the absence of the program[6].

[Insert Table A1, Figure A1 here]

# 3. Data and Empirical Strategy

In this section, we describe our data sources, construct our main variables, and specify our empirical strategy.

## 3.A. Sample and Data Sources

We use data primarily from two sources in this study. The basic registry data are obtained from the Accounting and Corporate Regulatory Authority (ACRA), the regulatory body overseeing business entities. According to Section 5 of the *Business Names Registration Act*, generally, all forms of businesses must be registered with the ACRA. Therefore, our registry data cover the universe of firms created from 1980 to 2015 in Singapore. For each newly built firm, the dataset includes the firm's name, the industry that the firm operates in, the registry date, and the firm's legal status (sole proprietorship, partnership, or company). It also provides the cease date for each firm, which is especially helpful for our survival analysis. More importantly, we can observe the name and national identification number of the founders, enabling us to merge these data with the demographic data discussed below. To avoid contamination from other relevant policies that may promote new firm formation in the event window, we restrict our analysis to January 1999 to March 2005. The sample starts from 1999 to avoid any contaminating effect of

[6] We plot the regional distribution of firm creation and public housing in Figure A1.

the 1997 Asian financial crisis. The ending period is chosen because an act permitting registration of limited liability partnerships came into operation in April 2005.

The second data source is a unique personal database containing demographic information on more than 2 million individuals in Singapore. It contains a full sample of Singapore's residents (Singapore citizens and Singapore permanent residents) over 18 years old, constituting nearly 60% of Singaporean population as of 2012 (Agarwal et al., 2016). The dataset includes a comprehensive set of demographic characteristics such as gender, date of birth, race, marital status, housing address (public or private), and postal code. Using the unique personal identification numbers, we can match the personal database with the firm registry database to obtain, with a high degree of accuracy, the demographic profile (including age, race, marital status, and gender) of every individual registered with each new firm. Compared with the survey approach used to acquire the demographic and personal information of entrepreneurs in previous studies (e.g., the SINE survey in Landier and Thesmar, 2009), our merged dataset contains a richer set of demographic variables with less measurement error[7]. When we merge the firm registry data and personal demographic data, the match rate is 90.25%. This suggests that Singapore's residents are more likely to start a business than foreigners.[8]

## 3.B. Methodology

### 3.B.1. Empirical Strategy

The greatest challenge in evaluating the program's impact on outcome variables is to obtain an estimate of the counterfactual level of entrepreneurship in the absence of the program to separate its effect from the effects of any other shocks to macroeconomic fundamentals. An important aspect of the policy for our purpose is that it explicitly laid out a list of industries

[7] Hurst, Li and Pugsley (2014) document evidence of underreporting by self-employed in U.S. household survey data

[8] In Table A4 of Online Appendix, we split the sample into entrepreneurs by local residents and foreigners and estimate our main specification by including a triple interaction term of Post, T and Local indicator. The results show that the Home Office Scheme leads to a relatively higher entry into self-employment among local entrepreneurs than the foreign counterparts.

prohibited from home-based operations. Specifically, firms that operate in nonpermissible sectors are ineligible for home office applications and, therefore, can serve as a control group. This enables us to circumvent the aforementioned obstacle by exploiting variation in exposure to the Home Office Scheme and defining the treatment and control groups using difference-in-differences analysis.

To ease the concern that the result may be driven by industry concentration in particular geographies, we analyze the impact on firm creation at the industry–district–year–quarter level. Specifically, our main difference-in-difference specification to estimate the reform effect is presented as follows in Equation (1).

$$Y_{j,d,t} = \alpha + \beta \times Treated_j \times Post_t + X'_{j,t}\gamma + \delta_j + \gamma_t + \theta_{yd} + \varepsilon_{j,d,t} \quad (1)$$

where $Y_{j,d,t}$ represents industry-level outcomes (such as the number of newly created firms in logarithms) for industry $j$ and district $d$ in month $t$. $Treated_j$ takes a value of one for industries in the treatment group, those not on the forbidden list of the Home Office Scheme, and zero for the control group. $Post_t$ takes the value of one for quarters after the policy (2002 Q1 to 2005 Q1) and zero otherwise. Vector $X_{j,t}$ contains both time-varying industry-level and macroeconomic controls, including (1) *industry productivity*, defined as the change in value added per worker in each industry, and (2) *GDP Growth*, the annual GDP growth in Singapore, as well as their interaction with the treated dummy, $Treated_j$. $\delta_j$ is the industry fixed effect. $\gamma_t$ is the year–quarter fixed effect. $\theta_{yd}$ is the year X district fixed effect. All specifications are weighted by industry size to address the concern that the conditional variance of the error term varies across industries, which is the basis of our treated dummy. The standard errors are clustered by industry and year-quarter. The coefficient β measures the effects of the program on the treatment group relative to the outcomes of the control.

$$Y_{j,d,t} = \alpha + \beta_{pre} \times Treated_j \times Pre_t + \beta_{post} \times Treated_j \times Post_t + X'_{j,t}\gamma + \delta_j + \gamma_t + \theta_{yd} + \varepsilon_{j,d,t} \quad (2)$$

Moreover, Equation (2) implements a test of the validity of our difference-in-difference design, which requires $\beta_{pre}$ to be not statistically or economically significantly different from zero. We choose the period before 2001 (1999Q1 to 2000Q4) as the benchmark period, and $Pre_t$ is a binary variable that equals 1 for the period before the announcement (2001Q1 to 2001Q4) and otherwise 0.

We also decompose the post-period dummy to study the dynamic responses at each stage of the reform in Equation (3).

$$Y_{j,d,t} = \alpha + \beta_{post,1} \times Treated_j \times Post\_Stage1_t + \beta_{post,2} \times Treated_j \times Post\_Stage2_t + X'_{j,t}\gamma + \delta_j + \gamma_t + \theta_{yd} + \varepsilon_{j,d,t} \quad (3)$$

Specifically, $Post\_Stage1_t$ is a binary variable that equals one for the observations from the first stage of the postreform period (2002Q1 to 2003Q2) and zero otherwise; $Post\_Stage2_t$ is a binary variable that equals 1 for observations in the second stage of the postreform period (2003Q3 to 2005Q1) and 0 otherwise.

In addition, we implement an event study version of Equation (1) by replacing the post indicator $Post_t$ with a list of quarter-since-policy dummies while using the same set of controls and fixed effects:

$$Y_{j,d,t} = \alpha + \sum_{k=-9}^{8} \beta_k \times Treated_j \times I(t = k) + X'_{j,t}\gamma + \delta_j + \gamma_t + \theta_{yd} + \varepsilon_{j,d,t} \quad (4)$$

where $I(t = k)$ indicates the $k_{th}$ quarter relative to the policy implementation time. The coefficient of interest, $\beta_k$, captures the difference in trends in business creation between the treatment group and the control group $k$ quarters before/after the policy. We dummy out the remaining sample periods.

### 3.B.2. Discussion of Identification Assumptions

We discuss the extent to which our empirical framework can estimate the causal effect of the housing reform. First, the identification of the difference-in-differences estimate hinges on the validity of the parallel trend assumption. In our context, this assumption is that, in the absence of the program, the treatment and control groups would have displayed similar patterns in the outcome variables during the period of the program. Although the assumption is not directly testable, we can gauge its plausibility by comparing the firm creation trends around the policy announcement time and see whether they are parallel across the treated and control groups during

the period leading up to the reform. To provide evidence to support this assumption, we plot the unconditional number of firm creations in Figure 1. As can be observed, there are no differential trends when we compare the treatment with the control group before the program.

As a formal test, we further plot in Figure 2 the estimates of the dynamic treatment effect using an event study regression in Equation (4). The trends in business creation are parallel for the treated and control groups before the policy, with most coefficients bundling around zero. We observe a substantial change in the coefficients immediately after the policy implementation. The differential becomes even larger for the period after the Home Office Scheme (HOS) was extended to all residential units. The observation is consistent with the appropriateness of the natural experiment and difference-in-difference regressions as an empirical method for evaluating the effects of the HOS policy.

The second concern is that the documented differences in firm creation may be driven by the differential responses of the treatment and control groups to fluctuations in overall economic conditions. If the assumption is violated, we might observe that industries in the treatment and control groups display different evolutions even without the reform. We include the interaction term of the treatment group dummy with both a time-varying industry control, *industry productivity*, and macroeconomic conditions, *GDP Growth*, to capture the impact on the outcome variables due to differential responses to macroeconomic fluctuations among treated and control industries.

One may still be concerned about the existence of secular trends that differ systematically between the treated and control groups that lead to parallel trend invalidity. Therefore, we also test the sensitivity of our estimates to violations of the parallel trends assumption following procedures described in Rambachan and Roth (2023). Specifically, we compare the 95% confidence intervals from our primary DiD model against those obtained after we allow for deviation from a linear trend of up to an arbitrary amount of M. In Figure A3a, we plot the confidence interval and show that the estimates in both periods are significantly different from zero when we include a treatment group–specific linear trend. The pattern persists even when we permit annual deviations from a linear trend by as much as 0.035 and 0.02 when we consider the deviation for the first ($\tau = 1\ or\ \tau_{2002Q1}$) and second ($\tau = 2\ or\ \tau_{2002Q2}$) posttreatment periods. We include a detailed discussion in Appendix B.

Another concern is that the Singapore government may have chosen industries with high aggregate demand in the Home Office Scheme. We argue that this concern is less of an issue for two reasons. First, as stipulated in the announcement, the negative list of industry types was specified to ensure that disturbance was minimized in the residential neighborhood. The policy requires that all applications be approved as part of the enforcement procedure. Residential property should not be used for activities falling under the negative list of unpermitted industry types. In the robustness check, as the first-stage reform was implemented in selected areas only, we also exploit the geographic variation in firm creation within the same industry. Specifically, we compare firm creation for the same industry in the selected versus not selected areas before and after the reform. Second, we show in Figure 1 that there are no significant differences in entrepreneurial activities between the treatment and control groups for the pre-policy period. Therefore, firm creations in the treatment and control groups during the pre-policy period are likely to follow common trends. More importantly, we observe a much larger impact of the scheme at stage 2, consistent with the staggered manner in which the reform was implemented.

[Insert Figures 1,2, A3 here]

### 3.C. Summary Statistics

The summary statistics for our sample are presented in Table 1. Panel A tabulates the mean, median, standard deviation, and quantile distribution of new firm creation for monthly aggregated accounts. Panels B and C report the statistics for the variables that describe the characteristics of newly created firms and entrepreneurs, the latter of which excludes foreigners due to data availability. This table shows that the number of new businesses created is 37, and the average number of employments at origination is 3.3 every month. On average, 7.4% of newly created businesses survive at least 2 years after origination, slightly higher than the share reported by Hombert et al. (2020). After merging these data with the Singaporean demographics database discussed in 3.A., we are left with a statistically large sample that enables us to depict the characteristics of entrepreneurs. The typical entrepreneur in the sample is best characterized as a middle-aged male, and this observation is largely consistent with findings from prior studies (i.e., Azoulay et al.,2020).

For the program evaluation, we match comparable treatment industries with control industries such that the latter can serve as a valid counterfactual in the analysis. Specifically, we

calculate the propensity score using a logistic regression based on industry-level characteristics, including productivity, risk, average firm size, capital contributed at origination, and the intensity of entrepreneurial activities (monthly business creation). Panel A of Table A2 presents the logistic regression result. Then, the matching is conducted according to the nearest-neighbor method based on the propensity score. Panel B of Table A2 compares these industry-level variables for matched treatment and control industries; the differences are statistically and economically insignificant.

[Insert Table 1 and A2 here]

# 4. Results

We start our analysis by estimating the impact of the reform on new business creation and checking the robustness of the main findings. Subsequently, we analyze heterogeneity in the response across different types of firms. After that, we investigate possible explanations for the increased firm creation. Finally, we examine the quality of the new firms with survival analysis.

## 4.A. Effect on New Firm Creation

### 4.A.1. Baseline Estimation

We start our analysis by first validating our research design. Specifically, we interact the pre- and postreform period dummies with the treatment group indicator *T* as in Equation (2), and the estimate is shown in Column (1), Table 2. The coefficient on the interaction term of *Pre* and *Treated* suggests that the firm creation for the treatment group during the pre-reform period was, on average, 1.9 percent higher than that of the control group, which is a small and statistically insignificant difference. This confirms the validity of the difference-in-differences design. However, the coefficients on the interaction term of *Post* and *Treated* display a statistically and economically significant effect: firm creation grew by 16 percent more following the reform for the treated industries than for the control group.

Column (2) of Table 2 estimates our main specification (Equation (1)) with month and industry fixed effects using the log of the number of new firms created in industry *j* at month *t* as the outcome variable. Again, we find the coefficient β to be positive and significant, which indicates that the reform indeed spurred entrepreneurial activities. To examine the sensitivity of β to the inclusion of other controls, we further include industry controls as well as their interaction with the treated dummy to account for differential responses to macroeconomic fluctuations in

Columns (3) and (4). The inclusion of these additional controls barely affects the estimate of β. Our estimate appears stable across specifications, suggesting that, to the extent that the observable characteristics in our data are representative of unobservables, the estimate of β is not due to omitted variable bias (Altonji et al. 2005).

The Home Office Scheme was implemented in two phases: first in selected areas and then applied to all residential units. We investigate the dynamic impact at different stages by decomposing the postreform period and interacting with a dummy for treated industries. The estimated results are shown in Columns (5) to (7), with controls included progressively. We find that the growth rate of newly created firms for the treated industries is significantly higher than that of the control group for both stages. Intuitively, we expect a larger impact in the second phase, as it implemented the reform at a much larger scale. The evidence is consistent with the expectation that the second stage would exhibit a much larger impact. Overall, the result suggests no significant differences in entrepreneurial activities between the treatment and control groups during the prepolicy period. The number of new firms for the treatment group outpaces that for the control only after the Home Office Scheme was implemented.

One concern of the above analysis is potential measurement error in business registrations. For example, people might have run unregistered businesses from home before and then registered them after the reform. However, the institutional setting in Singapore provides little incentive to do so. Registering with ACRA costs between 100 and 300 Singapore dollars (S$), depending on the business type. In Singapore, unregistered businesses are illegal and can be associated with significant penalties and social costs on people running illegal activities if caught. The strict enforcement by the government and hefty fines and imprisonment terms imposed as penalties on the convicted may deter such illegal practices and keep the incidence of unregistered businesses relatively low. For example, if individuals choose to conduct business without registration, they can be subject to a fine of up to S$10,000 or imprisonment for a term not exceeding 2 years or both. Due to data constraints, ex post measurement of the social costs or penalties for running a business without a formal license at home is difficult. Instead, we provide some anecdotes to illustrate the deterrence effects on the running of unregistered businesses and practices in Singapore. In May 2022, an ex-property agent was fined S$1.6 million for illegally subletting private homes for short-term accommodation on Airbnb. The agency regulator also revoked his

real estate salesperson license.[9] HDB owners and tenants who knowingly allowed their flats to be used for illicit vice-related activities faced the risk of their flats being repossessed by the HDB. Landlords may also face a jail term of up to three years and a fine of up to $3,000.[10] Moreover, they cannot enjoy government grants or tax exemptions for startups or open business accounts in the bank to apply for bank loans. Thus, the low cost of registering a firm and the high penalties associated with failing to do so discourage the running of unregistered businesses in Singapore.

To address the above concern regarding measurement error, we empirically explore variations in the entrepreneur's integrity or incentive to run an unregistered business. If the policy simply formalized unregistered businesses, we should expect the impact on business creation to be driven by entrepreneurs who are more unethical or incentivized. We first proxy for the extent to which such illegal activities can be discovered and punished by examining the distance between the company's address and its nearest police station. In addition, we utilize a proprietary dataset that compiles all lawsuits in Singapore and construct a measure of entrepreneurs' integrity. Specifically, we view an entrepreneur as having less integrity (or to be less compliant with the law) if he or she was a defendant in any lawsuit after the sample period (2005 or later). Our ex post approach aims to avoid the confounding impact of formalization of unregistered businesses on business creation. Table A5 shows the results. In Panel A, we find no significant differential treatment effect for addresses near or far from police. In Panel B, we find that the treatment effects are smaller for entrepreneurs with less integrity (or who are less compliant with the law). These findings suggest that our results are unlikely to be driven by registration of unregistered businesses.

[Insert Tables 2 and A5 here]

### 4.A.2. Robustness Check

In this section, we perform several robustness tests.

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[9] Source: "Ex-property agent fined record $1.16m for illegally subletting private homes on Airbnb, HomeAway," The Straits Times, 31 May 2022.

[10] "Jail for man who ran brothels in S'pore using mother's HDB flat sale proceeds," Todayonline, by Louisa Tang, 26 March 2021.

*Spillovers* – We address the concern regarding any potentially confounding effect from spillovers. This is crucial for the validity of our empirical design since the existence of a spillover effect would imply violation of the stable unit treatment value assumption (SUTVA) and bias the estimated treatment effects. Especially in our setting, if a large number of entrepreneurs responded to the policy by switching from the control industries to the treated ones, this would result in an upward bias in the coefficient estimates of the true treatment effect. We carry out three sets of tests to mitigate this concern.

First, we focus on treated and control industries with different levels of capital contribution where switching is less likely to be a concern. Specifically, we choose different cutoff points from the cross-sectional distribution of industry-level capital contribution and redefine the matched treated and controls list. Then, we re-estimate the DiD specification in Equation (1) and present the results in Panel A of Table A9. The results are qualitatively and quantitatively similar when we construct the sample with different thresholds. In the second test, we follow Berg et al. (2021) and implement the empirical version of their spillover model to account for the effect. Specifically, we include overall firm creation as an additional control in the main specification while excluding the time fixed effects. Columns 1 and 2 of Panel B report the results when the observation unit is an industry–month. Columns 3 and 4 present the estimates at a more granular level (industry–district–quarter, the same as in Equation (1)). The inferences remain unchanged, and the estimated magnitude is comparable to that in Section 4.A.1. Third, we exploit the first stage of the Home Office Scheme and compare its effect on the control industries in the treated area with the outcome of all industries in the control area. If spillover is a major concern, we should observe switching from the control industries to the treated industries in the treated areas. This predicts that the differences between the control industries in the treated area and those in the control area before and after the HOS would be negative and significantly different from zero. In contrast to the prediction associated with the existence of spillovers, Panel C of Table A9 shows no statistically significant differences in firm creation. Overall, the evidence in the three tests alleviates concerns regarding spillover effects.

*Home Office Firms* – We verify the impact of the Home Office Scheme on the probability of creation of new businesses with offices at home. Home office firms are identified in the following two ways. First, we use the residence information in the Singaporean demographic

datasets and check the match with the office addresses. Second, since the Singaporean demographic datasets cannot track the dynamics of home addresses, we infer residence information from a large dataset on Singaporean non-HDB house transactions. Using either approach requires us to face a tradeoff between coverage and precision. Then, we apply Equation (1) to estimate the impact on home-based business creation. Table A6 presents the OLS results. In both Panel A and B, the coefficients on the interaction term of *Post* and *Treated* suggest that the Home Office Scheme led to a significantly higher probability of creation of new home office businesses.

*Alternative Sample* – First, we consider the potential confounding impact of the *bursting of the dotcom bubble* by excluding high tech–related industries. To address the concern that our baseline results are potentially driven by small and insignificant sectors, we also consider excluding minor sectors in the forbidden list, including Card reading/Palm reading or fortune telling in any form, Funeral chapels or homes and Mausoleums. Second, to ensure that our results can be generalized to the full sample, we repeat the analysis in Table 2 on the unmatched sample. In particular, we include all 81 industries in the analysis. The results are presented in Table A7. To mitigate the endogeneity concern relating to the selection of industries into the treatment and control groups, we also exploit the geographic variation in firm creation conditional on the same industry, as the first-stage reform was implemented only in selected areas. Specifically, we compare firm creation for the same industry in selected areas with that in unselected areas before and after the reform in Table A8. The robustness checks show qualitatively and quantitatively similar effects for new firm formations. The details are found in the online appendix.

*Permutation Exercise* – To further examine the robustness of the results, we perform a falsification test. We examine the impact of the Home Office Scheme by randomly assigning the 81 industries into the treatment and control groups. This specification checks the validity of our design to identify the effect and excludes the possibility that we are establishing a spurious relationship between the reform and entrepreneurial outcome. Specifically, we randomly assign all industries into the treatment and control groups to generate 1,000 bootstrap simulations. In each permutation, we construct a pseudo-treatment indicator and then estimate the DiD regression model in Equation (1) to obtain the $\beta$ coefficient. Then, the *p* value is calculated by measuring the position of the absolute value of the true estimate under the null distribution from the estimates of

all the simulations. In Figure A4, we plot the distribution of the bootstrapped estimate, $\beta$. The coefficients peak at zero and exhibit a symmetrical distribution on both sides. In the figure, we also plot the coefficient of the true estimate using the vertical solid line. The real case coefficient has a $p$ value of 0.002 in this bootstrapped distribution, which is highly significant. In other words, our bootstrap analysis further confirms that the main findings are unlikely to hold by chance.

[Insert Tables A6–A9, Figure A4 here]

*Inframarginality* - According to the "inframarginality" concern, the marginal propensity cannot be determined by examining the average outcome given the potential omitted variables problem (Anwar and Fang, 2006). To address this concern, we adjust the growth rate for each industry in the treatment group based on the level in the control industries. We plot the distribution of the adjusted monthly growth rate in firm creation in treated industries between the prereform (January 1999 to November 2001) and postreform periods (December 2001 to March 2005) in Figure A2. As seen, the distribution mode falls in the range of (0, 30%], and more than 30% of industries in the treatment group experience a growth rate higher by less than 30% after the reform compared to the rates of the control group. These results suggest that the findings that we document in Section 4.A.1 are unlikely to be driven by outliers.

[Insert Figure A2]

### 4.B. Heterogeneity Test

Who are the marginal entrepreneurs attracted by the Home Office Scheme? In this subsection, we test the heterogeneity in the effects of the Home Office Scheme by firm size and entrepreneurs' prior experience.

*Firm Size* – While allowing operations from a home office fosters entrepreneurship by reducing the fixed costs of starting a new business, this should specifically be the case for sectors characterized by small-scale businesses with small workforces. This characteristic also implies compliance with the policy requirement that the newly established businesses do not cause disturbance to neighbors or the residential neighborhood. Therefore, it is reasonable to expect that it is relatively small-scale firms that mainly drive the positive effect of the reform on entrepreneurship. To test this hypothesis, we classify our full sample of newly created businesses into two groups based on size at registration. A new business is considered large (small) if the

number of individuals is more than (equal to or less than) three. We estimate our main specification by including a triple interaction term of *Post*, *Treated*, and our indicator for the small firm group (*Size_less_than_3*); the results are shown in Table 3[11]. The coefficient on the triple interaction term in the most saturated specification is 0.141 (significant at the 1% level), while the coefficient on *Post*Treated* is -0.009 (insignificant). The evidence indicates that the increase in new firm creation is mostly concentrated among small-scale businesses, lending further support for the impact of the Home Office Scheme in driving the difference-in-differences estimate.

*Prior Experience*: *Novice vs. Experienced Entrepreneurs* – We investigate the heterogeneity of the response to the entrepreneurship stimulus program among different entrepreneurs by comparing the new business creation induced by the Home Office Scheme among novice and experienced entrepreneurs. The novice sample includes first-time entrepreneurs who do not have prior business experience. Prior studies document evidence of performance persistence in entrepreneurship: entrepreneurs with a track record of success are more likely to succeed (i.e., Gompers et al., 2010). In view of this argument, we classify the sample of experienced entrepreneurs into failures and nonfailures depending on their status as of December 1998, right before the sample period's beginning. We run the regression in Equation (1) to obtain estimates of β for the three subsamples separately and report the results in Table 4. The results show that the Home Office Scheme led to significantly higher entry into self-employment among first-time entrepreneurs than among their counterparts. Decomposition based on past performance in Columns (3) to (6) suggests that the reform seems to have strengthened the "performance persistence" effect, with more successful, experienced entrepreneurs tending to open a second business.

[Insert Tables 3, 4 here]

We show above that firm creation was more pronounced among small-scale businesses and novice entrepreneurs. These results suggest that marginal and novice entrepreneurs were likely to

[11] In the heterogeneity test on firm size, we further classify our full sample of newly created business into two groups based on the size at registration so that the unit of observation is industry–district–quarter–size group (large/small). The additional size dimension makes the sample size twice that in the main specification (Table 2).

open small-scale businesses, consistent with the prediction about the selection of marginal entrepreneurs based on the model of Evans and Jovanovic (1989). Details about the model are described in Appendix A.

### 4.C. Possible Explanations

Why did the Home Office Scheme help to increase firm creation? There are three main possible explanations. First, the Home Office Scheme reduced the entry cost that entrepreneurs used to face, including office space rental and commuting expenditures. Second, workplace flexibility further enhances the nonpecuniary benefits of being an entrepreneur and allows engagement in joint market and household production. Third, prior literature has found that the social status of entrepreneurs (e.g., possible shame from a business failure) is an important driving force for interest in entrepreneurship (Begley and Tan, 2001). The reform in this study helped entrepreneurs obscure the salience of possible business failure and thus avoid experiencing the consequent shame and humiliation. In other words, it reduced the cost of experimentation and enabled entrepreneurs to work on an idea with fewer concerns about possible negative consequences.

According to the experimentation perspective, the foremost hurdle to entrepreneurship is the cost of experimenting with new business ideas or projects. If the Home Office Scheme increased firm creation due to a reduction in entry costs, the positive effect of the reform on the propensity to start a business is expected to be more prevalent among individuals with financial constraints, where fixed costs are likely the barrier to entrepreneurship. We conduct two sets of analyses to examine the role of financial constraints. First, we follow the idea of Hurst and Lusardi (2004) to exploit the difference in starting capital that entrepreneurs contribute to firm origination across sectors. Specifically, for each industry, we obtain the median contributed amount among businesses started from 1980 to 1998 (before our sample period).

Second, we obtain the income data of credit card holders covering the districts where the entrepreneurs in our sample live from a large local bank. We use the data to derive the district-level median income for the financially constrained entrepreneurs in the industry and geographic

area.[12] In Table 5, we report heterogeneous effects by interacting the treatment dummy, reform period indicator, and financial constraint measure. The results show that the effect of the Home Office Scheme is relatively larger when there is a higher level of financial constraints, i.e., among high-starting-capital industries and entrepreneurs with lower income.

In Figure 3, we plot the DiD estimates on business creation in Table 2 at each quantile of the two measures for financial constraints: capital contribution and income. The results are consistent with those of Table 5, which shows that industries with a higher level of financial constraints drive the effect of the Home Office Scheme.

While the Singaporean demographic data do not provide us with the exact annual income for each individual, we take advantage of a special feature in Singapore to construct an individual-level measure of financial constraints. In Singapore, there are two main types of residential property: public housing and private housing. Because of heavy subsidies from the Singapore government, public housing, or HDB apartments, are offered with strict eligibility criteria—only citizens with a family income below the stipulated cap can apply. Thus, we can refer to the property type (HDB or private) to proxy for the individual's wealth. We use the ratio of HDB residents as the dependent variable in Column (1) of Table A10 and check whether the Home Office Scheme changes the pool of new entrepreneurs by adding significantly more people with financial constraints.[13] Compared to the control industries, the treatment industries attract more entrepreneurs living in subsidized public (HDB) housing. These results corroborate that entry regulation with high fixed costs is a barrier to firm creation and that reducing entry costs is important to encourage firm creation. These results also suggest that marginal and financially constrained entrepreneurs benefit more from the reform. They are consistent with the predictions

[12] Specifically, we exploit the proprietary consumer financial transaction dataset from Agarwal and Qian (2014), which was obtained from the leading bank in Singapore. This dataset contains income information for each customer. We then aggregate all customers in the same district and use the median income as a proxy for the financial constraints of entrepreneurs.

[13] In Table A10, we provide further evaluation of the policy by examining its impact on the composition of new entrepreneurs. We find that newly created firms induced by the reform involve more "discouraged workers", namely, a higher ratio of young/old entrepreneurs, Singaporean entrepreneurs and entrepreneurs belonging to a local minority group (Azoulay et al., 2021).

related to the selection of financially constrained entrepreneurs based on the model of Evans and Jovanovic (1989). Details about the model are described in Appendix A.

If the Home Office Scheme increased firm creation by increasing nonpecuniary benefits, the treatment industries should attract more female entrepreneurs, who benefit directly from engagement in joint market and household production. Following this argument, we further classify the sample of entrepreneurs by gender and estimate our main specification by including a triple interaction term of *Post*, *Treated*, and the *Female* indicator. The results are reported in Table 6. Columns (1) through (4) show that the Home Office Scheme led to higher entry into self-employment among female entrepreneurs than among their male counterparts. Therefore, these findings confirm the importance of nonpecuniary benefits in explaining the overall effect of the reform.

The third possible explanation is that the scheme helped obscure the salience of possible business failure. This is especially relevant in the setting of Singapore, as a prior study has shown that "social status of entrepreneurship" and "shame from business failure" distinguish East Asian from Anglo-Saxon cultures and predict interest in entrepreneurship better in the former case. Since there is no readily available measure to quantitatively pin down the extent of such salience, we design a test as follows. Due to the workplace flexibility offered under the Home Office Scheme, entrepreneurs enjoyed a reduction in salience of failure if they created businesses in the treated industries under the reform that failed afterward. Therefore, we expect the Home Office Scheme to facilitate future (second) firm creation among those entrepreneurs. We focus on entrepreneurs whose first business was established during our sample period (January 1999–March 2005) but failed afterward. We adopt a difference-in-differences design to test the probability of second business creation between entrepreneurs who opened their first business in treated industries and control industries before and after the reform. The results are reported in Table 7. In Column (1), the estimated effect on *D(FirstBusi_Post)* is significant, suggesting that entrepreneurs with first business (but failed later) created post-reform in the control industries significantly increase the propensity to start a new business in the future, compared to those with first business (but failed later) before the reform. We do not find a statistically significant coefficient on the interaction term, *D(FirstBusi_Post)*D(FirstBusi_Treated Industries)*. Therefore, we cannot reject the null hypothesis that the reform had little effect in obscuring the salience of possible business failure.

Another potential concern is that individuals open a business at home to obtain tax benefits, which might provide a different interpretation of our results. Singapore's government allows business owners to claim tax deductions for certain business expenses. However, the tax deduction applies to all business owners regardless of whether they operate under the Home Office Scheme. Therefore, there is no additional tax incentive to open a home office.

In sum, we show that the effect is more pronounced for low-income and female individuals but find no evidence that the scheme helps obscure the salience of possible business failure. Thus, these results suggest that reducing entry costs and increasing nonpecuniary benefits are the most important mechanisms through which the scheme encourages firm creation. In contrast, the explanation of obscuring the salience of possible business failure plays a smaller role.

[Insert Table 5,6,7,A10 and Figure 3 here]

### 4.D. Quality of the Startups

Removing barriers to entrepreneurship has been a major objective for policy-makers in the design of programs. However, evaluating the welfare implication of policy depends on how individuals self-select into entrepreneurship because there exists a substantial amount of heterogeneity among would-be entrepreneurs. To explore whether the Home Office Scheme led to a significant change in the quality of newly built firms, we measure the quality of the firms in two different ways: survival rate for the first firm and the characteristics of the next firm. The first measure illustrates how well the startups could survive in the following years. A higher attrition rate (or lower survival rate) indicates lower firm quality—in other words, evidence that is less in favor of the welfare enhancement of this policy.

#### 4.D.1. Survival Rate

We use a Cox proportional hazard model to explicitly incorporate the history of each firm before it is terminated. In the model, we track all newly created firms during the sample period and consider two states of the firm: ceased or still existing. The regressions are estimated with month, firm's region, and industry fixed effects. Table 8 presents the hazard ratio estimate using all the newly created firms in our sample. As evident in Panel A, the difference-in-difference result suggests that the exit rate of newly created firms after the Home Office Scheme decreases by 29%

relative to that of comparable startups in the control industries. Columns (4) and (5) separately report the estimates for firms created in the pre- and postpolicy periods. Before the policy, new firms in the treated industries were 1.6% more likely to exit than firms in the control group, consistent with existing cross-country evidence on small ventures. However, the startups induced by the reform exhibit a lower cease rate of 22.4%.

We further assess the robustness of our results and investigate how the policy influences the firm's probability of exit at different time horizons. Specifically, we run OLS using dummies for the survival of startups during the first to fifth years and present the estimates in Panel B. As can be observed, the coefficients on the interaction term are all positive and significant except for those of the first two years. In other words, new firms created by the reform were likely to exit during the first two years but exhibit a much higher survival rate at a longer time horizon. The estimates are economically meaningful. If we consider 5-year survival, the rate for the additional firms created through the reform is higher in absolute terms by approximately 5.7%, which is equivalent to a 7.3% increase relative to the sample mean, as reported in Table 1.

Similarly, Figure 4 displays the survival curve for the treated and control groups during the five years since creation. As the figure illustrates, following the Home Office Scheme, the survival rate of treated firms became significantly higher, especially considering the longer time horizon. Overall, we provide consistent evidence that the additional startups exhibited higher survival rates due to the benefits of fixed cost reduction.

The Home Office Scheme affected firm survival through two channels. The first is the selection channel. Since the scheme attracted both marginal and financially constrained entrepreneurs, the firms' survival depends on the ability of these marginal entrepreneurs. The second is the treatment effect. Since the scheme reduced fixed costs, the payoff of an entrepreneur is higher, and the choice to become a worker is less attractive.

To better differentiate between the ex ante selection channel and the treatment effect brought by the reform, we perform a firm survival test across different industries, similarly to in Table 5. Specifically, we categorize all firms into groups with/without financial constraints based on the two measures that we use in Section 4.C. The results are tabulated in Figure A5. We find that the positive effect of the Home Office Scheme on the quality of startups concentrates mainly on firms with financial constraints. These results indicate that the treatment effect is stronger for

financially constrained entrepreneurs. The Home Office Scheme motivated individuals with financial constraints by reducing entry costs and positively influenced firms' future performance, as reflected by a higher survival rate.

[Insert Table 8 and Figures 4, A5 here]

### 4.D.2. Measurement

One concern with the survival analysis is the measurement of business closure. Suppose that people who cease business operations in a separate office are more likely to report a business closure to the registry than people who cease business operations from home. In this case, our survival results would reflect reporting bias rather than business quality. A more detailed discussion of the institutional setting of business closure in Singapore suggests that the reporting is independent of whether the firm is registered at home.

Every person must register with ACRA to conduct business in Singapore except those exempted under the Business Names Registration Act. When registering with ACRA, one can choose one of the following business types: sole proprietorship, partnership, limited liability partnership (LLP), and company[14]. The procedures to close a business entity depend on the business type. Sole proprietorships, partnerships, and LLPs must renew their registration every three years and pay the renewal fee. Carrying on the business after the expiry date is an offense, where the defaulter is liable to a fine, imprisonment, or both. If business owners intend to close their business, they are incentivized not to renew their registrations to avoid renewal fees and other reporting costs.

Companies must file annual returns (ARs) and hold annual general meetings (AGMs). Suppose that the company fails to hold its AGMs or file ARs on time. In this case, ACRA may allow the company to pay a late lodgment fee or penalty or consider prosecuting the company's directors in court. If business owners intend to close a business, they have an incentive to report to ACRA to avoid the cost of filing ARs and holding AGMs.

[14] Limited liability partnership became available for registration only starting from April 2005, which is beyond our sample period.

Therefore, the institutional setting in Singapore ensures that there are strong incentives to report business closures on time, and reporting bias is unlikely to be an issue. Even if reporting bias exists, it is independent of whether the firm is registered at home.

We also address the measurement concern by comparing the occurrence of extreme failure among firms in the treatment and control groups. Namely, we merge with the Singapore lawsuit datasets and identify firm failures that come with lawsuits or personal bankruptcy filings. While the exit dummy may suffer from measurement error, an observation of firm exit and concurrent lawsuits for the entrepreneurs very likely reflects the firm's performance. We re-estimate the specification as in Section 4.D.1, and the results are reported in Table A11. The pattern is consistent with that in Table 8. The coefficients on the interaction term are all positive and significant except for those for the first two years, indicating that the performance of the additional firms created under the reform is comparable to that of control firms during the first two years but becomes much better at a longer time horizon.

[Insert Table A11 here]

### 4.D.3. Explanations

Why did the Home Office Scheme increase the quality of new startups in terms of survival rate? First, the scheme reduced the fixed cost of rent and transportation for the startups and thus helped them to survive at low cost. Second, the scheme might have attracted individuals with more resources to start a new business. However, in Section 4.C., we show that the effect of the Home Office Scheme is relatively higher among entrepreneurs living in poor communities and that the treatment industries attract more entrepreneurs living in subsidized public (HDB) housing. Thus, our results contradict this explanation. Third, the scheme might have encouraged individuals to choose highly productive and low-risk industries. To investigate this hypothesis, we compute the industry-level average of the quarterly change in value added for 1992 (the earliest date for which we have industry productivity data) to 1998 and define an industry to have high/low productivity if its average in that period is above/below the median of the distribution.

Similarly, we define the risk of an industry by calculating the average 1-year survival rate among businesses created before our sample period and comparing it with the median of the sample distribution. The heterogeneous effect of the scheme across different industries is presented in Table A12. Panels A and B compare the estimated effects between these two classes of

industries. They indicate that business creation in high-productivity/low-risk industries responded more strongly to the reform. The results confirm that the response of new firm creation to the Home Office Scheme was particularly concentrated in industries with high productivity and low risk.

[Insert Table A12 here]

### 4.D.4. Future Business Creation

The additional dimension that we consider in measuring the quality of the startups is to look at the characteristics of the entrepreneur's next firm in the long run. According to the experimentation view, the first business creation often provides a good setting for the entrepreneur to experiment with new business ideas or projects. It allows him or her to learn from managing the firm. Thus, the characteristics of the second firm also reflect the quality of the first firm after the scheme. We compare the probability of a second business creation between entrepreneurs who opened their first business in the treated and control industries before and after the reform. We use difference-in-difference to study the relationship between business creation experience during the Home Office Scheme period and future entrepreneurial activities. Specifically, we focus on entrepreneurs whose first business was established during our sample period (January 1999 to March 2005) and carry out DiD regressions to study the impact of the reform on the creation of the second business. The results are presented in Panel A of Table 9. The coefficient of the interaction term is positive and significant at the 1% level. The results show that entrepreneurs who opened their first business in a treated industry after the reform are more likely to open a second business. Our findings imply that the reform encouraged serial entrepreneurship.

We further restrict our sample to serial entrepreneurs who established a second business. We carry out DiD regressions by examining the second business's characteristics: (1) the number of employees at origination and (2) survival rate (at 2 years). We progressively add time, region, and industry fixed effects for the first and second business to remove the potential influence of time-, region-, and industry-invariant city-level characteristics. Further analysis includes time-varying industry control variables such as industry productivity. The estimations are tabulated in Panel B of Table 9. The coefficient estimate on the interaction term, *D(FirstBusi_Post)*D(FirstBusi_Treated Ind)*, implies an impact of the Home Office Scheme on the quality of future business creation. Models (1) and (2) examine the number of employees,

whereas Models (3) and (4) focus on the survival rate. We can see that when the first business was created during the postreform period and belonged to the treated industries, there was significantly larger creation of second businesses among serial entrepreneurs relative to the counterfactuals. Adding various control variables neither affects this relationship nor changes its significance level. The survival rate remains similar between the two types of entrepreneurs. The results suggest that the Home Office Scheme encouraged serial entrepreneurs to open a larger business with a similar survival rate.

Why do we observe an increased probability of starting a second business in the treated group? There are two explanations. The first is the selection of serial entrepreneurs into the treatment group. Those planning to start several businesses may have taken advantage of the Home Office Scheme and are more likely to start a second business. The second is the treatment effect of the home office scheme. For example, the Home Office Scheme reduced entry costs and potentially increased profits and capital accumulation from the first business. Thus, the treated entrepreneurs would be more likely to start a second business. The results must be interpreted cautiously because we cannot distinguish the selection effect from the treatment effects in this paper.

[Insert Table 9 here]

Although the additional startups were likely to have financial constraints, they exhibited a higher survival rate and were created in industries with higher productivity and lower risk. Moreover, when the first firm was established after the scheme in the treatment industries, the entrepreneurs were more likely to start a second, larger firm with a similar survival rate. The evidence in this section indicates that the Home Office Scheme effectively spurred entrepreneurial activities and attracted more entry into self-employment without significantly lowering the average quality of the pool.

# 5. Conclusion

The barriers to entrepreneurship are complex and multifaceted, including issues related to funding, training, mentoring, and regulation. Addressing these barriers will require a coordinated effort from policymakers to relax and modify rules, thereby reducing impediments. One example of such efforts is offering home-based entrepreneurship, which reduces the fixed costs of starting

a new business and enables would-be entrepreneurs to engage in joint market and household production. This paper examines a policy reform that facilitated entry into home-based entrepreneurship. Relying on the difference-in-differences strategy, we find that the policy led to a significant increase in the level of new firm creation, especially in the sector of small-scale business. Moreover, the effect was more pronounced for low-income individuals, for whom access to capital is most likely to be the main barrier to entrepreneurship.

We go beyond the focus on the level of entrepreneurial activities and investigate the welfare implications of the reform. While the additional firms induced by the policy started relatively small at creation, they did not quickly fail but were as likely as control firms to exit during the first two years. More interestingly, our findings suggest that the newly created firms exhibited a much higher survival rate at a longer time horizon. The results confirm that regulatory entry barriers with high fixed costs are an important factor that deters entrepreneurship. Removing entry barriers benefits all entrepreneurs and encourages marginal entrepreneurs to start businesses. More importantly, it provides additional benefits to financially constrained individuals since they can use the savings on fixed costs to increase investment.

Entrepreneurship has long been embraced as a critical contributor to innovation, job creation, and overall economic growth (e.g., King and Levine, 1993; Guiso, Sapienza, and Zingales, 2004). Promoting the entrepreneurial ecosystem has become the focus of both policy-makers and academics in policy design and assessment. Such policies come in various forms, including funding support, entrepreneurial training, and access to mentoring and expertise. Although we study the effect of an entrepreneurship policy reform in Singapore, our results propose a broader link that applies to all relevant settings. The analysis highlights the importance of liberalizing regulations over home-based businesses and the necessity of accounting for heterogeneity in would-be entrepreneurs' ability in the design of policies for entrepreneurship promotion.

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**Figure 1 Business Creation: Treated vs. Control**

The figure plots the 12-month moving average of the number of businesses created in the treated and control industries from January 2000 to March 2005 (1999 does not appear because of the 12-month average). The vertical lines correspond to the reform period (1st stage, December 2001 to June 2003; 2nd stage, July 2003-)

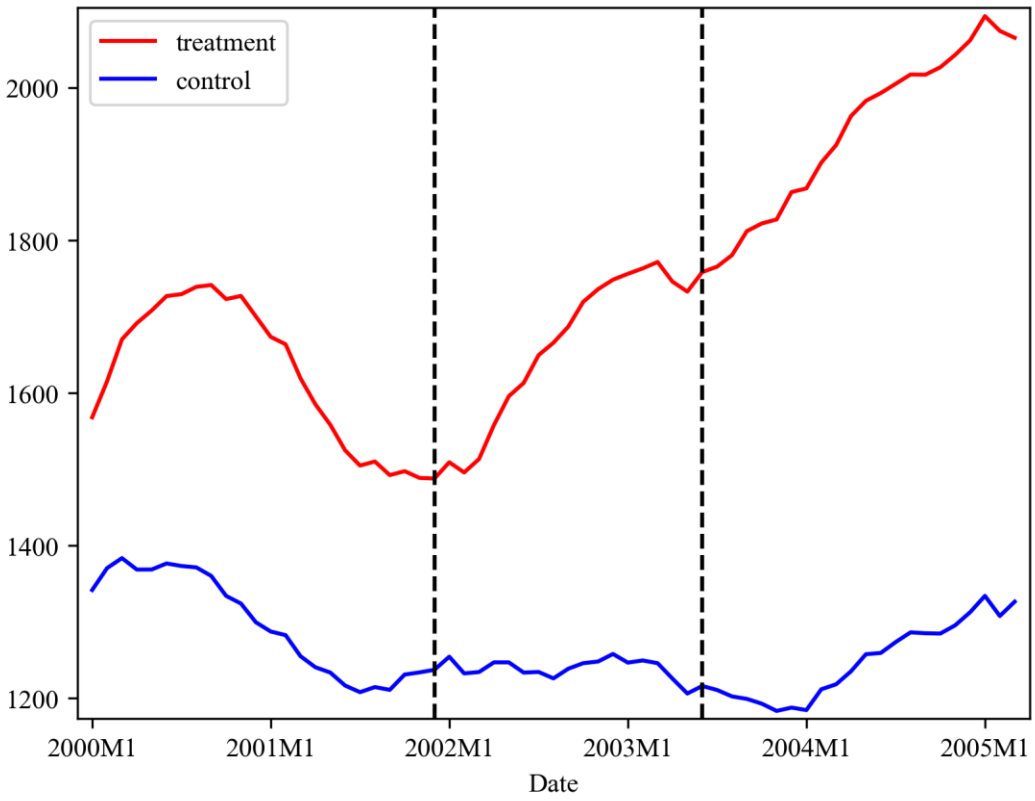
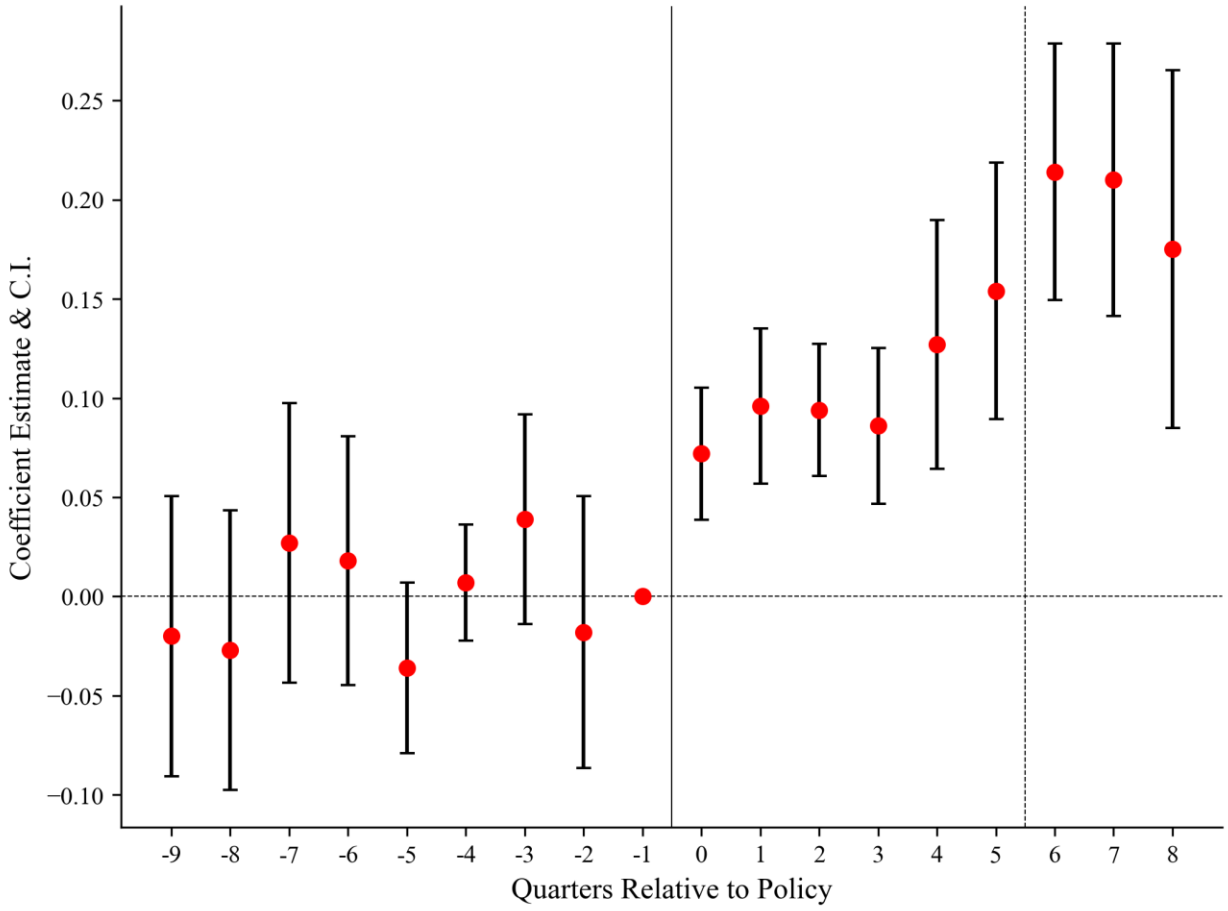


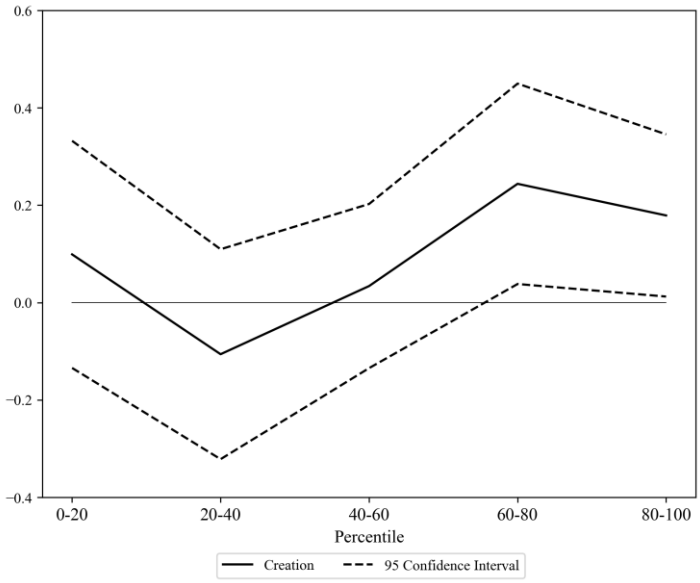
**Figure 2 Home Office Scheme and Business Creation: Dynamics**

The figure plots the path of OLS coefficient estimates (and 95% confidence interval) from a regression of firm creation as a function of quarters since the policy implementation, as in Equation (4). Each point represents a coefficient estimate. The solid and dashed vertical line indicate the timing for the first and second phase of the reform, respectively. Event quarter -1 is normalized to 0.

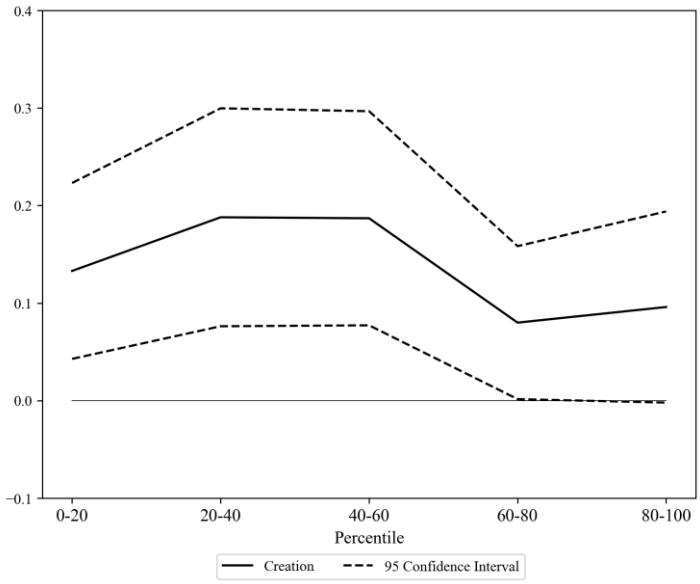


## Figure 3 Business Creation and Financial Constraint

The figure plots the DID estimates on business creation in Table 2 at each quantile of the two measures for financial constraints: capital contribution, and income, along with their 90 percent confidence interval.



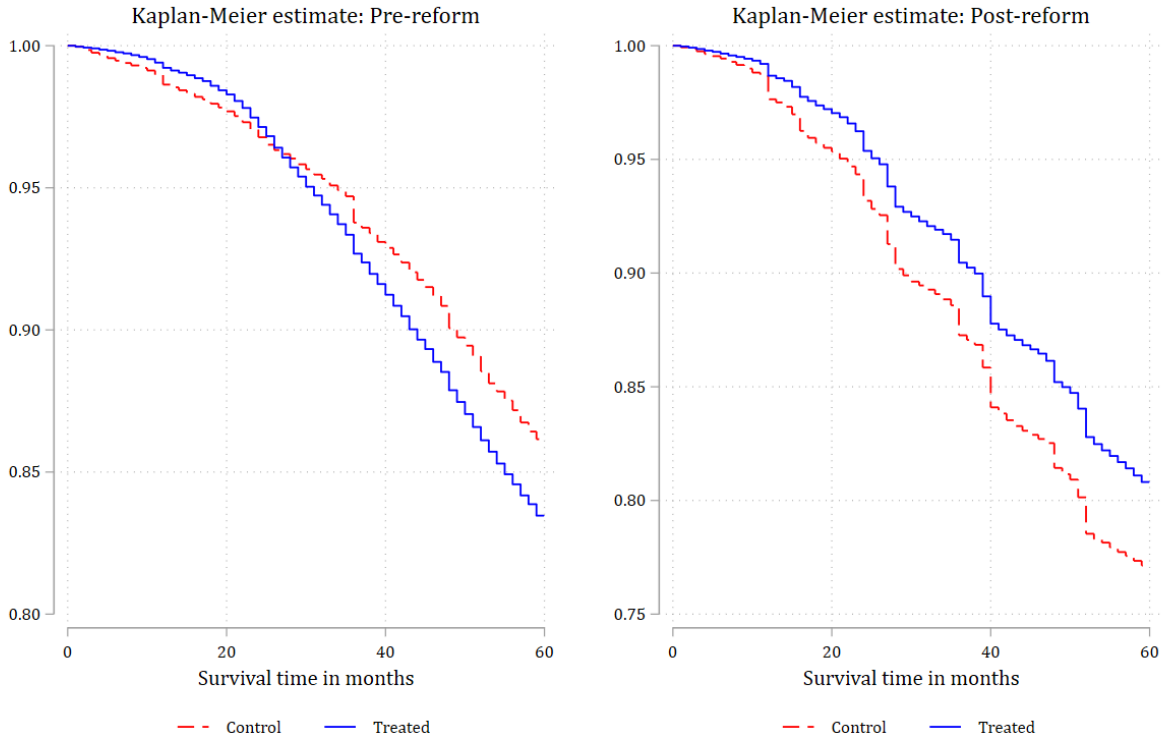

Panel A Capital Contribution




Panel B Income (district-level median)

## Figure 4 Survival Rate of New Business Created

The figure plots the Kaplan-Meier survival curve (along with the exponential greenwood confidence intervals) by treated and control group during the pre- (January 1999 to November 2001) and post-reform period (December 2001 to March 2005).



## Table 1 Summary Statistics

The table reports summary statistics for all new firms created during the sample period. Panel A reports the industry-level statistics on business creation, while Panels B and C present the firm-level statistics on business characteristics and entrepreneurs' demographics.

| | N | mean | sd | p25 | p50 | p75 |
|---|---|---|---|---|---|---|
| **Panel A Industry-level** | | | | | | |
| Number of firms created (monthly) | 6075 | 37.238 | 90.121 | 1 | 9 | 35 |
| **Panel B Firm Characteristics: Full Sample** | | | | | | |
| Entrepreneur plus employee at creation | 124204 | 3.302 | 3.762 | 1 | 2 | 4 |
| Employee at creation | 124204 | 0.782 | 1.308 | 0 | 0 | 1 |
| Survive during the first year | 124204 | 0.954 | 0.21 | 1 | 1 | 1 |
| Survive during first two years | 124204 | 0.926 | 0.262 | 1 | 1 | 1 |
| Survive during first three years | 124204 | 0.882 | 0.322 | 1 | 1 | 1 |
| Survive during first four years | 124204 | 0.833 | 0.373 | 1 | 1 | 1 |
| Survive during first five years | 124204 | 0.782 | 0.412 | 1 | 1 | 1 |
| **Panel C Entrepreneur Demographics: Singaporean Sample** | | | | | | |
| Age | 85770 | 39.535 | 8.714 | 33 | 39 | 45 |
| Male | 85770 | 0.666 | 0.401 | 0.5 | 1 | 1 |
| Married | 85770 | 0.585 | 0.439 | 0 | 0.667 | 1 |
| Chinese | 85770 | 0.898 | 0.289 | 1 | 1 | 1 |
| Malay | 85770 | 0.048 | 0.204 | 0 | 0 | 0 |
| India | 85770 | 0.039 | 0.184 | 0 | 0 | 0 |
| Others | 85770 | 0.014 | 0.106 | 0 | 0 | 0 |
| Non-Chinese | 85770 | 0.102 | 0.289 | 0 | 0 | 0 |

## Table 2 Home Office Scheme and Business Creation

The table investigates the impact of the Home Office Scheme on firm creation based on the matched sample. The observation unit is an industry-district-quarter, including 58 industries, from January 1999 to March 2005 (quarterly), 28 districts. The sample period is selected to avoid the confounding effect of the 97–98 Asian financial crisis and Singapore's Limited Liability Partnership Act implemented in April 2005. *Pre* is a dummy equal to 1 for the observations during the period before the reform (2001Q1 to 2001Q4); otherwise, 0. *Post* is a dummy equal to 1 for the observations after the reform (2002Q1 to 2005Q1); otherwise, 0. *Post_Stage_1* is a dummy equal to 1 for observations in the first stage of the reform period (2002Q1 to 2003Q2); otherwise, 0. *Post_Stage_2* is a dummy equal to 1 for observations in the second stage of the reform period (2003Q3 to 2005Q1); otherwise, 0. *Treated* is a dummy equal to 1 if the industry does not belong to the forbidden industry in the Home Office Scheme. Controls include (1) *Industry productivity*, the change in value-added per work in each industry; and (2) *GDP Growth*, the annual GDP growth in Singapore. All specifications are weighted by industry size. Robust standard errors clustered by industry and year-quarter are reported in parentheses. *, **, *** denote significance at 10%, 5%, and 1% levels, respectively.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| VARIABLES | Log (1+number of new firms created in industry j, district d, quarter t) | | | | | | |
| Treated*Pre | 0.019 | | | | | | |
| | (0.028) | | | | | | |
| Treated*Post | 0.160*** | 0.154*** | 0.152*** | 0.138*** | | | |
| | (0.049) | (0.046) | (0.045) | (0.045) | | | |
| Treated*Post_Stage1 | | | | | 0.111*** | 0.109*** | 0.096** |
| | | | | | (0.037) | (0.037) | (0.037) |
| Treated*Post_Stage2 | | | | | 0.191*** | 0.189*** | 0.187*** |
| | | | | | (0.053) | (0.053) | (0.056) |
| Controls | No | No | Yes | Yes | No | Yes | Yes |
| Treated*Controls | No | No | No | Yes | No | No | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year*District FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| No. of Industry | 58 | 58 | 58 | 58 | 58 | 58 | 58 |
| Observations | 40,600 | 40,600 | 40,600 | 40,600 | 40,600 | 40,600 | 40,600 |
| Adj R-squared | 0.754 | 0.754 | 0.754 | 0.754 | 0.754 | 0.754 | 0.754 |

## Table 3 Heterogeneity Test on Business Creation: Firm Size

The table explores the heterogeneity of the impact of the Home Office Scheme on business creation across different firm sizes in the matched sample. The unit of observation is industry-district-quarter-size-group, including 58 industries, January 1999 – March 2005 (quarterly), 28 districts, and two size groups(large vs. small). Controls include (1) *Industry productivity*, the change in value-added per work in each industry; and (2) *GDP Growth*, the annual GDP growth in Singapore. All specifications are weighted by industry size. Robust standard errors clustered by industry and year-quarter are reported in parentheses. *, **, *** denote significance at 10%, 5%, and 1% levels, respectively.

| | (1) | (2) |
|---|---|---|
| Dependent Var. = Log (1+number of new firms created in industry j, district d, quarter t) | | |
| Treated*Size_less_than_3*Post | 0.141*** | 0.141*** |
| | (0.031) | (0.030) |
| Treated*Post | 0.003 | -0.009 |
| | (0.019) | (0.020) |
| Treated* Size _less_than_3 | -0.041*** | -0.041*** |
| | (0.006) | (0.006) |
| Size_less_than_3*Post | 0.196 | 0.196 |
| | (0.192) | (0.192) |
| Size_less_than_3 | 0.588*** | 0.588*** |
| | (0.125) | (0.125) |
| Controls and Treated*Controls | No | Yes |
| Industry FE | Yes | Yes |
| Year-Quarter FE | Yes | Yes |
| Year*District FE | Yes | Yes |
| No. of Industry | 58 | 58 |
| Observations | 81,200 | 81,200 |
| Adj R-squared | 0.537 | 0.537 |

**Table 4 Heterogeneity Test on Business Creation: Novice vs. Experienced Entrepreneurs**

The table explores the heterogeneity of the impact of the Home Office Scheme on firm creation among Novice and Experienced Entrepreneurs in the matched sample. The novice sample includes first-time entrepreneurs who do not have prior business experience. We further classify the experience into two groups: failures and non-failures. The sample of failures includes individuals who started a business, then failed and remained failures as of December 1998, right before the beginning of the sample period in the main analysis. Similarly, the sample of non-failures includes individuals who started their business in or before December 1998 and survived throughout the sample period of the main analysis (i.e., it did not exist until March 2005). The observation unit is an industry-district-quarter, including 58 industries, from January 1999 to March 2005 (quarterly), 28 districts. Controls include (1) *Industry productivity*, the change in value-added per work in each industry; and (2) *GDP Growth*, the annual GDP growth in Singapore. All specifications are weighted by industry size. Robust standard errors clustered by industry and year-quarter are reported in parentheses. *, **, *** denote significance at 10%, 5%, and 1% levels, respectively.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Novice | | Experienced | | | |
| | | | Failures | | Non-Failures | |
| Treated*Post | 0.136*** | 0.115** | 0.004 | 0.005 | 0.034 | 0.024 |
| | (0.043) | (0.042) | (0.003) | (0.004) | (0.020) | (0.020) |
| Controls | No | Yes | No | Yes | No | Yes |
| Treated*Controls | No | Yes | No | Yes | No | Yes |
| Industry FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year-Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year*District FE | Yes | Yes | Yes | Yes | Yes | Yes |
| No. of Industry | 58 | 58 | 58 | 58 | 58 | 58 |
| Observations | 40,600 | 40,600 | 40,600 | 40,600 | 40,600 | 40,600 |
| Adj R-squared | 0.701 | 0.701 | 0.139 | 0.139 | 0.465 | 0.465 |

## Table 5 Business Creation and Financial Constraints

The table explores the role of financial constraint in the impact of the Home Office Scheme on firm creation in the matched sample. Panel A focuses on the initial capital contribution made by the entrepreneurs at the business creation. Specifically, *Contribution_capital* is the (logarithm of) median contributed amount among businesses that started from 1980 to 1998 for each industry. Panel B is based on the entrepreneurs' income, which we measure using the median income (in logarithm) at the district level. The observation unit is an industry-district-quarter, including 58 industries, from January 1999 to March 2005 (quarterly), 28 districts. All specifications are weighted by industry size. Controls include (1) *Industry productivity* and (2) *GDP Growth*. Robust standard errors clustered by industry and year-quarter are reported in parentheses. *, **, *** denote significance at 10%, 5%, and 1% levels, respectively.

| | (1) | (2) |
|---|---|---|
| Dependent Var. = Log (1+number of new firms created in industry j, district d, quarter t) | | |
| **Panel A Contribution Capital** | | |
| Treated*Contribution_capital*Post | 0.612** | 0.588* |
| | (0.293) | (0.287) |
| Treated*Post | 0.062 | 0.051 |
| | (0.062) | (0.059) |
| Contribution_capital*Post | -0.536** | -0.533** |
| | (0.201) | (0.195) |
| Controls and Treated*Controls | No | Yes |
| Industry, Year-Quarter and Year*District FE | Yes | Yes |
| Observations | 40,600 | 40,600 |
| Adj R-squared | 0.754 | 0.754 |
| **Panel B Entrepreneurs' Income** | | |
| Treated*Income*Post | -0.031* | -0.031* |
| | (0.015) | (0.015) |
| Treated*Post | 0.423** | 0.407** |
| | (0.153) | (0.147) |
| Treated*Income | 0.162*** | 0.162*** |
| | (0.050) | (0.050) |
| Controls and Treated*Controls | No | Yes |
| Industry, Year-Quarter and Year*District FE | Yes | Yes |
| Observations | 40,600 | 40,600 |
| Adj R-squared | 0.755 | 0.755 |

## Table 6 Business Creation and Entrepreneurs by Gender

The table explores the heterogeneity of the impact of the Home Office Scheme on firm creation among female and male entrepreneurs in the matched sample. The unit of observation is industry-district-quarter-gender type, including 58 industries, January 1999 – March 2005 (quarterly), 28 districts, and 2 gender groups. Controls include (1) *Industry productivity*, the change in value-added per work in each industry; and (2) *GDP Growth*, the annual GDP growth in Singapore. All specifications are weighted by industry size. Robust standard errors clustered by industry and year-quarter are reported in parentheses. *, **, *** denote significance at 10%, 5%, and 1% levels, respectively.

| | (1) | (2) |
|---|---|---|
| Dependent Var. = Log (1+ new firms created in industry j, district d, quarter t) | | |
| Treated*Female*Post | 0.032** | 0.032** |
| | (0.014) | (0.015) |
| Treated*Post | 0.070** | 0.055** |
| | (0.026) | (0.025) |
| Treated* Female | -0.041*** | -0.041*** |
| | (0.011) | (0.011) |
| Female *Post | 0.015 | 0.015 |
| | (0.051) | (0.051) |
| Female | 0.139*** | 0.139*** |
| | (0.036) | (0.036) |
| Controls and Treated*Controls | No | Yes |
| Industry FE | Yes | Yes |
| Year-Quarter FE | Yes | Yes |
| Year*District FE | Yes | Yes |
| No. of Industry | 58 | 58 |
| Observations | 81,200 | 81,200 |
| Adj R-squared | 0.656 | 0.657 |

## Table 7 Home Office Scheme and Future Entrepreneurial Activities

The table focuses on entrepreneurs with the experience of first business failure. It explores whether Home Office Scheme helps facilitate the probability of an entrepreneur's future (second) firm creation via reduced salience of first business failure in the matched sample. Specifically, we focus on those entrepreneurs whose first business was established during our sample period (January 1999 – March 2005) but failed afterward. Then we classify them into two groups based on the time of first business creation. *D(FirstBusi_Post)* equals one for those who establish the first business in the post-reform period (December 2001 – March 2005). *D(FirstBusi_Treated Industries)* equals one if the first business created falls in the treated industries. Robust standard errors clustered by industry of first business are reported in parentheses. Superscripts of *, **, and *** indicate significance levels at 10%, 5%, and 1%, respectively.

| | (1) | (2) | (3) |
|---|---|---|---|
| Dependent Var. = D (Second Business Created) | | | |
| D(FirstBusi_Post) | 0.026** | | |
| | (0.010) | | |
| D(FirstBusi_Post) *D(FirstBusi_Treated Industries) | 0.016 | 0.015 | 0.014 |
| | (0.014) | (0.014) | (0.014) |
| First Business Region FE | No | No | Yes |
| First Business Creation Month FE | No | Yes | Yes |
| First Business Industry FE | Yes | Yes | Yes |
| Observations | 37,932 | 37,932 | 37,932 |
| Adj R-squared | 0.010 | 0.006 | 0.008 |

## Table 8 Quality of Business Created: Survival Analysis

Panel A of the table reports the estimated hazard ratios from Cox-proportional hazard models of firm exit for the matched sample. Estimates on discrete variables represent the effect of moving from zero to one. Panel B presents the OLS estimate of the firm's survival. *D(Survival)* is a dummy equal to 1 if the business survives during the first /two/three/four/five years. The sample includes all firms in the 58 industries created from January 1999 to March 2005. Controls include (1) *Industry productivity*, the change in value-add per work in each industry; and (2) *GDP Growth*, the annual GDP growth in Singapore. Robust standard errors clustered by industry are reported in parentheses. *, **, *** denote significance at 10%, 5%, and 1% levels, respectively.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Panel A Hazard regression** | | | | | |
| | | Full Sample | | Pre-reform | Post-reform |
| Treated | | | | 1.016 | 0.776*** |
| | | | | (0.037) | (0.038) |
| Treated*Post | 0.697*** | 0.705*** | 0.634*** | | |
| | (0.022) | (0.022) | (0.022) | | |
| Controls | No | No | Yes | Yes | Yes |
| Treated*Controls | No | No | Yes | Yes | Yes |
| Region FE | No | Yes | Yes | Yes | Yes |
| Month and Industry FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 112,542 | 112,541 | 112,541 | 42,377 | 70,164 |
| **Panel B OLS regression** | | | | | |
| | | | D(Survival) | | |
| | 1 year | 2 years | 3 years | 4 years | 5 years |
| Treated*Post | -0.004 | 0.007 | 0.034*** | 0.052*** | 0.057*** |
| | (0.004) | (0.007) | (0.012) | (0.017) | (0.020) |
| Controls | Yes | Yes | Yes | Yes | Yes |
| Treated*Controls | Yes | Yes | Yes | Yes | Yes |
| Region FE | Yes | Yes | Yes | Yes | Yes |
| Month and Industry FE | Yes | Yes | Yes | Yes | Yes |
| Observations | 116,954 | 116,954 | 116,954 | 116,954 | 116,954 |
| Adj R-squared | 0.011 | 0.017 | 0.034 | 0.039 | 0.049 |

## Table 9 Learning by Venturing

Panel A investigates the impact of business creation experience during the Home Office Scheme period on future entrepreneurial activities for the matched sample. Specifically, we focus on entrepreneurs whose first business was established during our sample period (January 1999 – March 2005) and classify them into two groups based on the time of first business creation. *D(FirstBusi_Post)* equals one for those who establish the first business in the post-reform period (December 2001 – March 2005). *D(FirstBusi_Treated)* equals one if the first business created falls in the treated industries. Robust standard errors clustered by industry of first business are reported in parentheses. Panel B examines the intensive margin by focusing on serial entrepreneurs in the above sample, that is, those who have the second business established, and makes a comparison of the second business's characteristics: (1) number of employees at origination; (2) survival rate within 2 years (results are similar using 1,3,4 or 5 years). Our analysis is focused on the treated industries. Robust standard errors clustered by industry are reported in parentheses. *, **, *** denote significance at the 10%, 5%, and 1% levels, respectively.

**Panel A Extensive Margin Analysis: Probability of Second Business Creation**

| | (1) | (2) |
|---|---|---|
| Dependent Var. = D (Second Business Created) | | |
| D(FirstBusi_Post) *D(FirstBusi_Treated) | 0.026*** | 0.024*** |
| | (0.008) | (0.008) |
| First Business Region FE | No | Yes |
| First Business Creation Month and Industry FE | Yes | Yes |
| Observations | 127,044 | 127,036 |
| Adj R-squared | 0.009 | 0.012 |

**Panel B Intensive Margin Analysis**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent Var. | Number of employees | | D(survival within 2 years) | |
| D(FirstBusi_Post) *D(FirstBusi_Treated) | 0.078*** | 0.067*** | 0.002 | -0.000 |
| | (0.022) | (0.020) | (0.009) | (0.009) |
| Control for industry productivity | No | Yes | No | Yes |
| Second Business Region FE | No | Yes | No | Yes |
| Second Business Month FE | Yes | Yes | Yes | Yes |
| Second Business Industry FE | Yes | Yes | Yes | Yes |
| First Business Creation Month and Industry FE | Yes | Yes | Yes | Yes |
| Observations | 48,740 | 48,740 | 48,740 | 48,740 |
| Adj R-squared | 0.115 | 0.143 | 0.253 | 0.256 |