Credit Constraints, Job Mobility and Entrepreneurship: Evidence from a Property Reform in China*

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Abstract

This paper provides new evidence on the impact of private property rights on entrepreneurship. I explore this issue in the context of a housing reform in urban China that allowed state employees renting state-owned housing the opportunity to buy their homes at subsidized prices. Using the reform as an exogenous change in the capital constraints and mobility costs that influence individuals' entry into entrepreneurship, my estimates suggest that the property reform increased self-employment. I develop a model of job choice to test two mechanisms that might explain how the reform increased entrepreneurship. I find that the reform increased the ability of individuals to finance entrepreneurial ventures by allowing them to capitalize on the value of the real estate. The unbundling of housing benefits from state employment also contributed to the increase in entrepreneurship by facilitating labor mobility out of the state sector.

Keywords: credit constraints, labor mobility, entrepreneurship, China, housing reform, private property
JEL Classification: J23, J32, J48, O18, O53, P21

1 Introduction

State-owned housing represents a large share of housing stock in low-income countries. Data from the United Nations Human Settlements Indicators (2001) suggest that residents in state-owned housing represent 18% of households in Africa, 16% in Asia and 8% in Latin America. Unlike public housing in high income countries which is provided as a welfare benefit to low income households, the bulk of state-owned housing in Africa and Asia is provided as an in-kind benefit to employees in the government and in state-owned enterprises. While the potential problems associated with housing provision to state employees have been widely recognized (Fishback 1992, Wang and Murie 1999), there is a remarkable lack of evidence that measures the labor market consequences of such programs and explores the mechanisms through which the effects work.

This paper uses a reform of employer-provided housing in China as a natural experiment to analyze how the state provision of housing affects labor market decisions. The reform offered state employees who were provided rental housing from their employers the opportunity to purchase their homes at subsidized prices. It was a large-scale program that affected over forty percent of urban households in China. In this paper, I focus on the impact of the property reform on the decision to enter into entrepreneurship because entrepreneurs are regarded as key agents in fostering the innovation and investment necessary for economic development.[1] I explore two mechanisms through which state ownership of property might affect entrepreneurship. First, in a credit constrained environment, the privatization of state assets could allow individuals to capitalize on the value associated with the real estate in ways that they can not as renters. Second, the unbundling housing from the employment decisions of individuals could increase aggregate job mobility, including transitions into entrepreneurship.

The first mechanism focuses on the institutions that allow the capital embedded in property to be used in a productive manner. Economic research supports the importance of private property rights for economic development (Acemoglu, Johnson and Robinson 2001) as well as for household-level decisions regarding labor (Field 2007) and agriculture (Besley 1995). This paper contributes to the existing property rights literature by offering an empirical test of an idea popularized by De Soto (2000). He argued that the real estate occupied by urban squatters contains vast amounts of potential wealth that could be transformed into capital for entrepreneurial ventures through the formalization of property rights. In this paper, I explore a similar question with a focus on the creation of private property rights on the stock of state-owned housing.

The ability to access the value associated with property is relevant for entrepreneurial ventures in the presence of credit market imperfections. Evidence for severe credit constraints have been found for firms (Banerjee and Duflo 2004 in India; De Mel, McKenzie and Woodruff 2007 in Sri Lanka; McKenzie and Woodruff 2006 in Mexico) as well as for agriculture (Udry and Anagol 2006 in Ghana). This paper is more closely aligned with the credit constraints literature that examines the individual-level decision to enter into entrepreneurship. Research by Evans and Jovanovic (1989) in the U.S., Paulson and Townsend (2004) in

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[1]The model by Banerjee and Newman (1993) demonstrates how credit market imperfections can lead to barriers to entry for potential entrepreneurs, and have negative effects on aggregate economic growth and the income distribution.

Thailand, and Djankov, Qian, Roland and Zhuravskaya (2006) in China find a positive relationship between pre-existing wealth and the probability of entry into entrepreneurship. The notion that wealth predicts entrepreneurship is seen as evidence that wealthy individuals are able to finance their business ideas, while less wealthy individuals are unable to raise the capital necessary to start up or maintain their business ventures. However, if wealth is correlated with unobserved ability, then there can be a positive correlation between wealth and entrepreneurship in the absence of credit market imperfections. Research that attempts to instrument for wealth with inheritances and housing prices provides mixed results (Hurst and Lusardi 2004, Fairlie and Krashinsky 2006). However, inheritances are arguably not exogenous and the changes in housing prices used in these papers may be too small to produce a salient wealth effect. I use an exogenous change in access to housing wealth to separate the effect of credit constraints from omitted variables that affect an individual's decision to start a small business. Unlike the small-scale changes in wealth used in other papers, the value associated with the property occupied by state housing tenants was quite large.

In addition credit constraints, this paper considers job mobility as a second factor that affects entry into entrepreneurship. The special features of the housing system in China make it both interesting and necessary to consider job mobility when analyzing the labor market choices of individuals in China. Before the reform, a substantial portion of workers in the state sector were provided housing from their employers. The bundling of housing benefits with a specific employer could have reduced labor mobility because individuals were unable to stay in their current homes if they switched jobs. To my knowledge, this paper is the first analysis of the economic effects of employer-provided housing benefits. A related literature on in-kind benefits and job mobility focuses on the provision of health insurance in the U.S. (Madrian 1994, Kapur 1998, Buchmueller and Valleta 1996, Gruber and Madrian 2002). For both health insurance in the U.S. and housing provision in China, the level of employer-provided benefits does not directly reflect worker productivity. For example, American employees with large families receive greater compensation in the form of health insurance coverage than equally productive, single workers. The methods of the health insurance literature do not apply to employer-provided housing because the outside cost of health insurance varies based on individual characteristics such as pre-existing conditions whereas market housing prices do not vary across individuals. My research is also loosely related to work on the labor market effects of different systems of housing, such as welfare housing (Hughes and McCormick 1987) and rent control (Svarer, Rosholm and Munch 2004). My analysis focuses on housing that is tied with employment rather than tied to a neighborhood or a specific apartment.

My baseline identification strategy uses a differences-in-differences approach in comparing the outcomes of the same individuals before and after the reform as well as relative to a "control" group of similar individuals. I use both a state sector and a private sector comparison group and find substantial treatment effects relative to both groups. While the treatment group is not exogenously determined, the panel structure of the data set allows me to provide empirical support for the identification assumption of parallel trends in self-employment between the two groups before the reform.

3

To understand the mechanisms that underlie the program's effect on entrepreneurship and the identification issues in my empirical work, I develop a theoretical model of job choice that incorporates features of the Chinese labor market. The model allows for heterogeneity in unobserved individual characteristics such as entrepreneurial ability and connections in the state sector. The framework provides insight into how individuals sort into different types of housing and employment sectors. It also yields predictions that allow me to examine the two mechanisms through which the sale of state-owned housing during the housing reform could have led to an increase in entrepreneurship.

Using data from the China Health and Nutrition Survey, my estimates suggest that the vast majority of residents in state-owned housing choose to purchase their homes at the subsidized price. Furthermore, the reform increased the probability that former state housing residents entered into self-employment by two percentage points. This represents a doubling of the base rate of self-employment in the affected group. The data also indicate an increase in the rate of job changes among former residents of state-owned housing as well as a substantial growth in the amount of business capital that they owned. An extrapolation of the estimates suggest that the property reform freed around ten billion RMB from former state-owned housing for productive enterprises. This represents an increase of 14% on an estimated 67 billion RMB of capital investment in urban household enterprises in 1993. Overall, the empirical evidence suggests that entrepreneurship expanded both because the reform loosened credit constraints and because it increased overall mobility.

2 Institutional Background

2.1 Socialism and Early Reforms

Upon taking control in 1949, the Communist Party nationalized land in urban areas and established a system that guaranteed jobs for all workers. Households with private ownership of homes retained full property rights to their residences, but the government established public ownership of all new housing stock. Public housing stock was allocated to urban residents through state work units and nominal rents were charged.

Following the death of Chairman Mao in 1976, the new leadership initiated a gradual reform of the socialist system towards a mixed economy. A reform of the housing system was considered because the government recognized serious problems in the state provision of housing, including shortages, poor management and corruption in the distribution (Wang and Murie 1999). Private construction of housing was allowed and the supply of commercial housing expanded. The first experiments on the public housing system entailed the sale of newly built apartments at construction cost in Xian and and Nanning in 1979. During the 1980s, several other small-scale housing experiments were piloted in different cities. These included a program that split the cost of housing between workers and state work-units and a voucher system in which employees paid additional rent in cases where their housing allotment exceeded the value of the voucher. One of the most serious proposals outlined by the federal government included simultaneously increasing

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Figure 1: Aggregate Trends in Home Ownership in Urban Areas
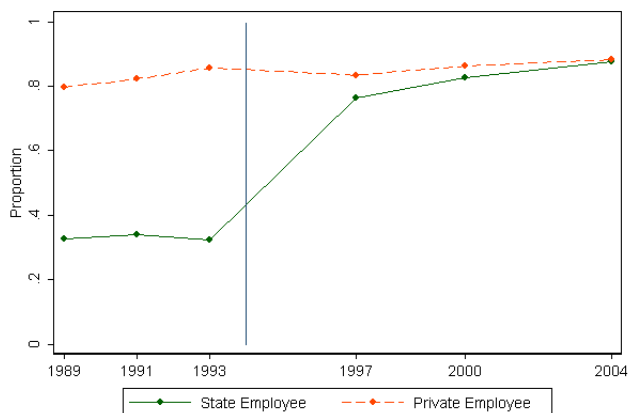
Data Source: CHNS

rents and wages and encouraging state tenants to buy their homes at full cost (Pudney and Wang 1995). The small-scale attempts at privatizing housing failed because people found the prices too high.

After the political protests and subsequent military crackdown in Tiananmen Square in 1989, the central government shifted the discussion about housing reform towards rent increases rather than privatization. The government realized its past ideas for privatization were financially infeasible as well as politically destabilizing (Davis 1993). Davis' interviews with urban residents confirm that the central and municipal governments hid their plans for full commodification of urban housing assets from most of the population through the early-1990s. While the experimentation in the 1980's clearly demonstrated the government's interest in housing reform, qualitative research suggests that the urban population did not foresee the timing and specific nature of the reform. In Section 5.2, my empirical analysis confirms that anticipation of the reform did not affect pre-reform labor market choices.

2.2 Privatization of Public Housing Units

In July 1994, the State Council outlined the procedures for state employers to sell public housing units to sitting tenants in urban areas throughout the country. Individuals in state-owned housing were given the opportunity to buy full or partial property rights to their current homes. Partial property rights included use rights for perpetuity, the right to bequeath and the right to use the home as collateral for loans. After five years of ownership, individuals with partial property rights gained the right to sell the home, but shared the profits from the sale with their work units.[2] In contrast, those purchasing full property rights faced no restrictions in the use or sale of their homes and retained all profits earned.

Learning from the public response to housing experiments in the 1980s, the government allowed work units to set prices for their housing stock below market value with additional discounts based on seniority.

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[2]In some cases, the subsequent sale of former public units had additional limitations. For example, apartments sold to public university employees that were located on campus could not be sold in the open market. On-campus property had to be sold back to the university or to other university employees. Such strict limitations on sale were not common outside of university housing systems.

According to the China News Analysis (1998), most buyers paid less than 15% of the market value for their homes. While the option to buy the home had no specified time limit, the government encouraged immediate purchase by specifying a schedule of price increases over time. As added incentive to purchase homes, the reform included proposals to increase rents in state-owned housing units from less than 1% up to 15% of average household income. The generous prices allowed most households to buy the homes outright. Households without the cash to purchase their homes had the incentive to take mortgage loans because they would gain the difference between the market value and the government sale price. The housing reform that began in 1994 transformed China into a country with one of the highest rates of home ownership in the world. The success of the reform in increasing the rates of home ownership among state employees is demonstrated in Figure 1, which displays the rates of home ownership by the employment status of the household head. The trend for households headed by state employees demonstrate a dramatic increase in home ownership following 1994 while the trend for households headed by private employees is relatively flat.

Data from the Chinese Household Income Project covering urban areas in eleven provinces in 1995 indicate that the average difference between the market value and the price charged by the government was 24,462 RMB, which is more than two times the average annual wages of a household.[3] While the terms of the sale were highly subsidized, the pre-reform rents charged by the government were also extremely subsidized. The average present discounted value of future rent subsidies under the pre-reform system is calculated to equal 20,223 RMB.[4] This is roughly equivalent to the 24,462 RMB subsidy in the sale price during the reform and suggests that there was there was little wealth effect associated with the reform. The effect of the housing reform operated through the transformation of use rights into private property rights rather than through a wealth effect associated with the terms of the sale. The reform gave individuals in state-owned housing the ability to capitalize on the wealth inherent in the housing that they had previously only been able to occupy.

While the value associated with each home was substantial, its relevance for entrepreneurship depends on whether the home equity could be accessed. There are at least three ways that individuals could access the value of their homes after the reform. First, after acquiring full property rights, individuals could sell their homes. Appendix table XII shows the households that were given the opportunity to buy their state-owned rentals were more likely than other groups to change residences within the sample area following the reform. While there were restrictions on outright sale among individuals who purchased partial property rights, they could still access the wealth either by renting out rooms or by using the home as collateral in loans from both formal and informal sources.[5] An article in the *Financial Times* (2002) emphasized the importance of

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[3]The calculation is based on the sample of 6,931 households in the Chinese Household Income Project of 1995, which was selected from a sample of 35,000 urban households from the State Statistical Bureau.

[4]This calculation is based on the assumption that individuals believed the current system would persist and on an expected life expectancy of 72 years and a real interest rate of 2% in China in 1994. The average age and pre-reform rent subsidy of state housing tenants used in the calculation are based on data shown in Table I.

[5]Formal sector lending by banking institutions in less common in China than in developed countries such as the U.S. However, evidence suggests that informal sector lending is common in China (Feder et. al. 1992). A 2004 survey by the Beijing Central University of Finance and Economics of 20 provinces estimated the amount of underground lending in China at $101 billion, equivalent to 28% of the the funds lent through formal sources (Li 2004). Further evidence indicates that loans from non-bank

Figure 2: Urban Aggregate Trends in State Employment and Self-Employment
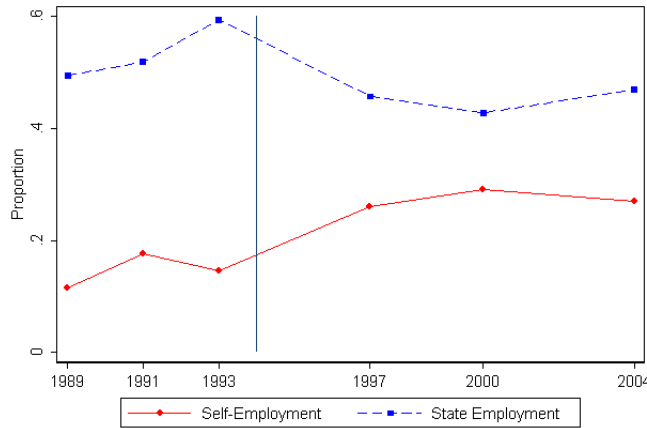
loans collateralized by housing: "Having recently bought an apartment from his state employer at a steep discount, he felt galvanized to put his asset to work... Mr. Yao is one of millions of Chinese discovering the delights of collateral. A boom in home ownership in most large cities over the past five years has furnished many people with their first genuinely valuable asset and the Chinese are proving enthusiastic borrowers." A Gallup poll on borrowing behavior in China in 2005 found that personal borrowing to start or operate a business was the top reason that individuals go into debt (42% of respondents).

Figure 2 shows rates of state employment and self-employment in urban areas from 1989 to 2004. The time-series patterns provide visual evidence for a break in employment trends following the start of the reform in 1994, and are consistent with movement from the state sector into self-employment. Aggregate employment in the state sector was 8% lower in the three periods after the reform than before, and self-employment rose by 12%.

3   Theoretical Framework

3.1   A Model of Employment and Housing Choices

I develop a static model of employment and housing decisions that incorporates elements of Evans and Jovanovic's (1989) model of entrepreneurial choice with liquidity constraints. An individual chooses to become a self-employed entrepreneur or work for a wage in either the state sector or the private sector. Embedded in the choice between the three types of employment is a decision regarding housing. If the individual becomes an entrepreneur or an employee in the private sector, she must purchase housing in the private market. If she becomes a state employee, then she receives a public housing unit in an amount denoted by $\widetilde{h}_i$, where $\widetilde{h}_i \geq 0$.[6] Individuals differ in their liquid wealth, W_i, and their observable characteristics, x_i.

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sources are often collateralized by assets including housing (Watts 2005, Guo and Mu 1998).

[6]For simplicity, I model $\widetilde{h}_i$ as a free in-kind benefit. In reality, public tenants are charged subsidized rents. A state employee's decision between taking the public housing allotment or entering the private market depends on her preferences over housing

Suppose the utility of individual i employed in sector l is a function of housing, h_{il}, and non-housing consumption, C_{il} is:

$$\max_{h_{il}, k_{il}} U(C_{il}, h_{il}) \tag{1}$$

where $l \in \{s, p, e\}$ and s indicates the state sector, p the private wage sector and e entrepreneurship. The constraints differ by sector because the model only allows borrowing to finance capital, and capital investment is only relevant in the entrepreneurial sector. The budget constraint for a wage employee in the private or state sector is

$$C_{il} + q_l h_{il} = Y_{il} + rW_i \tag{2}$$

and for an entrepreneur it is

$$rb_{il} + C_{il} + q_l h_{il} = Y_{il}(k_{il}) \tag{3}$$

$$k_{il} = W_i + b_{il} \tag{4}$$

$$-W_i \leq b_{il} \leq \lambda(q_l h_{il} + W_i) \tag{5}$$

where k_{il} is capital, r equals the cost of borrowing plus one, q_l is the price of a unit of housing and the price of consumption is normalized to equal one. Individuals are credit constrained in their investment in capital and the net amount that they can borrow, given by b_{il}. The net amount borrowed cannot exceed a proportion of their total wealth which includes the value of their homes, $q_l h_{il}$, and their liquid assets, W_i. The proportion is denoted by λ, where $0 < \lambda < 1$. If she chooses not to own capital ($k_{il} = 0$ and $b_{il} = -W_{il}$), she can lend out her wealth. From her earnings, Y_{il}, she repays the amount that she borrows (or receives the principal and interest on her loan). The amount the individual earns in sector l is Y_{il}. The utility function is assumed to be increasing and concave in both consumption and housing. Individuals have the same preferences over housing and consumption, so sorting into the three sectors is driven by differences in their housing compensation and earnings in the various sectors.[7]

Wages in the private and state sectors are functions of individual i's set of observable characteristics, x_i, given by $Y_{ip} = y_p(x_i, \zeta_i)$ and $Y_{is} = y_s(x_i, \psi_i)$, respectively, where ζ_i is an unobserved component of wages in the private sector and ψ_i the unobserved component of state wages. The market price for a unit of housing for entrepreneurs and private wage employees is q ($q_p = q_e = q$). Workers in the state sector receive an amount of housing allocation, $\widetilde{h}_i = \widetilde{h}(x_i, \mu_i)$, to rent for free ($q_s = 0$). The person's observable characteristics, x_i, and some unobservable characteristics, μ_i, determine the amount of public housing that the individual receives. This reflects the evidence that the allocation of public housing units depended on observable worker

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and consumption as well as the relative prices of housing in the public system and the private market.

[7]For details on how heterogeniety in preferences for housing would alter the model, see Appendix A.

characteristics such as job tenure and rank as well as unobservable traits such as social connections (Logan and Zhou 1996). I assume that μ_i and ψ_i are not perfectly correlated. Net entrepreneurial earnings are given by $Y_{ie} = \theta_i f(k_i)$, where θ_i reflects entrepreneurial ability, and $f(\cdot)$ is the production function that is concave in the amount of capital invested in the business, k_i. This form suggests that entrepreneurs with higher levels of ability have higher total product as well as higher marginal product at every level of capital.[8] Entrepreneurial ability, θ_i, state connections, μ_i, private wage unobservables, ζ_i, state wage unobservables, ψ_i, initial wealth, W_i, and the vector of characteristics, x_i, all vary across individuals. Furthermore, I assume that individuals know their own values for all measures, but only x_i is observed by the econometrician.

The total utility an individual derives from working in the state sector is given by[9]

$$V_{is} = U(Y_{is} + rW_i, \widetilde{h}_i). \tag{6}$$

The total utility of a worker in the private sector is

$$V_{ip} = U(Y_{ip} + rW_i - qh_i^*, h_i^*) \tag{7}$$

and h_i^* solves the following first-order condition

$$-qU_c + U_h = 0 \tag{8}$$

where U_c is the derivative of the utility function with respect to non-housing consumption and U_h is the derivative with respect to housing. The total utility of an entrepreneur is

$$V_{ie} = U(\theta_i f(\bar{k}_i) - r\bar{k}_i + rW_i - q\bar{h}_i, \bar{h}_i) \tag{9}$$

where the amount of housing chosen is such that

$$\bar{h}_i \text{ solves} \begin{cases} -qU_c + U_h = 0 & \text{if } b_i^* < \lambda(q\bar{h}_i + W_i) \\ -q(1 + r\lambda)U_c + U_h = 0 & \text{if } b_i^* = \lambda(q\bar{h}_i + W_i). \end{cases} \tag{10}$$

The entrepreneur also selects the amount of capital to invest in her business. The choice of k_i is equivalent to choosing the net amount to borrow, b_i. The net amount borrowed can be negative; in other words, the individual can also lend out her wealth and earn r. At an interior solution, the first-order condition with respect to k yields k_i^* that solves $\theta_i f'(k_i) = r$. However, the maximum capital accessible for the entrepreneur

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[8]This is consistent with the empirical evidence. See Evans and Jovanovic (1989) for an overview.

[9]This assumes that all state employees receive and accept the housing offered in the state sector. This simplification allows me to focus on the implications of employer-provided housing in the state sector, and does not change the fundamental results of the model. In reality, many state employees live in private housing either because they are waiting for a state housing assignment, or because their preferences and budget constraints bring them to consume a level of housing that is different from their state assignment.

Figure 3: Case of $Y_p > Y_s$
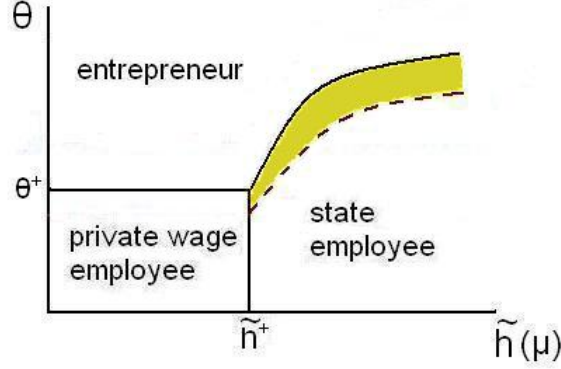
is constrained such that $k_i \leq (1 + \lambda)W_i + \lambda q \bar{h}_i$. Thus, $\bar{k}_i = \min\{k_i^*, k_i^{cc}\}$ where $k_i^{cc} = (1 + \lambda)W_i + \lambda q \bar{h}_i$. Because the production function f is increasing, for $k_i^{cc} < k_i^*$, we have that $\theta_i f(k_i^{cc}) < \theta_i f(k_i^*)$ and the total value of being an entrepreneur, V_{ie}, is lower for individuals with binding credit constraints.

The individual will become an entrepreneur if $V_{ie} > max\{V_{is}, V_{ip}\}$, a state employee if $V_{is} > max\{V_{ie}, V_{ip}\}$, and a private wage employee if $V_{ip} > max\{V_{ie}, V_{is}\}$. The value of V_{ie} depends on the level of capital input, k_i, which is limited by the sum of her liquid wealth and the amount that she can borrow. Thus, it is possible that a constrained individual chooses not to be an entrepreneur even though she would have if she were able to achieve the optimal level of capital investment, k_i^*. Credit constraints will produce a correlation between individuals' total wealth and their probability of entering entrepreneurship.

For a worker with given values of W_i, x_i, ζ_i and ψ_i such that $y_p(x_i, \zeta_i) > y_s(x_i, \psi_i)$, her optimal choice across job sectors can be represented by the solid lines in Figure 3.[10] The values θ^+ and $\tilde{h}^+$ are the reservation values that delineate the choices of workers based on their unobserved heterogeneity in μ_i and θ_i. More specifically, θ^+ is the value of θ_i at which an individual is indifferent between private wage employment and self employment; in other words, θ^+ solves $V_{ip} = V_{ie}$, which simplifies to

$$Y_{ip} = \theta_i f(\bar{k}_i) - r\bar{k}_i. \tag{11}$$

The value of θ^+ that solves equation 11 is independent of $\tilde{h}(\mu_i)$, and individuals with levels of entrepreneurial ability above θ^+ will prefer self-employment over a private wage job. Similarly, $\tilde{h}^+$ is the reservation value of housing benefits needed to make the individual indifferent between working for the state or a private firm; $\tilde{h}^+$ solves $V_{ip} = V_{is}$. People with high draws of public housing benefits, $\tilde{h}(\mu_i)$, will choose the state job over the private job. Finally, for individuals with high draws of both θ and μ, there are values of θ_i and $\tilde{h}_i$ such that the individual is indifferent between entrepreneurship and state employment, $V_{ie} = V_{is}$. This locus of

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[10]For an individual for whom $y_p(x_i, \zeta_i) < y_s(x_i, \psi_i)$, the choice simplifies to a binary choice between entrepreneurship and the state job. The individual will not choose the private sector because she will receive higher wages and a nonnegative housing benefit in the state sector.

points in $(\theta, \widetilde{h})$ space is determined by the following equation:

$$U(Y_{is} + rW_i, \widetilde{h}_i) = U(\theta_i f(\bar{k}_i) - r\bar{k}_i + rW_i - q\bar{h}_i, \bar{h}_i). \tag{12}$$

This is represented by the solid curve in Figure 3 and defines the employment decision for workers with high levels of both θ_i and $\widetilde{h}(\mu_i)$. The concave shape of the boundary is quite intuitive. As the housing that individuals receive in the state sector increases, they need higher levels of θ_i to make them indifferent between entrepreneurship and state sector employment ($\partial\theta/\partial\widetilde{h} > 0$). Furthermore, at high levels of $\widetilde{h}(\mu_i)$, the amount of $\widetilde{h}(\mu_i)$ needed for the individual to remain indifferent increases relative to θ_i ($\partial^2\theta/\partial\widetilde{h}^2 < 0$). This occurs because of the concavity of the utility function in housing. The marginal utility from increasing housing in the state sector is diminishing, but higher levels of θ_i yield more income that can be allocated to both housing and consumption. Appendix B derives this result formally.

As a result of the sorting process, individuals with higher levels of $\widetilde{h}(\mu)$ also have higher average levels of θ. Holding constant other characteristics, individuals with high levels of θ and low levels of $\widetilde{h}(\mu)$ will work as entrepreneurs. Thus, high θ types will only locate in the state sector if their housing allocation $\widetilde{h}(\mu)$ exceeds the threshold level. A similar explanation applies to the relationship between $\widetilde{h}(\mu)$ and the unobservable factor, ζ_i, that influences wages in the private sector.[11] High levels of $\widetilde{h}(\mu)$ observed in the state sector will also correspond with higher average amounts of ζ_i. The key assumption of the model is that μ_i and ψ_i are not perfectly correlated.[12] After the single period in which individuals make their optimal labor market decisions, positive correlations between $\widetilde{h}(\mu_i)$ and θ_i and between $\widetilde{h}(\mu_i)$ and ζ_i will result.

3.2 Theoretical Implications of Housing Reform

As I have discussed, loosening credit constraints increases the likelihood of self-employment by allowing individuals to move closer to k^* from k^{cc}. In Figure 3, this would cause the curved line defined by equation 12 to shift down from the solid line to the dotted line.[13] However, the housing reform only loosens credit constraints for state employees in public housing. Thus, only individuals in the shaded area in Figure 3 (in between the solid and dotted curved lines) shift from state employees to entrepreneurs. The credit constraints of private wage employees are not changed by the housing program, so none of the boundaries that determine their optimal employment decisions adjust.

By allowing workers to purchase their homes, the program removed the flow benefit of housing provision in the state sector. Because $\partial V_{is}/\partial\widetilde{h} > 0$, the housing program reduced an individual's value of V_{is} and increased the likelihood of moving out of the state job. By decreasing the flow of housing benefits tied to state employment, $\widetilde{h}(\mu_i)$, to zero for state employees who purchase the home, the reform shifts individuals

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[11]Figure 3 delineate the optimal employment decisions of individuals holding ζ_i constant. As ζ_i increases, the threshold levels θ^+ and $\widetilde{h}^+$ will also increase and expand the area over which private wage employment is the optimal decision. It is possible to imagine a three-dimensional space that shows how ζ, θ, and $\widetilde{h}$ define the optimal employment choices.

[12]Section 6.1 provides empirical support for this assumption.

[13]The corresponding calculations are shown in Appendix B.

left to their corresponding points on the y-axis, and cause individuals to shift left in Figures 3. In this case, all state workers for whom $Y_p > Y_s$ will enter the private sector or entrepreneurship. Whether a person becomes self-employed rather than a private wage employee depends on whether her entrepreneurial ability exceeds θ^+ where the threshold level of θ^+ increases with ζ_i. Even without any housing benefits, individuals may still optimally choose to remain in the state sector; these are the people for whom state wages exceed private wages and have lower levels of entrepreneurial ability ($\theta_i < \theta^{++}$).

As the theoretical framework demonstrates, there are two ways that the housing reform could increase transitions from employment in a state-owned enterprise into entrepreneurship. The creation of private property rights during the housing reform allowed individuals to invest more capital into a small business and increased the returns to entrepreneurship in the presence of credit constraints. On top of that, the unbundling of employment and housing provision reduced the non-wage benefits of state employment, and increased the relative attractiveness of non-state jobs. The simple model not only explains the possible effects of the housing reform on labor market choices, but it also provides a context for empirically testing the relevance of both the alleviation of credit constraints and the unbundling of housing from employment.

I look for evidence for credit constraints in two empirical tests. The model predicts that the wealth that people hold and the value of their homes should predict self-employment ($\partial V_{ie}/\partial(W_i + qh_i) > 0$) only if there are credit constraints. I examine exogenous variation in the post-reform appreciation of housing prices to avoid the potential identification that arises if total wealth is positively correlated with unobservable entrepreneurial ability. More specifically, I use regional heterogeneity in the appreciation of housing prices to test whether housing wealth influenced entry into entrepreneurship. The second implication of the credit constraints hypothesis is that the property reform should have increased the levels of capital investment for pre-reform residents of public housing. Individuals who realized the ability to capitalize on the value associated with the newly privatized property should move from k^{cc} towards k^*.

The model also generates two predictions to test whether the provision of housing benefits in the state sector was internalized by individuals in their labor market decisions. A reduction in the value of housing subsidies, $\widetilde{h}$, induces individuals to shift out of the state sector and into both self-employment and private wage employment. In contrast, a reduction in credit constraints only predicts transitions into self-employment and not into private wage jobs. Thus, the movement of state sector workers with housing benefits into private wage employment supports the unbundling hypothesis. A second prediction is generated from a simple extension of the model into a dynamic context. If the pre-reform system of providing state-owned housing with state jobs reduced labor mobility, we should see that the average wage gains accompanying job changes out of the state sector in the pre-reform period should be larger than the average gains in the post reform period. In other words, $w_{p,t} - w_{s,t-1}$ should be larger before the reform because individuals leaving the state sector would need additional compensation for the loss of subsidized housing.

Furthermore, the amount of rent subsidy that individuals received *before* the reform provides heterogeneity in the effects of bundling housing with state employment. The model demonstrated that individuals

with more $\widetilde{h}$ should have higher levels of abilities in private wage employment, ζ, and in entrepreneurship, θ. Not only does the bundling hypothesis imply an increase in mobility that is not limited to transitions into entrepreneurship, but it also suggests that the effect on mobility should increase with pre-reform levels of $\widetilde{h}$. Similarly, not only should the wage gains be larger for individuals leaving the state sector before the reform, but the pre-reform wage gains of job changers should be positively correlated with $\widetilde{h}$.

4   Data and Methodology

4.1   China Health and Nutrition Survey

The data used in this analysis come from the China Health and Nutrition Survey (CHNS). The CHNS covers nine provinces (Guangxi, Guizhou, Heilongjiang, Henan, Hubei, Hunan, Jiangsu, Liaoning, and Shandong), which vary considerably in their geography and levels of economic development. The survey was sampled with a multistage, random cluster design. Counties were stratified into three levels of income, and a weighted sampling technique randomly selected four counties in each province. In addition, the data include the provincial capital and one low-income city. The data cover approximately 4,400 households and 16,000 individuals in the years 1989, 1991, 1993, 1997, 2000, and 2004. Thus, the data include three waves before and three waves after the beginning of the housing reform in 1994. While the survey contains both urban and rural households, the sample used in this analysis is limited to the urban sample because the housing reform was only implemented in urban areas.[14] This paper focuses on labor market choices, so I limit the sample to working age adults between 18 and 60 years old. Households with more than one generation of adults are not uncommon in urban China, but I restrict the individual-level analysis to household heads and spouses of heads in 1993. Furthermore, my analysis focuses on the impact of the reform on individuals who were already in the labor force in 1993 so I exclude individuals who were enrolled in school or retired.

Entrepreneurs are defined as individuals who report their primary occupation as self-employed. This categorization excludes individuals who engage in entrepreneurial activities in addition to a full-time wage job. While less than 4% of adults in the CHNS report a secondary job, three-quarters of this sub-sample engage in self-employment as their secondary labor market activity. Although not shown, the results in this paper are robust to the inclusion of people whose secondary job is self-employed. I also examine a definition of entrepreneurship as households that own a small business. This measure is highly correlated with individual self-employment. The results are similar with slightly larger standard errors. Household business ownership is subject to more measurement error because the survey question changes between 1993 and 1997.

The theoretical model simplifies the employment structure of the Chinese economy by splitting the world into three sectors of employment (state, private and self-employed). The survey allows individuals to categorize themselves into six sectors of employment (state, private, small collective, large collective and

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[14]I define urban areas as neighborhoods where the majority of households have urban registrations.

other). In China, employment in collective enterprises is less relevant in urban areas than in rural areas. It is rare for collective work units to provide employees with subsidized housing, so for the purposes of my analysis, it is not important to distinguish them from private wage employees. The empirical results are robust to the exclusion of collective employees.

4.2 Econometric Methodology

To evaluate the causal impact of the policy change on individual outcomes, I use a differences-in-differences framework. The idea underlying the identification strategy is to compare the outcome not only before and after the reform, but also between a treatment group and a control group. The CHNS does not explicitly ask whether tenants of public housing received the opportunity to buy their home during the reform, so I employ an intention-to-treat (ITT) approach.[15] I identify the treatment group as all household heads and spouses of heads in public housing with at least one member of the couple in state employment in 1993. Classification of individuals into the treatment and control groups occurs at the couple level.[16] While the start of the implementation of the housing reform varied at the regional level, I am unable to exploit this variation because the data set used in this analysis is not collected in the years between 1993 and 1997.

One "control" group includes all household heads and spouses living in privately-owned homes and not employed in a state-owned enterprise in 1993. These individuals should not experience a direct effect from the reform of public housing. I also define a second "control" group that encompasses household heads and spouses not living in public housing but with at least one member in state employment in 1993. There are a few anecdotal accounts of state-owned enterprises offering monetary compensation to employees not living in public housing at the time of the reform to offset the associated loss for workers on the waiting list for public housing allocations. However, the evidence suggests that this compensation was not universal and the value did not approach the large transfer associated with the subsidized home sale. Furthermore, any wealth transfer to individuals in the state-employed control group would bias the estimates towards accepting the null hypothesis of equality in the outcome between the treatment and the control group. The state-employed control group offers the advantage of absorbing other changes occurring in the state sector around the time of the housing reform. Thus, this control group removes the effect of changes in the wage structure, increases in lay-offs in the public sector, or decreases in provision of other in-kind benefits.

Table I presents summary statistics for the groups in the year immediately before the implementation of the reform. The treatment group is statistically similar to the control groups along several dimensions, including gender, age, marital status, ethnicity and durable non-housing assets (which equals the sum of a household's self-reported market value of durable goods but does not include housing or financial assets).[17]

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[15]To the degree that individuals declined or did not receive the opportunity to purchase, this approach will yield underestimates of the impact of the housing reform. Over 80% of households living in state-owned housing in 1993 transitioned into private housing by 1997, so the ITT estimates would be quite similar to estimates of the treatment on the treated.

[16]In other words, public housing tenants who are not employed in the state sector but married to state employees are categorized in the treatment category. A substantial fraction of state employees are married to other state employees, and it would not be possible to identify private property rights at the individual level for these couples.

[17]Variables in units of RMB are converted into real 1990 RMB using the GDP deflator for mainland China provided by the

Table I: Summary Statistics in 1993

	Treatment	Control 1: Other State Employees	Control 2: Non-State Home Owners
Male	0.52	0.55	0.54
	(0.50)	(0.50)	(0.50)
Age	41.72	40.91	41.45
	(8.98)	(9.16)	(9.54)
Married	0.98	0.98	0.98
	(0.15)	(0.13)	(0.15)
Durable Non-housing Assets	4177	3698	4705
	(7686)	(5450)	(7433)
Years of Education	10.76	8.22*	7.22*
	(3.61)	(3.62)	(3.29)
Han Ethnicity	0.95	0.94	0.93
	(0.21)	(0.24)	(0.26)
Household Size	3.62	4.07*	4.21*
	(1.24)	(1.55)	(1.53)
Real Hourly Wage	0.62	0.67	0.92*
	(0.52)	(0.57)	(0.69)
Cadre in Household	0.47	0.26*	0.08*
	(0.71)	(0.55)	(0.28)
Rent Subsidy per Month	72.03	0.13*	0.57*
	(67.18)	(2.41)	(8.21)
Service Occupation	0.17	0.26*	0.20*
	[0.37]	[0.44]	[0.40]
Self-Employed	0.02	0.07*	0.37*
	(0.13)	(0.25)	(0.45)
Capital	31.1	86.0*	549.2*
	(361.5)	(423.2)	(3295.9)
Observations	590	303	380

Notes: Table reports individual averages. Standard deviations in parentheses. * denotes the average for the control group is significantly different from the treatment group at the 5% level. Wage, assets, capital, and rent subsidy are in real 1990 RMB.

As expected, the treatment group is different from the control groups along a number of labor market characteristics. Households living in state-owned housing in 1993 have more members who hold positions as cadres.[18] The real hourly wage of individuals in the treatment group is significantly lower than the privately employed control, but not significantly different from the control group of individuals in state employment.[19] The rent subsidy is calculated as the difference between the self-reported market rental value and the rent paid per month. It is 72 RMB per month for the treatment group, and approximately zero for both control groups. The average level of self-employment and capital assets are lowest in the treatment group, and the state control group is more similar to the treatment group than the private control group.

The treatment group has more education than the other groups. Individuals in the treatment group have an average of 10.8 years of education, which is 2.5 to 3.5 years more than the average in the control groups. This difference raises two potential concerns about the identification strategy. While I can control for differences in observed characteristics, such as education, it is possible that my estimates reflect time-varying returns to education and other observable characteristics. I use a propensity score approach to address this issue in section 6.3. The second potential concern is that differences in observable characteristics may suggest the presence of differences in unobservables. The panel structure of the data allows me to remove the effects of any unobservable factors that are time invariant through the inclusion of individual fixed effects. The data indicate that observable characteristics and individual fixed effects explain approximately 80% of the variation in pre-reform housing subsidies among state employees (Table V). Furthermore, I will show that the empirical results do not support this explanation.

The regressions with binary outcomes in this analysis are estimated with a logistic regression. Because the mean rate of self-employment is low, the logistic model will perform better than a linear probability model. The baseline differences-in-differences estimator is implemented as a logistic regression of the form

$$Pr(y_{ijt} = 1) = m(\alpha_0 + \alpha_1 Treat_i * Post_t + \alpha_2 Post_t + \alpha_3 Treat_i + \alpha_4 \mathbf{x_{it}} + \varphi_j + \epsilon_{ijt}) \qquad (13)$$

where y_{ijt} is a dummy variable for self-employment for individual i in year t and province j, $Treat_i$ identifies the treatment group, $Post_t$ is a dummy variable that equals 1 in the three periods following the reform and φ_j are province fixed effects. The vector of covariates, $\mathbf{x_{it}}$, includes a quadratic in age, years of education, and an indicator for male. The coefficient, α_1, is the estimated effect of the housing program.

While the basic logistic regression yields marginal effects that are easy to interpret, the theoretical model suggests that the proper estimating equation includes individual fixed effects to control for the un-observed time-invariant characteristics, θ_i, ζ_i, ψ_i and μ_i, that affect the decisions regarding housing and

United Nations.

[18]Cadres hold administrative or managerial positions in state-owned enterprises, government or the Communist party.

[19]The hourly wage is calculated as the sum of an individual's average monthly wage, one-twelfth of the annual bonus and earnings from labor provided to a collective, all divided by average hours worked per month.

employment. The fixed effects (or conditional) logit estimator of the following equation

$$Pr(y_{it} = 1) = g(\alpha_1 Treat_i * Post_t + \alpha_2 Post_t + \alpha_4 \mathbf{x_{it}} + \gamma_i + \epsilon_{it}) \tag{14}$$

corresponds to equation 13 with the inclusion of time-invariant individual fixed effects, γ_i.[20] The fixed effects logit has the disadvantage of not yielding estimates of average partial effects without strong assumptions regarding the distribution of γ_i (Wooldridge, 2002). For the fixed effects logit models, I report the results in coefficients, or log odds ratios, which are interpreted as the log of the relative odds of the outcome occurring for a group relative to another. Again, α_1 is the coefficient representing the program effect.

Identification of a panel differences-in-differences estimator requires a "parallel trends" assumption, which assumes the gap in the outcome between the treatment and the control group would remain the same in the absence of the reform. In order to examine this assumption, I allow the effects of the program to vary over time by estimating the following more flexible logit regression

$$Pr(y_{ijt} = 1) = m(\alpha_0 + \sum_{t \geq 1993} \beta_t \cdot Treat_i + \alpha_1 Treat_i + \alpha_2 \mathbf{x_{ijt}} + \tau_t + \varphi_j + \epsilon_{ijt}) \tag{15}$$

The corresponding fixed effects logit regression is

$$Pr(y_{ijt} = 1) = g(\alpha_0 + \sum_{t \geq 1993} \beta_t \cdot Treat_i + \alpha_2 \mathbf{x_{ijt}} + \tau_t + \gamma_i + \epsilon_{ijt}) \tag{16}$$

Relative to the baseline models of 13 and 14, the flexible specifications allow us to examine differences in trends in self-employment before and after the housing reform began in 1994. An estimate of β_{1993} equal to zero would show that the gap in the outcome between the treatment and control groups did not vary before the reform. Hence, it would support the key identification assumption that trends in self-employment across the different groups would have been similar in the absence of the reform.

5   Main Results

5.1   Impact on Entrepreneurship

Columns 1 and 3 in Table II present estimates of equation 13 where the dependent variable is an indicator for self-employment.[21] Column 1 includes the treatment group and the control group of employees of state-owned enterprises who were not living in public housing in 1993, and the sample in column 3 contains the treatment group and the control group of non-state employees who were home owners in 1993. Columns 2 and 4 display the corresponding average marginal effect. The estimates in Column 2 suggest that the housing

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[20]For the fixed effects estimator, the logit model is generally preferred over the probit model because the fixed effects probit is computationally more difficult to implement and its estimates are inconsistent for a small number of time periods (Wooldridge 2002).

[21]The corresponding results with the impact of the reform on home ownership are presented in Appendix C.

Table II: The Impact of the Reform on Self-Employment

	Logit: Self-Employment[a]				FE Logit: Self-Employment[b]	
	Control=State Workers		Control=Private Workers		Control=State	Control=Private
	Coefficient	*Marginal*	Coefficient	*Marginal*	Coefficient	Coefficient
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Post	0.86	*0.015*	1.892	*0.075*	1.745	2.133
	[0.580]		[0.554]**		[0.682]*	[0.655]**
Post	1.646	*0.028*	0.383	*0.009*	0.624	-1.306
	[0.262]**		[0.192]*		[0.489]	[0.329]**
Treat	-2.091	*-0.043*	-4.232	*-0.274*		
	[0.486]**		[0.479]**			
Age	-0.231	*-0.003*	-0.095	*-0.002*	-0.12	0.42
	[0.110]*		[0.087]		[0.160]	[0.100]**
Age^2	0.002	*0.000*	0.001	*0.000*	0.002	-0.003
	[0.001]+		[0.001]		[0.002]	[0.001]*
Education	-0.19	*-0.002*	-0.133	*-0.003*		
	[0.031]**		[0.028]**			
Male	-0.179	*-0.002*	-0.116	*-0.003*		
	[0.233]		[0.138]			
Obs	2805		2811		457	1045

Notes: ** denotes significance at the 1% level, * at the 5% level, + at the 10% level.
[a] Robust standard errors clustered by household in brackets. Additional controls are indicators for province and a constant term.
[b] Robust standard errors in brackets. Additional controls are individual fixed effects.

reform resulted in a 1.5 percentage point increase in self-employment for the treatment group relative to the control group of individuals not employed in the state sector in 1993. Column 4 shows that the implied increase in entrepreneurship relative to the control group of home owners in 1993 who were employed in the state sector is 7.5 percentage points. While the estimated effect of the program is smaller relative to the state control group than relative to the non-state control group, they are both large relative to the base rate of self-employment for the treatment group.

There are several possible explanations for the estimated effect to be smaller relative to state employees. The smaller estimate may be explained in the credit constraints framework by the possible monetary transfer given to some state employees that did have the option of buying a home at a subsidized price. However, the results in Section 7.2 do not support the idea that the access to wealth experienced by the treatment group was larger relative to private employees than other state employees. The difference may also be the result of other changes occurring in the state sector. The estimate of α_1 in the sample that includes the state control group removes the effect of other changes in the state sector.

The last two columns of Table II show the impact of the reform on self-employment with the inclusion of individual fixed effects. The effect of the reform on entrepreneurship remains robust in the specifications that control for individual unobserved heterogeneity. The estimates suggest that the reform significantly increased the log odds of self-employment by 1.7 to 2.1 times for the treatment group relative to the state control group and the private control group, respectively. The gap in the estimates for the two samples found in the logit estimates decreases with the inclusion of individual fixed effects.[22] This suggests that the gap in the logit estimates is driven by differences in time-invariant unobservable characteristics between workers in the private sector and state employees not residing in public housing rather than by non-housing changes occurring in the state sector.

5.2 Alternative Specifications

Alternative specifications that address potential biases from sample attrition and serial correlation are considered in Appendix E. This section presents the results from the flexible equations given by equations 15 and 16.[23] In Table III, the coefficient estimates of the interaction, *Treat*1993*, are small in magnitude and statistically equivalent to zero. This indicates that relative to earlier years, the pre-reform trends in self-employment of individuals in state-owned housing were not different from the trends for state-employed or privately-employed individuals in private housing. The results support the identifying assumption that the pre-reform trends in self-employment for residents of state housing were similar to residents in private housing. The estimates of the coefficients shift in 1997, which is the first survey wave following the start of the reform. Thus, individuals who had the opportunity to buy their residences from the state transitioned into entrepreneurship beginning in 1997 at a greater rate than individuals already living in private housing. An χ^2-test reveals that the coefficient on *Treat*1993* is significantly different from *Treat*1997* at the 5% level in columns 3, 5 and 6. In comparing the interactions among each of the post-reform years (1997, 2000, 2004) and the treatment indicator, the coefficients are not statistically different from each other with one exception at the 10% level. This indicates that the conditional, post-reform trends in self-employment were similar for the treatment and control groups. In general, the coefficient estimates on *Treat*1997* are similar in magnitude to the estimates of $Treat * Post$ in the more parsimonious estimates shown in Table II.

5.3 Alternative Explanations

5.3.1 Time-Varying Returns to Characteristics

In this section, I use a propensity score approach, outlined by Rosenbaum and Rubin (1983) and Wooldridge (2002), to evaluate whether the results are driven by changes over time in the *returns* to observable char-

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[22]Logit coefficients are scaled by the residual variation of the estimated equation (Amemiya 1985). Comparison of coefficients across regressions depends on the assumption that the unobserved variation is the same across the samples. The results of a test proposed by Allison (1999) fails to reject the null hypothesis of equal residual variation for the two samples ($\chi^2 = 1.07$).

[23]The interactions with 1989 and 1991 are omitted. The specification that only omits the 1989 wave produces similar results with larger standard errors.

Table III: Flexible Estimates of Program Effect on Self-Employment

	Logit: Self-Employment[a]				FE Logit: Self-Employment[b]	
	Control=State Workers		Control=Private Workers		Control=State	Control=Private
	Coefficient	*Marginal*	Coefficient	*Marginal*	Coefficient	Coefficient
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*1993	0.383	*0.00*	0.996	*0.02*	0.062	-0.398
	[1.094]		[1.027]		[0.885]	[0.819]
Treat*1997	0.65	*0.01*	2.718	*0.16*	1.826	2.411
	[0.923]		[0.905]**		[0.855]*	[0.837]**
Treat*2000	1.047	*0.02*	2.101	*0.09*	1.112	1.608
	[0.935]		[0.892]*		[0.853]	[0.800]*
Treat*2004	2.412	*0.09*	2.115	*0.10*	2.722	2.028
	[1.073]*		[0.949]*		[1.068]*	[0.984]*
Treat	-2.285	*-0.04*	-4.527	*-0.28*		
	[0.770]**		[0.739]**			
year 1993	-0.175	*0.00*	-0.869	*-0.01*	0.015	-1.085
	[0.452]		[0.276]**		[0.633]	[0.359]**
year 1997	1.851	*0.04*	-0.388	*-0.01*	1.000	-3.314
	[0.385]**		[0.322]		[1.194]	[0.737]**
year 2000	1.49	*0.03*	0.192	*0.00*	1.414	-4.348
	[0.399]**		[0.299]		[1.679]	[1.028]**
year 2004	0.983	*0.02*	0.928	*0.02*	1.199	-5.343
	[0.638]		[0.405]*		[2.342]	[1.439]**
$\chi^2 : 1993 = 1997$[c]	0.10		3.85*		3.78*	8.80**
$\chi^2 : 1997 = 2000$	0.24		0.74		0.86	1.25
$\chi^2 : 1997 = 2004$	3.60+		0.58		0.96	0.19
$\chi^2 : 2000 = 2004$	2.06		0.00		2.88	0.26
Obs	2805		2811		457	1045

Notes: * denotes significance at the 5 level, + at the 10% level. [a]Robust standard errors clustered by household in brackets. Additional controls are a quadratic in age, years of education, indicators for male and province and a constant term. [b]Robust standard errors in brackets. Additional controls are a quadratic in age and individual fixed effects. [c]χ^2 test of equality between *Treat*1993* and *Treat*1997*.

acteristics, such as education, along which the treatment group is different. This procedure first estimates a propensity score for treatment across all individuals in the sample. The estimated propensity score is included in the regression of interest to act as the control function, and it should contain all the information in the covariates that is pertinent in the estimation of the treatment effect. In the regression framework, I include both the propensity score and its interaction with the *Post* dummy. This removes the effect

Table IV: Estimates for Self-Employment including Propensity Score

| | Logit: Self-Employment[a] | | | | FE Logit: Self-Employment[b] | |
| | Control=State Workers | | Control=Private Workers | | Control=State | Control=Private |
| | Coefficient | Marginal | Coefficient | Marginal | Coefficient | Coefficient |
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Post	0.79	0.009	1.79	0.053	1.57	2.324
	[0.681]		[0.685]**		[0.816]+	[0.740]**
Treat	-1.849	-0.028	-4.16	-0.24		
	[0.559]**		[0.556]**			
Propensity*Post	1.075	0.01	-0.355	-0.006	-0.153	-2.351
	[1.271]		[0.937]		[1.492]	[1.006]*
Propensity	-0.887	-0.008	-2.057	-0.036		
	[1.547]		[1.356]			
Post	1.394	0.017	0.711	0.014	1.027	-0.635
	[0.467]**		[0.325]*		[0.692]	[0.464]
Obs	2586		2569		338	842

Notes: ** denotes significance at the 1% level, * at the 5% level and + at the 10% level. [a] Robust standard errors clustered by household in brackets. Additional controls are a quadratic in age, years of education, indicators for male and province and a constant term. [b] Robust standard errors in brackets. Additional controls are a quadratic in age and individual fixed effects.

of differences in observable characteristics between the treatment and control groups on the coefficient on $Treat * Post$.

The first stage estimates of the propensity to live in public housing in 1993 are shown in the first column of Appendix Table XIII. Table IV reports the self-employment estimates with controls for the propensity to be in the treatment group. The estimates of $Treat * Post$ remain quite similar to the coefficient estimates in the baseline regressions displayed in Table II. The magnitude of the effect of treatment relative to the comparison group of other state employees decreases slightly. The coefficient estimates of $Propensity * Post$ indicate that individuals who are observably similar to treatment individuals but not directly affected by the housing sale are not more likely to enter into self-employment as a result of the reform. In fact, the fixed effects regression in column 6 suggests that privately employed individuals with similar characteristics to the treatment group are significantly less likely to be entrepreneurs. Overall, the impact of the reform on entrepreneurship remains robust to addressing a change in the returns to observable differences between the treatment and the comparison groups.

Sorting into the treatment and control groups is not random, and a potential alternative explanation for the results is that a change in the returns to the time-invariant, *unobservable* characteristics occurred at the same time as the housing reform. For example, an increase in corruption during the period of the housing reform might increase the returns to state connections for an entrepreneur, and state connections
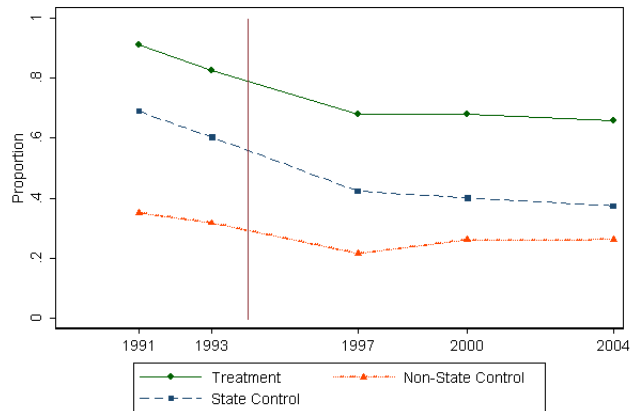
Figure 4: Rates of Unemployment
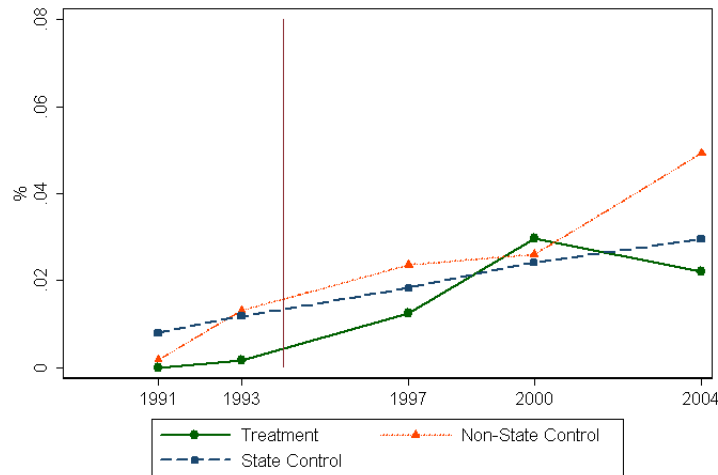
are higher among the treatment group. However, the coefficient estimates of the treatment effect relative to the two control groups are similar in the specifications that include individual fixed effects. For a change in the returns to time-invariant, unobservables to be driving the estimates, a specific relationship in the unobservable characteristics among the treatment and control groups must hold. It would require that the average levels of the unobservable characteristics that influence the decision to enter into self-employment are similar between the two control groups but different from treatment group. This seems unlikely because the state-employed control and the privately-employed control group are significantly different from each other along several characteristics, such as education and household cadres.

5.3.2   Other Changes in the State Sector

The mid-1990s was a time of continued economic growth and the Chinese government introduced numerous policies to reform the socialist system. In this section, I consider alternative explanations for the rise in entrepreneurship. For another explanation to be plausible, it would not only have to occur at the same time as the housing reform, but its impact would have to affect state employees residing in public housing and private employees in a similar manner. Any reforms that affected all state employees or did not affect private employees would not explain the significant, positive effect on entrepreneurship that I find for the treatment group relative to the two control groups.

Privatization of state assets occurred as state-owned enterprises were converted into private companies. The firm-level reforms in the state sector resulted in numerous layoffs as newly privatized companies shed excess labor to achieve profitability. It is plausible that substantial layoffs would increase the rate of self-employment if some individuals preferred self-employment over unemployment. However, the lay-offs that resulted from the reform of state-owned enterprises can only explain the increase in self-employment if state employees in the treatment group were more likely to lose their jobs than state employees in the control group

Figure 5: Health Insurance Coverage

and the break occurred around the same time as the housing reform. Figure 4 shows the unemployment rates of individuals in the CHNS by treatment and control groups from 1991 to 2004.[24] The rates of unemployment among the different groups moved together in general. A spike in the rate of unemployment for the treatment group relative to the state control group occurred between 1997 and 2000, but this was not the same time as the break in home ownership. While the CHNS does not have information about lay-offs (or *xiagang*), aggregate data on lay-offs in state-owned enterprises also indicates that the peak in lay-offs occurred in the years 1997, 1998 and 1999 (see Table 1 in Giles, Park and Zhang, 2005). The share of workers laid off in state-owned enterprises ranged from 5.7 to 7.2% in the years between 1997 and 1999. In 1995, the corresponding figure was 3.2%. The unemployment data in the CHNS and the aggregate data do not support the argument that entrepreneurship increased as a result of lay-offs associated with the privatization of state-owned enterprises.

Other reforms were occurring in the state sector as the central government moved away from a socialist economic system that combined low wages with the provision of in-kind benefits such as housing, health care and child care. The value of health care and child care are quite small in comparison to housing costs, so even if the state monetized or removed health and child care benefits precisely around the time of the housing reform, it is likely that the effect of the housing program would dwarf other changes in benefits. Figure 5 displays the trends in health insurance coverage for the treatment and the control groups across time.[25] The trends indicate that individuals in the treatment group were more likely to have some type of health insurance coverage than either of the control groups. The numbers of individuals with health insurance declined substantially over the early 1990s but this reduction occurred more gradually than the sale of housing units to sitting tenants. Not only does the timing of the decline precede the housing reform, but the trends are also moving in parallel for the three groups. This suggests that changes in health insurance provision in the state sector cannot explain the positive effect of the housing reform on entrepreneurship.

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[24]The CHNS did not ask about unemployment in the 1989 wave of the survey.

[25]I have dropped the data for health insurance in 1989 because the survey question in 1989 was different from the subsequent years.

For the most part, the economic reforms pursued by the Communist government were embodied by Deng Xiaoping's phrase, "*mozhe shitou guo he*" or "crossing the river by feeling for stones." The statement underscores the government's emphasis on a gradual reform process. The trends for unemployment and health insurance show gradual changes over time that were similar for the treatment group and control groups. Thus, other elements of the state reform process do not correspond to the sharp break in home ownership shown in figure 1 and cannot explain the estimated impact of the housing program on entrepreneurship.

6   Impact of Bundling Housing and Employment

6.1   Evidence on Rent Subsidy and Wage Compensation

The relationship between employer-provided housing and job mobility implied by the model depends on the system of housing allocation to state employees in China. The key assumption is that wages in the state sector are not fully adjusted based on the amount of rental subsidy that an individual receives. Wages in the state sector in China are set according to official tables that depend on observable characteristics such as occupation and tenure. Thus, if two individuals with identical characteristics work in the same position at a state-owned enterprise, their wages will be equal regardless of differences in the value of their housing allocations. In this section, I present empirical evidence to support the assumption that the wages of state employees with similar characteristics are not adjusted to offset differences in their housing benefits. First, I examine whether the wages of state employees with public housing are significantly lower than similar state sector workers living in private housing. Second, I analyze whether the amount of rental subsidy is negatively correlated with the wages of state employees conditional on observable characteristics.

The results of the first test are displayed in columns 1 and 2 of Table V. The dependent variable is the logarithm of an individual's wage. The sample is limited to state employees in the pre-reform waves. The inclusion of one-digit occupation fixed effects in column 2 absorbs any effects from differences in occupations of treatment individuals and non-treated state employees. The coefficient estimates of $Treat$ are similar for both specifications and neither is significantly different from zero. Conditional on observable characteristics such as age and education, the average wages of employees in state-owned enterprises are the same regardless of whether they receive a subsidized housing allocation or not. Furthermore, the point estimates are quite small. The average pre-reform rent subsidy was worth 23% of the wages of a state employee, and wage differences of 1.7 and 1.4% implied by columns 1 and 2, respectively, are considerably smaller than the average value of the rental subsidies. I also include the number of household members who are cadres to capture connections in the state sector. The estimates indicate that state connections do not influence wages, and this supports the assumption of the model that μ_i is not highly correlated with ψ_i.

The second test utilizes the actual amount of rent subsidy rather than just a binary division between whether or not a person lives in a state-subsidized home. In the estimates in columns 3 and 4 in Table V, the dependent variable is the logarithm of the rent subsidy received in each year plus one and the sample is

Table V: Wages and Housing Benefits of State Employees in Pre-Reform Years (1989-1993)

	Logarithm of Wages		Logarithm of Rent Subsidy	
	OLS	OLS	OLS	FE
	(1)	(2)	(3)	(4)
Treat	-0.017	-0.014		
	[0.013]	[0.013]		
Monthly Wage			0.066	-0.098
			[0.070]	[0.086]
Household Cadre	-0.002	-0.005	0.211	0.171
	[0.008]	[0.008]	[0.102]*	[0.114]
Occupation FE	No	Yes	No	No
Observations	2628	2581	1355	1355
Adjusted R^2	0.10	0.11	0.32	0.79

Notes: ** denotes significance at the 1% level, * at the 5% level and + at the 10% level. Robust standard errors clustered by household in brackets. The log wage regressions are at the individual level and the log rent subsidy regressions are at the household level. The regressions also include indicators for year and province, a quadratic in age and a constant term. In addition, the OLS regressions in columns 1 and 2 include education and a male indicator, and columns 3 includes the education of the head of house.

limited to pre-reform years. In column 3, the positive and significant impact of cadres in the household on the pre-reform amount of rent subsidy confirms that unobserved connections, μ_i, determine a significant portion of the variation in $\widetilde{h}_i$. While the lack of significance of the coefficient on *Household Cadre* in the estimation with household fixed effects provides some support for the theoretical assumption that people's unobserved connections in the state sector are time-invariant, the standard errors are too large to be conclusive. The coefficients on *Monthly Wage*, which is measured as the logarithm of a household's real monthly wages, are economically small and statistically equivalent to zero.[26] The point estimate with household fixed effects indicates that a doubling of wages corresponds to a decrease in rent subsidies of 9.8%. Overall, the results reinforce the findings of the first test. The correlation between wages and housing benefits are not statistically different from zero and the magnitude of the relationship is too small to support the idea that wages are adjusted to offset differences in the value of the state housing allocation.

6.2 Mobility

As the theoretical model suggests, we can determine whether the unbundling of housing from employment decisions contributed to the increase in entrepreneurship by testing whether the reform resulted in an increase in general mobility out of the state sector. In other words, the hypothesis on unbundling housing provision from employment can explain an increase in movement from state-owned enterprises towards private wage

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[26]Measuring the wages and rent subsidies in levels, which relaxes of the assumptions associated with a log-log regression, produce similar results.

jobs, but the credit constraints story cannot. In addition, the effects of bundling housing with employment must work at the individual level while loosening credit constraints can affect other household members who are not working for the state sector. The previous analyses of the impact of the reform have defined treatment at the couple level; thus, an individual residing in state-owned housing who was not employed in the state sector but married to a state employee has been categorized in the treatment group. For the tests of the hypothesis of the mobility effects of unbundling housing from employment, I separate individuals living in state-owned housing before the reform and not employed in the state sector from the treatment group. This separation provides an additional test for whether the impact of the reform on labor mobility can be attributed to the unbundling of housing from state employment.

In addition to estimating equation 14, I also estimate the following fixed effects logit equation

$$Pr(y_{it} = 1) = g(\alpha_1 TreatInd_i * Post_t + \alpha_2 Post_t + \alpha_3 TreatSpouse_i * Post_t + \alpha_6 \mathbf{x_{it}} + \gamma_i + \epsilon_{it}) \qquad (17)$$

where the dependent variable is an indicator that takes a value of 1 if the individual is employed in a private wage job and zero otherwise. The sample includes the treatment group and the control group of state employees in private housing before the reform. The variable $TreatInd$ equals one for state employees who are living in state-owned housing in 1993. $TreatSpouse$ is an indicator that takes on a value of one for residents of public housing who are not state employed; these individuals are the spouses of workers who received housing allocations from their employers, and are excluded from $TreatInd$. A positive estimate of the coefficient on $TreatInd * Post$ supports the hypothesis that the pre-reform system of employer-provided housing reduced job mobility. Furthermore, the mobility of spouses of individuals who received housing from their state employers should not be affected by the reform so coefficient estimates on $TreatSpouse * Post$ equal to zero would provide further support for the validity of the bundling hypothesis.

The model predicted that individuals with more generous rent subsidies before the reform should also have higher average abilities in as entrepreneurs (θ_i) and private wage employees (ζ_i). I estimate

$$Pr(y_{it} = 1) = g(\alpha_1 TreatInd_i * Post_t + \alpha_2 Post_t + \alpha_3 TreatInd_i * Post_t * \widetilde{h}_i + \alpha_4 TreatSpouse_i * Post_t$$
$$+ \alpha_5 TreatSpouse_i * Post_t * \widetilde{h}_i + \alpha_6 \mathbf{x_{it}} + \gamma_i + \epsilon_{it}) \ (18)$$

where $\widetilde{h}_i$ is measured as the logarithm of the average amount of rent subsidy that an individual received in three periods before the reform. More specifically, the rent subsidy is calculated as the difference between self-reported market value and the rent paid for a state-provided housing unit. A positive estimate of α_3 and a zero estimate of α_5 provide additional evidence that the reform increased job mobility among state sector workers.

The results are presented in Table VI. The coefficient estimates on $TreatInd * Post$ in all regressions show that transitions into private wage employment by state employees in public housing in 1993 increased significantly after the reform. The estimates of equations 14 and 17 in columns 1 and 2, respectively, show

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Table VI: Fixed Effects Logit Estimates of Private Employment

	(1)	(2)	(3)
TreatInd*Post	1.463	2.016	0.365
	[0.287]**	[0.329]**	[0.566]
TreatSpouse*Post		-0.955	-1.461
		[0.655]	[1.600]
TreatInd*Post*$\widetilde{h}$			0.570
			[0.159]**
TreatSpouse*Post*$\widetilde{h}$			0.176
			[0.488]
Post	0.954	0.884	0.831
	[0.333]**	[0.335]**	[0.339]*
Observations	1131	1121	1105

Notes: Coefficients shown. ** denotes significant at the 1% level, * at the 5% level and + at the 10% level. Standard errors in brackets. Regressions also include a quadratic in age.

that the log odds of switching out of the state sector was 1.46 to 2.02 times higher for the treatment group than the state-employed comparison group. The estimates of equation 18 in column 3 indicate that a doubling of the average pre-reform rent subsidy corresponded with a 0.57 times increase in log odds of leaving the state sector for private employment. The coefficients on $TreatSpouse * Post$ indicate the mobility of individuals residing in public housing allocated to their spouses in 1993 was not statistically different from individuals in the control group. Finally, the coefficient estimates of $TreatSpouse * Post * \widetilde{h}$ are not significantly different from zero. The results suggest that an increase in general job mobility associated with unbundling housing from state employment can explain at least part of the increase in transitions into entrepreneurship for the treatment group following the reform.

6.3 Wage Growth

Patterns in wage growth for individuals who change sectors provide a second test of the relevance of bundling housing with state employment in explaining the effect of the housing reform on entrepreneurship. The model implies that the wage growth for individuals who leave the state sector should be higher before the reform than after the reform because individuals switching before 1994 needed to be compensated for the loss of housing benefits. This suggests examining the following first-difference equation over a selected sample of individuals who leave the state sector for a private job:

$$w_{it}^p - w_{i,t-1}^s = \beta_0 + \beta_1(PH89_i * Post_t) + \beta_4(x_{it} - x_{i,t-1}) + \tau_t + v_{it} \qquad (19)$$

Table VII: First-Difference Estimates of Wage Growth of Former State Employees

	Logarithm Specification		Level Specification	
	(1)	(2)	(3)	(4)
PH89*Post	-0.160	-0.158	-0.168	-4.70
	[0.057]**	[0.060]**	[44.83]	[46.61]
PH89*Post*$\widetilde{h}$			-0.16	-1.22
			[0.368]	[1.028]
Propensity$_{89}$*Post		0.046		-64.46
		[0.151]		[120.46]
Propensity$_{89}$*Post*$\widetilde{h}$				1.922
				[1.611]
Observations	231	225	229	223
Adjusted R^2	0.21	0.20	0.20	0.19

Notes: ** denotes significance at the 1% level, * at the 5% level and + at the 10% level. Robust standard errors clustered by household in brackets. Regressions also control for a first-difference quadratic in age, a constant term and year dummies. Sample includes heads and spouses who have left the state sector.

where w_{it}^p is the monthly real earnings of the individual who is no longer in the state sector in period t, and $w_{i,t-1}^s$ is the monthly real wage in the state sector in period $t-1$. The specification also includes year dummies, denoted by τ_t, rather than just a post-reform indicator. This flexible specification for time captures differences in the intervals between surveys which vary from two to four years. The variable, $PH89_i$, is an indicator that takes on a value of 1 if individual i resides in public housing in 1989. I use this measure of treatment defined by public housing residence in 1989 rather than 1993 because the latter definition would preclude switching in the pre-reform period. If bundling reduced mobility before the reform and was eliminated or reduced by the reform, then we would expect $\beta_1 < 0$. I also examine heterogeneity in the impact of the reform by the pre-reform levels of housing subsidy with the following regression:

$$w_{it}^p - w_{i,t-1}^s = \beta_0 + \beta_1(PH89_i * Post_t) + \beta_2(PH89_i * Post * \widetilde{h}) + \beta_4(x_{it} - x_{i,t-1}) + \tau_t + \upsilon_{it} \qquad (20)$$

A negative coefficient on β_2 suggests that state provision of housing reduced job mobility.

I estimate equation 19 with w_{it}^p, $w_{i,t-1}^s$, and $\widetilde{h}$ in levels. While the distributions of wages and rent subsidy appear to be suited for such a specification where they are both measured in logs, the estimate of the triple interaction, $PH89 * Post * \widetilde{h}$, requires a strong assumption on the relationship between the dependent variable and $\widetilde{h}$. In this case, it is difficult to believe that a doubling of $\widetilde{h}$ at 10 RMB per month has the same percentage effect on wages as a doubling of $\widetilde{h}$ at 100 RMB per month. In the levels specification, we would expect that an increase in $\widetilde{h}$ corresponds with the same amount of wage gain for an individual leaving the state sector. In other words, the model predicts a coefficient on $PH89 * Post * \widetilde{h}$ of -1.

I also control for the differential trends in the returns of characteristics of individuals in $PH89$ and state employees not living in public housing in 1989. Following section 6.3, I estimate the propensity to be in public housing in 1989. The results of this step are shown in column 2 of Appendix Table XIII. I include the propensity score and its interactions with $Post$ and $Post * \widetilde{h}$ to control for differences in observable characteristics between those in public housing and those in private housing.

Columns 1 and 2 of Table VII correspond with equation 19, and columns 3 and 4 with equation 20. Columns 1 and 2 imply that public housing residents leaving the state sector needed 16% higher wages to compensate them for the loss of housing subsidies before the reform than after the reform. In the CHNS, the reported value of the rental subsidy was approximately 23% of an individual's total wages in the pre-reform period, so the coefficient estimates correspond fairly well with the idea that the value of housing benefits that state workers received influenced their job tenure decisions. In the specifications that include $PH89 * Post * \widetilde{h}$ in columns 3 and 4, the coefficient estimates on the triple interaction are negative, but they are not statistically different from zero at the 10% level. In column 4, the point estimate of the triple interaction is close the predicted value of -1.

The empirical evidence supports the hypothesis that the system of housing provision by state work-units reduced mobility before the reform, and that the housing reform removed this barrier to mobility. Both tests give consistent support for the argument that the reduction in housing benefits associated with state jobs can explain at least part of the increase in self-employment following the housing reform.

7   Testing Credit Constraints

7.1   Appreciation in Housing Prices

The model suggested that a positive relationship between capital investment or entry into entrepreneurship and post-reform gains in housing wealth would provide evidence that individuals were accessing the value associated with their state-owned housing to alleviate credit constraints. I use heterogeneity in housing price appreciation across provinces and time to test for credit constraints. The data on housing price are discussed in Appendix D. Changes in housing prices after the reform determine the housing wealth realized by the new home owners.

Hurst and Lusardi (2004) used fluctuations in housing equity as exogenous changes in wealth to analyze the impact of credit constraints on entrepreneurship. Using housing price appreciation to instrument for wealth, they found that wealth had no significant effect on the probability of becoming an entrepreneur in the U.S. In contrast, later work by Fairlie and Krashinsky (2006) did find that appreciation in housing prices predicted entrepreneurship in the U.S. They included housing appreciation in reduced form estimates of entry into self-employment and used a finer geographic unit of analysis for housing price averages than the nine Census regions used by Hurst and Lusardi. Similar to Fairlie and Krashinsky, I include housing price changes directly in my estimation rather than using an instrumental variables approach because the

Table VIII: Impact of Housing Price Appreciation on Self-Employment and Capital

	FE Logit		FE Logit		Fixed Effects	
	Self-Employment		Positive Capital		Log(Capital)	
	Control=State	Private	State	Private	State	Private
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Post*Δq	2.989	3.877	6.059	6.48	6.767	8.285
	[2.063]	[1.384]**	[1.488]**	[1.775]**	[1.688]**	[1.438]**
Treat*Post	1.555	2.23	0.859	0.911	-0.45	0.498
	[0.774]*	[0.895]*	[0.414]*	[0.562]	[0.512]	[0.244]+
Post	0.558	-1.54	-1.816	-1.787	1.261	0.171
	[0.694]	[0.960]	[0.299]**	[0.297]**	[0.551]+	[0.297]
Post*Δq	-0.238	0.524	-0.95	-1.319	1.66	-0.748
	[1.264]	[0.972]	[1.103]	[1.414]	[0.532]*	[0.805]
Observations	389	893	871	1224	330	555

Notes: ** denotes significant at the 1% level, * at the 5% level and + at the 10% level. Standard errors clustered by province in brackets. The self-employment regressions are at the individual level, and the capital regressions at the household level. Additional controls are a quadratic in age and fixed effects. The capital regressions also include a constant term.

CHNS does not ask about total wealth.

I estimate the following equation:

$$y_{ijt} = g(\alpha_1 Treat_i * Post_t + \alpha_2 Treat_i * Post_t * \Delta q_{jt} + \alpha_3 Post_t * \Delta q_{jt} + \alpha_4 Post_t + \alpha_5 x_{ijt} + \gamma_i + \epsilon_{ijt}) \quad (21)$$

where Δq_{jt} represents changes in housing prices in province j and time t.[27] The estimate of α_2 is identified by differences in housing price appreciation across provinces and across time. Because the alleviation of credit constraints can affect other members of a household, the analysis in this section defines $Treat$ at the couple level. I examine two dependent variables: self-employment and capital. When y_{ijt} is the self-employment status of individual i or whether individual i owns productive assets or not, I estimate the equation with a fixed effects logit. When y_{ijt} is the logarithm of the amount of productive assets owned by household i, a fixed effects linear regression is estimated. For both dependent variables, a positive estimate of α_2 supports the hypothesis that the reform increased entrepreneurship by relaxing capital constraints.

Table VIII present the results of equation 21 with self-employment and capital as dependent variables. Columns 1 and 2 suggest that a one standard deviation increase in housing price appreciation (13%) following the reform increases the relative log odds of the treatment group entering self-employment by 0.39 and 0.50

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[27]I do not include $Treat_i * \Delta q_{jt}$, which would reflect the the impact of changes in housing prices for the treatment group before the reform. First, it is not possible because the data series for q_{jt} begin in 1993. Second, the theoretical model suggests that housing prices should not affect the treatment group before the reform because by definition treated individuals do not own a home before the reform.

times, respectively. However, the estimate of the triple interaction, $Treat * Post * \Delta q$, is only significantly different from zero for the sample that includes private sector workers. Appreciation in housing prices may be correlated with changes in other conditions across provinces, so the estimate of α_2 may reflect changes other than appreciation in home values. For example, an inflow of workers to an area due to local labor demand will lead to an increase in housing prices and affect individuals' decisions regarding entrepreneurship in ways other than through the value of their homes. I attempt to deal with this issue by including changes in average wages and consumer prices (as well as their interactions with $Treat * Post$) to capture changes in local economic conditions. The additional control variables do not have a large effect on the estimates of the coefficients of interest.[28]

The estimates of equation 21 with capital as the dependent variable provide stronger evidence that appreciation in home values contributes to the effect of the reform on entrepreneurship. The coefficients on the triple interaction in columns 3 through 6 in Table VIII are all significant and positive. For existing capital owners, a one-standard deviation increase in housing prices corresponds with an increase in capital for treated households of over 100% above the increase in capital for similar households living in areas of stagnant house prices. The fixed effects logit estimates of positive capital ownership confirm that the reform had a larger effect on capital in areas where house prices experienced greater growth. The coefficients on $Treat * Post * \Delta q$ in columns 3 and 4 indicate that a one-standard deviation increase in house prices led to an increase in the relative log odds of a transition from zero capital to positive capital ownership by approximately 0.8 times.

7.2 Private Property Rights and Access to Capital of Entrepreneurs

Another test for credit market failures involves an examination of the impact of the housing reform on capital invested into small business enterprises. In an environment with credit constraints, we would expect the post-reform ability to capitalize on housing to increase asset investment in household businesses. However, if the unbundling of housing from state employment increased entry into entrepreneurship, then the housing reform could increase capital even in the absence of credit constraints. To isolate the relevance of credit constraints on entrepreneurship and avoid the confounding influence of unbundling benefits from employment, I consider a sample limited to households that either operated a small business or had a self-employed member in 1993. In the limited sample, a positive effect of the reform on the amount of capital owned by households provides evidence that the reform's alleviation of credit constraints is independent from unbundling employment and housing.

The results are presented in Table IX. Columns 1 and 2 display the estimates for the full sample, and columns 3 and 4 correspond with the limited sample of households involved in a small business in 1993. In columns 1 and 2, the dependent variable is the logarithm of the household's total market value of the

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[28]Results available from the author.

Table IX: Impact of Reform on Capital

	Full Sample of Households		1993 Business Households	
	Control=State	Control=Private	Control=State	Control=Private
	(1)	(2)	(3)	(4)
Treat*Post	1.09	1.47	1.87	1.52
	[0.136]**	[0.143]**	[0.601]**	[0.640]*
Post	-1.17	-1.53	-2.51	-2.10
	[0.111]**	[0.111]**	[0.326]**	[0.247]**
Obs	2942	3551	396	995

Notes: Standard errors in brackets. ** denotes significant at the 1% level, * at the 5% level and + at the 10% level. Capital is measured at the household level, and these regressions also include household fixed effects, a quadratic in the age of the head, and a constant term.

professional and productive equipment that they own plus one.[29] The coefficient estimates of $Treat * Post$ for the full sample and the limited sample are all significant and with somewhat larger estimates for 1993 business households. For the full sample, the coefficients indicate that the reform increased treated households' ownership of productive assets by over 300% relative to privately-employed households and by around 200% relative to state-employed households in privately-owned housing. The corresponding estimates for households already engaged in small business activities are 357% and 549%, in columns 3 and 4, respectively. They indicate that the increase in capital is not merely driven by entry into entrepreneurship. The magnitude of the impact of the housing reform on capital is quite large, but this is not surprising given the low initial levels of capital investment and the large difference between the market value and the government price charged on the homes. While there is a positive relative effect of the reform on the value of capital owned by the treatment group, the coefficient on $Post$ is negative and significant. This suggests that there was a trend towards decreasing investment in small businesses among non-treatment households.

The empirical evidence suggests that the impact of the housing reform on increasing entrepreneurship can be attributed to both an alleviation in credit constraints and the unbundling of housing from state employment. Comparing the impact of the reform on capital for the full sample and the sample limited yields a lower-bound estimate of the contribution of the relaxation of credit constraints on the increase in capital investment by the treatment group. The total effect of the reform on capital owned by treated households is given by the coefficients on $Treat * Post$ in columns 1 and 2 in Table IX. The lower-bound contribution of the relaxation of credit constraints can be measured with the corresponding coefficients in columns 3 and 4. Households that were residing in public housing and engaged in a business in 1993 make up less than ten percent of treatment hounseholds, and their average level of capital holdings in 1993 is much higher at 276 RMB. These numbers combined with the average capital owned by all treatment households shown in Table I suggest that at least 58% of the total increase in capital of the treatment group can be

[29]Other specifications that demonstrate that the results are not driven by a mass of zero values are available from the author.

attributed to the relaxation of credit constraints associated with the housing reform. The evidence suggests that the alleviation of credit constraints played a larger role in the expansion of investment in business enterprises than the unbundling of housing provision from state employment.

8   Conclusion

This paper presents new evidence on the economic implications of employer-provided housing in the state sector. My results suggest that the participation of state employers in the housing market introduced distortions to individuals' labor market choices. By deterring job mobility and the private accumulation of property, the pre-reform system of in-kind housing benefits created inefficiencies in the labor market. The empirical evidence indicates that the sale of state-owned housing encouraged entrepreneurship among individuals who received the opportunity to purchase the homes that they had been renting from their state employers. This paper provides a framework for understanding the two mechanisms through which the privatization of state housing assets can increase entrepreneurship and aggregate productivity. One explanation is that the conversion of state assets to private property allowed individuals to capitalize on the value associated with their real estate and hence relieved credit constraints. The other explanation highlights the potential impact of unbundling housing provision from employment on job mobility. While it is impossible to use the reform to examine either credit constraints or the bundling of housing and employment in isolation, the model yielded predictions to test the relevance of each argument.

The empirical results support the idea that both mechanisms played a role in the positive impact of the reform on entrepreneurship among the former tenants of state housing. This work indicates that credit markets fail to provide efficient levels of capital for small businesses in China, and this affects both entry into entrepreneurship and the accumulation of productive assets. While individuals respond to the total compensation package and make optimal choices, the bundling of housing benefits with employment in the state sector resulted in matches between workers and employers that were not the most productive from the perspective of a social planner. Furthermore, a system that allows state bureaucrats to allocate housing units among state sector workers generates the potential for corruption. This work has policy relevance for other developing countries that continue to provide subsidized state-owned housing for workers in civil service and in state-owned enterprises. Policy makers have the opportunity to encourage small businesses by promoting private ownership of assets and by increasing small business lending in the formal sector.

The program represented a large shift in the ideology of the government regarding private property rights as it encouraged private ownership of homes. It succeeded in turning China into a country with one of the highest rates of home ownership in the world. I have argued that this reform allowed constrained individuals with entrepreneurial ability the opportunity to begin business ventures and increased labor mobility out of the sizable state sector. However, an understanding of the returns to privatizing public housing assets in other countries requires more analysis on the legal and financial institutions that are

necessary for such a housing program to have an effect. It is also important for policy to understand the implications of the housing reform on the distribution of wealth. The potential general equilibrium effects of the dramatic increase in the supply of private housing is a topic for future research.

YALE UNIVERSITY

A   Model of Housing Preferences

To examine the role that housing preferences would play in the model, I specify each individual's utility to be a Cobb-Douglas function with weights η_i and π_i on consumption and housing, respectively:

$$U_i(C_{ij}, h_{ij}) = \eta_i \ln C_{ij} + \pi_i \ln h_{ij} \tag{22}$$

The budget constraints remain the same as specified in equations 2 - 5. In the private sector, the optimal value of consumption is given by

$$C_i^* = \frac{Y_{ip} + rW_i}{1 + \frac{\pi_i}{\eta_i}} \tag{23}$$

and the optimal value of housing is given by

$$h_i^* = \frac{\pi_i}{\eta_i + \pi_i} \frac{Y_{ip} + rW_i}{q}. \tag{24}$$

This yields the total value of private employment as

$$V_{ip} = \eta_i \ln\left[\frac{\eta_i}{\eta_i + \pi_i}(Y_{ip} + rW_i)\right] + \pi_i \ln\left(\frac{\pi_i}{\eta_i + \pi_i}\frac{Y_{ip} + rW_i}{q}\right). \tag{25}$$

When heterogeneity in housing outcomes is determined by connections in the state sector, μ_i, a simple sorting rule results where the probability of an individual choosing the state sector increases with μ_i. It is unclear how variation in preferences for housing will affect choices regarding sector of employment. In order to answer this question, I examine how a change in housing preferences affects the utility in each sector. By the envelope theorem, the derivative of V_{ip} with respect to π_i is

$$\frac{\partial V_{ip}}{\partial \pi_i} = \ln h_{ip}^*. \tag{26}$$

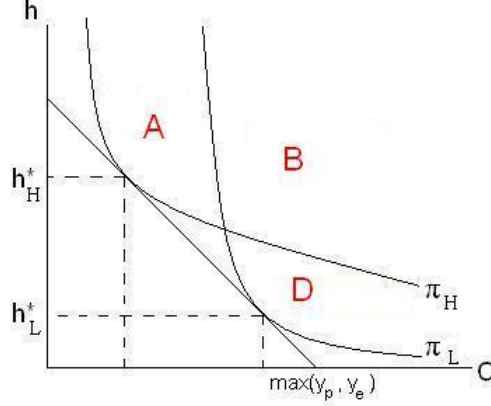Similarly, for an entrepreneur

$$\frac{\partial V_{ie}}{\partial \pi_i} = \ln \overline{h}_{ie}. \tag{27}$$

Finally, in the state sector, the total value of employment is

$$V_{is} = \eta_i \ln(Y_{is} + rW_i) + \pi_i \ln \widetilde{h}_i \tag{28}$$

Figure 6: Employment Choices Varying by Housing Preferences

and the derivative of V_{is} with respect to π_i is

$$\frac{\partial V_{is}}{\partial \pi_i} = \ln \widetilde{h}_i. \tag{29}$$

These derivatives suggest that sector choice is not monotonically related to housing preferences as it was to state connections (μ_i) in Section 3. How an increase in an individual's preference for housing affects the relative utility from each of the three sectors depends on the relative magnitude of the level of housing that the individual would get in the state sector ($\widetilde{h}$) and the level they could afford in the private sector (h^*). Figure 6 delineates this result. The figure shows two individuals who are identical in their compensation in each sector and their initial wealth. They only differ in terms of their preferences for housing. One person has a high (H) preference for housing while the other has a low (L) preference. Outside of the state sector, they face the same budget constraint shown in the figure, but they would allocate their budget differently across the two goods. As an entrepreneur or a private wage employee, person H will choose an larger amount of housing than person L in the private housing market. Both types will enter the state sector if their state compensation falls in the area B. However, their sector locations differ in areas A and D. In area A, only the type with high preference for housing chooses the state sector. In area D, the high type chooses non-state employment while the low type prefers state employment.

This section demonstrates that allowing for heterogeneity in housing preferences will lead to different patterns of sorting into sectors of employment than the model where all individuals have the same preferences. If we assume that preferences for housing are not correlated with entrepreneurial ability, then the sorting process implied by this model will not lead to biased estimates of the impact of the reform on entry into entrepreneurship or capital accumulation. The estimates regarding job mobility and the impact of pre-reform levels of rent subsidy require the additional assumption that housing preferences are not correlated with individuals' unobserved abilities in the private wage sector, ζ_i. The independence of housing preferences with entrepreneurial ability and unobserved ability as a private wage employee seem quite plausible.

B Boundary of V_e and V_s

To see that equation 12 yields a boundary that is concave in $(\widetilde{h}, \theta)$ space, we can use the implicit function theorem on equation 12. In the case of a constrained entrepreneur,

$$\frac{\partial \theta}{\partial \widetilde{h}} = \frac{U_h^s}{U_c^e[(\theta f'(k^{cc}) - r)\frac{\partial k}{\partial \theta} + f(k^{cc})] - (qU_c^e - U_h^e)\frac{\partial \bar{h}}{\partial \theta}} \tag{30}$$

We know that for constrained entrepreneurs, k is equal to their initial wealth and the amount that they can borrow. A individual's utility is increasing in both housing ($U_h > 0$)and non-housing consumption ($U_c > 0$). From equation 10, we know that $-q(1 + r\lambda)U_c + U_h = 0$ for the constrained entrepreneur, so $qU_c - U_h < 0$. Furthermore, for a constrained entrepreneur, $\theta f'(k^{cc}) > r$. Thus, we have $\frac{\partial \theta}{\partial \widetilde{h}}$ is positive.

For an unconstrained entrepreneur,

$$\frac{\partial \theta}{\partial \widetilde{h}} = \frac{U_h^s}{U_c^e f(k^*)} \tag{31}$$

and the slope of the curve given by 31 is also positive. While figure 3 shows the boundary with a relaxation of credit constraints as fairly parallel to the credit constrained boundary, this does not have to be the case. The relative magnitude of the slopes defined by equations 30 and 31 depend on the difference in output that results from accessing more capital $(f(k^*) - f(k^{cc}))$ and degree of the distortions in the credit constrained case, which are captured in the size of $(\theta f'(k^{cc}) - r)\frac{\partial k}{\partial \theta}$ and $(qU_1^e - U_2^e)\frac{\partial \bar{h}}{\partial \theta}$.

The second derivative of equation 12 for the constrained entrepreneur is

$$\frac{\partial^2 \theta}{\partial \widetilde{h}^2} = \frac{U_{22}^s}{U_1^e f(k^{cc}) + \frac{\partial \bar{h}}{\partial \theta}(qU_c^e - U_h^e)} \tag{32}$$

and for the unconstrained entrepreneur, it is

$$\frac{\partial^2 \theta}{\partial \widetilde{h}^2} = \frac{U_{hh}^s}{U_c^e f(k^*)}. \tag{33}$$

For both cases, the sign of $\partial^2 \theta / \partial \widetilde{h}^2$ is negative because $U_{hh} < 0$. Thus, the boundary given by equation 12 is concave as shown in Figures 3-3.

To see that the threshold level of θ^+ falls as credit constraints are alleviated, as shown in Figure 3, recall that θ^+ is the level of θ_i that solves equation 11. For a given level of θ_i, we know that $\theta_i f(k^*) - rk^* > \theta_i f(k^{cc}) - rk^{cc}$ because $k^* > k^{cc}$. For equation 11 to hold for the same earnings in the private sector, Y_{ip}, the threshold level of θ_i must fall as $k^{cc} \to k^*$.

Appendix Table X: The Impact of the Reform on Home Ownership

	Logit: Home Ownership[a]				FE Logit: Home Ownership[b]	
	Control=State Workers		Control=Private Workers		Control=State	Control=Private
	Coefficient	*Marginal*	Coefficient	*Marginal*	Coefficient	Coefficient
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Post	3.946	*0.687*	5.051	*0.633*	5.313	6.204
	[0.452]**		[0.415]**		[0.666]**	[0.630]**
Post	0.403	*0.100*	-0.666	*-0.158*	-0.464	-1.337
	[0.378]		[0.322]*		[0.546]	[0.473]**
Treat	-5.807	*-0.871*	-6.882	*-0.916*		
	[0.359]**		[0.448]**			
Obs	2819		2821		1317	1350

Notes: ** denotes significance at the 1% level, * at the 5% level, + at the 10% level.
[a] Robust standard errors clustered by household in brackets. Additional controls are a quadratic in age, years of education, indicators for male and province and a constant term.
[b] Robust standard errors in brackets. Additional controls are a quadratic in age and individual fixed effects.

C   Impact of the Reform on Home Ownership

Figure 1 demonstrates that the trends in home ownership were fairly parallel for the two groups before and after the state sale of housing.[30] Columns 1 and 3 of Table X present estimates of equation 13 where the dependent variable is an indicator for home ownership. Columns 2 and 4 display the corresponding average marginal effect. The two comparison groups yield similar estimates. The results indicate that the reform increased the treatment group's probability of home ownership by over 60 percentage points.

Columns 5 and 6 display coefficients of fixed effects logit estimates of equation 14 where the dependent variable is home ownership. The sample sizes for the fixed effects logit regressions are smaller because they only include individuals who transition into home ownership at some point during the sample. The housing program increased the log odds of home ownership for the treatment group relative to the control group by approximately 5 to 6 times. Overall, the results in Table X confirm that the housing program was successful in increasing the home ownership rate among public housing tenants.

D   Supplementary Data Sources

Data on housing prices that begin as early as the mid-1990s are not common. To my knowledge, the only aggregate series that extend as far back as 1993 are provincial statistics on the value and the floor space of residential homes sold in the China Statistical Yearbooks, published by the National Bureau of Statistics.

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[30]A breakdown by the particular definitions of the treatment group, the state control group and the private control group provides similar support for the parallel trends assumption.

Because my focus is on urban areas, I use the price series covering cities, towns and industrial and mining areas rather than entire provinces. The average price per square meter of residential space is constructed as the total value divided by the floor space sold from 1993 to 2000.[31] Other province-level data used as controls, such as average wages and the urban consumer price index, are also from the China Statistical Yearbooks. The average province-level wages and housing prices are converted into real 1990 RMB using the GDP deflator for mainland China provided by the United Nations. The appreciation in the price of residential housing is constructed as the percent change in the real average price per square meter since the last survey.

E Robustness Checks

E.1 Sample Attrition

While attrition in my sample of analysis is low in the first two waves following the launch of the longitudinal survey in 1989 (averaging less than 1% each year), it increases substantially in the last three waves of the survey. Approximately 14% of the sample attrites between 1993 and 1997. Furthermore, the rates of attrition differ across the treatment and control groups with the treatment group being more likely to leave in each wave following the reform. In the 1997 round, 18% of individuals in state-owned housing in 1993 had attrited compared with 7% of individuals privately employed in 1993 and 16% of state employees without state housing in the previous sample period. This is not surprising because the theoretical framework predicts an increase in mobility for the treated relative to the untreated group following the reform. However, sample attrition can bias the estimates of the impact of the housing reform on entrepreneurship.

I address the potential bias from sample attrition by using the inverse probability weighting (IPW) method. The key assumption underlying this method is that attrition is based on observable characteristics.[32] The IPW method places more weight on individuals who are likely to attrite in order to create a sample that better reflects the original full sample. The procedure involves two steps. First, for every wave following 1989, I estimate the probability that each individual remains in the sample conditional on their presence in the previous samples. In other words, for each wave following the first, I estimate a logit regression where the dependent variable is whether the individual remained in the survey. The estimates are shown in the Appendix Table XIV. However, the predicted *conditional* probabilities are not representative of the original sample population. Wooldridge (2002a) shows that the inverse of the joint probability can be used as weights in the second step of the estimation with the joint probability calculated directly from the conditional probabilities.

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[31]Urban housing price data in 2004 are excluded because of clear evidence of at least one error in the published data in that year. There is a dramatic drop in the value of residential sales in urban areas for at least one province (Liaoning) by a factor of one hundred, but no corresponding drop for the same series in the flanking years, the area of residential homes sold in urban areas in 2004, province-wide housing prices, or the prices of commercial buildings.

[32]This assumption is often called ignorability of selection. See Wooldridge (2002) or Wooldridge (2002a) for more details on inverse probability weighting.

Appendix Table XI: Estimates of Self-Employment: Robustness Checks

	Logit: Self-Employment[a]				FE Logit: Self-Employment[b]	
	Control=State Workers		Control=Private Workers		Control=State	Control=Private
	Coefficient	Marginal	Coefficient	Marginal	Coefficient	Coefficient
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Estimates with Attrition Correction (Inverse Probability Weighting)						
Treat*Post	1.066	*0.019*	1.972	*0.080*	1.937	1.895
	[0.543]+		[0.503]*		[0.592]**	[0.660]**
Post	1.627	*0.028*	0.452	*0.012*	0.427	-1.43
	[0.274]*		[0.174]*		[0.452]	[0.337]**
Treat	-2.007	*-0.048*	-4.196	*-0.307*		
	[0.497]*		[0.474]*			
Obs	3082		3129		444	1020
Panel B: Two-Period Collapsed Estimates (Serial Correlation Correction)						
Treat*Post	1.237	*0.041*	2.456	*0.167*	1.375	1.835
	[1.111]		[1.070]+		[0.694]+	[0.656]*
Post	1.463	*0.039*	0.317	*0.014*	-0.115	-0.536
	[0.448]*		[0.284]		[1.567]	[1.287]
Treat	-2.721	*-0.141*	-4.842	*-0.488*		
	[1.045]*		[0.978]*			
Obs	953		1014		160	240

Notes: ** denotes significance at the 1% level, * at the 5% level and + at the 10% level. [a] Robust standard errors clustered by household in brackets. Additional controls are a quadratic in age, years of education, indicators for male and province and a constant term. [b] Robust standard errors in brackets. Additional controls are a quadratic in age and individual fixed effects.

Panel A of Table XI displays the results using inverse probability weighting to correct for sample attrition. Comparing these results to the ones shown in Table II, the attrition-corrected estimates of $Treat * Post$ imply a larger effect of the housing sale on self-employment for all of the estimates except one. Thus, the results indicate that sample attrition based on observable characteristics tends to bias the results downward. These results depend on the assumption that conditional on the variables in the attrition equation, attrition is ignorable with respect to self-employment.

The problem is more difficult if sample attrition is driven by unobservable characteristics. Heckman's two-step estimator provides a method for handling attrition based on unobservables, but the procedure relies on finding an exclusion restriction in the selection equation. Unfortunately, the data set does not provide a variable that would plausibly predict attrition but not job mobility and other labor market choices in this context. In the absence of an empirical method for addressing attrition based on unobservables, I suggest another approach to thinking about this problem. Preference for risk is a likely candidate for

an unobservable trait that affects both moving out of the survey area and entry into entrepreneurship. If individuals with greater preferences for risk are more likely to become entrepreneurs and more likely to move far enough to leave the sample area, then the bias stemming from attrition based on risk preferences will be downward. Attrition based on unobservable characteristics is likely to have a similar impact as attrition based on observables in working against finding a significant effect of the reform on entrepreneurship.

E.2 Serial Correlation

Bertrand, Duflo and Mullainathan (2004) find that differences-in-differences estimates that use several years of data may lead to substantial underestimates of the standard errors if serial correlation in the outcomes is not addressed. Following their approach, I collapse the six waves of data into two periods, pre-reform and post-reform. The results for self-employment are shown in Panel B of Table XI. The average marginal effects implied by the logit estimates suggest that the program increased self-employment for the treatment group by 4 percentage points relative to the state control group and by 16 percentage points relative to the privately-employed control group. These estimates are approximately twice as large as the average marginal effects estimated in the baseline regressions in Table II. The coefficient estimates with inclusion of individual fixed effects in columns 5 and 6 are similar in magnitude and significance to the corresponding estimates in the baselines regressions.

F Appendix Tables

Appendix Table XII: Fixed Effects Logit Estimates of Residential Move of Household

	Control = State	Control = Private
	(1)	(2)
Treat*Post	1.37	1.02
	[0.41]*	[0.33]*
Post	0.11	0.38
	[0.33]	[0.24]
Observations	563	837

Notes: Coefficients shown. ** denotes significant at the 1% level, * at the 5% level and + at the 10% level. Standard errors in brackets. Regressions also include a quadratic in age.

Appendix Table XIII: Estimates of the Propensity Scores

	Treatment (1993)	Public Housing in 1989
	(1)	(2)
Age	0.122	0.122
	[0.060]*	[0.036]**
Age^2	-0.001	-0.001
	[0.001]	[0.000]**
Years of Education	0.273	0.073
	[0.021]**	[0.016]**
Male	-0.370	-0-.409
	[0.136]**	[0.116]**
Household Cadre	0.392	0.331
	[0.126]**	[0.097]**
Observations	1372	1603

Notes: Coefficients of logit estimates shown. ** denotes significance at the 1% level, * at the 5% level, + at the 10% level. Standard errors in brackets. Additional controls are province indicators and a constant term.

Appendix Table XIV: Logit Estimates of Attrition

	Pr(Stay 1991)	Pr(Stay 1993)	Pr(Stay 1997)	Pr(Stay 2000)	Pr(Stay 2004)
	(1)	(2)	(3)	(4)	(5)
Treat	0.000	0.002	-0.053	-0.043	-0.051
	[0.002]	[0.004]	[0.022]*	[0.023]*	[0.035]
Age	0.000	-0.001	0.015	-0.026	0.018
	[0.001]	[0.002]	[0.009]+	[0.012]**	[0.018]
Age^2	0.000	0.000	0.000	0.000	0.000
	[0.000]	[0.000]	[0.000]+	[0.000]*	[0.000]
Education	-0.001	0.000	-0.004	-0.003	-0.018
	[0.000]*	[0.000]	[0.003]	[0.003]	[0.004]**
Male	0.000	0.000	0.003	0.001	0.009
	[0.002]	[0.003]	[0.019]	[0.017]	[0.025]
Log(HH size)	-0.007	0.001	-0.008	0.072	0.025
	[0.005]*	[0.004]	[0.034]	[0.032]*	[0.045]
Obs	912	509	1292	920	883

Notes: Average marginal effects shown. Standard errors in brackets. ** denotes significance at the 1% level, * at the 5% level, and + at the 10% level. Additional controls are province fixed effects.

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