

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

## Job Market Paper

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### Abstract

In 1994, the Chinese government began a program that offered state employees living in state-owned housing the opportunity to buy their homes at subsidized prices. This paper analyzes the impact of housing policy and the creation of private property on individuals' labor market decisions with a focus on entrepreneurship. Using the reform as an exogenous change in the mobility costs and credit constraints that influence entry into entrepreneurship, my estimates suggest that the housing reform significantly increased self-employment. I develop a theoretical framework that incorporates two mechanisms to explain the linkage between housing reform and entrepreneurship. First, I explore the idea that unbundling housing and employment decisions increased labor mobility from the state sector, including transitions into self-employment. Second, I hypothesize that the creation of private property and the wealth effect associated with the subsidized sale increased entrepreneurship by relaxing credit constraints. The empirical evidence in urban China suggests that both mechanisms played a role in increasing self-employment.

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# 1 Introduction

Entrepreneurs are regarded as key agents in fostering the innovation and investment necessary for economic development. In thinking about the conditions that facilitate entrepreneurship, policy makers and economists have focused on the importance of incomplete markets in credit, property and labor. The model developed 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. Their work supports the idea that interventions by government agencies and other organizations are necessary because credit markets fail to provide efficient levels of capital for new businesses.

Another approach to analyzing the relationship between entrepreneurship and economic development focuses on property rights and increasing the liquidity of assets, such as land. De Soto (2000) popularized the idea that the transformation of unused capital in low-income countries is critical to entrepreneurship and economic growth. His arguments highlight the large amounts of potential wealth that could be accessed through the formalization of property rights of urban squatters. Field (2007) provides empirical evidence on the response of households to increasing the security of property rights. Field finds that a program that provided property titles to urban squatters in Peru increased labor supply and the probability of working outside of the home. Besley (1995) develops a theoretical structure to analyze various mechanisms through which increasing ownership rights influence investment in an agricultural setting, but his results do not provide clear evidence in support of any specific mechanisms.

While Besley focused on land in an agricultural setting and Field and De Soto emphasized the property of urban squatters, this paper addresses a similar idea about the latent value of capital in low-income countries in the form of state-owned housing. Government-owned housing represents a large share of the housing stock in low-income countries. Data from the United Nations Human Settlements Indicators (2001) suggest that residents in state-owned housing represent 16% of households in Africa, 10% in Asia and 14% in Latin America. In the U.S., less than 2% of households live in public housing. The bulk of state-owned housing in the U.S. is provided as a welfare benefit to individuals on the low end of the income distribution. In contrast, in many developing countries, in particular in countries of South Asia, East Asia and sub-Saharan Africa, the bulk of the stock of state-owned housing is provided to employees in the government and in state-owned enterprises. I analyze the impact of state housing provision on entrepreneurship in the context of a property reform in urban China where individuals living in housing provided by their state employers were given the opportunity to purchase their homes at subsidized prices. Unlike the situations considered by Besley and Field, the fundamental issue in China is not the security of property rights but the conversion of state assets into private property. The key similarity is the consideration of institutions that prevent the capital embedded in real estate from being used in a productive manner.

In this paper, I demonstrate that the sale of state-owned housing in urban areas of China beginning in 1994 resulted in a significant increase in self-employment. I use the reform as a natural experiment to analyze the effects of housing policy and the creation of private property on individuals' labor market

choices. My baseline identification strategy takes uses a difference-in-difference 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 also exploit the panel structure of my data set with a fixed effects estimator. I consider two mechanisms through which the sale of state-owned housing during the housing reform could have led to an increase in entrepreneurship. First, in a credit constrained environment, the privatization of state assets could have allowed individuals to capitalize on the value associated with the real estate. Second, by unbundling housing from the employment decisions of individuals, the reform could have increased aggregate job mobility, including transitions into entrepreneurship.

Numerous empirical studies have evaluated the effect of credit constraints on entrepreneurship. One relevant strand of literature in developing countries estimates the returns to capital in order to demonstrate the the impact of credit constraints on small businesses. High returns to capital provide evidence that credit constraints prevent individuals from entry. The firm-level analyses of McKenzie and Woodruff (2002) in Mexico and Banerjee and Duflo (2004) in India find evidence of severe credit constraints. Another body of literature examines the individual-level decisions regarding entry into entrepreneurship. Work by Evans and Jovanovic (1989) in the United States and by Paulson and Townsend (2004) in Thailand 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 necessary capital to start up and maintain their business ventures. One challenge to this interpretation occurs if wealth is correlated with unobserved ability. In that case, 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).

My paper contributes to the emerging literature that attempts to address the omitted factors that complicate the analysis of a credit market failure for small businesses. In this paper, I use a large, exogenous change in housing wealth to separate the effect of credit constraints from the omitted variables that affect an individual’s decision to start a small business. The establishment of private ownership rights allowed individuals to capitalize on the value of the property. Furthermore, the large discrepancy between the market value and the price charged during the privatization of state-owned housing represented a substantial wealth effect.

The economic research on entrepreneurship focuses primarily on the role of credit constraints in entry decisions. In this paper, I consider job mobility as another factor that affects entry in entrepreneurship. There is a general consensus that the efficiency of the housing market influences job mobility by affecting the costs of residential mobility. 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 state-owned enterprises were provided housing from their employers. Housing provided by state work-units was highly subsidized with the rents averaging less

than one percent of the household's total expenditures. 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. There is a literature on in-kind benefits and job mobility that focuses on the provision of health insurance in the U.S. (Madrian 1994, Kapur 1998, Buchmueller and Valleta 1996, Gruber and Madrian 2002).<sup>1</sup> The literature identifies the effects of health insurance on mobility by using variation in health status, household size and health insurance coverage from a spouse's employer. However, these variables are choices that are arguably determined endogenously with decisions about employment and job mobility. The lack of exogenous variation is also a problem in the approach used in papers that evaluate the consequences of public housing on residential and labor mobility. Hughes and McCormick (1987) find that the provision of public housing as a welfare benefit in the United Kingdom reduced residential mobility and led to an increase in unemployment.

My estimates suggest that the 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 2-8%. This is a substantial amount that represents at least a doubling of the rates of self-employment among the affected group. The data also indicate an increase in the rates of job changes among former residents of state-owned housing as well as a substantial growth in the amount of capital that they owned. The estimates suggest that the property reform freed over two billion RMB from former state-owned housing for productive enterprises. Overall, the empirical evidence suggests that entrepreneurship expanded both because the reform loosened credit constraints and because it increased overall mobility.

The following section of the paper describes the institutional background in China and provides details regarding the housing reform. Next, I develop a theoretical framework to understand the empirical implications of credit constraints and bundling housing with employment for entrepreneurship. Section 4 describes the data, presents the empirical strategy and analyzes the results. The last section concludes.

## 2 Institutional Background

### 2.1 Socialism and Early Reforms

Upon taking control of the government in China in 1949, the Communist Party nationalized urban land and established a system that guaranteed jobs for all workers. Households with private ownership of their homes were allowed to retain 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

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<sup>1</sup>This literature uses the term job lock to describe the immobility that arises when workers don't want to risk losing employer-provided health insurance even when higher match-specific productivities exist.

work units and nominal rents were charged.

Following the death of Chairman Mao Zedong in 1976, the new leadership initiated a gradual reform of the socialist system towards a mixed economy. Private construction of housing was allowed and the supply of commercial housing expanded. The first experiments with public housing in 1979 entailed the sale of newly built apartments at construction cost in Xian and Nanning. During the 1980s, several other small-scale housing experiments were piloted in different cities. These included a program that split the cost of a private home between workers and their state work-units, a mandatory housing savings contribution, and a voucher system in which employees were expected to pay 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 increasing rents and wages simultaneously in the state sector and encouraging 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. They realized their ideas for privatization were financially infeasible at current wage levels 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 mid-1990s. While the experimentation throughout 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 5.3, I use data to confirm 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 home. 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.<sup>2</sup> 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. According to the China News Analysis (1998), most buyers paid less than 15% of the market value for

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<sup>2</sup>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.

their homes. Urban data from the Chinese Household Income Project covering eleven provinces in 1995 suggest that the average price for a public housing unit was one-quarter of its market value; the average wealth gain for purchasers was 24,462 RMB, which is more than two times the average annual wages of a household.<sup>3</sup> 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 price. The large windfall associated with purchasing homes ensured the success of the reform, and transformed China into a country with one of the highest rates of home ownership in the world.

While the amount of windfall was substantial, its relevance 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. 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.<sup>4</sup> Chen (2007) argues that the ability to capitalize on housing assets through market transactions and collateralized loans has contributed significantly to economic growth in China. This point is also made in an article in the *Financial Times* (2002): “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... This new willingness to borrow, particularly among young people, has touched off a virtuous circle in economic activity.” A Gallup poll on borrowing behavior in China in 2005 found that personal borrowing to start or operate a business is the top reason that individuals go into debt (42%); this is followed by borrowing to make purchases in the housing market (19%). Furthermore, in section 6, I empirically test whether individuals who bought a subsidized home internalized the wealth effect associated with the purchase.

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. The top portion of Table 1 demonstrates that by 1997, the rates of home ownership among households in public housing in 1993 exceeded 50% in 1997 for all provinces in the sample with the exception of Shandong. This outlier may be explained by the small number of individuals sampled in that province. The

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<sup>3</sup>The sample of 6,931 households in the Chinese Household Income Project of 1995 was selected from a sample of 35,000 urban households from the State Statistical Bureau.

<sup>4</sup>Formal sector lending by banking institutions is 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) and loans from non-bank sources are often collateralized by assets including housing (Watts 2005, Guo and Mu 1998).

Table 1: Transition Rate of Residents in Public Housing in 1993

	% Home Owners in 1997	Std. Dev.	Obs.
<u>By Province</u>			
Liaoning	0.72	0.45	116
Jiangsu	0.64	0.46	59
Shandong	0.35	0.49	26
Henan	0.50	0.50	113
Hubei	0.68	0.47	117
Hunan	0.71	0.46	150
Guangxi	0.52	0.50	62
Guizhou	0.60	0.49	62
<u>By Employer Size</u>			
Less than 20 Employees	0.64	0.48	76
20-100 Employees	0.67	0.47	112
Over 100 Employees	0.65	0.48	363

Note: The statistics shown for Liaoning province are for 2000 rather than 1997 because the area was not surveyed in the 1997 wave.

inclusion of province fixed effects will remove the effect of any time-invariant differences across provinces. I also looked for heterogeneity in the implementation of the reform by characteristics of the state-owned enterprise. As shown in bottom of Table 1, there are no differences in the transition into home ownership by the size of the firm. Overall, the data suggest that over the window of 1993 to 1997, there was not much heterogeneity in the timing of implementation across firms.

### 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. In my model, the individual must choose 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  $\tilde{h}_i$ , where  $\tilde{h}_i \geq 0$ .<sup>5</sup> Individuals differ in their level of wealth,  $W_i$ , and their observable characteristics,  $x_i$ .

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

<sup>5</sup>For simplicity, I model  $\tilde{h}_i$  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 the relative prices of housing in the public system and the private market.

consumption,  $C_{il}$ :

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

subject to the constraints

$$rb_{il} + C_{il} + q_l h_{il} = Y_{il} \quad (2)$$

$$k_{il} = W_i + b_{il} \quad (3)$$

$$-W_i \leq b_{il} \leq \lambda(q_l h_{il} + W_i) \quad (4)$$

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  $\lambda$ , where  $0 < \lambda < 1$ . 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 earnings and compensation in the various sectors.<sup>6</sup>

In the sequence of events within the static framework, an individual realizes her level of wealth,  $W_i$ . She chooses the amount to borrow or lend, and this determines  $k_i$ . The model only allows individuals to borrow to finance capital. 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), and allocates the remaining money between housing and consumption.

Wages in the private and state sectors are functions of individual  $i$ 's set of observable characteristics,  $x_i$ , given by  $Y_{pi} = y_p(x_i, \zeta_i)$  and  $Y_{si} = y_s(x_i)$ , respectively, where  $\zeta_i$  is an unobserved component of wages in the private sector. Workers in the state sector receive an amount of housing allocation,  $\tilde{h}_i$ , to rent for free ( $q_s = 0$ ). The market price for a unit of housing for entrepreneurs and private wage employees is  $q$  ( $q_p = q_e = q$ ). The person's observable characteristics,  $x_i$ , and some unobserved characteristics,  $\mu_i$ , determine the amount of public housing that the individual receives,  $\tilde{h}(x_i, \mu_i)$ . This reflects the evidence that the allocation of public housing units depended on observable worker characteristics such as job tenure and rank as well as unobservable traits such as social connections (Logan and Zhou, 1996). Net entrepreneurial earnings are given by  $Y_{ei} = \theta_i f(k_i)$ , where  $\theta_i$  reflects entrepreneurial ability, and  $f(\cdot)$  is the production function that is concave in the amount of capital invested in the business,  $k_i$ . Entrepreneurial ability,  $\theta_i$ , state connections,  $\mu_i$ , private wage unobservables,  $\zeta_i$ , initial wealth,  $W_i$ , and the vector of characteristics,  $x_i$ , vary across individuals. I assume that individuals know their own values for all measures, but only  $x_i$  is

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<sup>6</sup>For details on how heterogeneity in preferences would alter the model, see Appendix A.

observed by the econometrician.

Individuals in wage employment do not experience any gains from investing in capital, so they choose  $k_i = 0$ . The total utility an individual derives from working in the state sector is given by

$$V_{si} = U(Y_{si} + rW_i, \tilde{h}_i) \quad (5)$$

The total utility of a worker in the private sector is

$$V_{pi} = U(Y_{pi} + rW_i - qh_i^*, h_i^*) \quad (6)$$

and  $h_i^*$  solves the following first-order condition

$$-qU_c + U_h = 0 \quad (7)$$

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. This produces the standard result that individuals choose their levels of consumption and housing to equate the marginal rate of substitution between consumption and housing with the price ratio.

The total utility of an entrepreneur is

$$V_{ei} = U(\theta_i f(\bar{k}_i) - r\bar{k}_i + rW_i - q\bar{h}_i, \bar{h}_i) \quad (8)$$

where

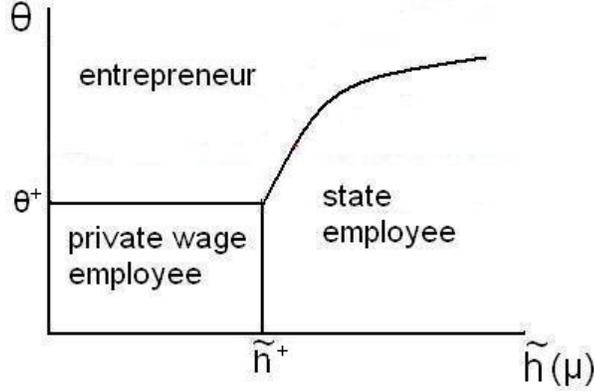
$$\bar{h} = \begin{cases} -qU_c + U_h = 0 & \text{if } b_i^* < \lambda(q\bar{h} + W_i) \\ -q(1+r\lambda)U_c + U_h = 0 & \text{if } b_i^* = \lambda(q\bar{h} + W_i) \end{cases}$$

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 yields  $k_i^*$  that solves

$$\theta_i f'(W_i + b_i) = \theta_i f'(k_i) = r \quad (9)$$

However, the maximum capital accessible for the entrepreneur 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 concave, for

Figure 1: Case of  $Y_p > Y_s$



$k_i^{cc} < k_i^*$ , we have that

$$\theta_i f(k_i^{cc}) < \theta_i f(k_i^*) \quad (10)$$

and the total value of being an entrepreneur,  $V_{ei}$ , is lower for individuals with binding credit constraints.

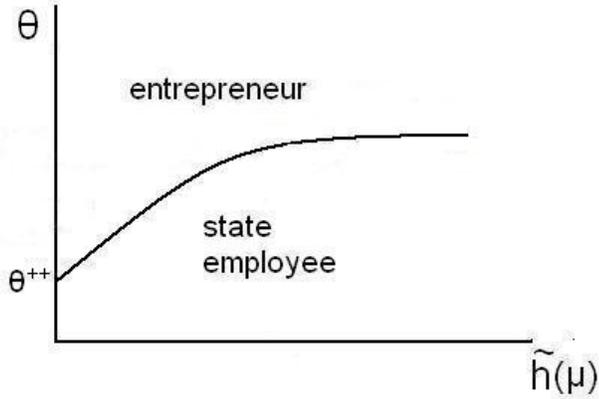
The individual's employment (and housing) decision is simple. She will become an entrepreneur if  $V_{ei} > \max[V_{si}, V_{pi}]$ , a state employee if  $V_{si} > \max[V_{ei}, V_{pi}]$ , and a private wage employee if  $V_{pi} > \max[V_{ei}, V_{si}]$ . The value of  $V_{ei}$  depends on the level of capital input,  $k$ , 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 was able to achieve the optimal level of capital investment,  $k^*$ . Credit constraints will produce a correlation between individual's total wealth and their probability of entering entrepreneurship.

For a worker with given values of  $W_i$ ,  $x_i$  and  $\zeta_i$  such that  $y_p(x_i, \zeta_i) > y_s(x_i)$ , her optimal choice across job sectors can be represented by Figure 1. The values  $\theta^+$  and  $\tilde{h}^+$  are the reservation values that delineate the choices of workers based on their unobserved heterogeneity in  $\mu_i$  and  $\theta_i$ . More specifically,  $\theta^+$  is the value of  $\theta_i$  at which an individual is indifferent between private wage employment and self employment; in other words,  $\theta^+$  solves  $V_{pi} = V_{ei}$ , which simplifies to:

$$Y_{pi} = \theta_i f(\bar{k}_i) - r\bar{k}_i \quad (11)$$

The value of  $\theta^+$  that solves equation 11 is independent of  $\tilde{h}(\mu_i)$ , and individuals with levels of entrepreneurial ability above  $\theta^+$  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  $V_{pi} = V_{si}$ . The threshold level of  $\tilde{h}^+$  determined by  $V_{pi} = V_{si}$  is independent of  $\theta$ . People with high

Figure 2: Case of  $Y_p < Y_s$



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  $\theta$  and  $\mu$ , there are values of  $\theta_i$  and  $\tilde{h}_i$  such that the individual is indifferent between entrepreneurship and state employment,  $V_{ei} = V_{si}$ . These values are determined by the equation

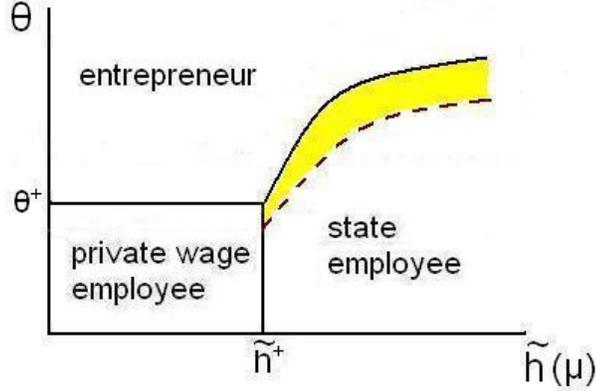
$$U(Y_{si} + rW_i, \tilde{h}_i) = U(\theta_i f(\bar{k}_i) - r\bar{k}_i + rW_i - q\bar{h}_i, \bar{h}_i) \quad (12)$$

This is represented by the curved line in Figure 1 and defines the employment decision for workers with high levels of both  $\theta_i$  and  $\tilde{h}(\mu_i)$ . 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  $\theta_i$  to make them indifferent between entrepreneurship and state sector employment ( $\frac{\partial \theta}{\partial h} > 0$ ). Furthermore, at high levels of  $\tilde{h}(\mu_i)$ , the amount of  $\tilde{h}(\mu_i)$  needed for the individual to remain indifferent increases relative to  $\theta_i$  ( $\frac{\partial^2 \theta}{\partial h^2} < 0$ ). This occurs because of the concavity of the utility function. The marginal utility from increasing housing in the state sector is diminishing, but higher levels of  $\theta_i$  yield more income that can be allocated to both housing and consumption. Appendix B derives this result formally.

While Figure 1 describes the employment decisions of an individual with given levels of  $W_i$ ,  $x_i$  and  $\zeta_i$  where  $y_p(x_i, \zeta_i) > y_s(x_i)$ , Figure 2 shows the corresponding employment choices of an individual for whom  $y_p(x_i, \zeta_i) < y_s(x_i)$ . In this case, the choice simplifies to a binary choice between entrepreneurship and the state job. The individual will never choose the private sector because she will receive higher wages and a nonnegative housing benefit in the state sector.

As a result of the sorting process, individuals with higher levels of  $\tilde{h}(\mu)$  also have higher average levels of  $\theta$ . Holding constant their observable characteristics, individuals with high levels of  $\theta$  and low levels of  $\tilde{h}(\mu)$  will work as entrepreneurs. Thus, high  $\theta$  types will only locate in the state sector if their housing allocation  $\tilde{h}(\mu_i)$  exceeds the threshold level. A similar explanation applies to the relationship between  $\tilde{h}(\mu_i)$  and the

Figure 3: Case of  $Y_p < Y_s$



unobservable factor,  $\zeta_i$ , that influence their wages in the private sector.<sup>7</sup> High levels of  $\tilde{h}(\mu)$  observed in the state sector will also correspond with higher average amounts of  $\zeta_i$ .

### 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.<sup>8</sup> 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  $\partial V_{si}/\partial \tilde{h} > 0$ , the housing program reduced an individual's value of  $V_{si}$  and increased the likelihood of moving out of the state job. An exogenous reduction in housing benefits in the state sector would cause individuals to shift left in Figures 1 and 2. By decreasing  $\tilde{h}(\mu_i)$  to zero for state employees who purchase the home, the reform shifts individuals left to their corresponding points on the y-axis. In this case, all state workers for whom  $Y_p > Y_s$  (in Figure 1) 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  $\theta^+$  where the threshold level of  $\theta^+$  increases with  $\zeta_i$ . 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 ( $\theta_i < \theta^{++}$  in Figure 2).

<sup>7</sup>Figures 1 and 2 delineate the optimal employment decisions of individuals holding  $\zeta_i$  constant. As  $\zeta_i$  increases, the threshold levels  $\theta^+$  and  $\tilde{h}^+$  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  $\zeta$ ,  $\theta$ , and  $\tilde{h}$  define the optimal employment choices.

<sup>8</sup>The corresponding calculations are shown in Appendix B.

As the theoretical framework demonstrates, there are two ways that the housing reform could increase the transition from employment in a state-owned enterprise into entrepreneurship. The wealth effect associated with the reform allowed individuals to invest more capital into a small business and increased the potential returns to selecting entrepreneurship over wage employment 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 through an alleviation of credit constraints or the unbundling of housing from employment, but it also provides a context for empirically testing the relevance of each of them. 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 home should predict self-employment ( $\partial V_{ei}/\partial(W_i + qh_i) > 0$ ) if there are credit constraints. Furthermore, the use of exogenous variation in housing wealth avoids the potential identification problem that arises if wealth is positively correlated with unobservable entrepreneurial ability. I control for an individual's time-invariant unobserved characteristics, such as ability, with individual fixed effects. While the China Health and Nutrition Survey, the primary data set in this analysis, lacks a broad measure of total wealth or the wealth effect experienced during the reform, I use regional heterogeneity in the appreciation of housing prices to test whether  $\partial V_{ei}/\partial(W_i + qh_i) > 0$ .<sup>9</sup>

The second implication of credit constraints is that the levels of capital investment should increase to reflect the relaxation of credit constraints. Individuals who realize a wealth gain from buying a subsidized home should move from  $k^{cc}$  towards  $k^*$ . However, an average increase in capital is also consistent with the mobility hypothesis about bundling housing and employment. If the unbundling of housing from state employment increases entry into entrepreneurship, then capital can increase from zero to a positive amount even in the complete absence of credit constraints. To separate the effect of credit constraints from entry decisions that may be affected by bundling, I test whether capital increased for individuals who were entrepreneurs *before* the reform and experienced the wealth shock of the housing program.

The model also generates two predictions that allow me to test whether the provision of housing benefits with state employment was relevant in individuals' labor market decisions. A reduction in the value of housing subsidies,  $\tilde{h}$ , will induce individuals to shift out of the state sector and into both self-employment and private wage employment. In contrast, as shown in Figure 3, a reduction of credit constraints only predicts transitions into self-employment and not into private wage jobs. 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 public 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.

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<sup>9</sup>In Appendix D, I consider another method of measuring the wealth transfer that households living in state-owned housing experienced during the reform.

While the value of the wealth transfer *during* the reform yields information about credit constraints, 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  $\tilde{h}$  should also have higher average levels of  $\zeta$  and  $\theta$ . The information contained in  $\tilde{h}$  provides additional tests for the relevance of unbundling in explaining changes in labor choices after the reform. 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  $\tilde{h}$ . Similarly, not only should the wage gains be larger before the reform for individuals leaving the state sector, but the pre-reform wages gains of job changers should be positively correlated with  $\tilde{h}$ .

## 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 and span the years 1989, 1991, 1993, 1997, 2000, and 2004. Thus, the data includes 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.<sup>10</sup> The research focuses on labor market choices, so I limit the sample to working age adults between 18 and 60 years old who are not retired or enrolled in school. Households with more than one generation of adults are not uncommon in urban China, but I restrict the sample to household heads and spouses of heads in 1993. Furthermore, my analysis focuses on the impact of the property reform on individuals who were already in the labor force in 1993. The privatization of public housing may also have indirect effects on the decisions of newcomers to the labor force or other household members, but I leave these questions for future research.

Entrepreneurs are defined as individuals who report their primary occupation as self-employed. Thus, this categorization excludes individuals who engage in entrepreneurial activities in addition to a full-time wage job. While only a small fraction (less than 4%) of adults in the CHNS report a secondary job, approximately 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.

<sup>10</sup>I define urban areas as neighborhoods where the majority of households have urban registrations.

This measure is highly correlated with self-employment. The results are similar, but the standard errors are slightly larger. Household small business ownership is subject to more measurement error because the survey question changes between 1993 and 1997. For the results in this paper, I focus the analysis on entrepreneurs defined as individuals who report self-employment as their primary work.

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 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 Supplementary Data Sources

One of the model's implications regarding credit constraints requires data on regional housing prices. Data on housing prices that begin as early as the mid-1990s are not common. To my knowledge, the only series that extend as far back as 1993 are provincial statistics on the value and the floor space of residential homes sold from the China Statistical Yearbooks, published by the National Bureau of Statistics. 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.<sup>11</sup> 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 wages and housing prices as well as the CHNS series that are in nominal yuan are converted into real 1990 yuan 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 year.

## 4.3 Econometric Methodology

To evaluate the causal impact of the policy change on individual outcomes, I use a difference-in-difference 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 and identify the treatment group as all household heads and spouses of heads in public housing with at least one member in state employment in 1993. Classification of

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<sup>11</sup>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.

individuals into the treatment and control groups occurs at the couple level.<sup>12</sup>

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 loss associated 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 experiencing 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 2 presents summary statistics for the treatment and control 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 assets (which equals the sum of a household’s self-reported market value of durable goods but does not include housing and financial assets). However, the treatment group has substantially more education than the other groups. The theoretical framework allows for differences across sectors in the returns to observable characteristics,  $x_i$ , as well as for these characteristics to enter into the housing allocation function in the state sector. The model suggests that the differences in education across groups reflect higher returns to education in the state sector. My estimates control for education, and the panel structure of the data allows me to remove the effect of any time-invariant factors by including individual fixed effects. I will also use a propensity score approach to examine whether the results are sensitive to the differences in education and other observable characteristics.

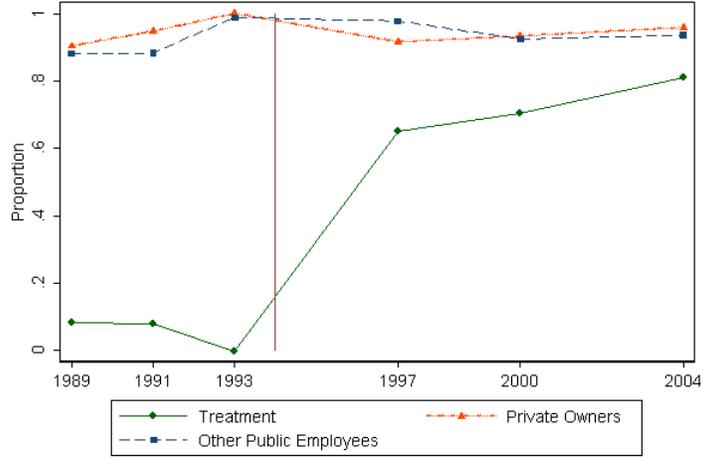
As expected, the treatment group is different from the control groups along a number of labor market characteristics. The real hourly wage of individuals in the treatment group is significantly lower than the privately employed control, but not significantly different from the control group of individuals in state employment. This supports the notion that the structure of wage compensation in the state sector is separate from the provision of state-owned housing.<sup>13</sup> The difference between the market rental value and the rent charged is 127 yuan 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 second control group is more similar to the treatment group than the first control group. The percentages

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<sup>12</sup>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, so it would not be possible to identify private property rights at the individual level.

<sup>13</sup>See section 7.1 for more detailed analysis of the system of compensation in the state sector.

Figure 4: Rates of Home Ownership



of self-employed individuals in the treatment group and the state control group are not zero because these groups are defined at the couple level.

Identification of a panel difference-in-difference estimator requires that the differential trends in behavior of the treatment group relative to the control group are constant before and after the reform. In other words, consistent estimates of a DID depends on a parallel trends assumption, that the difference in the outcome between the treatment and the control group would remain the same in the absence of the reform. Figure 4 shows that the difference in rates of home ownership between the treatment and control groups were quite constant before and after the reform in 1994.

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 difference-in-difference estimator on home ownership or self-employment is implemented as a logistic regression of the form

$$y_{ijt} = m(\alpha_0 + \alpha_1 Treat_i * Post_t + \alpha_2 Post_t + \alpha_3 Treat_i + \alpha_4 \mathbf{x}_{it} + \varphi_j + \epsilon_{ijt}) \quad (13)$$

where  $y_{ijt}$  is a dummy variable for home ownership or 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  $\varphi_j$  are province fixed effects. The vector of covariates,  $\mathbf{x}_{it}$ , includes a quadratic in age, years of education, and male. The coefficient,  $\alpha_1$ , on the interaction term,  $Treat_i * Post_t$ , is the estimated effect of the housing program.

While the basic logistic regression yields estimates that are easy to interpret, the theoretical model suggests that that the proper estimating equation includes individual fixed effects to control for the time-invariant characteristics,  $\theta_i$ ,  $\zeta_i$  and  $\mu_i$ , that affect the decisions regarding housing and employment. The

Table 2: Summary Statistics in 1993

	Treatment	Control 1: Non-State Home Owners	Control 2: Other State Employees
Male	0.52 (0.50)	0.54 (0.50)	0.55 (0.50)
Age	41.72 (8.98)	41.45 (9.54)	40.91 (9.16)
Married	0.98 (0.15)	0.98 (0.15)	0.98 (0.13)
Durable Non-housing Assets	4090.7 (7299)	4245.4 (7210)	4187.7 (6214)
Education (Years)	10.76 (3.78)	7.44* (3.55)	7.86* (3.47)
Han Ethnicity	0.95 (0.21)	0.93 (0.26)	0.94 (0.24)
Household Size	3.62 (1.24)	4.21* (1.53)	4.07* (1.55)
Cadre in Household	0.47 (0.71)	0.08* (0.28)	0.26* (0.55)
Real Hourly Wage	0.62 (0.52)	0.92* (0.69)	0.67 (0.57)
Rent Subsidy per Month	72.03 (67.18)	0.57* (8.21)	0.13* (2.41)
Self-Employed	0.02 (0.13)	0.37* (0.45)	0.07* (0.25)
Capital	31.1 (361.5)	549.2* (3295.9)	86.0* (423.2)
Obs	590	380	303

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 yuan

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

$$y_{it} = g(\alpha_1 Treat_i * Post_t + \alpha_2 Post_t + \alpha_4 \mathbf{x}_{it} + \gamma_i + \epsilon_{it}) \quad (14)$$

corresponds to equation 13 with the inclusion of time-invariant individual fixed effects,  $\gamma_i$ . However, the fixed effects logit has the disadvantage of not yielding estimates of average partial effects without strong assumptions regarding the distribution of  $\gamma_i$  (Wooldridge, 2002, p.492). 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,  $\alpha_1$  is the coefficient representing the program effect.

## 5 Main Results

### 5.1 Effect on Home Ownership

Columns 1 and 3 of Table 3 present estimates of equation 13 where the dependent variable is an indicator for home ownership. Column 1 includes the treatment group and the control group of non-state employees who are home owners in 1993, and the sample in column 3 contains the treatment group and the control group of employees of state-owned enterprises who were not living in public housing in 1993. 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%.

Columns 5 and 6 display coefficients of fixed effects logit estimates of equation 14 where the dependent variable is home ownership. The sample size 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 3 confirm that the housing reform program was quite successfully in increasing the home ownership among public housing tenants.

### 5.2 Effect on Entrepreneurship

The same identification strategy can be used to estimate the impact of privatizing state-owned housing on the probability of becoming an entrepreneur. I estimate equations 13 and 14 with  $y$  as self-employment. The results are presented in Table 4. Columns 2 and 4 provide the average marginal effect of the reform on self-employment as specified by equation 13. The estimates in Column 2 suggest that the housing reform resulted in a 7.4% 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 1.5%. While the estimated effect of the program is considerably smaller relative to the state control group as compared to the

Table 3: The Impact of the Reform on Home Ownership

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

Notes: \*\* denotes significance at the 1% level, \* at the 5% level, + at the 10% level.

<sup>a</sup> 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.

<sup>b</sup> Robust standard errors in brackets. Additional controls are a quadratic in age and individual fixed effects.

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. Other changes occurring in the state sector can explain the differences in the estimated program effect in column 1 and 2. The estimate of  $\alpha_1$  in the sample that includes the state control group removes the effect of other changes in the state sector, such as changes in job security, wages and other benefits. The smaller estimate of the coefficient on the interaction term,  $Treat_i * Post_t$ , can also be explained in the credit constraints framework by the wealth transfer given to some state employees that did have the option of buying a home at a subsidized price.<sup>14</sup>

The gap in the estimates for the two samples decreases in the estimates with individual fixed effects. The last two columns of Table 4 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 the specifications that control for individual unobserved heterogeneity. The estimates suggest that the reform significantly increased the relative log odds of self-employment by 1.7 to 2.1 times for the treatment group relative to the private control group and the state control group, respectively. In column 5, the negative and significant coefficient on  $Post$  indicates that the reform resulted in a decrease in self-employment among private employees. One potential explanation is that the housing reform had general equilibrium effects that resulted in a decrease in the entrepreneurship among existing home owners.<sup>15</sup> However, the analysis of this paper focuses on the

<sup>14</sup>This monetary compensation for non-treated state employees was discussed in Section 4.3. However, the results in Section 6 indicate that the wealth effect experienced by the treatment group is the same relative to state employees or private employees.

<sup>15</sup>In a separate paper, I explore the idea that the privatization of state-owned enterprises caused a fall in the equilibrium price

Table 4: The Impact of the Reform on Self-Employment

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

Notes: \*\* denotes significance at the 1% level, \* at the 5% level, + at the 10% level.

<sup>a</sup> Robust standard errors clustered by household in brackets. Additional controls are indicators for province and a constant term.

<sup>b</sup> Robust standard errors in brackets. Additional controls are individual fixed effects.

partial equilibrium impact of the housing reform on individuals who were given the opportunity to purchase the state-owned home.

### 5.3 Alternative Specifications

I allow the effects of the program to vary over time by estimating the following more flexible logit regression

$$y_{ijt} = m(\alpha_0 + \sum_{t \geq 1993} \beta_t \cdot Treat_i + \alpha_1 Treat_i + \alpha_2 \mathbf{x}_{ijt} + \omega_t + \varphi_j + \epsilon_{ijt}) \quad (15)$$

The corresponding fixed effects logit regression is

$$y_{ijt} = g(\alpha_0 + \sum_{t \geq 1993} \beta_t \cdot Treat_i + \alpha_2 \mathbf{x}_{ijt} + \omega_t + \gamma_i + \epsilon_{ijt}) \quad (16)$$

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. A coefficient on the interaction of *Treat* with the 1993 year dummy equal to zero 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.

Table 5 presents the estimation results from the flexible equations. 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 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 starting in 1997, which is the first survey wave following the start of the reform. In 1997, individuals who had the opportunity to buy their residences from the state transitioned more into entrepreneurship than individuals already living in private housing. The impact of the reform on entry into entrepreneurship is largest in this year relative to privately employed individuals. The coefficient estimates on *Treat\*1997* are similar in magnitude to the estimates of *Treat \* Post* in the more parsimonious estimates shown in Table 4. The estimates also suggest that the magnitude of the increase in self-employment continued in 2000 and 2004. This may reflect lags in the labor market response of individuals to the purchase or some delays in the implementation of the reform.

Another potential specification would be to use an instrumental variables approach. Estimates of equation 13 with home ownership as the dependent variable represent the first stage with the interaction term, *Treat \* Post*, as the instrument. This estimation strategy requires the additional assumption that the reform only affected entrepreneurship through its impact on home ownership. While this is a plausible assumption and consistent with the mechanisms described in the model, the instrumental variables estimates

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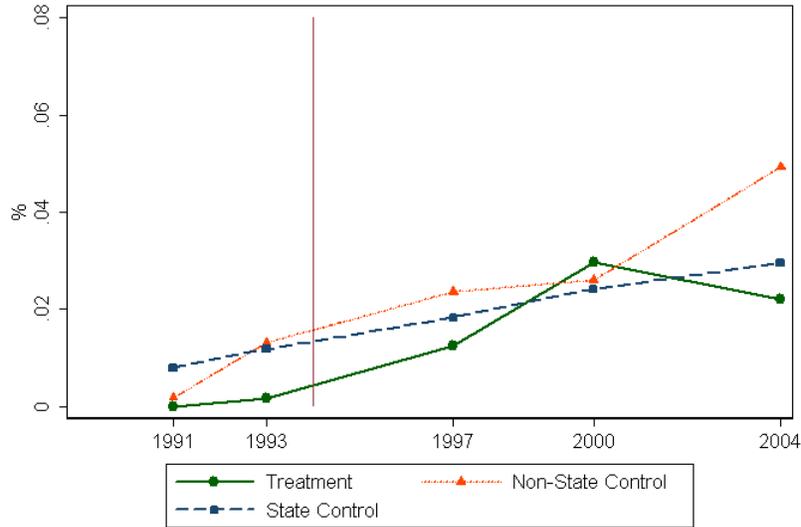
of privately-owned homes. In an environment with credit constraints, a fall in housing wealth for the existing home owners could explain the decrease in self-employment associated with the housing sale. This idea has been examined in reference to the privatization of state-owned housing in Hong Kong (Ho and Wong, 2006) and in Singapore (Bardan et al., 2003).

Table 5: Flexible Estimates of Program Effect on Self-Employment

	Logit: Self-Employment <sup>a</sup>				FE Logit: Self-Employment <sup>b</sup>	
	Control=Private Workers		Control=State Workers		Control=Private	Control=State
	Coefficient	<i>Marginal</i>	Coefficient	<i>Marginal</i>	Coefficient	Coefficient
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*1993	0.996 [1.027]	<i>0.02</i>	0.383 [1.094]	<i>0.00</i>	-0.398 [0.819]	0.062 [0.885]
Treat*1997	2.718 [0.905]**	<i>0.16</i>	0.65 [0.923]	<i>0.01</i>	2.411 [0.837]**	1.826 [0.855]*
Treat*2000	2.101 [0.892]*	<i>0.09</i>	1.047 [0.935]	<i>0.02</i>	1.608 [0.800]*	1.112 [0.853]
Treat*2004	2.115 [0.949]*	<i>0.10</i>	2.412 [1.073]*	<i>0.09</i>	2.028 [0.984]*	2.722 [1.068]*
Treat	-4.527 [0.739]**	<i>-0.28</i>	-2.285 [0.770]**	<i>-0.04</i>		
year=1993	-0.869 [0.276]**	<i>-0.01</i>	-0.175 [0.452]	<i>0.00</i>	-1.085 [0.359]**	0.015 [0.633]
year=1997	-0.388 [0.322]	<i>-0.01</i>	1.851 [0.385]**	<i>0.04</i>	-3.314 [0.737]**	1.000 [1.194]
year=2000	0.192 [0.299]	<i>0.00</i>	1.49 [0.399]**	<i>0.03</i>	-4.348 [1.028]**	1.414 [1.679]
year=2004	0.928 [0.405]*	<i>0.02</i>	0.983 [0.638]	<i>0.02</i>	-5.343 [1.439]**	1.199 [2.342]
Obs	2677		2719		1048	462

Notes: \* denotes significance at the 5 level, + at the 10% level. <sup>a</sup> 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. <sup>b</sup> Robust standard errors in brackets. Additional controls are a quadratic in age and individual fixed effects.

Figure 5: Rates of Unemployment



would simply scale the coefficients of the reduced form estimates. In this particular context, the IV estimates (not shown) are very similar to the difference-in-difference estimates. This is not surprising because vast majority of individuals in public housing accepted the offer to purchase a home at a subsidized price.

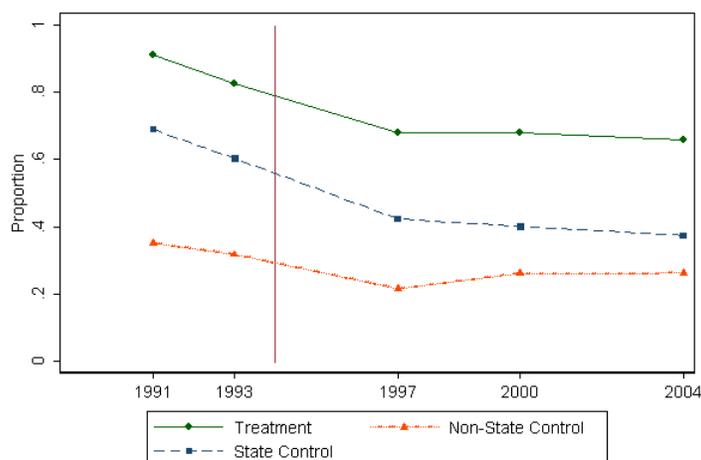
### 5.3.1 Alternative Explanations

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 several 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 be limited to state employees residing in public housing. Any reforms that affected all state employees would not explain the significant, positive results that I find for the treatment group relative to the control group of state workers not residing in public housing.

While it is clear that the housing reform had a large impact on employees in state-owned enterprises, there were other important reforms occurring in the state-sector. Privatization of state assets also occurred at the firm level with the conversion of state-owned enterprises 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 and the break occurred around the same time as the housing reform. Figure 5 shows the unemployment rates of individuals in the CHNS by treatment and control groups.<sup>16</sup> The rates of unemployment among the different groups

<sup>16</sup>The CHNS did not ask about unemployment in the 1989 wave of the survey.

Figure 6: Health Insurance Coverage



moved together in general. There appears to be a small spike in the rate of unemployment for the treatment group relative to the state control group between 1997 and 2000, but this did not occur at the same time as the break in home ownership. The unemployment 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 decided to move 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, the effect of the housing program would dwarf other changes in benefits. Figure 6 displays the trends in health insurance coverage for the treatment and the control groups across time.<sup>17</sup> The trends indicate that individuals in the treatment group was more likely to have some type of health insurance coverage than either of the control groups. Furthermore, it appears that 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.

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 the control group of state employees who were not living in public housing in 1993. Thus, other elements of the state

<sup>17</sup>I have dropped the data for health insurance in 1989 because the survey question in 1989 was different from the subsequent years.

Table 6: Estimates for Self-Employment including Propensity Score

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

Notes: \*\* denotes significance at the 1% level, \* at the 5% level and + at the 10% level. <sup>a</sup> 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. <sup>b</sup> Robust standard errors in brackets. Additional controls are a quadratic in age and individual fixed effects.

reform process do not correspond to the sharp break in home ownership shown in figure 4 and cannot explain the estimated impact of the housing program on entrepreneurship.

## 5.4 Robustness Checks

### 5.4.1 Non-Random Treatment

The summary statistics showed that individuals in the treatment group differ from individuals in the control group along observable characteristics such as education. 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 the non-random assignment of treatment. 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. In other words, the estimated propensity score should contain the information in the covariates that is pertinent in the estimation of the treatment effect. In my framework, I include both the propensity score and its interaction with the *Post* dummy. This removes the effects of differences in observable characteristics of the treatment and control group on the estimate of *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 15. Table 6 reports the self-employment estimates controlling for the effects of 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 4. The magnitude of the effect of treatment relative to the comparison group of other state employees decreases slightly. The 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 5 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 controlling for observable differences between the treatment and the comparison groups.

#### 5.4.2 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 year following the reform. In the 1997, 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.<sup>18</sup> The IPW 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 each 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 has remained in the survey. These estimates are shown in the Appendix Table 16. However, these predicted *conditional* probabilities are not representative of the original sample population. Wooldridge (2002a) shows that we can use the inverse of the joint probability as weights in the second step of the estimation with the joint probability calculated directly from the conditional probabilities.

Panel A of Table 7 displays the results using the inverse probability weighting to correct for sample attrition. Comparing these results to the ones shown in Table 4, 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, sample attrition based on observable characteristics tends to bias the results downward. These results depend

<sup>18</sup>This assumption is often called ignorability of selection. See Wooldridge (2002) or Wooldridge (2002a) for more details on inverse probability weighting.

Table 7: Estimates of Self-Employment: Robustness Checks

	Logit: Self-Employment <sup>a</sup>				FE Logit: Self-Employment <sup>b</sup>	
	Control=Private Workers		Control=State Workers		Control=Private	Control=State
	Coefficient	Marginal	Coefficient	Marginal	Coefficient	Coefficient
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel A: Estimates with Attrition Correction (Inverse Probability Weighting)</b>						
Treat*Post	1.972	0.080	1.066	0.019	1.895	1.937
	[0.503]**		[0.543]*		[0.660]***	[0.592]***
Post	0.452	0.012	1.627	0.028	-1.43	0.427
	[0.174]**		[0.274]**		[0.337]***	[0.452]
Treat	-4.196	-0.307	-2.007	-0.048		
	[0.474]**		[0.497]**			
Obs	3129		3082		1020	444
<b>Panel B: Two-Period Collapsed Estimates (Serial Correlation Correction)</b>						
Treat*Post	2.456	0.167	1.237	0.041	1.835	1.375
	[1.070]*		[1.111]		[0.656]**	[0.694]*
Post	0.317	0.014	1.463	0.039	-0.536	-0.115
	[0.284]		[0.448]**		[1.287]	[1.567]
Treat	-4.842	-0.488	-2.721	-0.141		
	[0.978]**		[1.045]**			
Obs	1014		953		240	160
Notes: ** denotes significance at the 1% level, * at the 5% level and + at the 10% level. <sup>a</sup> 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. <sup>b</sup> Robust standard errors in brackets. Additional controls are a quadratic in age and individual fixed effects.						

on the assumption that attrition is ignorable with respect to self-employment conditional on the variables in the attrition equation.

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, I cannot think of a variable that would 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 has a similar impact as attrition based on observables, and both work against finding a significant effect of the reform on entrepreneurship.

### 5.4.3 Serial Correlation

Bertrand, Duflo and Mullainathan (2004) find that difference-in-difference estimates that use several years of data may lead to substantial underestimates of the standard errors if serial correlation in the outcomes are 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 7. The average marginal effects implied by the logit estimates suggest that the program increased self-employment for the treatment group by 4% relative to the state control group and by 16% 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 4. 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.

## 6 Testing Credit Constraints

This section explores the relevance of incomplete credit markets in explaining the positive impact of the housing reform on entry into self-employment. This hypothesis assumes that the housing reform produced a wealth effect on the treatment group through the generous terms of the sale. I test this assumption by investigating whether the reform had a positive impact on consumption. I use food consumption specifically because the CHNS does not ask about other types of consumption. Furthermore, it is a standard way to evaluate a wealth effect in the development literature (Rosenzweig and Stark 1989, Kazianga and Udry 2006). These papers measure the effect of wealth on food consumption in poor, rural areas. My analysis is for an urban sample that is not living at subsistence level; thus, using food consumption in this context will yield an underestimate of the wealth effect. The dependent variable is the logarithm of the food consumption

Table 8: Impact of Reform on Consumption and Capital

	<u>Fixed Effects</u>		<u>Fixed Effects</u>		<u>FE Logit</u>		<u>Fixed Effects</u>	
	<u>Log(Food Consumption)</u>		<u>Log(Capital+1)</u>		<u>Positive Capital</u>		<u>Log(Capital)</u>	
	<i>Control=Private</i>	<i>State</i>	<i>Private</i>	<i>State</i>	<i>Private</i>	<i>State</i>	<i>Private</i>	<i>State</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Panel A: Full Sample</b>								
Treat*Post	0.06	0.05	1.47	1.09	1.01	1.24	1.30	0.90
	[0.018]**	[0.021]*	[0.143]**	[0.136]**	[0.307]**	[0.358]**	[0.618]*	[0.621]
Post	-0.12	-0.11	-1.53	-1.17	-1.82	-2.09	-3.89	-3.39
	[0.021]**	[0.025]**	[0.111]**	[0.111]**	[0.184]**	[0.265]**	[0.293]**	[0.332]**
Obs	4775	3922	3551	2942	1428	1003	762	398
<b>Panel B: Sample of 1993 Business Households</b>								
Treat*Post	0.06	0.02	1.52	1.87	0.78	1.86	0.47	0.32
	[0.068]	[0.070]	[0.640]*	[0.601]**	[0.667]	[0.803]*	[1.100]	[1.091]
Post	-0.19	-0.07	-2.10	-2.51	-1.90	-3.06	-3.90	-3.44
	[0.037]**	[0.057]	[0.247]**	[0.326]**	[0.251]**	[0.534]**	[0.382]**	[0.492]**
Obs	1388	526	995	396	654	255	546	204

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

where food consumption is measured as the daily average from individuals' diaries of food intake over three days. The estimates in columns 1 and 2 in panel A of table 8 suggest the housing reform caused a 5 to 6% increase in total food consumption among individuals in the treatment group. This evidence supports the assumption that the reform resulted in a substantial wealth effect for public housing tenants.

## 6.1 Wealth and Access to Capital of Entrepreneurs

According to the model, we can test for incomplete credit markets by examining the impact of the housing reform on capital. In an environment with credit constraints, we would expect wealth effect associated with the reform to increase capital. However, if the unbundling of housing from state employment increased entry into entrepreneurship, then the housing reform would 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 that includes only individuals who were in 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 for credit constraints that is independent from unbundling employment and housing.

Households in the CHNS are asked to report the total market value of the professional and productive equipment that they own. There are numerous households, including households involved in a small business enterprises, that report the total value of their productive assets at zero. Thus, I analyze several specifications of capital. My first approach is fairly standard in measuring capital as the logarithm of the value plus one. However, the logarithm of one plus capital is not normally distributed because of the preponderance of zero values. This is likely to violate the linear regression model's assumption of normality in the error term. In order to understand whether the parameter estimates are sensitive to the mass of zero capital holdings, I examine two other specifications. I estimate a fixed effects logit model where the dependent variable is an indicator that takes on the value of one if the household reports any capital and zero otherwise. I also estimate a log capital regression that only includes non-zero values of capital.

The results are presented in Table 8. Panel A shows the estimates for the full sample, and Panel B shows the estimates that the limited sample of households involved in a small business in 1993. In columns 3 and 4, the dependent variable is the logarithm of capital plus one. The coefficient estimates of  $Treat * Post$  are significant and similar in magnitude for the full sample and the limited sample with the estimates for 1993 business households somewhat larger. For the full sample, the coefficients indicate that the reform increased treated households' ownership of productive assets by 147% relative to privately-employed households and by 109% relative to state-employed households in privately-owned housing. The corresponding estimates for households already engaged in small business activities are 152% and 187%, and 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 for the treatment group is quite large, but this is not surprising given the large wealth effect associated with the terms of the sale. While there is a positive effect of the reform on the value of capital owned by the treatment group relative to the control groups, the coefficient on  $Post$  is negative and significant. This suggests that the reform led to a decrease in the value of productive assets of the non-treatment groups. This may be explained by general equilibrium effects from the privatization of state-owned housing.<sup>19</sup>

I also examine a fixed effect logit model of positive holdings of capital (columns 5 and 6) and a linear fixed effects model of increases in the capital assets for households that already owned positive amount of capital (columns 7 and 8). For the full sample, the evidence suggests that the reform caused both an increase in capital among treated households who already owned capital as well as an increase in transitions of treated households into owning capital at all. The coefficients of  $Treat * Post$  for the sample of households involved in small businesses in 1993 are uniformly positive, but only significant for one of the four coefficients. Overall, the empirical results on productive assets confirm that capital market imperfections exist in urban China and the wealth effect associated with the housing reform allowed individuals to reach higher levels of capital.

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<sup>19</sup>This corresponds with a negative coefficient on  $Post$  for some of the estimates of self-employment. I am exploring the potential for general equilibrium effects in another paper.

## 6.2 Appreciation in Housing Prices

The model suggested that a positive relationship between entry into entrepreneurship and the size of the wealth effect that individuals experienced would provide evidence that the reform loosened credit constraints. I cannot measure the actual wealth transfer that each household received because the CHNS does not have information about the price paid by household for their homes.<sup>20</sup> I use heterogeneity in housing price appreciation across provinces and time to test for credit constraints. Changes in housing prices after the reform determines the housing wealth realized by the new home owners.

Hurst and Lusardi (2004) were the first to use gains in housing equity as exogenous change 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 for 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 CHNS does not ask about total wealth.

I estimate the following equation:

$$y_{ijt} = g(\alpha_1 Treat_i * Post_t + \alpha_2 Treat_i * Post_t * \Delta q_{jt} + \alpha_3 Post_t * \Delta q_{jt} + \alpha_4 Post_t + \alpha_5 x_{ijt} + \gamma_i + \epsilon_{ijt}) \quad (17)$$

where  $\Delta q_{jt}$  represents changes in housing prices in province  $j$  and time  $t$ .<sup>21</sup> The estimate of  $\alpha_2$  is identified by differences in housing price appreciation across provinces and across time. I examine two dependent variables, self-employment and capital. When  $y_{ijt}$  is the self-employment status of individual  $i$ , the dependent variable is an indicator so I estimate the equation with a fixed effects logit. When  $y_{ijt}$  is the amount of productive assets owned by household  $i$ , a fixed effects linear regression is estimated. For both dependent variables, a positive estimate of  $\alpha_2$  supports the hypothesis that the reform increased entrepreneurship by relaxing capital constraints.

Table 9 present the results of equation 17 with self-employment and capital as dependent variables. Columns 1 and 2 suggests 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 0.50 to 0.39 times, respectively.<sup>22</sup> However, the estimate of the triple interaction,  $Treat * Post * \Delta q$ , is only significantly

<sup>20</sup>Appendix D discusses an approach for estimating the wealth effect with parameters estimated in an external data set, but the model is limited by information contained in both the CHNS and the external data set. I am unable to capture much of the variation in the wealth effect associated with the housing sale.

<sup>21</sup>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  $q_{jt}$  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.

<sup>22</sup>A one standard deviation increase corresponds to an increase in the relative odds of 6.3 ( $=\exp(3.877) \times 13\%$ ) and 2.6 ( $=\exp(2.989) \times 13\%$ ) times in columns 1 and 2 respectively.

Table 9: Impact of Housing Price Appreciation on Self-Employment and Capital

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

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.

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  $\alpha_2$  may reflect changes other than appreciation in home values. I attempt to deal with this issue by controlling for other prices. Appendix Table 17 displays the regressions that also include changes in average wages and consumer prices, as well as their interactions with  $Treat * Post$ . The additional control variables do not have a large effect on the estimates of the coefficients of interest,  $\alpha_2$  and  $\alpha_1$ , but the standard errors are larger.

The estimates of equation 17 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 9 are all significant and positive. For existing capital owners, a one-standard deviation increase in housing prices correspond with an increase in capital for treated households ranging from 87% to 107% 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 (or the relative odds by over 50 times).

The theoretical model and the available data offer two methods to test whether capital constraints could explain the positive effect of the housing reform on entrepreneurship. Patterns in consumption and capital confirmed that individuals internalized the wealth effect associated with the subsidized purchase of

their homes. I also find that rates of self-employment and capital ownership increased with regional growth in the average value of privately-owned homes. Overall, the results support the hypothesis that the reform reduced credit constraints and provide evidence that capital constraints matter for small business formation in urban China.

## 7 Impact of Bundling Housing and Employment

### 7.1 Empirical Evidence on Housing Subsidies and Wage Compensation

The theoretical framework and the relationship between state-provided housing and job mobility depend on the system of allocation of housing units to state employees in China. The key assumption is that the value of housing benefits does not affect wage compensation in the state sector. In other words, the amount of rental subsidy that an individual receives does not simply reflect a compensating wage differential. 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 observably identical individuals work in the same position in 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 this assumption.

As shown in Table 2, the unconditional hourly wages of state employees with and without public housing are statistically equivalent. However, the table also indicates these two groups are different along some observable characteristics. I offer two methods to test that the wage compensation of state employees with similar observable characteristics are independent of their housing benefits. First, I test whether the hourly wages of state employees with public housing (the treatment group) and in private housing (the state control group) are statistically equal conditional on observable characteristics that determine wages. Second, I analyze whether a correlation exists between the amount of the rental subsidy and the wages of state employees.

The results of the first test are displayed in columns 1 and 2 of Table 10. The inclusion of one-digit occupation fixed effects in column 2 absorbs any wage effects from different occupations between treatment individuals and non-treated state employees. The coefficient estimates of *Treat* are similar for both specifications and neither are 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. In the pre-reform period, the average rent subsidy is worth 23% of the wage of a state employee who lives in a state-owned housing unit, and wage differences of 1.4 to 1.7% are considerably smaller than the average value of the rental subsidies.

The second test utilizes the actual amount of rent subsidy that individuals received rather than just a binary division between whether or not a person lives in a state-subsidized home. Estimates of the conditional correlation between the amount of rent subsidy and wages are shown in columns 3 and 4 of Table 10. I include

Table 10: Wages and Housing Benefits of State Employees in Pre-Reform Years (1989-1993)

	<u>Log Hourly Wage</u>		<u>Log Rent Subsidy</u>	
	OLS (1)	OLS (2)	OLS (3)	FE (4)
Treat	-0.017 [0.013]	-0.014 [0.013]		
Log Wage			0.066 [0.070]	-0.098 [0.086]
Household Cadre			0.211 [0.102]*	0.171 [0.114]
Age	0.001 [0.005]	-0.001 [0.005]	0.019 [0.044]	0.004 [0.085]
Age <sup>2</sup>	0.000 [0.000]	0.000 [0.000]	0.000 [0.000]	0.000 [0.001]
Education	0.006 [0.001]**	0.006 [0.002]**	0.088 [0.016]**	
Male	0.043 [0.007]**	0.035 [0.008]**		
Occupation FE	No	Yes	No	No
Observations	2628	2581	1353	1355
Adjusted R <sup>2</sup>	0.10	0.11	0.31	0.62

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 regressions also include indicators for year and province, sand a constant term. The log wage regressions are at the individual level and the log rent subsidy regressions are at the household level.

the number of household members who are cadres.<sup>23</sup> In column 3, the positive and significant impact of this variable on the amount of rent subsidy received supports the assumption that unobserved connections,  $\mu_i$ , determine a significant portion of the variation in  $\tilde{h}_i$ . The lack of significance of the coefficient on *Household Cadre* in the estimation with household fixed effects supports the theoretical assumption that people’s unobserved connections in the state sector are time-invariant. The coefficients on *Log Wage* are economically small and statistically equivalent to zero. The point estimate with household fixed effects indicates that a doubling of wages corresponds to a decrease in rent subsidies of 9.8%. These results reinforce the findings of the first test. The correlation between wages and housing benefits are statistically equal to zero and the magnitude of the relationship is too small to support compensating wage differentials.

## 7.2 Mobility

As the theoretical model suggests, we can determine whether the unbundling of housing benefits 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 movements towards private wage jobs, but the credit constraints story cannot. I estimate the following fixed effects logit equation

$$\Delta Job_{it} = g(\alpha_1 Treat_i * Post_t + \alpha_2 Post_t + \alpha_4 \mathbf{x}_{it} + \gamma_i + \epsilon_{it}) \quad (18)$$

where  $\Delta Job_{it}$  is an indicator that takes a value of 1 if the individual changed her sector of employment. The sample does not include transitions into self-employment. I have already demonstrated that the reform increased transitions into self-employment for individuals offered the option to purchase a state-owned home; these results focus on other transitions from the state sector. An estimate of  $\alpha_1 > 0$  supports the hypothesis that employer-provided housing reduces job mobility. I also estimate

$$\Delta Job_{it} = g(\alpha_1 Treat_i * Post_t + \alpha_2 Post_t + \alpha_3 Treat_i * Post_t * \tilde{h}_i + \alpha_4 \mathbf{x}_{it} + \gamma_i + \epsilon_{it}) \quad (19)$$

where  $\tilde{h}_i$  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. The model predicted that individuals with more generous rent subsidies before the reform should also have higher average unobservable abilities in as entrepreneurs ( $\theta_i$ ) and private wage employees ( $\zeta_i$ ). Thus, a positive coefficient of the triple interaction,  $\alpha_3 > 0$ , provides evidence that the housing benefits of the reform reduced job mobility among state sector workers.

The results of equation 19 are presented in columns 1 and 3 of Table 11, and equation 18 in columns

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<sup>23</sup>Cadres are individuals who hold administrative or managerial positions in state-owned enterprise, government or the Communist party.

Table 11: Fixed Effects Logit Estimates of Job and Residential Mobility

	Change in Employment Sector				Residential Move			
	Control = Private		Control = State		Control = Private		Control = State	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat*Post	0.758 [0.286]*	-0.758 [0.566]	0.808 [0.321]*	-0.726 [0.595]	1.012 [0.333]*	0.654 [0.773]	1.354 [0.406]*	1.265 [0.817]
Treat*Post* $\tilde{h}$		0.455 [0.148]**		0.439 [0.152]**		0.051 [0.197]		0.056 [0.197]
Post	-0.283 [0.297]	-0.259 [0.300]	-0.297 [0.367]	-0.258 [0.370]	0.463 [0.247]+	0.315 [0.255]	0.194 [0.329]	-0.274 [0.353]
Obs	1231	997	928	793	803	631	546	431

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 cubic in age and a constant term. Employment sector is an individual-level variable, and residential move is measured at the household level. The regressions of change in employment sector do not include transitions into self-employment.

2 and 4. The estimates show that sectoral mobility of state employees in public housing before the reform increased significantly relative to both of the control groups. The relative odds of switching out of the state sector was over two times higher (and the relative *log* odds was 0.8 times higher) for the treatment group relative to either the state-employed comparison group (column 3) or privately-employed comparison group (column 1). The estimates of equation 18 are also similar across both samples. They indicate that a 100% increase in the average pre-reform rent subsidy correspond with a 0.45 times greater relative log odds of leaving the state sector. The results suggest that the increase in 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.

I also examine the impact of the reform on residential mobility. While I have demonstrated that the housing reform affected transitions into self-employment and other changes in employment, residential mobility also provides insight into the economic effects of the housing reform. Residential mobility directly affects the range of employment opportunities that individuals face and the employment choices that individuals make. Columns 5 and 7 of Table 11 show the impact of the reform on residential mobility within the sample area.<sup>24</sup> The coefficients on *Treat \* Post* suggest that the housing reform significantly increased the physical mobility of individuals. The relative odds of an individual in the treatment group moving increased by a factor of 2.8 (=  $\exp(1.01)$ ) relative to the non-state control group, and by a factor of 3.9 (=  $\exp(1.35)$ ) relative to the state control group. I also estimate 18 with the dependent variable as residential moves rather than sector changes. While the estimates of *Treat \* Post \*  $\tilde{h}$*  are positive, they not statistically different from

<sup>24</sup>The survey is not designed to follow individuals who move to cities or provinces outside of the sample areas, but the patterns in attrition from the sample across the treatment and control groups are similar to the patterns of migration within the sample area.

zero. Overall, the evidence suggests that the pre-reform provision of housing by state work-units served to decrease job and residential mobility of state employees.

### 7.3 Wage Growth

Patterns in wage growth for individuals who change sectors provide a second test of the relevance of bundling housing and employment in the state sector. 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:

$$w_{p,t} - w_{s,t-1} = \beta_0 + \beta_1 PH_{89} * Post + \beta_2 \mathbf{Year}_t + \beta_3(x_t - x_{t-1}) + v_t \quad (20)$$

where  $w_{p,t}$  is the log real earnings of the individual who is no longer in the state sector in period  $t$ , and  $w_{s,t-1}$  is the log real wage in the state sector in period  $t - 1$ . The specification also includes year dummies, denoted by the vector  $\mathbf{Year}_t$ , 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,  $PH_{89}$ , is an indicator that takes on a value of 1 if in public housing in 1989. I use this measure of public housing tenants in 1989 rather than the treatment group defined by housing status in 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 estimate a regression that includes the triple interaction,  $PH_{89} * Post * \tilde{h}$ . In this specification, a negative coefficient on the triple interaction suggests that state provision of housing reduced job mobility.

The first column of Table 12 supports the idea that housing benefits influenced labor mobility out of the state sector. Public housing residents leaving the state sector needed 31% 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 estimate corresponds well with the idea that the value of housing benefits that state workers influenced their job tenure decisions. Column 2 also includes  $PH_{89} * Post * \tilde{h}$ . While the coefficient estimates on  $PH_{89} * Post * \tilde{h}$  and  $PH_{89} * Post$  are both negative, they are not individually different from zero at the 10% level. As a robustness check, I also control for differences in the characteristics between individuals in  $PH_{89}$  and state employees not living in public housing in 1989. Following section 7.3, I estimate the propensity to be in the public housing in 1989. The results of this step are shown in column 2 of Table 15. I include the propensity score and its interactions with  $Post$  and  $Post * \tilde{h}$  to control for differences in observable characteristics between those in public housing and those in private housing. These results are displayed in column 3 of Table 12. Again, the coefficients on  $PH_{89} * Post * \tilde{h}$  and  $PH_{89} * Post$  are negative, but they are not significantly different from zero.

Table 12: First-Difference Estimates of Log Hourly Wage Growth of Former State Employees

	(1)	(2)	(3)
PH <sub>89</sub> *Post	-0.312 [0.138]**	-0.273 [0.274]	-0.233 [0.274]
PH <sub>89</sub> *Post* $\tilde{h}$		-0.032 [0.061]	-0.06 [0.106]
Propensity <sub>89</sub> *Post			-0.966 [0.466]*
Propensity <sub>89</sub> *Post* $\tilde{h}$			0.086 [0.121]
Observations	275	272	266
Adjusted R <sup>2</sup>	0.15	0.14	0.14

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.

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. I examined two simple tests of job lock and both results 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.

## 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 introduces distortions to people's 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 through better job matches. One explanation suggests that the the conversion of state assets to private property allowed individuals to capitalize on the value associated with the 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 of this paper 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 in China 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. Furthermore, policy makers have the opportunity to encourage small businesses by promoting private ownership of assets and 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 fully 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. However, the costs associated in the generous transfer of state housing assets were enormous. I have argued that this reform allowed constrained individuals with entrepreneurial ability the opportunity to begin profitable business ventures and increased labor mobility out of the sizable state sector, but measuring the returns to privatizing public housing assets requires more analysis on the costs associated with the reform. In addition to a cost-benefit analysis of the housing reform, it is also important for policy to understand the implications of the housing reform on the distribution of wealth. Research on the general equilibrium effects of the dramatic increase in the supply and demand for private housing is a topic for future research.

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## 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  $\eta_i$  and  $\pi_i$  on consumption and housing, respectively:

$$U_i(C_{ij}, h_{ij}) = \eta_i \ln C_{ij} + \pi_i \ln h_{ij} \quad (21)$$

The budget constraints remain the same as specified in equations 2 - 4. In the private sector, the optimal value of consumption is given by

$$C_i^* = \frac{Y_{pi} + W_i}{1 + \frac{\pi_i}{\eta_i}} \quad (22)$$

and the optimal value of housing is given by

$$h_i^* = \frac{\pi_i}{\eta_i + \pi_i} \frac{Y_{pi} + W_i}{q} \quad (23)$$

This yields the total value of private employment as

$$V_{pi} = \eta_i \ln \left[ \frac{\eta_i}{\eta_i + \pi_i} (Y_{pi} + W_i) \right] + \gamma \ln \left( \frac{\pi_i}{\eta_i + \pi_i} \frac{Y_{pi} + W_i}{q} \right) \quad (24)$$

When heterogeneity in housing outcomes is determined by connections in the state sector,  $\mu_i$ , a simple sorting rule results where the probability of an individual choosing the state sector increases with  $\mu_i$ . 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 changing housing preferences affect the utility in each sector. The derivative of  $V_{pi}$  with respect to  $\pi_i$  is

$$\frac{\partial V_{pi}}{\partial \pi_i} = \frac{-\eta_i}{\eta_i + \pi_i} + \ln \left( \frac{\pi_i}{\eta_i + \pi_i} \frac{Y_{pi} + W_i}{q} \right) + \frac{\eta_i}{\eta_i + \pi_i} = \ln \left( \frac{\pi_i}{\eta_i + \pi_i} \frac{Y_{pi} + W_i}{q} \right) = \ln h_{pi}^* \quad (25)$$

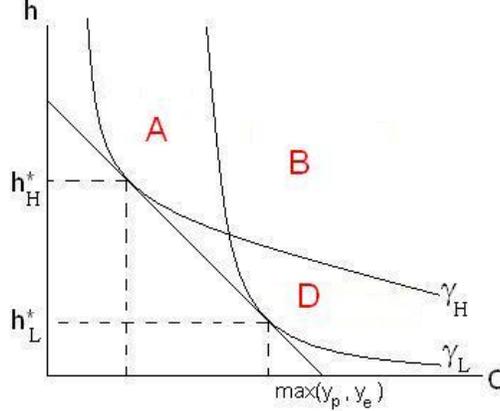
Similarly, for an entrepreneur

$$\frac{\partial V_{ei}}{\partial \pi_i} = \ln \bar{h}_{ei} \quad (26)$$

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

$$V_{si} = \eta_i \ln(Y_{si} + W_i) + \pi_i \ln \tilde{h}_i \quad (27)$$

Figure 7: Employment Choices Varying by Housing Preferences



and the derivative of  $V_{si}$  with respect to  $\pi_i$  is

$$\frac{\partial V_{si}}{\partial \pi_i} = \ln \tilde{h}_i \quad (28)$$

These derivatives suggest that sector choice is not monotonically related to housing preferences as it was with state connections ( $\mu_i$ ). How an increase in an individual's preference for housing while holding all else constant 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 ( $\tilde{h}$ ) and of the level they could afford in the private sector ( $h^*$ ). Figure 7 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. Clearly, 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 fell 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,  $\zeta_i$ . 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  $(\tilde{h}, \theta)$  space, we can use the implicit function theorem on equation 12. In the case of a constrained entrepreneur,

$$\frac{\partial \theta}{\partial \tilde{h}} = \frac{U_h^s}{U_c^e[(\theta f'(k^{cc}) - r) \frac{\partial k}{\partial \theta} + f(k^{cc})] - (qU_c^e - U_h^e) \frac{\partial \tilde{h}}{\partial \theta}} \quad (29)$$

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 9, 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  $\frac{\partial \theta}{\partial \tilde{h}}$  is positive.

For an unconstrained entrepreneur,

$$\frac{\partial \theta}{\partial \tilde{h}} = \frac{U_h^s}{U_c^e f(k^*)} \quad (30)$$

and the slope of the curve given by 30 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 29 and 30 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 captures in the size of  $(\theta f'(k^{cc}) - r) \frac{\partial k}{\partial \theta}$  and  $(qU_c^e - U_h^e) \frac{\partial \tilde{h}}{\partial \theta}$ .

The second derivative of equation 12 for the constrained entrepreneur is

$$\frac{\partial^2 \theta}{\partial \tilde{h}^2} = \frac{U_{hh}^s}{U_c^e f(k^{cc}) + \frac{\partial \tilde{h}}{\partial \theta} (qU_c^e - U_h^e)} \quad (31)$$

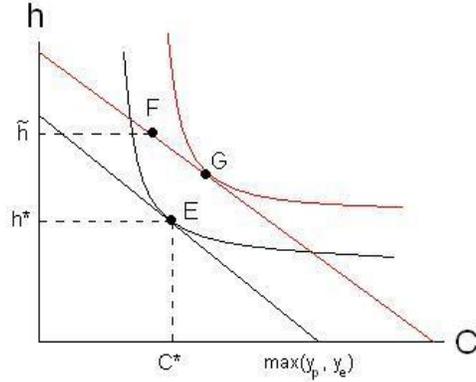
and for the unconstrained entrepreneur, it is

$$\frac{\partial^2 \theta}{\partial \tilde{h}^2} = \frac{U_{hh}^s}{U_c^e f(k^*)} \quad (32)$$

For both cases, the sign of  $\partial^2 \theta / \partial \tilde{h}^2$  is negative because  $U_{hh} < 0$ . Thus, the boundary given by equation 12 is concave as shown in Figures 1-3.

To see that the threshold level of  $\theta^+$  falls as credit constraints are alleviated, as shown in Figure 3, recall that  $\theta^+$  is the level of  $\theta_i$  that solves equation 11. For a given level of  $\theta_i$ , 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,  $Y_{pi}$ , the threshold level of  $\theta_i$  must fall as  $k^{cc} \rightarrow k^*$ .

Figure 8: Efficiency Loss of State Housing Provision



## C Inefficiency of Employer-Provided Housing

To understand how the state provision of housing leads to inefficient job matches, consider an individual who would select  $C^*$  and  $h^*$  if she worked as a private wage employee or as an entrepreneur. The optimal allocation of her earnings between consumption and housing is given by point E in figure 8. She will only work in the state sector if her wages and state housing allocation place her on a higher utility curve than the one she can achieve working outside of the state sector. Suppose her compensation in the state sector is given by point F. Working in the state sector brings her to a higher utility curve, so she chooses to work at a state-owned enterprise. However, the state provision of housing leads to a social loss in this case. For the same amount of resources, as denoted by the budget constraint that runs through point F, she could achieve a higher level of utility (at point G) if she were paid that amount in cash and allowed to determine how much of her compensation to spend on housing. In the area over which we observe state employees (to the right of the utility curve that could be achieved in the private sector), there is only a single path of points (including points E and G) that is socially efficient. Given the limited stock of state-owned housing in a local area and the lumpy nature of housing provision, it is unlikely that the assignment of state housing can correspond well to the single path of efficient allocations.

## D Heterogeneity in the Value of Wealth Transfer

As suggested by the theoretical framework in section 3, *ceteris paribus*, a positive relationship between wealth and entrepreneurship provides evidence that credit constraints matter for small business formation. By exploiting the exogeneity of the wealth transfers associated with the reform, I avoid the potential correlation between wealth and individual unobservables. In this section, I examine the prediction that larger wealth transfers correspond to greater likelihood to enter self-employment. Because the questions in the CHNS do not allow me to quantify the household-level wealth effect associated with the housing reform, I use

Table 13: Estimates of Net Wealth Effect of the Housing Reform

	Coefficient	Standard Error
Age	1.649	[0.442]**
Age <sup>2</sup>	-0.016	[0.005]**
Education	-0.335	[0.201]+
Log(Area of Home)	11.530	[1.946]**
Province Population	-0.002	[0.000]**
Province GDP	0.004	[0.001]**
Observations	1866	
Adjusted R <sup>2</sup>	0.05	

Notes: \*\* denotes significance at the 1% level, and + at the 10% level. The regression also includes a constant term. Area of the home is in meters squared. The net wealth effect is in tens of thousands of yuan and equals the difference between the market value and the government price charged.

supplementary data from the 1995 wave of the Chinese Household Income Project (CHIP). The goal of this survey is to measure the income distribution in rural and urban areas of China, so numerous detailed questions about employer-provided benefits were asked.

In the CHIP data, I construct the net gain in housing wealth associated with the subsidized sale as the difference between the self-reported market value of the home and the self-reported purchase price for individuals who bought their home from their state employers. I predict the value of this household-level transfer with variables that exist in both the CHNS and the CHIP. I construct an predicted value for the wealth transfer for each household in the CHNS using the values of the explanatory variables in the CHNS weighted by the coefficients estimated in the CHIP. In other words, I estimate the following equation with the urban sample in the CHIP:

$$T_i = \beta_0 + \beta_1 \mathbf{z}_i + \beta_2 \mathbf{s}_j + \psi_{ij} \quad (33)$$

where  $T_i$  is the net gain in housing wealth,  $z_i$  are characteristics of the house or the household, and  $s_j$  refers to province-level characteristics.<sup>25</sup> I use the coefficients from equation 33 to estimate  $\widehat{T}_i$  in the CHNS. I then include the predicted values of  $\widehat{T}_i$  directly in regression given by

$$d_{ijt} = m(\alpha_1 \text{Treat}_i * \text{Post}_t + \alpha_2 \text{Treat}_i * \text{Post}_t * \widehat{T}_i + \alpha_3 \text{Post}_t + \alpha_4 \mathbf{x}_{ijt} + \gamma_i + \epsilon_{ijt}) \quad (34)$$

where  $d$  is either self-employment or capital. A positive estimate of  $\alpha_2$  would support the hypothesis that the housing reform reduced credit constraints.

The estimates of the net transfer of wealth with the 1995 CHIP data are presented in Table 13. The

<sup>25</sup>I use province-level characteristics rather than province fixed effects because the majority of the provinces in the CHIP and the CHNS do not overlap.

Table 14: Heterogeneity in the Impact of the Reform by the Value of the Transfer

	FE Logit		FE Logit		Fixed Effects	
	Self-Employment		Positive Assets		Log(Capital)	
	<i>Control=Private</i>	<i>State</i>	<i>Private</i>	<i>State</i>	<i>Private</i>	<i>State</i>
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Post* $\widehat{T}$	0.06	0.04	0.03	0.03	0.07	0.02
	[0.19]	[0.20]	[0.03]	[0.03]	[0.06]	[0.06]
Treat*Post	1.91	1.94	0.49	0.85	-0.45	0.54
	[2.70]	[2.88]	[0.84]	[0.87]	[1.54]	[1.74]
Post	-1.29	0.83	-1.70	-1.90	0.18	1.11
	[0.33]**	[0.51]	[0.20]**	[0.26]**	[0.27]	[0.36]**
p-Value of Treat*Post and Treat*Post* $\widehat{T}$	0.03	0.06	0.01	0.01	0.03	0.16
Observations	994	406	1104	776	492	282

Notes: Standard errors in brackets. \*\* denotes significant at the 1% level and \* at the 5% level. Controls include a quadratic in age and fixed effects. The regressions of log(capital) also include a constant term.

explanatory variables include the age and education of the household head, the total area of the home, and province-level GDP and population.<sup>26</sup> A doubling of the floor space of the home implies an increase of over 11,000 yuan in the value of the wealth gain. A one standard deviation increase in average GDP of the province translates into a higher gain of 4,820 yuan. These coefficient estimates are combined with the data in the CHNS to calculate  $\widehat{T}$ .

The results with  $\widehat{T}$  are shown in Table 14. While all of the coefficients for  $Treat * Post * \widehat{T}$  are positive, they are not significantly different from zero. The magnitude of the estimates are also quite small. A one-standard deviation increase in the value of the transfer (6,800 yuan) translates into a 1.3 to 4.8% increase in capital. I also test for the joint significance of  $Treat * Post * \widehat{T}$  and  $Treat * Post$  and find them to be jointly different from zero at the 5% level in a majority of the estimates. This suggests that there is not enough variation in the estimates of  $\widehat{T}$ . Indeed, the adjusted  $R^2$  in table 13 is only 5%. The variables that I use are not able to capture much of the variation in the transfer of wealth at the time of the reform. Unfortunately, this analysis is limited by the variables that are available in both the CHNS and the CHIP. The results in Table 14 do not contradict the idea that individuals with greater gains in wealth were more likely to enter into entrepreneurship following the reform, but there is not enough power in my estimates of the wealth transfer to provide strong support for the credit constraints hypothesis.

A strong, positive correlation between the size of the wealth transfer during the reform and the the size of the rent subsidies received before the reform would complicate the interpretation of the coefficient of  $Treat * Post * \widehat{T}$ . However, the data do not provide evidence for a high correlation between  $\widehat{T}$  and  $\tilde{h}$ . In

<sup>26</sup>The dependent variable is in levels because the distribution of those values appear normal.

the CHNS, the correlation between the predicted value of  $\hat{T}$  and the pre-reform, reported level of  $\tilde{h}$  is quite low. For individuals in the treatment group, the correlation is 0.068. Furthermore, using two waves of data from the Chinese Household Income Project, I find a small, negative correlation between the province-level correlations between the average rent subsidy in 1988 and the average difference between the market value and the price paid by former public housing tenants.

## E Appendix Tables

Table 15: Estimates of the Propensity Scores

	Treatment (1993)	Public Housing in 1989
	(1)	(2)
Age	0.131	0.124
	[0.067]*	[0.036]**
Age <sup>2</sup>	-0.001	-0.001
	[0.001]	[0.000]**
Years of Education	0.286	0.082
	[0.021]**	[0.016]**
Male	-0.370	-0.421
	[0.136]**	[0.116]**
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.

Table 16: Probit 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]	0.002 [0.004]	-0.053 [0.022]*	-0.043 [0.023]*	-0.051 [0.035]
Age	0.000 [0.001]	-0.001 [0.002]	0.015 [0.009]+	-0.026 [0.012]**	0.018 [0.018]
Age <sup>2</sup>	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]*	0.000 [0.000]	-0.004 [0.003]	-0.003 [0.003]	-0.018 [0.004]**
Male	0.000 [0.002]	0.000 [0.003]	0.003 [0.019]	0.001 [0.017]	0.009 [0.025]
Log(HH size)	-0.007 [0.005]*	0.001 [0.004]	-0.008 [0.034]	0.072 [0.032]*	0.025 [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.

Table 17: Impact of Housing Price Appreciation on Self-Employment and Capital

	<u>FE Logit</u>		<u>FE Logit</u>		<u>Fixed Effects</u>	
	<u>Self-Employment</u>		<u>Positive Capital</u>		<u>Log(Capital)</u>	
	<i>Control=Private</i>	<i>State</i>	<i>Private</i>	<i>State</i>	<i>Private</i>	<i>State</i>
	(1)	(2)	(3)	(4)	(5)	(6)
Treat*Post* $\Delta p$	2.654 [4.157]	2.162 [4.515]	9.951 [1.776]**	8.880 [2.039]**	13.138 [1.841]**	8.439 [1.808]**
Treat*Post	0.809 [2.499]	0.422 [2.425]	0.438 [0.553]	0.777 [0.646]	-1.548 [0.265]**	-3.474 [0.474]**
Post	-1.498 [1.320]	1.053 [1.103]	-0.738 [0.382]+	-1.193 [0.380]**	0.519 [0.292]	2.810 [0.514]**
Post* $\Delta p$	-0.498 [2.387]	0.374 [1.324]	-3.300 [0.563]**	-2.335 [0.586]**	-0.931 [0.443]+	1.231 [1.829]
Treat*Post* $\Delta CPI$	0.012 [1.497]	0.161 [1.692]	-2.078 [1.285]	-1.946 [1.410]	-2.957 [1.212]*	-0.389 [1.007]
Treat*Post* $\Delta \bar{W}$	6.089 [7.278]	4.401 [8.209]	5.894 [4.136]	3.463 [3.797]	15.497 [3.520]**	15.807 [2.500]**
Post* $\Delta CPI$	0.632 [0.978]	-0.561 [0.688]	1.062 [0.436]*	0.958 [0.407]*	0.045 [0.321]	-0.294 [0.826]
Post* $\Delta \bar{W}$	-1.764 [4.296]	-0.087 [2.098]	-7.147 [1.346]**	-4.518 [2.128]*	-1.931 [1.523]	-7.104 [2.287]*
Observations	893	389	1224	871	555	330

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.