Unbundling Property Rights:
Urban Housing Privatization and Labor Mobility in China[*]

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(Preliminary Version)

Abstract

This paper examines the effect of urban housing privatization on labor mobility in China. The reform untied housing access from state sector employment and transferred property rights from the state to individuals. We exploit city-specific timing of the reform for identification and show that the reform can explain approximately one-fourth of the increase in urban labor supply for the private sector during 1986-2005. We do not find any effects on entrepreneurship, credit or consumption. This suggests that the main impact of housing privatization was to decrease the opportunity cost of working for the private sector.

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1   Introduction

Tying access for urban housing to working in state sector jobs is a common feature in communist countries such as China, the former U.S.S.R. and Vietnam. In their transition to market economies, all of these countries have undergone urban housing privatization reforms, where access to housing was disassociated from state employment and property rights were transferred from the state to individuals. Private property rights are considered to be essential for economic development by many economists.[1] In the specific context of employer-provided housing, untying housing access from state jobs decreases the opportunity cost of working for the potentially more productive private sector.[2] The right to private property could facilitate the conversion of property into other usable assets through credit channels. For example, De Soto (2000) argues that the real estate occupied by urban squatters contains vast amounts of potential wealth that can be transformed into capital for entrepreneurial ventures through the formalization of property rights.[3] The right to private property may also be associated with an increase in wealth, which may in turn affect labor market and investment decisions.[4] That said, there is almost no empirical evidence on the effects of transferring property rights from the state to individuals in the context of transition economies, or on the effects of untying housing access from state employment in any context. As of now, we simply have little idea of how important these reforms are for the economic development of transition economies.

This paper aims to fill this gap by investigating the effect of urban housing reforms in China. During the late 1980s and the 1990s, households in most Chinese cities were offered the chance to purchase the apartments that they rented from the state, thereby.untying access to housing from working in the state sector and giving urban residents a chance to become private homeowners. These reforms were enacted in at least 50 cities, potentially affecting more than 90 million people. In this sense, this is the largest urban housing reform in the

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[1]See, among others, North and Thomas (1973), Knack and Keefer (1995) and Acemoglu, Johnson and Robinson (2001) for analyses of general property rights institutions.

[2]See Fishback (1992) and Wang and Murie (1999) for studies on some of the problems of state housing.

[3]Banerjee and Newman (1993) provides a theoretical framework of the relationship between credit constraints and entreprenurialship. See Banerjee and Duflo (2004), De Mel, McKenzie and Woodruff (2007), and McKenzie and Woodruff (2006) for microeconomic studies of the importance of credit constraints for firms; Udry and Anagol (2006) provide evidence on the importance of credit for farmers.

[4]Evans and Jovanovic (1989), Paulson and Townsend (2004), and Djankov, Qian, Roland and Zhuravskaya (2006) have shown that wealth is positively correlated with private entreprenuership in the United States, Thailand and China respectively.

world. We study the effects of these reforms on the labor market decisions of households, specifically on private sector employment and entrepreneurship. We then exploit the richness of our data to investigate the forces that drive these reduced form effects.

Most empirical studies of the impact of changing urban property rights focus on reforms which extended secure property rights to squatters or other informal residents on the property. Two recent examples of this are papers by Galiani and Schargrodsky (2006), who find that urban land titles lead to increased investment in housing in Buenos Aires, and Field (2007), who finds that providing secure property rights to squatters in Peru is associated with an increase in labor market participation.[5]

Our study differs from these previous ones in three major ways. First, the reforms in China were comprehensive and affected the vast majority of the urban population. Given that migration between cities is severely restricted by policy, we can assume that each city forms a self-contained labor market. Our study will thus capture the net of general equilibrium and individual effects of the reform. In particular, if we interpret the housing privatization as a large supply-side shock to labor supply in the private sector, our data, under reasonable assumptions, enable us to estimate the elasticity of labor demand in the private sector. Second, tenancy for state owned housing was reasonably secure in urban China. The increased security of tenure, which is the mechanism emphasized in prior studies of squatters and illegal residents, is not likely to be a big factor in the Chinese context. Third, private property rights were phased in. The privatization reforms we study transferred only the rights to use, mortgage and bequeath housing. The rights to lease, buy and sell such housing were granted by separate regulations enacted 5-7 years after this privatization reform. The current version of the paper focuses on the privatization reforms; we plan to analyze the impact of trading reforms in future work.

Our empirical strategy exploits city level variation in the timing of the reform that privatized housing. We collected data on the dates of the privatization reform from official city newspapers stored in the archives of the National Library of China in Beijing. The reform dates for cities in our sample range from 1988-1997. We then match this data to household

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[5]Interestingly, both these studies find little impact of property rights on the credit market, which may partly be a result of the fact that these reforms extended private property rights to people at the bottom of the income distribution. Some studies of rural land reform also fail to find an effect on access to credit (Do and Iyer, 2008; Braselle, Gaspart and Platteau, 2002).

level data from the Urban Household Survey (UHS) conducted by the National Bureau of Statistics. The matched data set forms a repeated cross section of households from a panel of 24 cities over the period 1986-2005, with approximately 100,000 household-year observations. We then compare household outcomes across cities that had implemented privatization reforms and those that had not, before and after the reforms were implemented. We control for a set of factors such as household characteristics and city government budgets, as a way to adjust for the fact that housing reforms may be enacted at times when the city budget is particularly low. Our identification assumption is that the reforms are exogenous to other factors that affect labor market decisions beyond our controls. We show that our results are robust to including controls for province level employment trends, as a proxy for labor demand shocks over the same period, and for city-level GDP growth rates.

We find that the privatization reform caused private ownership of housing to increase from zero to approximately 50% of urban households. This led to a three percentage-point increase in labor in the private sector. But it had no effect on entrepreneurship as measured by the probability that a household head or his/her spouse will own a private business. Between 1986 and 2005, the fraction of our sample that worked in the private sector increased from zero percent to approximately 6%. A simple back of the envelope calculation using our estimated effects of the reform on the probability that a household works for the private sector suggests that the housing privatization reform can explain approximately 24% of the total observed increase in private sector labor supply in China during this period.

The untying of housing from state employment is probably the main driving force for this result, rather than the acquisition of private property rights. While we cannot test this directly, we can individually test some of the implications of the different channels of private property rights. First, we examine the importance of the credit channel. Though households in China at the time did not have access to credit from formal financial institutions for small businesses, private property can potentially be used as collateral for informal loans. We do not expect this to play an important role in the China context because the extent to which housing can be used as collateral in a secured loan is limited by the ability of the informal lender to evict defaulting borrowers. The lack of a well-developed judicial system for contract enforcement therefore greatly decreased the value of apartments as collateral in the informal market. In addition, there is some reason to believe that strong local networks, high private

savings rates, and government migration restrictions which decrease monitoring costs have allowed family networks and informal lenders to supply most of the credit needs of small businesses so that small private entrepreneurs in China are not severely credit constrained.[6] We do not need to take these assertions for granted. Using household data on loans, we can check whether credit plays an important role by estimating the effect of the reform on the probability of a household having a loan, or on the amount of loans households carried. We find no effect.

Then, we investigate whether moving to the private sector improved wages. We find no effect on either total household income or labor income, suggesting that individuals move into jobs in the private sector which pay similarly to state sector jobs. The lack of a wage difference may be surprising since most economists think of the private sector as the more productive sector. But note that since we capture the general equilibrium effects, the increase in labor supply to the private sector is likely to decrease wages in that sector. In future work, we will investigate whether this massive movement of labor into the private sector increased production by estimating the effect of the reform on city level GDP.

Any potential wealth effects are likely to be unimportant in the case of the privatization reforms, because households could not sell the newly acquired housing until the cities enacted the trading reforms. We will address this issue in our future analysis of trading reforms.

To the best of our knowledge, there is only one other micro-econometric study of the effects of untying housing from state employment. Wang (2008) studies the effect of the same reform as in this paper on labor market decisions. There are two main differences between her study and ours. The first pertains to the identification strategy. Wang (2008) assumes that the reform occurred on or after 1994 for all cities, probably because the *Urban Real Estate Administration Act* was officially enacted by the national government in 1994. But as our data shows, that majority of the cities in China had already passed regulations on housing privatization by then.[7] Wang's control group– of people in private housing on or before 1994– therefore includes people who had already been exposed to, and taken advantage

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[6]Recent field work by Dollar, Qian and Wei (2008) suggests that small businesses rely on a well-functioning informal credit market that provides competitive interest rates (as low as 11% per annum).

[7]Such action by provincial and local governments is a feature of many institutional changes in China. For instance, at least half the provinces had decollectivized agriculture using the Household Responsibility System before the national government enacted the official policy in 1980.

of, the housing reforms. Her treatment group of those who did not have private housing in 1994, in turn, includes people who had been exposed to the housing reforms, but had not yet taken advantage of it, creating a potential selection bias. The second difference between our studies is that she finds a large positive effect on entrepreneurship and the importance of credit channels in driving that effect. We find no effect on entrepreneurship or credit. The latter is not surprising given the institutional context of China.[8]

Our study makes several important contributions. As a study of property rights, it shows that in a context where tenancy is secure and credit channels do not operate, then property rights do not have obvious benefits in terms of income, consumption, or the quality of housing. If we view housing access as a form of employer benefits, our results show that untying this benefit has large effects on labor mobility. Our context differs from the usual context of studies of employer benefits in that before the reform, workers could not obtain housing from the market. Hence, it is a rather extreme example. As a study of transitional economies, our results show that a relatively simple reform can make a huge impact on providing labor for the private sector. Interestingly, the findings indicate that the shift to the private sector does not improve wages. The market competition effect caused by the increase in labor supply may have offset potential gains in individual wages caused by switching to the more productive sector.

The rest of the paper is structured as follows. Section 2 documents the process of urban housing reforms in China's cities and Section 3 outlines the potential impacts of such reforms. Sections 4 and 5 describe our data and empirical strategy. Section 6 presents the results. Section 7 interprets the results. And section 8 offers concluding remarks.

2 Urban Housing Privatization in China

Prior to economic reforms in 1978, housing in Chinese cities was provided to households through their work units in return for a highly subsidized rent.[9] As a result of subsidized

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[8]Wang's use of housing price appreciation as a measure of relaxing credit constraints is also not adjusted for the fact that households could not trade their housing to realize this value. In our data, no city enacted the trading regulations before 1998.

[9]Formal ownership of land in China rests with the state, and households are now given long-term use-rights on the land (typically 50-75 year leases).

housing and inadequate provision by the work units, the demand for housing had always exceeded the supply by a wide margin, and long waiting lists for state-owned apartments were common. Another problem emerged during the reform era as the government began to develop the private sector: the linking of housing to the work unit restricted labor mobility (Meng, 2000).

Housing reform began in the mid-1980s, as part of the general movement towards a market economy and also because the maintenance cost of the state-owned apartments exceeded the nominal rents paid by the residents (Duda, Zhang and Dong, 2005). The first steps in the reform process were to increase rents, and move from an implicit to an explicit rental subsidy. In 1986 the State Council chose six cities (Shenyang, Tangshang, Yantai, Bangbu, Changzhou, and Jiangmen) to experiment with this reform and in 1988, at a State Council Housing Reform Meeting, all other cities were encouraged to implement a similar type of reform. The rent increases did not resolve the problem of the restriction on labor mobility, and after 1988, some cities pushed the reform agenda further to include selling of old state owned housing (Yuan, 2000 and Pan, 2000).

The completely privatization of urban housing was only officially adopted by the central government several years later. In 1991, the State Council organized the second housing reform meeting which decided that part of the work unit owned housing could be sold to its own employees at a subsidized price. Work units were instructed to set up Housing Provident Funds, where workers could save money to buy the newly privatized public housing.[10] In 1993, at the third housing reform meeting the State Council announced pricing rules for selling new or old public housing (Yuan, 2000 and Pan, 2000). The basic idea was that the price of a new apartment should not be higher than three times the average household annual income in a city. If buying an older house, the price should be adjusted according to a depreciation formula that fully depreciated the house over 75 years. In addition, there were different concessions implemented. One concession was based on job tenure. The longer the tenure at the work unit the higher the concession (the price reduction). Furthermore, work units had

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[10] This scheme was copied from the Singaporean model. It is similar to a Roth IRA in the U.S. Individuals can contribute up into a savings deposit and employers must match X% up to Y amount. Both X and Y are determined by policy, which varies by region. The central government have certain rules. For example, the amount cannot be withdrawn within two years.

The scheme was introduced in 1991 and taken up by some cities by 1993 and nationally later. The actual nationalization of the policy probably did not happen until many cities were already doing it (reference).

discretion to price houses differently according to location and quality. Of course, at highly subsidized prices, the number of square meters one could purchase was capped, with the cap level depending upon the workers rank. For example, if a minister was entitled to 250 square meters then a governor-general was entitled to 180 square meters and so on (Wang, 1993).

The reforms served as a one time transfer of housing from the state to individuals. Once the property right was accepted, workers lose the right to subsidized rent. However, in most cases, there was the option of continuing to rent. The exception is for regions designated for development by the city. For example, residents in many older neighborhoods of central Beijing were forced to move to newer housing in other parts of the city in order to clear land for development related to the 2008 Olympics. Anecdotally, most residents were happy to move since they were given newly constructed housing of better quality in the same city, and also compensated with a cash transfer from the state.

In 1994, the national government enacted the Urban Real Estate Administration Act (which took effect in 1995), which further facilitated the transfer of land-use rights from the state to land users by means of tender, auction or negotiated agreements. At this time, the State Council decided that housing purchased from work units could be sold in the market five years after the purchase date. All cities then passed a specific regulation regarding the granting of trading rights to households. The timing of this trading reform also varied considerably across cities; we plan to analyze the impact of these reforms in future work.

In 1998, the State Council made the decision that there would be no more in-kind allocation of housing from work units to employees; all housing assets had to be purchased either from the market or from the work unit at market prices. By 2000, this had been implemented in most provinces (Yuan, 2000; Pan, 2000). After 2000, reforms in the housing sector have been focused on developing and regulating the housing loan market.

In sum, like many economic reforms in China, housing reform was carried out at different times and with slightly different procedures across cities; local implementations came first and central approval in the form of official policies followed.

3 Potential Impact of Housing Reforms

The urban housing reforms are likely to affect household labor market behavior through three main channels. First, the privatization reforms broke the link between housing and state employers i.e. it increased workers' job mobility, since they did not need to worry about access to housing. We are thus more likely to observe workers changing jobs, and perhaps moving from the state-controlled sector to the private sector or starting their own businesses. We can directly test this using data on the sector of occupation of the household head and his/her spouse. We can also look whether households started business ventures, now that they had an asset as well as a place of business to operate from.

The second effect is with respect to property-related financial transactions. The privatization reforms gave households a mortgageable asset, which they could use to obtain loans for other investments or expenses. We therefore check whether households are able to obtain more credit after the reforms are enacted.

Finally, the reform may affect the wealth of households. In the context of the Chinese reforms, the direction of the wealth effect in not a priori obvious because state housing was made available at very subsidized rents. Private property rights only have a positive wealth effect if the net present value of gains from real estate price increases outweigh the value of losses due to the loss of government subsidized rent. If this is not true, then households will be made worse off unless if they are given the option of not taking up private property rights. For the most part, households were given the option to continue renting from the state at the subsidized rate. So, for simplicity, we assume that the wealth effect was zero or positive in this discussion. (See section on background for more details). In the absence of a credit market, an increase in wealth could relieve constraints and lead to more entrepreneurship. More generally, increased wealth could encourage households to undertake riskier enterprises which could encourage workers to move to jobs in the newer and perhaps riskier private sector or private entrepreneurship.

Note that because formal banks did not make loans to small businesses and informal lenders could not easily evict defaulting borrowers to secure housing as collateral, any wealth effects are most likely not realized until the apartments can be sold. Therefore, we will use the second reform that allowed trading of privately owned housing on the market to estimate

wealth effects.

4 Data

We constructed a unique data set of reform dates by collecting information on the first date when private property rights were introduced in the city i.e. when workers were allowed to buy their houses from their work units, or when cities started to allocate land to private developers. We call this the date of the privatization reform. This information was obtained by a search of the local city newspapers, and we obtained these dates for 50 of China's largest cities. We match this data at the city level to a repeated cross section of households from the Urban Household Surveys (UHS) conducted by China's National Bureau of Statistics. The UHS is a 0.01% stratified random sample of the urban population; we have access to the UHS data for cities in 15 provinces. The matched sample gives us a repeated cross-section of households in 24 cities over 20 years (1986-2005), with approximately 100,000 household-year observations. We also collected information on overall GDP growth rates and employment rates at the provincial and city level from province and city level yearbooks.

Table 1 presents the years in which the cities in our sample implemented the privatization reform. The table clearly shows that different cities adopted the reform measures at different points in time. In particular, we note that most of the privatization reforms were already enacted before the Urban Real Estate Administration Act of 1994 was enacted by the national government.

Table 2 presents the descriptive statistics from the UHS data. On average, 68% of the sample lives in private housing. This proportion increased from 11% in 1986 to 80% in 2005, with a corresponding decline in the fraction of households living in public housing from 85% to 17% (Figure 1). Over the same period, the fraction of households where the household head was employed in a state-owned or collective-owned enterprise declined from 98% to 67% (Figure 2). At the beginning of our sample period, almost no household heads were employed in the private sector or had a business of their own; by 2005, 6% of household heads worked in a private sector enterprise and 4.4% had become entrepreneurs. 31% of household heads and 42% of spouses are not listed as being employed; these are mostly retirees or students since the employment rates in China are generally very high.

On average, households save 20% of their total income. Very few households, less than 1%, are recorded as having any housing loans, suggesting that the most households used their savings and/or informal sources of credit to finance their home purchases. This was partially because the housing loan sector was not well developed in the initial phase of privatization reforms, and the only formal source for most people would have been the Housing Provident Funds, which were set up and administered at the discretion of the work unit or sometimes the city governments. Almost no households are recorded as having other types of formal loans, such as car loans or education loans. Approximately 15% of households have a non-housing loan, and this fraction declines steadily over time, from 51% in 1986 to 5.7% in 2005 (Appendix Table 1 documents the summary statistics over time for more of our variables). Only 22% of households spend any money on home improvements or maintenance.

In terms of household characteristics, the average household head is 50 years of age (Table 2, Panel B). Almost 60% of household heads are men, and 12 % have a college education. The average household size is three individuals. Panel C reports the city and province level data. Average provincial GDP growth rates are 14%, and average city GDP growth rates are 19% over this period. Overall urban employment increased at a rate of 5% annually and the state share of urban employment declined steadily over time.

5 Empirical Strategy

Our empirical strategy will be to compare the decisions of households in different cities before and after the city passed the reform, using a panel data regression of the form:

$$Y_{ijt} = \alpha_j + \beta_t + \gamma Post_{jt} + \delta \mathbf{X}_{ijt} + \epsilon_{ijt} \tag{1}$$

where Y_{ijt} represents the outcome for household i living in city j and year t and $Post_{jt}$ is a dummy variable indicating whether city j has announced the privatization reform by year t. α_j is a fixed effect for city j, which will control for all time-invariant city characteristics such as geographic characteristics, initial GDP or initial involvement of the private sector. We include a set of year fixed effects β_t to control for changes over time which affect all the cities, such as nationwide economic growth, or nationwide reform policies. During the 1990s, China

witnessed a number of nationwide reforms, such as the end of the dual exchange rate system, reforms in the tax system, and of course the enactment of the 1994 real estate law. Our estimates are thus net of the effects of these nationwide changes. $\mathbf{X}_{ijt}$ is a vector of household characteristics such as age, gender and education of the household head. Note that because our sample of 24 cities is spread out over 16 provinces, we do not have much within-province variation. Hence, we are not able to control for province level controls in addition to city-level controls. The standard errors are clustered at the city-year level, to adjust for the fact that households in the same city are subject to common time-varying shocks.

Our coefficient of interest is γ, which represents the difference in outcomes before and after the privatization reforms are announced. The primary outcomes we will focus on are household labor market decisions, as well as outcomes such as income, consumption and savings.

Our estimates of the impact of housing privatization can be biased if the timing of such reforms is correlated with other city level unobservable (to the researchers) characteristics, such as local macro economic or political environment. For example, if a city adopted a certain housing reform measure at a particular year because its budgetary situation was most favorable for the reform at that point in time, simply controlling for the timing of the introduction of the reform will not allow us to disentangle the effect of a favorable budgetary situation from that of the housing reform.[11] We therefore control for lagged city incomes in the main specification. Another potential concern is that the housing reforms occurred in cities that were already experiencing rapid private sector growth. To address this, we will control for contemporaneous and lagged changes in province level urban employment and private-sector employment in our robustness checks. We will also control explicitly for pre-trends in private sector employment as a robustness check.

A third cause for bias in our estimates is that the housing reforms may have occurred at exactly the same time as other reforms that facilitated economic growth or private sector employment. In particular, we are concerned about a series of reforms commonly called the "enterprise reforms" during 1993-98 which shut down unprofitable state-owned enterprise reforms. For instance, in 1994, the Chinese government began an ambitious program to restructure state-owned enterprises (SOEs), called "grasping the big ones and letting go of the

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[11] Alternatively, cities might enact the housing reform at a time when their budget is particularly low, since one of the goals of the reform was to relieve the state of the maintenance cost of these apartments.

small ones" (*zhuada fangxiao*). This involved retaining government control of a core group of 1000 large SOEs, privatization small SOEs controlled by local governments and allowing bankruptcies and mergers of non-performing SOEs. To the extent that this was a national reform, our identification strategy which depends on city-level timing of the housing reforms will not be confounded. But if there was city-level variation in the timing of the enterprise reforms, which we have every reason to believe was the case, then our estimates will be confounded unless if our controls are correlated with the determinants of the enterprise reforms. Lagged city level GDP is most likely correlated with the health of the SOEs in a city since SOEs dominated the Chinese urban economy until very recently. We are also collecting data on city level GDP by sector. Then, we will be able to control for the pre-trend in city level GDP growth by sector, which is arguably a good predictor of the profitability of SOEs and whether enterprise reforms were carried out to shut them down.

6 Results

6.1 Did Privatization Reforms Convert Tenants to Owners?

We begin our analysis by verifying that our data on housing reform dates accurately reflects the implementation of housing reforms. We estimate a flexible specification for household housing choices where we allow the effect of the reform to vary by the number of years since the reform:

$$HomeOwnership_{ijt} = \alpha_j + \beta_t + \sum_{k=-2}^{6} \gamma_k Post_{jkt} + \epsilon_{ijt} \qquad (2)$$

where $HomeOwnership_{ijt}$ equals 1 if household i in city j lives in privately owned housing at time t, α_j and β_t are city fixed effects and time fixed effects as in (1), and $Post_{jkt}$ is a dummy which equals 1 if city j implemented the reform k years after year t. This specification also reveals whether there are any pre-trends by explicitly including dummies for one and two years before the city-level reforms (the omitted category is 3 or more years after the reform). We estimate γ_k for two years before $(\gamma_{-2}, \gamma_{-1})$ and five years after the reform $(\gamma_1, ..., \gamma_5; \gamma_6$ captures the effect 6 or more years after the reform. If the reform increased private ownership, then we expect $\hat{\gamma}_k > 0$ for $k > 0$ when we estimate the effect on private ownership; in particular we test whether the sum of post-reform coefficients $\sum_{k=1}^{6} \gamma_k$ is greater than zero. We expect

the opposite relationship for households living in publicly owned housing.

We see that the proportion of households living in private housing increases significantly after the city enacts the privatization reform. Figure 3 plots the coefficients obtained from the regression (2), along with their 95% confidence intervals. The coefficients γ_k are statistically greater than zero for $k >= 2$. Equally important from our identification standpoint, we do not see any evidence of pre-trends in housing choices before the reform occurred: the coefficients γ_{-2} and γ_{-1} are statistically indistinguishable from zero. We also plot the coefficients from running the regression (2) with the dependent variable as a dummy for whether the household lives in public housing. We see that the reform leads to a significant decrease in the probability of a household living in public housing, almost exactly mirroring the increase in private housing (black line in Figure 3). Overall, we conclude that the privatization reforms were successful in their goal of converting tenants into homeowners.

Table 3 presents the results from our main specification (1) for private home ownership. The baseline estimate shows that the privatization reform increased the fraction of households living in privately owned housing by nearly 10 percentage points (Table 3, Column 1). This baseline specification controls for household characteristics (household size, age, gender and education of the household head) and for lagged city incomes. The estimates in columns (2)-(5) present the results of different robustness checks, as discussed in Section 5. We find that the results remain essentially unchanged when we control for a three-year lagged moving average of city income rather than simply lagged income (column 2), when we add controls for contemporaneous growth of urban employment or share of state employment in the province (column 3), and when controlling for lagged city-level GDP growth (column 4), suggesting that the enactment of reforms was not driven by prior growth of the city's economy.

6.2   Did Privatization Improve Housing Quality?

We find that privatization reforms do not significantly improve the quality of the housing obtained by households; neither the total floor area nor the probability of having their own bath and toilet is significantly affected by the privatization reforms (Table 3, Panels B and C). This is partly because one particular way of improving quality–moving to a better house–was not possible in the initial years following privatization. But these results show that households

themselves did not invest significantly in either increasing the floor area or the amenities available in the newly privatized home. We also examined the type of water supply, heating source and cooking fuel used by the households, and find that none of these are significantly improved by the privatization reforms.[12] Of course, these measures depend in large part on city-level investments in public services and our results therefore demonstrate that housing privatization reforms were not systematically accompanied by other city initiatives to improve living conditions.

6.3   Housing Privatization and Occupational Choices

We find that housing privatization results in a significant increase in the probability of a household member choosing to work in the private sector (Table 4). This is consistent with our prior that such reform reduced the opportunity cost of moving to the private sector. The fraction of household heads working in the private sector increased by 1.5 percentage points, and the fraction of spouses working in the private sector increased by 2.5 percentage points in the post-reform period (Table 4, Panels B and C, Column 1). As in the housing choice regressions, these results are robust to the addition of a number of control variables for province-level employment and GDP trends, city budget variables and lagged city-level GDP growth.

On the other hand, we find no effects of the privatization reform on the fraction of household heads or spouses who have their own businesses (Table 4, Panels D, E and F). All the coefficients are much smaller than those for private sector employment, and statistically insignificant. This suggests that the decision to enter entrepreneurship has different determinants than the decision to switch from state to private sector. In particular, it is likely that the restrictions on trading combined with the lack of a well-developed credit market made it difficult for households to capitalize on their newly acquired housing wealth in order to finance entrepreneurship.

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[12]Results available upon request.

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6.4 Credit, Consumption and Wages

We present evidence that the housing privatization reform did not increase households' ability to obtain credit, in keeping with the institutional setting in China. Households are not more likely to have either a housing loan or a non-housing loan following the enactment of housing privatization reforms (Table 5, Panels A and B). We also find that the estimated coefficients for the effect of the reform on total household consumption and expenditure on housing improvements are small, negative and statistically insignificant (Table 5, Panels C and D). All of this suggests that the reform did not significantly increase household wealth. Similarly, there is no effect on savings rate (Table 5, Panel E).

Do households earn more as a result of moving to the private sector? We find negative and statistically insignificant coefficients for the estimated effects of housing privatization on the log of total household labor income and total per capita income, which includes business income. This shows that although workers moved into the private sector, they did not earn more as a result.

7 Interpretation

The findings that privatization of housing had no effect on entrepreneurship and credit suggests that the reduced form effect of the reform on labor mobility does not operate through credit channels. The finding that it had no effect on consumption, savings or household income suggest that it also does not operate through wealth channels. Together, these results point to the change in opportunity cost for working for the state sector as the most likely explanation of the main results.

Is the effect of privatization economically significant? As documented earlier, the fraction of household heads employed in the private sector increased from zero to 6% over our sample period; the corresponding figures for spouses are zero and 8.4%. (See Appendix Table A1). A back-of-the-envelope calculation suggests that the privatization of housing explains 24% of the observed increase in household heads working in the private sector, and 27% of the increase in spouses working in the private sector – thus showing that a relatively simple reform (in the context of the many reforms implemented by the Chinese government during the period) had an enormous effect on increasing labor supply for the private sector.

The finding that the move to the private sector did not result in higher wages for workers is consistent with our belief that we are capturing general equilibrium effects with this wide-sweeping reform. Even if the workers were more productive in the private sector, wages might have been decreased because of the large increase in labor supply. In a standard supply-demand framework, if the elasticity of labor demand is not very high, then the increase in labor supply is likely to lead to a fall in wages. If we assume that the reform did not affect private sector labor demand, we can use it as an instrument for labor supply to estimate the elasticity of demand for labor in the private sector. The finding that the reform has no effect on wages implies that labor demand elasticity is likely to be large. If we think that we are capturing long run effects, the results may also reflect the increase in capital inputs in the private sector as a response to the increase in labor supply. We will explore this further in future research.

8   Conclusions

This paper presents novel evidence of how the largest property rights reforms in the world affected household labor decisions. We find that this relatively simple reform can explain one-fourth of the increase in labor supply for the private sector, which presumably helped fuel growth and productivity increases in one of the most rapidly growing economies in the past quarter of a century. The evidence suggests that the standard theories of the benefits of private property rights do not play an important role here. Instead, the results point to the simple mechanism of untying housing access from state employment as the main driver of the result. While caution should be used in applying the results outside of the China context, many of the relevant institutional details (such as the state monopolization of urban housing before the reforms, and the lack of contract enforceability for informal credit markets) are similar to other developing countries, especially those transitioning from communist regimes.

Thus far, we have only examined the impacts of privatization. We will next use similar strategies and data on city-level reform dates of the second reform that allowed households to trade on the market to examine the impact of that reform.

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Figure 1: Fraction of households in private and public housing over time
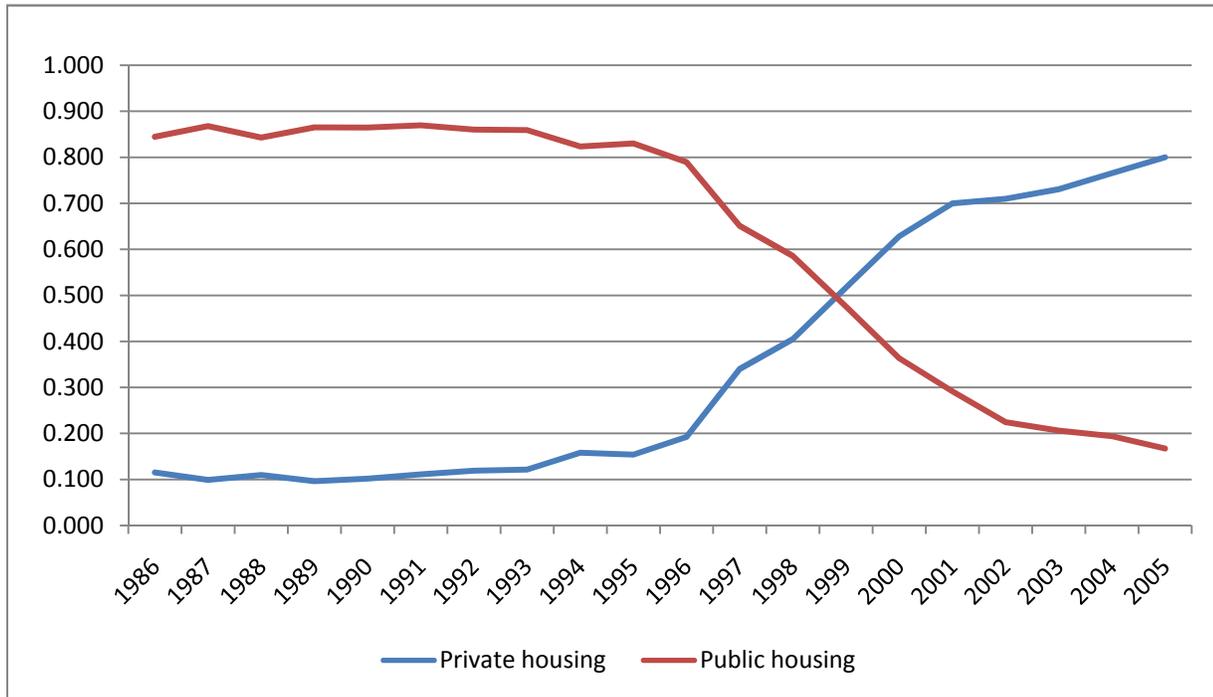
Figure 2: Fraction of Household Heads in the State and Private Sectors over Time
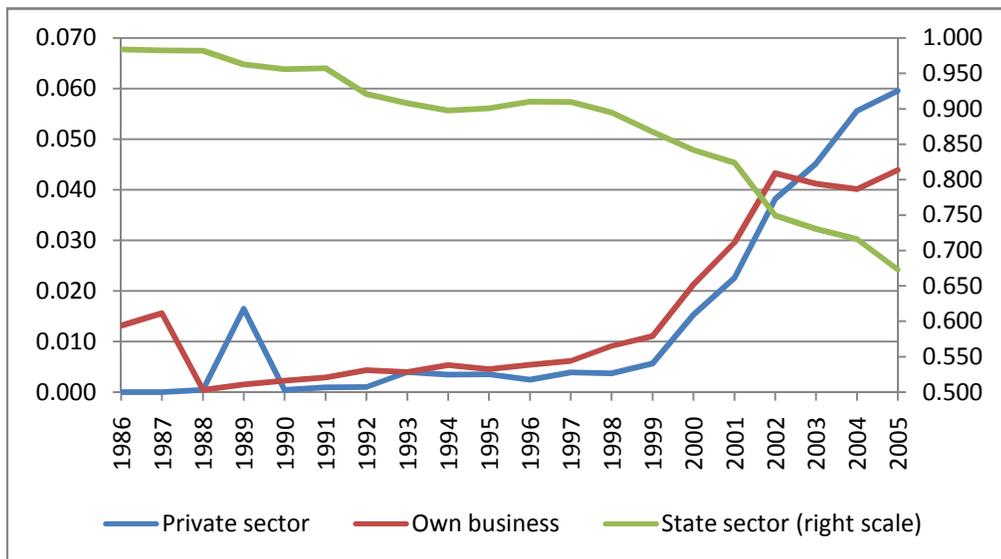
Figure 3: Effect of Privatization on Home Ownership
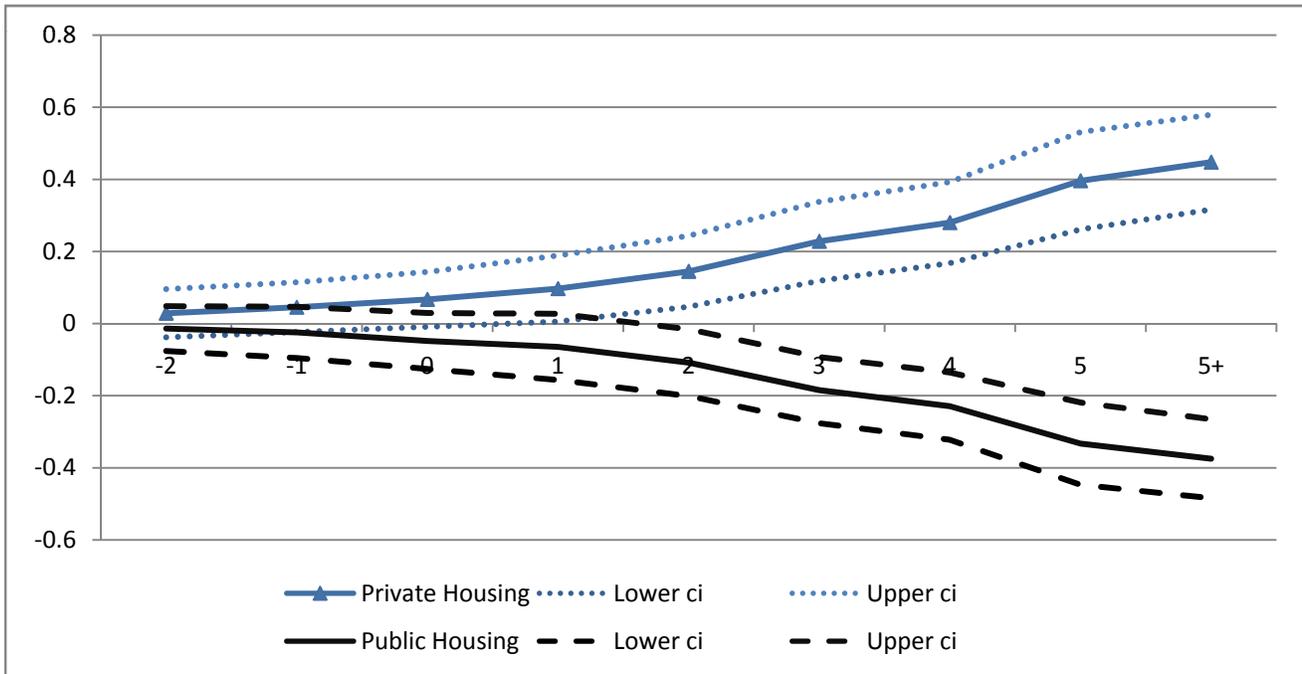
Coefficients and confidence intervals obtained from regression specification (2), controlling for city and year fixed effects. Standard errors are clustered at city-year level.

"Lower ci" and "upper ci" refer respectively to the lower and upper bounds of the 95% confidence interval.

The values on the X-axis refer to the number of years since the privatization reform; zero indicates the year of reform.

Dotted lines represent the 95% confidence intervals.

Coefficients and confidence intervals obtained from regression specification (1), controlling for city fixed effects and year fixed effects.

Table 1: Housing Privatization in Chinese Cities

Year of Privatization reform	# cities	#obs
1988	2	5,729
1989	1	4,490
1990	1	1,600
1991	2	14,018
1992	10	47,419
1993	5	19,621
1994	1	2,340
1995	1	2,707
1997	1	2,878
Total	24	100802

Table 2: Summary Statistics of Main Variables

Variable	Obs	Mean	S.D.
Panel A. Dependent variables			
Live in private housing (fraction)	100801	0.68	0.47
Live in publicly rented housing (fraction)	100801	0.28	0.45
Total floor area (square meters)	95516	46.9	21.4
Fraction of households who have their own bath and toilet	96078	0.66	0.47
Household head or spouse employed in private sector (fraction)	80736	0.08	0.27
Household head employed in private sector (fraction)	80736	0.04	0.19
Spouse employed in private sector (fraction)	80736	0.05	0.21
Household head or spouse owns a small private business (fractic	80736	0.05	0.22
Household head owns a small private business (fraction)	80736	0.03	0.18
Spouse owns a small private business (fraction)	80736	0.03	0.17
Household head unemployed (fraction)	100801	0.31	0.46
Spouse unemployed (fraction)	100801	0.42	0.49
Household labor income per capita (real yuan)	100801	7609	7876
Total household income per capita (real yuan)	100801	11369	8695
Household per capita consumption (real yuan)	100801	8313	7010
Household savings rate (%)	100801	20.2%	34.2%
Household has a housing loan (%)	94042	0.49%	6.98%
Household has a non-housing loan (%)	100801	14.8%	35.5%
Household spends any money on home improvements (%)	100801	21.8%	41.3%
Panel B. Household characteristics			
Age of household head	100440	50.1	11.4
Household head is male	100801	0.59	0.49
Household head has a college education	100418	0.12	0.32
Household size	100801	2.99	0.80
Panel C. City and province level controls			
Log City Income (lagged)	90639	13.83	1.32
Log City Income (lagged 3 year average)	91339	13.68	1.28
City GDP growth rate (lagged)	82280	19%	54%
Province per capita GDP growth rate	100801	14.3%	7.7%
Change in state share of province employment	100801	-0.036	0.034
Province urban employment growth	100801	5.1%	7.2%

All computations are adjusted for sampling weights.

Table 3: Privatization Reforms and Housing Choices

	Baseline (1)	City income moving average (2)	Province controls (3)	City GDP growth (lagged) (4)
A. Household lives in privately owned housing				
Post-privatization dummy	0.099	0.099	0.094	0.112
	(0.032)	(0.032)	(0.033)	(0.036)
Observations	86252	86891	86252	81649
R-squared	0.20	0.20	0.20	0.18
B. Total floor area				
Post-privatization dummy	-0.177	-0.295	-0.566	0.130
	(0.987)	(0.981)	(0.937)	(1.071)
Observations	85697	86336	85697	81095
R-squared	0.17	0.17	0.17	0.16
C. Household has own bath and toilet				
Post-privatization dummy	-0.081	-0.075	-0.085	-0.069
	(0.052)	(0.052)	(0.051)	(0.056)
Observations	86252	86891	86252	81649
R-squared	0.18	0.18	0.18	0.17
Year dummies	Y	Y	Y	Y
City dummies	Y	Y	Y	Y
Household controls	Y	Y	Y	Y
City income (lagged)	Y		Y	Y

Robust standard errors in parantheses, adjusted for clustering at city-year level
"Household controls" include age of the household head and its square, a dummy for whether the household head is male, and dummies for education categories of the household head.
"Province controls" include the growth rate of per capita GDP in the province, the growth rate of urban employment in the province and the change in the share of state employment in total urban employment in the province.
We exclude households where the head is below 15 years of age or above 70 years of age.
All regressions are adjusted for sampling weights.

Table 4: The Effect of Housing Privatization on Labor Market Decisions

	Baseline (1)	City income moving average (2)	Province controls (3)	City GDP growth (lagged) (4)
A. Household head or spouse employed in private sector				
Post-privatization dummy	0.027	0.027	0.029	0.031
	(0.013)	(0.013)	(0.013)	(0.014)
Observations	71394	71939	71394	67365
R-squared	0.06	0.06	0.06	0.06
B. Household head employed in private sector				
Post-privatization dummy	0.015	0.015	0.015	0.017
	(0.009)	(0.008)	(0.009)	(0.009)
Observations	66217	66719	66217	62401
R-squared	0.04	0.04	0.04	0.04
C. Spouse employed in private sector				
Post-privatization dummy	0.024	0.025	0.026	0.027
	(0.010)	(0.010)	(0.010)	(0.011)
Observations	57133	57574	57133	53753
R-squared	0.05	0.05	0.05	0.05
D. Household head or spouse owns a small private business				
Post-privatization dummy	0.006	0.005	0.007	0.005
	(0.006)	(0.006)	(0.006)	(0.006)
Observations	71394	71939	71394	67365
R-squared	0.04	0.04	0.04	0.04
E. Household head owns a small private business				
Post-privatization dummy	0.004	0.003	0.005	0.003
	(0.005)	(0.005)	(0.005)	(0.005)
Observations	66217	66719	66217	62401
R-squared	0.05	0.05	0.05	0.05
F. Spouse owns a small private business				
Post-privatization dummy	0.005	0.004	0.005	0.004
	(0.004)	(0.004)	(0.004)	(0.005)
Observations	57133	57574	57133	53753
R-squared	0.03	0.03	0.03	0.03
Year dummies	Y	Y	Y	Y
City dummies	Y	Y	Y	Y
Household controls	Y	Y	Y	Y
City income (lagged)	Y		Y	Y

Each entry in the table represents the coefficient on the post-privatization dummy obtained from a regression of the dependent variable on the post-privatization dummy, controlling for the controls listed under each column.

Robust standard errors in parantheses, adjusted for clustering at city-year level

"Household controls" include age of the household head and its square, a dummy for whether the household head is male, and dummies for education categories of the household head.

"Province controls" include the growth rate of per capita GDP in the province, the growth rate of total employment in the province and the change in the share of state employment in total employment in the province.

We exclude households where the head is below 15 years of age or above 70 years of age.

All regressions are adjusted for sampling weights.

Table 5: The Effect on Credit, Consumption, Savings and Wages

	Baseline (1)	City income moving average (2)	Province controls (3)	City GDP growth (lagged) (4)
A. Household has a housing loan				
Post-privatization dummy	-0.002	-0.002	-0.003	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	86252	86891	86252	81649
R-squared	0.00	0.00	0.00	0.00
B. Household has a non-housing loan				
Post-privatization dummy	0.014	0.009	0.026	0.015
	(0.044)	(0.044)	(0.041)	(0.047)
Observations	86252	86891	86252	81649
R-squared	0.13	0.13	0.14	0.13
C. Log(per capita consumption)				
Post-privatization dummy	-0.016	-0.017	-0.013	-0.015
	(0.038)	(0.038)	(0.037)	(0.040)
Observations	86252	86891	86252	81649
R-squared	0.48	0.48	0.48	0.45
D. Household spends money on house improvements				
Post-privatization dummy	-0.014	-0.012	-0.013	-0.016
	(0.027)	(0.027)	(0.029)	(0.029)
Observations	86252	86891	86252	81649
R-squared	0.03	0.03	0.03	0.03
E. Household savings rate				
Post-privatization dummy	0.013	0.013	0.012	0.004
	(0.014)	(0.014)	(0.014)	(0.014)
Observations	86252	86891	86252	81649
R-squared	0.03	0.03	0.03	0.03
F. Log (labor income)				
Post-privatization dummy	-0.027	-0.032	-0.029	-0.030
	(0.045)	(0.045)	(0.045)	(0.049)
Observations	79310	79912	79310	74982
R-squared	0.37	0.37	0.37	0.36
G. Log (total per capita income)				
Post-privatization dummy	-0.012	-0.012	-0.010	-0.023
	(0.037)	(0.037)	(0.037)	(0.038)
Observations	86252	86891	86252	81649
R-squared	0.56	0.56	0.56	0.53
Year dummies	Y	Y	Y	Y
City dummies	Y	Y	Y	Y
Household controls	Y	Y	Y	Y
City income (lagged)	Y		Y	Y

Each entry in the table represents the coefficient on the post-privatization dummy obtained from a regression of the dependent variable on the post-privatization dummy, controlling for the controls listed under each column.

Robust standard errors in parantheses, adjusted for clustering at city-year level

"Household controls" include age of the household head and its square, a dummy for whether the household head is male, and dummies for education categories of the household head.

"Province controls" include the growth rate of per capita GDP in the province, the growth rate of total employment in the province and the change in the share of state employment in total employment in the province.

We exclude households where the head is below 15 years of age or above 70 years of age.

All regressions are adjusted for sampling weights.

Appendix Table 1: Summary statistics over time

	1986	2005
Panel A. Dependent variables		
Live in private housing (fraction)	0.115	0.800
Live in publicly rented housing (fraction)	0.845	0.167
Total floor area (square meters)	31.0	51.8
Fraction of households who have their own bath and toilet	0.081	0.770
Household head or spouse employed in private sector (fraction)	0.000	0.101
Household head employed in private sector (fraction)	0.000	0.060
Spouse employed in private sector (fraction)	0.000	0.084
Household head or spouse owns a small private business (fraction)	0.013	0.044
Household head owns a small private business (fraction)	0.013	0.039
Spouse owns a small private business (fraction)	0.024	0.045
Household head unemployed (fraction)	0.119	0.348
Spouse unemployed (fraction)	0.233	0.464
Household labor income per capita (real yuan)	862	9806
Total household income per capita (real yuan)	1100	14888
Household per capita consumption (real yuan)	985	10372
Household savings rate (%)	9.7%	24.5%
Household has a housing loan (%)	.	0.5%
Household has a non-housing loan (%)	51.3%	5.7%
Household spends any money on home improvements (%)	17.1%	21.1%
Panel B. Household characteristics		
Age of household head	43.58	51.25
Household head is male	0.576	0.604
Household head has a college education	0.103	0.116
Household size	3.59	2.91
Panel C. City and province level controls		
Log City Income (lagged)		14.31932
Log City Income (lagged 3 year average)		14.18138
City GDP growth rate (lagged)		19.7%
Province per capita GDP growth rate	10.3%	12.5%
Change in state share of province employment	0.001	-0.030
Province urban employment growth	3.4%	6.3%

All regressions are adjusted for sampling weights.