

# Property Rights and Household Decisions: The Impact of China's Urban Housing Reforms

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Preliminary and incomplete

Comments welcome

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## Abstract

We examine the impact of housing reforms in China which transferred urban housing from state control to individual households, using variation across cities in the timing of the reform. Our preliminary results indicate that the urban housing reforms are associated with more households working in the private sector; households who are eligible for housing loans are more likely to be running private businesses after the reforms. These reforms do not increase households' access to non-housing loans, or the propensity of households to invest in housing improvements.

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# 1 Introduction

Many economists consider property rights to be one of the main institutions affecting economic development.<sup>1</sup> Scholars such as De Soto (2000) have argued that the major barrier to prosperity in developing countries is the inability to convert property into usable assets, because of a lack of clear-cut legally recognized rights. In the last two decades, there have been major changes to property rights in developing and transition economies. These changes have been both in the rural sector (e.g. the decollectivization of agriculture in China and Vietnam) and in the urban sector, where the property rights mainly relate to housing. This paper examines the impact of urban housing reforms in China undertaken during the late 1980s and the 1990s. The reforms in China transferred housing and land use rights from state ownership to private households and developers in at least 50 cities, potentially affecting more than 90 million people. In this sense, this is the largest reform in urban property rights undertaken in the world.

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 Schargrotsky (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.<sup>2</sup> Most such studies emphasize the decreased risk of eviction due to property rights reforms. In contrast, the Chinese reforms did not directly increase security of tenure: households which were renting from the state ran very little risk of being evicted from their homes. The reforms granted ownership—the rights to mortgage and bequeath the housing asset—and later, the right to buy, sell and lease housing. Another contrast with prior studies is that this was a change to the overall property rights regime which affected all urban residents i.e. the effects we estimate are likely to include general equilibrium changes. The third important aspect of the Chinese urban housing re-

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

<sup>2</sup>Interestingly, both these studies find little impact of property rights on the credit market—a result echoed in some studies of rural land reform (Do and Iyer, 2008; Braselle, Gaspart and Platteau, 2002).

forms is the way they were phased in: ownership rights (including the right to bequeath and mortgage) were given in the first phase, while the right to buy and sell housing was granted five or six years later. This enables us to separately estimate the additional impact of being able to trade the real estate asset, which is typically not possible to do in other settings.

Our empirical strategy relies on the fact that different cities in China initiated these reforms at different times. We collected data on the date when these cities initiated the privatization reform i.e. allowed work units to sell housing to their residents, as well as the date when the cities allowed households to begin trading their housing assets. This information was obtained from local city-level newspapers. We combined this data with household survey data from 37 cities over a 20-year period (1986-2005). We then use panel regressions to estimate the impact of these housing reforms on labor market and home investment decisions of the households.

Our preliminary results confirm that the housing reforms led to a large shift of households from state-owned housing to privately owned housing. We also find that households in cities which enacted reforms earlier are more likely to be employed in the private sector, and more likely to be owning a private business if they were eligible for housing loans. Perhaps as a consequence, we find that housing reforms are associated with higher labor incomes. In contrast, we do not find any impact of either the privatization or the trading reforms on households' access to credit, or the amounts they choose to spend on housing improvements, or the quality of housing they live in.

The paper is structured as follows: Section 2 reviews the process of urban housing reforms in China's cities and Section 3 outlines the potential impacts of such reforms. Section 4 describes our data and empirical strategy, and Section 5 summarizes our results.

## 2 Urban Housing Reforms in China

### 2.1 Housing Reforms before the 1990s

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.<sup>3</sup> As a result of subsidized housing and inadequate provision by the work units, the demand for housing had always exceeded the supply by a wide margin. The linking of housing to the work unit (who owned the house) restricted labor mobility (Meng, 2000).

Housing reform began in the middle 1980s by increasing rental levels and shifting from an implicit to an explicit rental subsidy in the hope that this would ease housing demand. In 1986 the State Council chose 6 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 rental reform 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).

### 2.2 Housing Reforms in the 1990s

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.<sup>4</sup> In 1993, at the third housing reform meeting the State Council announced pricing rules for selling new or old public

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<sup>3</sup>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).

<sup>4</sup>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).

housing (Yuan, 2000 and Pan, 2000). The basic idea was that the price of a new house (flat) should not be higher than 3 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 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 worker's rank. For example, a minister was entitled to 250 square meters, a governor-general was entitled to 180 square meters and so on (Wang, 1993).

In 1994 the State Council decided that housing purchased from work units could be sold in the market 5 years after the purchasing date. The Urban Real Estate Administration Act of 1994 (which took effect in 1995) also speeded up the establishment of the commercial housing market by providing for the transfer of land-use rights from the state to land users by means of tender, auction or negotiated agreements. By 1997, 50 per cent of public housing had been sold (Yuan, 2000). In 1998, the State Council made the decision that there would be no more free allocation of housing from work units to employees. All housing assets had to be purchased either from the work units at market price or directly from the market. By 2000, this was realized in most provinces (Yuan, 2000 and Pan, 2000). By then the initial aims of housing reform were largely achieved.

## **2.3 Housing Loan Reforms**

Since 2000, housing reform has been focused mainly on the housing loan market. Although housing credit was first introduced in 1991 in some state owned banks, it was not until 1995 that the People's Bank of China announced draft rules and regulations guiding the housing loan business, which covers both bank lending and lendings from the Housing Provident Funds. Initially, the rules were very restrictive: 1. there had to be double collateral pledged for the repayment of a loan; 2. the maximum loan repayment period was 10 years; and 3. the 30% downpayment had to be paid 6 months prior to the purchase of the property. The rules and regulations have been changing over the years and becoming more detailed as the

market develops, but, in general, they have become less restrictive.

Two things, which are most relevant to this study, are the repayment period rule and the restriction on the age of the borrower. With regard to the repayment period, the rule changed from 10 years in 1995 to 20 years in 1998 and the current rule (1999) allows individuals to repay over a 30 year period. The age restriction, mainly relevant to the Housing Provident Funds loan, has not been mentioned in the formal rules and regulations but it has been a common practice in almost all cities. The unwritten rule used to be very restrictive and, like most other rules in this area, has been gradually relaxed. The most recent practice limits loans to those under 60 to 70 years of age for males and 55 to 65 years of age for females. In addition the loans must be fully paid by these ages. Thus, if a male borrows when he is 40 he has 20 to 30 years to pay the mortgage, while for a forty year old woman the time limit is 15 to 25 years. The age restriction varies across different cities (reference).

To sum up, like many economic reforms in China, housing reform implemented in a piece-meal manner. Local experiments came first and nation wide decisions came second. Reforms were carried out at different times and with different procedures across cities, and sometimes even across work units in the same city.

## 2.4 Housing Reform Dates

In this paper we focus on two major reform measures. The first measure is the date when each city permitted the sale of public housing to employees (henceforth the “privatization reform”). The second is the date when the city allowed purchased public housing to be traded in the market (henceforth the “trading reform”). Table 1 presents the years at which the number of sample cities implemented each of these two reform measures. 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.

An issue arises as to what led city governments to adopt each of the housing reform measures at a certain time. Such a motivation might be correlated with other city level unobservable characteristics (to the researchers), 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 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. In future work, we will put together city level data to examine whether any such factors were at work. At present, because of the availability of a long time series, we are able to explicitly check whether there are pre-trends in our outcomes even before the reforms are enacted. We are also able to control for city-level time trends, so that any smoothly varying city characteristics would not contaminate our estimates.

Another potential issue of concern is the fact that during the entire period of housing reform, urban China was undergoing many other reforms, such as enterprise reform, health care reform, pension reform and education reform. These reforms were carried out in a similar piece-meal manner, in that some cities experimented with a particular reform and then other cities followed the experience some years later. If, within the same city, other reforms were introduced at the same time as the housing reforms, simply identifying the year housing reform measures were implemented will not allow us to separate out the housing effects from that of other reforms. A partial check for this is provided by examining only the sub-population we believe will be most affected by the housing reform by exploiting the housing loan eligibility rules. However, this is not completely fool-proof. We discuss this issue in greater detail in later sections.

### **3 Potential Impact of Housing Reforms**

The urban housing reforms are likely to affect household behavior through three main channels. First, the privatization reforms broke the link between housing and employer 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.

We use the variation in housing loan rules to identify sub-populations who are most likely to be affected by the reforms. As described in Section 2, the Housing Fund was most likely to give loans to men under the age of 60 and women under 55. These people are therefore the most likely to take advantage of the housing privatization and/or the opportunity to trade housing (so as to obtain a better piece of real estate). In fact, since housing loans were not very common in the early part of the 1990s, and the banking regulations with regard to housing rules were clarified nationally only in 1995, we expect that those most eligible for the Housing Fund loans are going to be the ones who respond most to the trading reforms.

The second major impact 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. Another set of property transactions we can examine relates to renting or leasing out of the properties acquired. However, our data set does not record variables such as income from rentals or property sales prior to 2002, and hence we are unable to examine this set of outcomes.

The third major influence on household behavior is that households might be able to obtain a better quality of housing. They might now be inclined to spend more on improving their housing quality, since they can expect to obtain both consumption utility from a better quality of housing, and perhaps a higher price when they sell their apartment. If the latter effect predominates, we expect to see greater investments by households in housing quality after the trading reforms are enacted. Another way to improve housing quality would be to “trade up” i.e. obtain better housing through market transactions which would not have been possible before the reforms. A final possibility is that the wealth effect of obtaining an asset at cheaper-than-market rates might result in increased investment by households in other dimensions, such as education or health improvements, or even increased consumption.<sup>5</sup>

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<sup>5</sup>Wang (2008) notes that the extent of subsidy in the sale price was not very different from the extent of subsidy the household was already receiving in terms of rents.



## 4 Data and Empirical Strategy

As described earlier, we collected information on the date of the privatization and the trading reforms for over 50 of China’s largest cities, using local city-level newspapers. This city-level detail distinguishes our work from that of Wang (2008), which simply compares outcomes before and after 1994. We combined the data on city-level reform dates with household survey data from the Urban Household Income and Expenditure Surveys conducted by the National Bureau of Statistics. We have data on households in 37 cities in 15 provinces over the period 1986-2005; the sample size is more than 3000 for each year. We then run difference-in-difference regressions to assess the impact of the housing reforms as follows:

$$Y_{ijt} = \alpha_j + \beta_t + \sum \gamma_k Post_{jkt} + \delta X_{ijt} + \epsilon_{ijt} \quad (1)$$

where  $Y_{ijt}$  is the outcome variable for household  $i$  in city  $j$  and year  $t$ ,  $\alpha_j$  is a city fixed effect,  $\beta_t$  is a year fixed effect,  $Post_{jkt}$  is a dummy which equals 1 if city  $j$  implemented the reform  $k$  years after year  $t$ ,  $X_{ijt}$  are other control variables and  $\epsilon_{ijt}$  is an error term. The coefficients of interest are  $\gamma_k$ , which document the increase in the outcome variable  $k$  years after the reform. Our identification is thus based on the differential timing of the housing reforms across cities, which of course can be potentially correlated with other city-level characteristics. We can explicitly check for any pre-trends in these outcomes by computing  $\gamma_k$  for up to two years before the reform. In all our regressions, we restrict our sample to six years before and five years after the reform date, and estimate  $\gamma_k$  for  $k = -2, -1, 0, 1, 2, 3, 4, 5$ . The omitted category in these regressions is therefore the period three or more years prior to the reform date. Since outcomes for households within the same city and same year can be correlated due to common city-year shocks, we cluster all standard errors at the city-year level.

For outcomes such as access to credit, where there are age-specific legal constraints to accessing mortgages, we use the specification below:

$$Y_{ijt} = \alpha_j + \beta_t + \sum \gamma_{1k} Post_{jkt} + \sum \gamma_{2k} Post_{jkt} * Eligible_{it} + \delta X_{ijt} + \epsilon_{ijt} \quad (2)$$

where  $Eligible_i$  is a measure of the eligibility of the household for loans from the Housing Provident Funds. This equals the difference between the age of the household head

and 60 (55 years for a female head of the household), and equals zero if the household head is aged over 60. We expect the coefficients  $\gamma_{1k}$  to be close to zero, and the coefficients  $\gamma_{2k}$  to be positive and significant in period after the reforms were enacted ( $k > 0$ ). As described earlier, this specification is relevant only for the trading reforms since the housing loan market was not well developed at the time of the privatization reforms.

## 5 Results

### 5.1 Transfer of Housing from Public to Private Sector

We first verify that the urban housing reforms achieved their primary goal—to decrease the state’s involvement in the housing market. We do this by running the specification (1) with the dependent variable equal to a dummy for whether the household lives in privately owned housing or rents from a public source. We should note that the share of households living in public housing declined from 84% in 1986 to 15% in 2005, while the fraction of households living in privately owned housing increased from 12% to 81% (Figure 1A).

We find that the fraction of households living in privately owned housing increases significantly about three years after the city passes a housing privatization reform, while the fraction of households living in public rental housing decreases by almost the same amount at the same time (Table 2, Columns 1 and 2). This confirms that the city level reform dates indeed had a strong and significant impact on the type of housing in Chinese cities (we test explicitly whether the sum of the  $\gamma_k$  coefficients is statistically significant for  $k > 0$ ). Further, there do not appear to be any significant pre-existing differences in housing types across cities before they passed this reform since none of the coefficients  $\gamma_k$  is significant for  $k \leq 0$ . As a robustness check, we examined the same trends for the trading reform: as expected, we do not find any significant impact of the trading reforms on the shift from public to private housing (Table 2, Columns 4 and 5). These coefficients are also graphed in Figures 1B and 1C to illustrate the trends.

Table 2A lists some robustness checks for this basic result. First, we see that the fraction of households living in public housing is significantly lower about three years after

the privatization reform is enacted (Column 2), consistent with Figure 2A. This specification controls only for time-invariant city characteristics. We show that the results are robust to controlling for household characteristics (Column 3), as well as any time-varying city characteristics which are controlled for by including city-specific time trends (Column 4). The sample for these regressions includes more years than in Table 2, to enable us to estimate city-level trends accurately. As before, the trading reforms do not show any major relationship with the transition of households from the public to the private sector.

## 5.2 Housing Reforms and Occupational Choice

As pointed out earlier, the transfer of housing from the state to the household broke the link between employment and access to housing. We therefore expect such changes to lead to a greater share of employment in non-state sectors, as well as to provide households with additional resources to start up household businesses. We examine these effects by looking at household occupational choices. The UHS data set classifies the sector of employment into 4 broad categories: employment in state-owned enterprises, collective enterprises, private enterprises and owning a private business. Over this time period, there has been a secular decline in the fraction of households employed in the state-owned sector from 80% to 60%, and a rise in the fraction of household heads employed in the private sector from zero to 7% (Figure 2A).

We find that the housing reforms are associated with an increase in the fraction of households where either the household head or the spouse is employed in the private sector (Table 3, Column 2). While the trends for the household head having a job in the state sector are declining, and the probability of the household running a private business increases after the privatization reform (Figure 2B), we do not find these effects to be statistically significant (Table 3, Columns 1 and 3). As with the housing choice variables, the trading reforms also do not appear to affect the occupational choices of households.

As described earlier, the Housing Fund rules made it difficult for older households to acquire housing loans, and hence they might be less able to take advantage of the trading reforms. Since starting up a business usually requires the household to have some investment capital, we check whether eligibility for credit affects the probability of starting a business.

We find that households which are eligible for receiving credit have a greater response to trading reforms in terms of their occupational choices: they are less likely to work in the state sector, and more likely to either have a private sector job or have a household private business after the trading reforms were passed. Table 3B shows the coefficients from running specification (2) for occupational choices, and also tests explicitly for differential response to reforms across eligibility categories. We find that households which are not eligible for a housing loan show no response to the trading reforms in terms of their occupational choices, while the response of eligible households is significantly larger. The coefficients are also shown graphically in Figure 2D. However, it is possible that the effects we document reflect the effect of age rather than loan eligibility, since loan eligibility is defined as the number of years before the household head reaches 60 (55 if female). It is possible that older household heads might be less likely to move to the private sector, either because they are risk-averse or because they are less likely to find a job in the private sector.

### 5.3 Housing Reforms and Household Investment

One of the major benefits of having use rights over housing is the possibility of using the asset as collateral, in order to access formal sources of credit. In this section, we investigate whether the reforms in China had this effect, and whether this led to greater investments by households in housing quality. Looking at the data on total income from credit, we find that one-third of Chinese households in our data had taken out a loan of some kind in the 1980s; this fraction declined to one-fifth by 2000, and was below 10% in 2004 and 2005. Very few households report having a home loan, outside of the Housing Funds. We therefore exclude home loans from this analysis, and focus instead on other borrowing behavior by households.

Does housing reform make it easier for households to obtain credit for other purposes? We do not find any statistically significant relationships between city-level housing reforms and households' access to credit, measured by the amount of (non-housing) loans taken out by households, or the probability of them having a loan (Table 4). This is true for both the privatization reforms and the trading reforms. Interestingly, we do find that households' labor income increases after the privatization reforms. This is most probably due to the earlier result that the household is more likely to have a member employed in the private

sector after the privatization reforms, which presumably pays higher wages.

The age-specific eligibility criteria for access to credit do not seem to matter for the impact of the reform (results available on request). We see that even households which are eligible for credit are not more likely to be carrying a loan after the housing reforms are enacted. This suggests that the guidelines regarding housing loans do not carry over into other types of loans.

Even without accessing credit, households may be more willing to invest in their homes if they know that they can capture the value of such investments by selling their home at an increased price in the future. They can also take advantage of the trading reforms to move to a better quality house. We therefore look at the impact of the trading reforms in particular on the quality of housing, as well as household expenditures on renovation and maintenance. Over this period, there was a general trend towards improved housing amenities such as housing area, whether the household has access to piped water, heating and gas cooking fuel and whether they have their own bath and toilet. However, we do not find any significant increase in these amenities when housing reform measures are enacted by the city. Table 5 shows the results for floor space and whether the household had their own bath and toilet. We also considered other housing amenities such as piped water, cooking gas and home heating, and found no statistically significant effects of the housing reforms (results available upon request). This could be because many of these amenities depend on local government provision and hence individual households may have limited control over such facilities. We also do not see any increases in the amounts households spend on housing decorations or building materials for housing renovations. This could indicate either that households did not perceive a large return to housing improvements, or that they could not obtain the resources needed to make such improvements.

Finally, we investigate whether households changed their living arrangements in response to the expansion in their housing possibilities. We find a small decrease in household size and the proportion of two-generation households in response to the privatization reform, and the opposite effect for the trading reforms. However, none of these coefficients are statistically significant.

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**Table 1: Dates of Urban Housing Reforms**

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Year of Privatization reform	# cities	#obs
1987	1	500
1988	3	6,133
1989	1	4,490
1990	1	1,600
1991	2	14,018
1992	14	50,838
1993	9	22,334
1994	3	3,655
1995	2	3,107
1997	1	2,878
Total	37	109553

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Year of Trading reform	# cities	#obs
1997	2	16,368
1998	11	29,588
1999	17	48,895
2000	5	9,274
2001	2	5,428
Total	37	109553

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**Table 2: Housing Reforms and the Move to Private Housing**

	Privatization Reform			Trading Reform		
	Live in public rental housing (1)	Live in privately owned housing (2)	Housing payment (3)	Live in public rental housing (4)	Live in privately owned housing (5)	Housing payment (6)
2 years before reform	-0.000 (0.018)	0.004 (0.016)	-2.359 (1.939)	0.059** (0.028)	-0.057* (0.032)	1.138 (8.838)
1 year before reform	0.002 (0.026)	0.006 (0.024)	-3.948 (2.808)	0.109* (0.061)	-0.110 (0.067)	-17.685 (18.372)
year of reform	-0.002 (0.032)	0.007 (0.033)	-6.404 (3.880)	0.154* (0.088)	-0.151 (0.092)	-3.469 (19.861)
1 year after reform	-0.014 (0.033)	0.022 (0.037)	-7.498 (5.334)	0.157 (0.113)	-0.200 (0.144)	-46.940 (28.428)
2 years after reform	-0.059 (0.046)	0.067 (0.049)	-7.847 (6.460)	0.192 (0.130)	-0.138 (0.127)	-14.538 (56.253)
3 years after reform	-0.131** (0.054)	0.148** (0.061)	-5.858 (7.195)	0.205 (0.151)	-0.120 (0.121)	3.599 (75.103)
4 years after reform	-0.159** (0.069)	0.181** (0.073)	-7.542 (8.213)	0.213 (0.168)	-0.118 (0.130)	28.661 (115.667)
5 years after reform	-0.201** (0.089)	0.224** (0.092)	-7.259 (10.084)	0.241 (0.187)	-0.108 (0.145)	112.199 (165.709)
F-statistic for sum of post-reform coefficients	4.24	4.68	1.03	1.85	1.15	0.04
p-value	0.05	0.04	0.32	0.18	0.29	0.85
Year dummies	YES	YES	YES	YES	YES	YES
City dummies	YES	YES	YES	YES	YES	YES
Observations	42570	42570	37082	72001	72001	72001
R-squared	0.23	0.26	0.06	0.22	0.19	0.03

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

\*\*\*represents significance at 1% level, \*\* at 5%, \* at 10%

**Table 2A: Robustness Checks for Housing Choices**

	Privatization Reform				Trading reform			
	Live in public rental housing				Live in public rental housing			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform dummy	-0.039 (0.041)				0.006 (0.037)			
Three or more years after reform		-0.123*** (0.037)	-0.125*** (0.037)	-0.102*** (0.031)		-0.004 (0.022)	-0.000 (0.021)	0.002 (0.014)
Year dummies	YES	YES	YES	YES	YES	YES	YES	YES
City dummies	YES	YES	YES	YES	YES	YES	YES	YES
Household controls			YES				YES	
City-specific time trend				YES				YES
Observations	74892	74892	74555	74892	84173	84173	83802	84173
R-squared	0.38	0.38	0.38	0.38	0.26	0.26	0.27	0.27

	Privatization Reform				Trading reform			
	Live in privately owned housing				Live in privately owned housing			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform dummy	0.046 (0.042)				-0.057 (0.073)			
Three or more years after reform		0.135*** (0.041)	0.125*** (0.038)	0.110*** (0.033)		0.044 (0.045)	0.052 (0.042)	0.022 (0.049)
Year dummies	YES	YES	YES	YES	YES	YES	YES	YES
City dummies	YES	YES	YES	YES	YES	YES	YES	YES
Household controls			YES				YES	
City-specific time trend				YES				YES
Observations	74892	74892	74555	74892	84173	84173	83802	84173
R-squared	0.34	0.34	0.37	0.35	0.23	0.23	0.25	0.23

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

\*\*\*represents significance at 1% level, \*\* at 5%, \* at 10%

Sample includes up to 10 years before and 5 years after the privatization reform; up to 10 years before and 5 years after the trading reform.

**Table 3: Housing Reforms and Occupational Choice**

	Privatization Reform				Trading Reform			
	HH head employed by state-owned enterprise (1)	HH head or spouse employed in private sector (2)	HH head or spouse owns a private business (3)	HH reports having business income (4)	HH head employed by state-owned enterprise (5)	HH head or spouse employed in private sector (6)	HH head or spouse owns a private business (7)	HH reports having business income (8)
2 years before reform	-0.043 (0.041)	0.039 (0.030)	0.005 (0.006)	0.004 (0.004)	-0.002 (0.022)	-0.001 (0.003)	0.005 (0.004)	0.004 (0.003)
1 year before reform	-0.046 (0.057)	0.027 (0.017)	0.009 (0.008)	0.007 (0.005)	-0.003 (0.028)	-0.001 (0.005)	0.012 (0.007)	0.012** (0.006)
year of reform	-0.066 (0.069)	0.029 (0.019)	0.017 (0.011)	0.012 (0.007)	0.008 (0.044)	0.002 (0.008)	0.011 (0.008)	0.013** (0.006)
1 year after reform	-0.080 (0.093)	0.033 (0.021)	0.021 (0.014)	0.013 (0.009)	0.002 (0.051)	0.011 (0.009)	0.010 (0.010)	0.013* (0.008)
2 years after reform	-0.086 (0.114)	0.041* (0.024)	0.026 (0.016)	0.015 (0.011)	0.005 (0.068)	0.020 (0.018)	0.006 (0.011)	0.008 (0.009)
3 years after reform	-0.124 (0.122)	0.051* (0.026)	0.032 (0.020)	0.018 (0.011)	0.011 (0.068)	0.028 (0.025)	0.006 (0.015)	0.008 (0.014)
4 years after reform	-0.150 (0.153)	0.058* (0.029)	0.035 (0.025)	0.018 (0.012)	-0.007 (0.068)	0.045 (0.037)	0.021 (0.019)	0.019 (0.018)
5 years after reform	-0.177 (0.169)	0.061* (0.030)	0.048 (0.028)	0.025* (0.013)	-0.005 (0.069)	0.053 (0.052)	0.016 (0.023)	0.025 (0.022)
F-statistic for sum of post-reform coefficients	0.90	3.58	2.53	2.74	0.00	1.37	0.63	1.21
p-value	0.35	0.07	0.12	0.11	0.98	0.25	0.43	0.28
Year dummies	YES	YES	YES	YES	YES	YES	YES	YES
City dummies	YES	YES	YES	YES	YES	YES	YES	YES
Observations	34357	36176	36176	42570	52576	56551	56551	72001
R-squared	0.03	0.06	0.02	0.89	0.05	0.05	0.03	0.02

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

\*\*\*represents significance at 1% level, \*\* at 5%, \* at 10%

**Table 3A: Robustness Checks for Occupational Choices**

	Privatization Reform				Trading reform			
	HH head or spouse employed in private sector				HH head or spouse employed in private sector			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform dummy	0.013 (0.009)				-0.003 (0.015)			
3+years after reform dummy		0.019** (0.008)	0.017** (0.008)	0.011** (0.004)		0.005 (0.008)	0.008 (0.008)	-0.006 (0.009)
Year dummies	YES	YES	YES	YES	YES	YES	YES	YES
City dummies	YES	YES	YES	YES	YES	YES	YES	YES
Household controls			YES				YES	
City-specific time trend				YES				YES
Observations	60891	60891	60883	60891	67089	67089	67082	67089
R-squared	0.06	0.06	0.08	0.06	0.05	0.05	0.07	0.06

	Privatization Reform				Trading reform			
	HH head or spouse owns a private business				HH head or spouse owns a private business			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-reform dummy	0.001 (0.005)				-0.009 (0.006)			
3+years after reform dummy		0.004 (0.006)	0.003 (0.005)	0.005 (0.004)		0.001 (0.007)	0.003 (0.008)	0.004 (0.006)
Year dummies	YES	YES	YES	YES	YES	YES	YES	YES
City dummies	YES	YES	YES	YES	YES	YES	YES	YES
Household controls			YES				YES	
City-specific time trend				YES				YES
Observations	60891	60891	60883	60891	67089	67089	67082	67089
R-squared	0.03	0.03	0.05	0.03	0.03	0.03	0.05	0.03

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

\*\*\*represents significance at 1% level, \*\* at 5%, \* at 10%

Sample includes up to 10 years before and 5 years after the privatization reform; up to 10 years before and 5 years after the trading reform.

**Table 3B: Housing Reforms, Loan Eligibility and Occupational Choices**

	HH head or spouse owns a private business	HH head employed in state-owned enterprise	HH head or spouse employed in private enterprise
2 years before reform	-0.000 (0.005)	0.010 (0.031)	-0.001 (0.004)
1 year before reform	0.003 (0.007)	0.028 (0.035)	0.001 (0.006)
year of reform	0.006 (0.010)	0.033 (0.043)	-0.001 (0.010)
1 year after reform	-0.002 (0.011)	0.030 (0.048)	0.002 (0.013)
2 years after reform	-0.035** (0.017)	0.115 (0.077)	0.005 (0.019)
3 years after reform	-0.021 (0.017)	0.089 (0.078)	-0.007 (0.026)
4 years after reform	-0.011 (0.021)	0.062 (0.083)	-0.005 (0.038)
5 years after reform	-0.018 (0.022)	0.076 (0.084)	0.001 (0.046)
Eligible	0.000** (0.000)	0.007*** (0.002)	-0.000 (0.000)
Eligible * 2 years before reform	0.000 (0.000)	-0.001 (0.001)	0.000 (0.000)
Eligible * 1 year before reform	0.001* (0.000)	-0.002 (0.001)	-0.000 (0.000)
Eligible * Year of reform	0.000 (0.000)	-0.002 (0.001)	0.000 (0.000)
Eligible * 1 year after reform	0.001** (0.000)	-0.002 (0.001)	0.001 (0.001)
Eligible * 2 years after reform	0.003*** (0.001)	-0.007** (0.003)	0.001* (0.001)
Eligible * 3 years after reform	0.002*** (0.001)	-0.005* (0.003)	0.003*** (0.001)
Eligible * 4 years after reform	0.003*** (0.001)	-0.004 (0.003)	0.004*** (0.001)
Eligible * 5 years after reform	0.003*** (0.001)	-0.004* (0.002)	0.004*** (0.001)
F-statistic for sum of post-reform coefficients [p-value]	1.44 [0.24]	1.12 [0.30]	0.00 [0.98]
F-statistic for sum of eligible post-reform coefficients [p-value]	41.38 [0.00]	4.36 [0.04]	26.46 [0.00]
Year dummies	YES	YES	YES
City dummies	YES	YES	YES
Observations	56546	52576	56546
R-squared	0.03	0.05	0.06

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

\*\*\*represents significance at 1% level, \*\* at 5%, \* at 10%

**Table 4: Housing Reforms and Access to Credit**

	Privatization reform			Trading reform		
	Log (labor income)	Log (other loans)	HH has a non- housing loan	Log (labor income)	Log (other loans)	HH has a non- housing loan
	(1)	(3)	(4)	(5)	(7)	(8)
2 years before reform	0.061 (0.040)	-0.336 (0.333)	-0.052 (0.052)	0.019 (0.028)	-0.312* (0.175)	-0.028 (0.033)
1 year before reform	0.073 (0.055)	0.003 (0.483)	-0.128 (0.078)	0.014 (0.084)	-0.378 (0.255)	-0.042 (0.058)
year of reform	0.098 (0.070)	-0.025 (0.585)	-0.133 (0.094)	0.079 (0.105)	-0.459* (0.256)	-0.057 (0.084)
1 year after reform	0.114 (0.088)	0.239 (0.642)	-0.200 (0.120)	-0.009 (0.133)	-0.593* (0.312)	-0.131 (0.105)
2 years after reform	0.186* (0.103)	0.384 (0.790)	-0.258 (0.154)	0.139 (0.186)	-0.476 (0.415)	-0.145 (0.139)
3 years after reform	0.230* (0.125)	0.118 (1.007)	-0.164 (0.200)	0.222 (0.246)	-0.782 (0.473)	-0.103 (0.165)
4 years after reform	0.303* (0.147)	-0.113 (1.231)	-0.235 (0.223)	0.126 (0.272)	-0.500 (0.616)	-0.127 (0.155)
5 years after reform	0.372* (0.186)	-0.318 (1.421)	-0.339 (0.254)	0.145 (0.318)	-0.713 (0.742)	-0.079 (0.158)
F-statistic for sum of post- reform coefficients	3.60	0.00	1.64	0.32	1.84	0.71
p-value	0.07	0.95	0.21	0.58	0.18	0.41
Year dummies	YES	YES	YES	YES	YES	YES
City dummies	YES	YES	YES	YES	YES	YES
Observations	39661	11063	42570	64335	14300	72001
R-squared	0.39	0.21	0.13	0.16	0.22	0.21

Robust standard errors in parantheses, adjusted for clustering at city-year level  
 \*\*\*represents significance at 1% level, \*\* at 5%, \* at 10%

**Table 5: Housing reforms and housing characteristics**

	Privatization reform					Trading Reform				
	Total housing area (2)	Household has own bath and toilet (4)	Log (HH spending on housing improvement) (6)	Household size (1)	Two-generation household (3)	Total housing area (8)	Household has own bath and toilet (10)	Log (HH spending on housing improvement) (12)	Household size (7)	Two-generation household (9)
2 years before reform	0.242 (0.977)	-0.005 (0.031)	-0.412*** (0.118)	-0.031 (0.044)	-0.040 (0.026)	0.237 (0.524)	-0.008 (0.028)	0.026 (0.189)	0.038* (0.021)	0.002 (0.012)
1 year before reform	1.182 (1.249)	-0.042 (0.032)	-0.317** (0.136)	-0.054 (0.077)	-0.026 (0.035)	0.850 (1.022)	-0.011 (0.039)	-0.159 (0.271)	0.037 (0.036)	0.014 (0.024)
year of reform	2.073 (1.753)	-0.047 (0.038)	-0.447*** (0.121)	-0.037 (0.082)	-0.041 (0.035)	1.418 (1.808)	-0.051 (0.058)	-0.098 (0.402)	0.074 (0.050)	0.021 (0.031)
1 year after reform	3.335 (2.237)	-0.080 (0.056)	-0.493** (0.193)	-0.037 (0.111)	-0.051 (0.049)	2.191 (2.709)	-0.096 (0.085)	-0.156 (0.477)	0.064 (0.066)	0.023 (0.044)
2 years after reform	3.148 (2.599)	-0.096 (0.088)	-0.606** (0.230)	-0.027 (0.141)	-0.054 (0.066)	3.253 (3.630)	-0.075 (0.110)	-0.327 (0.613)	0.083 (0.083)	0.055 (0.065)
3 years after reform	3.204 (3.167)	-0.100 (0.124)	-0.505* (0.282)	-0.051 (0.160)	-0.042 (0.077)	4.002 (4.397)	-0.120 (0.139)	-0.474 (0.733)	0.085 (0.093)	0.057 (0.082)
4 years after reform	3.384 (3.762)	-0.080 (0.150)	-0.434 (0.336)	-0.076 (0.181)	-0.059 (0.083)	5.277 (5.025)	-0.039 (0.166)	-0.444 (0.864)	0.114 (0.105)	0.053 (0.103)
5 years after reform	3.266 (4.276)	-0.080 (0.181)	-0.526 (0.418)	-0.123 (0.201)	-0.060 (0.088)	6.141 (5.814)	0.014 (0.196)	-0.345 (0.994)	0.124 (0.113)	0.061 (0.126)
F-statistic for sum of post-reform coefficients	1.07	0.54	3.57	0.16	0.55	0.95	0.21	0.23	1.07	0.36
p-value	0.31	0.47	0.07	0.69	0.47	0.34	0.65	0.63	0.31	0.55
Year dummies	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
City dummies	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	42561	42570	6305	42570	41533	71411	72001	12547	72001	70003
R-squared	0.14	0.31	0.18	0.04	0.03	0.15	0.15	0.07	0.02	0.02

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

\*\*\*represents significance at 1% level, \*\* at 5%, \* at 10%

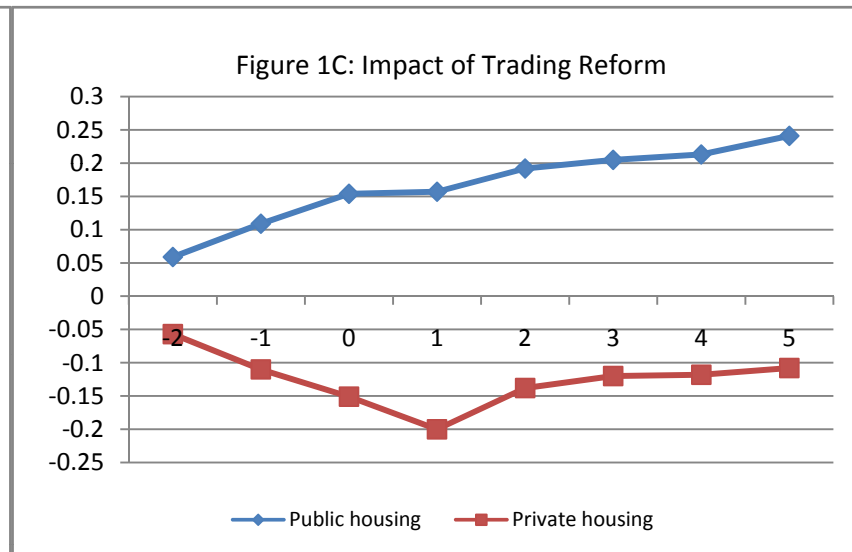
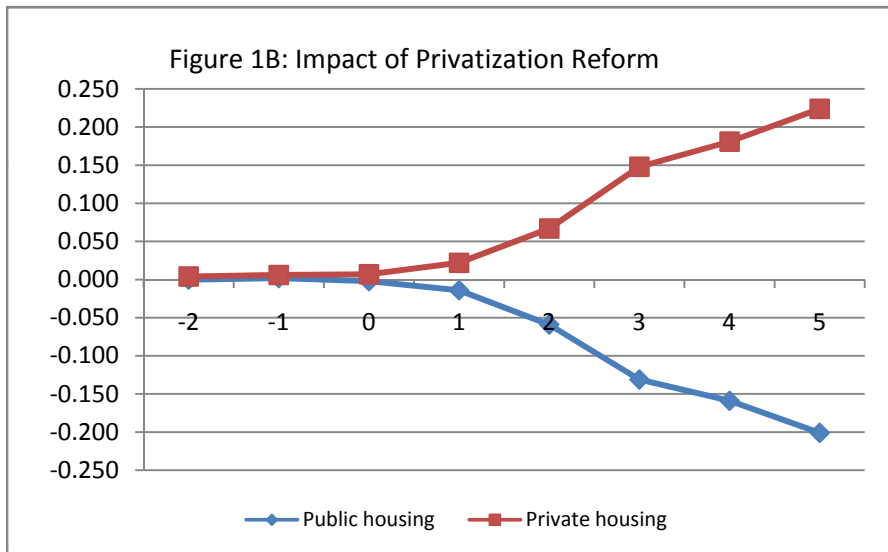
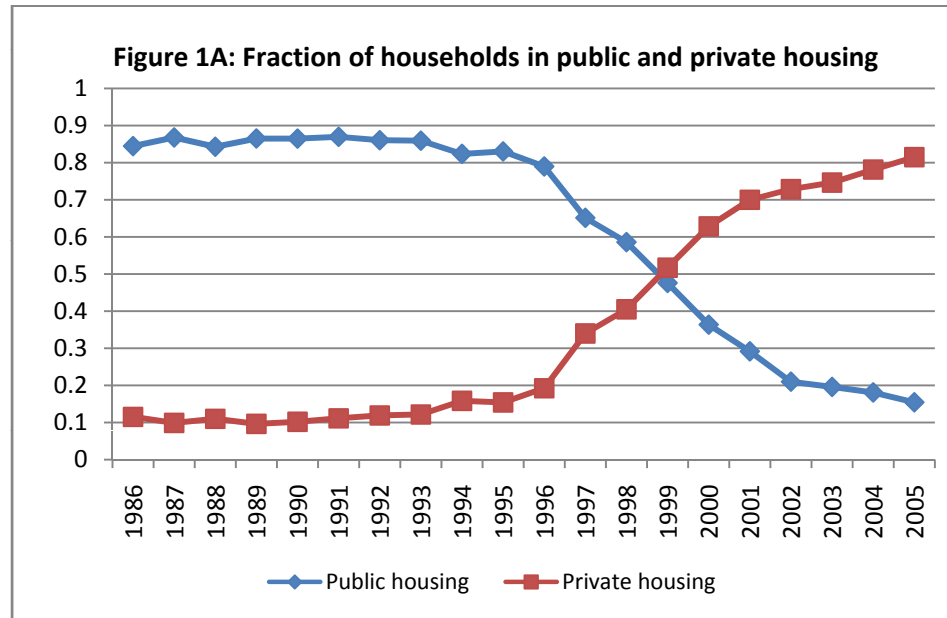




Figure 2A: Employment Trends over Time

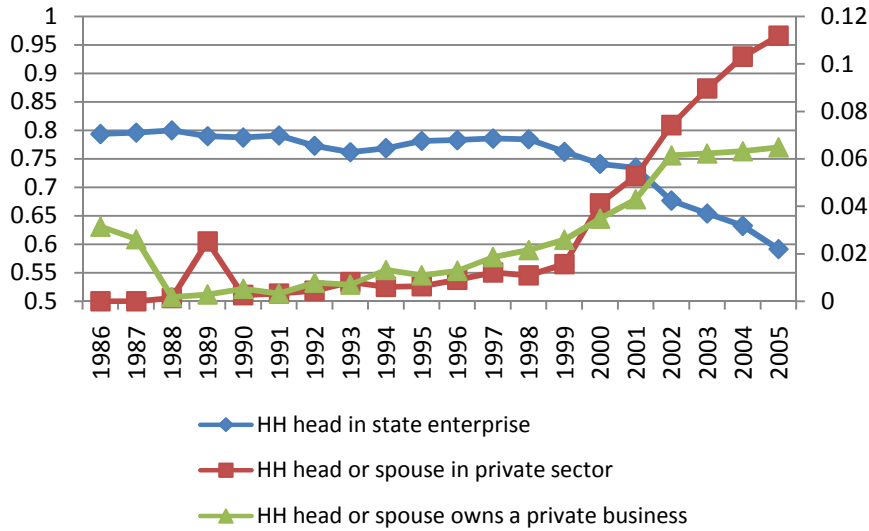


Figure 2B: Impact of Privatization Reforms

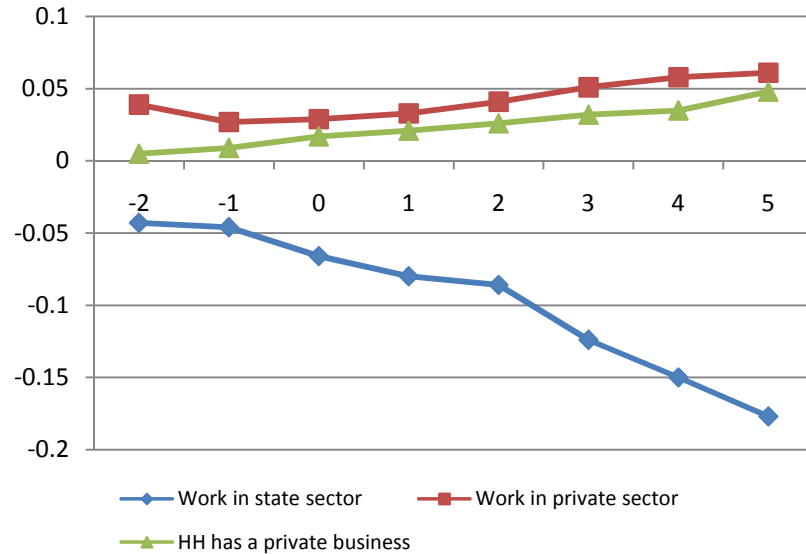


Figure 2C: Impact of Trading Reforms

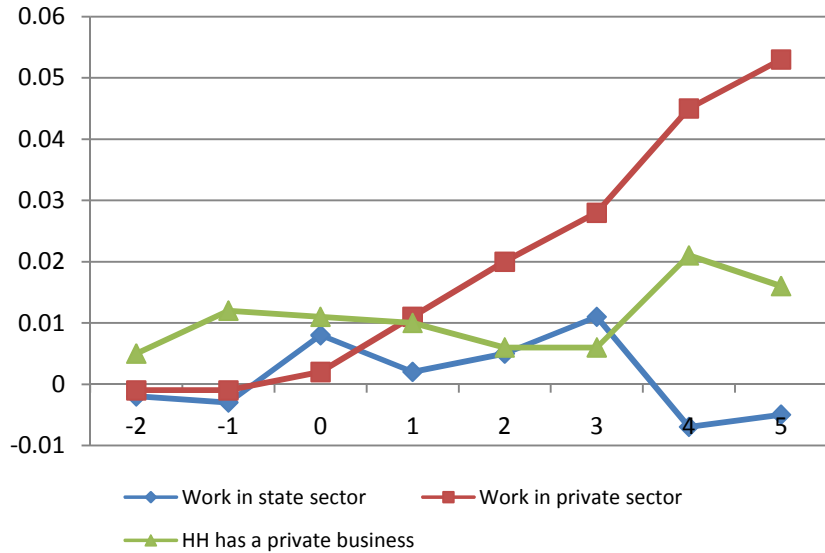


Figure 2D: Impact of trading by loan eligibility

