



Housing demand and household saving rates in china: Evidence from a housing reform[☆]



Binkai Chen^a, Xi Yang^{b,*}, Ninghua Zhong^{c,*}

^a School of Economics, Central University of Finance and Economics, China

^b Department of Economics, University of North Texas, United States

^c School of Economics and Management, Tongji University, China

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ABSTRACT

China's urban household saving rate has increased markedly since the mid-1990s, accompanied by a dramatic increase in home ownership. Is there a causal link between those two phenomena? This paper takes advantage of a unique natural experiment in China, which reformed the nationwide employer-based public housing system in 1998. This reform created an exogenous variation in housing demand among urban households. Using a difference-in-differences estimation strategy, we find evidence that the reform increased household saving rates during the reform period (1998–2001) by shifting the cost of housing services from the state to households. We also provide evidence that suggests that the 1998 housing reform affects household saving behaviors even after the reform period (2002–2009).

1. Introduction

In the past few decades, the Chinese housing market has dramatically changed. According to the Chinese Urban Household Survey (UHS), the home ownership rate among urban households was around 20% in the early 1990s, when most of the urban houses were publicly owned. The rate climbed sharply after 1998 and reached 90% in 2009, which is among the highest in the world. By comparison, the 2010 American home ownership rate was only 65.1% according to the U.S. Census Bureau. Such a rapid privatization of the housing market was unprecedented in Chinese history. During the same period, the average floor area per capita of Chinese households tripled, from 13 square meters in 1992 to 32 square meters in 2009 (Table A1), which indicates a significant improvement in living conditions in less than two decades. This increased housing demand led to soaring prices—housing prices almost tripled in China's major cities between 2000 and 2009 (Wu et al., 2012). As suggested by Fang et al. (2016), an 8 to 10 price-to-income ratio has been reached in major Chinese cities.

Despite such dramatic changes in the housing market, very little is

known about the consequences. This paper fills in the gap by linking China's volatile housing market with its economic imbalance, especially the country's unusually high saving rates. China has witnessed a significant increase in savings rates in the past two decades, with household saving as a share of disposable income nearly doubling, from 16% in 1992 to 30% in 2009. This increase resulted in a large current account surplus, which is considered a major contributor to global macroeconomic imbalances and a trigger of the recent global financial crisis (Chamon et al., 2013). From a theoretical perspective, the rising household saving rate seems puzzling because it was accompanied by rapid growth in household income. The permanent income hypothesis theory suggests that consumption instead of saving should increase with income. Both the importance of understanding global current account imbalances and the theoretical conflict have motivated a growing body of research into this unusual saving behavior. Various reasons have been given to explain the "Chinese saving puzzle," including an aging population, lack of social safety nets, precautionary saving motives, an underdeveloped financial market, and a cultural tradition of thrift (Modigliani and Cao, 2004; Chamon and Prasad, 2010; Chamon

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* Corresponding author.

E-mail addresses: chenbinkai@cufe.edu.cn (B. Chen), xi.yang@unt.edu (X. Yang), ninghua.zhong@gmail.com (N. Zhong).

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et al., 2013).

Rising housing demand has been mentioned as one of the explanations behind rising saving rates; however, current evidence on this mechanism is still relatively limited. For example, [Chamon and Prasad \(2010\)](#) give several potential explanations for China's rising saving rates during the transition period and mention that the privatization of the housing market created a private burden and new motives for saving during the transition period. In addition, [Wei and Zhang \(2011\)](#) and [Wei et al. \(2017\)](#) argue that families with sons tend to save more for buying a house to improve their sons' competitiveness in the marriage market. The main challenge of identifying and quantifying the causal effects of housing demand on household saving is the endogenous problem, because unobserved abilities or preferences could increase housing demand and household savings simultaneously, which creates a spurious correlation.

In this paper, we deal with the endogenous problem by taking advantage of a unique natural experiment in China that reformed the employer-provided public housing system in 1998 and, thus, created an exogenous variation in housing demand among urban households. Before the reform, the majority of urban residents worked for a state-owned enterprise and were living in public housing units provided by their employers with zero or highly subsidized rents.¹ To transform the public housing system into a market-based system, the reform abolished the provision of public housing across the nation, which created sudden and unexpected changes in housing demand among urban households. Before the reform, public-sector employees who lived in houses that lacked essential facilities expected their employers to help them improve their living conditions by allocating them a public house, so they did not need to save much. The 1998 reform eliminated that possibility. Now the only way for employees to improve their living conditions was to buy a private house on the market, which was usually expensive and forced them to increase their savings. In a sense, this reform acted like a negative wealth shock for those households. In contrast, the reform allowed public-sector employees who already lived in good-quality public housing units to purchase their houses at below-market prices. Because they were satisfied with their current living conditions, they had fewer incentives to save for housing. Hence, the reform provided exogenous variations in households' needs for housing demand, which allows us to better investigate the reform's impacts on household savings.

The primary data source for this paper is the UHS, which is a repeated cross-sectional data set that covers more than 230,000 households across 16 provinces between 1992 and 2009. As far as we know, it is the only nationally representative household data set in China that contains yearly information dating back to the early 1990s. This feature enables us to study the link between the 1998 housing reform and the rising household saving rates.

Using this data set and the exogenous variation created by the 1998 reform, we provide two sets for empirical tests of the housing demand hypothesis. First, we adopt a difference-in-difference (DID) approach and compare public-sector employees' saving rates before and after the reform for households that had and had not obtained high-quality housing under the old system.² We also consider another control group, which includes households that did not include state employees. Those households did not expect to receive public housing benefits before or after the reform, so they were not affected by the reform except through the general equilibrium effect. The DID model allows us to use the policy shocks by comparing changes in saving rates among otherwise

similar households before and after the housing reform. We find a significantly larger increase in saving rates of households that lived in low-quality housing units in 1999–2001 compared to those in 1992–1998, which suggests that the sudden increase in housing demand played an important role in explaining the rising household savings during the reform period. Overall, our baseline estimation suggests that the household saving rate increases by around 2 percentage points during the reform period (1998–2002). The average household saving rate increased by about 4 percentage points from 17% in 1998 to 21% in 2002, suggesting that the reform can explain about half of the increase in the household saving rates.³

More important, because of the rising housing prices and underdeveloped mortgage market, we expect that the suddenly generated housing demand kept driving up China's saving rates even after the 1998 reform. To test this conjecture, we explore household saving rates between 2002 and 2009. In particular, we divide households by whether they obtained their housing from the old system or the private market (public and private housing residents). We find that private housing residents save about 1.3% more than public housing residents, which suggests that rising housing demands were indeed contributing to the high saving rates in more recent years.

The rest of this paper is structured as follows. [Section 2](#) introduces China's household saving rate and the development of its urban housing market over time. [Section 3](#) describes the theoretical framework for how the housing reform boosts household savings. [Section 4](#) describes the UHS data. [Section 5](#) presents our empirical strategies, estimation results, and robustness checks. [Section 6](#) explores the long-term effects of the housing reform. We conclude with [Section 7](#).

2. Institutional background and literature review

2.1. China's rising saving rate: facts and explanations

China's household saving rate experienced a significant increase during the past two decades. As shown in [Fig. 1](#), according to the National Bureau of Statistics of China, the average saving rate (as a share of household disposable income) for urban households in China increased by about 5 percentage points during the 1990s, and then it rose sharply by another 10 percentage points during the next decade, reaching around 30% by 2009.⁴

The cross-country comparison in [Table 1](#) further illustrates China's high saving rate. In 2011, China's aggregate saving rate was among the highest in the world. It was not only higher than rates in developed countries (Germany, the United Kingdom, and the United States) but also higher than those in countries at a similar stage of development (Brazil and India), as well as than those with a similar culture (Japan and South Korea). The results suggest that neither economic development status nor cultural norms can fully explain China's high saving rates.

Many other explanations have been put forth in the existing literature. The first is based on the life-cycle theory ([Ando and Modigliani, 1963](#); [Modigliani and Cao, 2004](#)), which argues that China's saving rates are driven up by the rising share of the labor force in the population. However, [Chamon and Prasad \(2010\)](#) find that this explanation

³ Our baseline estimation suggests that the household saving rate increased by around 2 percentage points during the reform period. The average household saving rate increased by about 4 percentage points from 17% in 1998 to 21% in 2002, which suggests that the reform can explain about half of the increase in household saving rates.

⁴ According to the UHS, the household saving rate is around 22%. This is because the UHS over-samples public-sector employees who face relatively less income uncertainties and thus have lower saving rates than the general population. [Chamon and Prasad \(2010\)](#) also adopted the UHS to describe household saving rates in China, and they reported a similar pattern of household saving rates in China as in our paper.

¹ Unlike public housing in high-income countries, which is provided as a welfare benefit to low-income households, before 1998, public housing in China was an in-kind benefit to employees in the public sector.

² High-quality housing includes two- or three-bedroom apartments, while low-quality housing mainly consists of staff dormitories without individual bathrooms or kitchens.

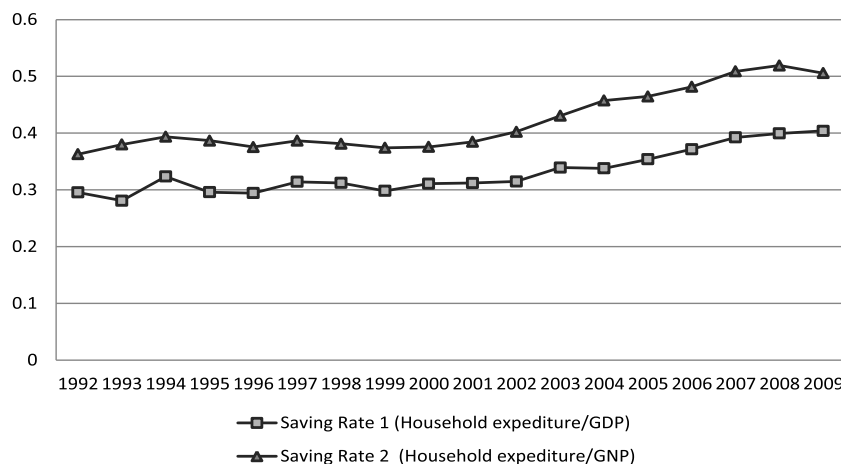


Fig. 1. Household saving rates: 1992–2009.

Table 1
Household saving rates of different counties (2011).

Country	U.S.	U.K.	Germany	Japan	South Korea	India	Brazil	China
Total Consumption as % of GDP	84	85	75	82	66	70	82	51
Household Consumption as % of GDP	68	65	56	61	51	59	62	37
Saving as % of GDP	16	15	25	18	34	30	18	49

Source: World Development Indicator (WDI). Available at <http://data.worldbank.org/indicator/NE.CON.TETC.ZS>.

is inconsistent with the profile of consumption and savings at the household level in China, since older people save more than middle-aged people. The second explanation is related to liquidity constraints (Kraay, 2000; Aziz and Cui, 2007), which suggests that the underdevelopment of China’s financial market has forced households to save more. Nevertheless, the efficiency of these markets improved even as the household saving rate kept rising, which suggests that the level of financial market development plays, at best, a minor role in household saving. The third explanation involves precautionary saving motives (Meng, 2003; Blanchard and Giavazzi, 2006; Giles and Yoo, 2007; Chamon and Prasad, 2010; Song and Yang, 2010; Chamon et al., 2013; He et al., 2017). This perspective argues that China’s pension, education, housing system, and state-owned enterprise (SOE) reforms have increased the uncertainty of household income and expenditure and, hence, have correspondingly increased household saving.

The rising housing demand has been mentioned as an important contributor to China’s high saving rates. For example, Chamon and Prasad (2010) argue that the rapidly rising private burdens of housing accounts for a 3 percentage point increase in savings rates since the early 1990s. They also find that the saving rate is higher for younger and older households than for middle-aged households, which is consistent with the narrative that younger and older households are more likely to save for purchasing a house (personally or for adult children). The housing demand explanation also appears in Wei and Zhang (2011) and Wei et al. (2017), who argue that, as the sex imbalance rises, families with sons tend to save more for buying a house to improve their sons’ competitiveness in the marriage market. However, few papers have directly studied the effect of changes in the housing demand on household savings. Our paper adds to the literature by studying this link using the 1998 reform of employer-provided housing as a natural experiment. We briefly introduce this reform next.

2.2. China’s urban housing reform

When the Chinese Communist Party took control in 1949, it established a system that guaranteed jobs and houses for all urban workers. Under this system, the majority of urban residents were employed in the public sector⁵ and lived in public housing units. The housing units were allocated, usually free or at a highly subsidized price, to state employees as in-kind compensation. Since the nominal rent collected did not even cover the cost of basic maintenance, there was little incentive for housing investment and improvement. As a result, housing stock was continually shrinking, and urban living conditions were continuously deteriorating. The per capita living space, for example, declined from 4.5 square meters in the early 1950s to 3.6 square meters in the late 1970s. The majority of urban residents had to live in shared dormitories, which were usually small and lacked essential facilities. Public employees might move into public housing with better living conditions, but only after several years of waiting. This scheme of housing allocation not only largely depressed housing consumption and generated serious complaints from the public, but also caused a large financial burden for the central government.⁶ In the late 1970s, the government was forced to reform the old system.

In the early stage of urban housing reforms (1979–1988), the government took a progressive approach. The reforms were implemented in certain selected cities, and included raising rents and promoting sales of public housing (Wang and Murie, 2000). Nationwide housing reform began in 1991,⁷ when public housing was sold to current tenants for the

⁵ Public-sector employees include SOE and government employees.

⁶ As summarized in Wang and Murie (1999), state-owned housing had other problems including poor management and corruption with the distribution.

⁷ The initial attempt of nationwide housing reform began in 1988. However, it was interrupted in 1989 by a political event—the Tiananmen Square protests.

first time. In 1994, the government established a more comprehensive framework to facilitate the privatization of public housing stocks. On the demand side, the 1994 reform specified the contracts of purchasing public housing,⁸ while on the supply side, private-sector firms were allowed to enter the real estate industry and construct commercial houses for the first time.⁹

The goal of the 1994 reforms was to establish a functional housing market in which families could directly purchase housing, so that the government could be relieved from its housing responsibility. Unfortunately, this aim did not happen easily. Immediately after the 1994 reform, China saw an unprecedented housing construction boom. However, instead of housing units being sold to individuals, most were purchased by work units, which resold them at deeply discounted prices to their employees (Wang and Murie, 1996). Since many of the housing units were owned by work units and were not subject to hard budget constraints, their purchase significantly distorted the emerging housing market.

In 1998, the central government, to speed up urban housing reform and to encourage individual participation in the housing market, decided to abolish the public housing system completely. Work units were prohibited from building or providing housing units for their employees. Urban employees had to buy either available public houses from their work units or commercial houses from the market. The 1998 reform marked the turning point of China's housing reform. With the reform's implementation, China had finally established a market mechanism for both housing production and consumption. Since then, private-market housing transactions have become more and more prevalent for Chinese households. Since 2002, more than 80% of public housing has been sold to individuals (Wang and Murie, 2000). After 2002, reforms in the housing sector have focused on developing and regulating the housing loan market.

Although the consequences of the housing market reforms are widely recognized in the literature (Chen, 1996; 1998; Wang and Murie, 1996; 1999; 2000; Fu et al., 2000; Huang and Clark, 2002; Wang, 2011), not much is known about the effects of housing reform beyond the housing market. To the best of our knowledge, only two other papers have studied the effects of housing reform outside of the housing market, and they both focus on labor market outcomes. The first is by Wang (2012), who estimates the effect of one housing reform on job mobility and entrepreneurship. The second is by Iyer et al. (2009), who examines the effects of the city-specific timing of one reform on labor mobility. Both papers made use of the 1994 reform in which households became entitled to property rights by purchasing previously state-owned housing units. Our paper is different for two reasons. First, we emphasize the effects of the 1998 reform and the abolishment of public housing. Second, instead of labor mobility, we focus on the effects of the housing reform on household savings.

3. Analytical framework

The abolishment of public housing in 1998 created a sudden and unexpected change in housing demand among urban households, which resulted in divergent saving behaviors among urban households in the short and long run. Before 1998, most urban households worked for the public sector and lived in public houses for free or with highly

⁸ To be more specific, here are two examples. On the one hand, households could pay the market price and have full property rights, including the right to resell on the open market. On the other hand, households could pay the subsidized price and have partial property rights with imposed restrictions on the resale of the house. Homeowners who have partial property rights cannot sell their house within five years, and when they sell the house after five years, they have to share the profits with their work units.

⁹ While the state owned all the land during this period, private-sector firms purchased land use rights for 70 years. Land use rights include the right to participate in secondary markets and to rent out the use of the land to others. See Lin and Ho (2003) for more details on land use rights.

subsidized rent, so they had no motive to own a house or to save money for purchasing one. The 1998 reform fundamentally changed the way households obtained their housing services by shifting the financial burden from the government to individual households. Depending on their current living conditions and employment status, households reacted differently to this policy change.

First, public-sector employees who were living in houses that lacked essential facilities had stronger incentives to improve their living conditions. Before the reform, they could only wait for their employers to help them, but afterward, they could improve their living conditions by purchasing a private-market house. Those houses, however, were much more expensive. During the reform period (1998–2001), a decent house in China's major cities cost around US\$10,000, which might seem low compared to today's prices, but was about four times the average annual income at that time. Without a mature mortgage market, purchasing a house from the private market required a substantial one-time payment, which forced some households to save disproportionately more than other households. Thus, the abolishment of public housing created a new and strong saving motive for public-sector employees who were living in low-quality houses.

On the contrary, public-sector employees who already lived in good-quality public housing units had relatively less incentive to save for housing because they were already satisfied with their current houses and the 1998 reform allowed them to purchase the houses at below-market prices.¹⁰ Even if they wanted to purchase a newer house, they faced a smaller financial burden because they could sell their privatized public house at market value and use the income from the sale to finance a new house.

Private-sector employees were not eligible for those housing benefits, so their saving behaviors are less likely to have been affected by the abolishment of public housing except through a general equilibrium effect. Overall, we expect that public-sector employees living in low-quality public housing experienced a much greater increase in household saving rates compared to public-sector employees living in better public housing or to private-sector employees. The fact that different groups of households reacted differently to the housing reform make it possible for us to identify the causal link between housing reform and household saving rates through a DID approach, as detailed in Section 5.

After the reform period, the private burden of housing continued to rise because housing prices soared throughout major cities. In recent years, the average housing price-to-income ratio (for a 30-square-meter living space) has reached 12 in Beijing and Shanghai, and around 8 in most other cities (Fang et al., 2016). Considering that China's mortgage market is underdeveloped and that personal saving is still the most common way for homebuyers to finance their homes, those numbers suggest that a household has to save 8 to 10 times its annual disposable income before buying a home. Without a mortgage, many home buyers rely on borrowing money from parents and immediate family members. This borrowing could be viewed as a hidden loan; children receive their parents' financial support for housing but must pay it back by supporting their parents in old age. Even with a mortgage, expenditures on housing could still consume a substantial fraction of household income. For example, suppose the price-to-income ratio is 8, and the household made a down payment of 40% and took a loan with a 30-year maturity and a 6% annual interest rate.¹¹ Buying a home would require the household to save 3.2 times the annual household income to make the

¹⁰ Data from the Chinese Household Income Project, which covers urban areas in 11 provinces, indicate that the average difference between the market value and the price charged by the government was 24,462 yuan, which is more than 2 times the average annual income of a household. The generous price subsidies allowed most households to buy their homes outright.

¹¹ The annual interest rate of 6% is rather low relative to the rate observed in recent years.

¹² According to the UHS, only 2.20% of households purchased their homes with mortgages in 2001, and this number only increased to 26.56% in 2009.

down payment and another 45% of its annual income to service the mortgage loan.

Given those factors, we expect that the abolishment of public housing in 1998 has had lasting effects on Chinese household saving. To investigate the extent to which housing-related motives can help explain the high and rising household saving rates in more recent years, we continue to use the exogenous variation in housing demand created by the 1998 reform. We hypothesize that with the rising costs of owning a house, households with housing demands that were satisfied by the old public housing system were likely to save less than households that needed to purchase houses from the private market. In particular, we expect to see a growing gap in saving rates between those two groups of households as housing prices rise. We test the hypotheses in Section 5.

4. Data

The primary data used in this study come from the UHS, which was collected annually by China's National Bureau of Statistics. The UHS is a repeated cross-sectional data set that surveyed urban households from 1992 to 2009. It provides comprehensive information regarding socioeconomic and demographic characteristics among urban households.¹³ In particular, it provides detailed information on household income and expenditures, property type, and housing characteristics that are critical for our paper. To our knowledge, the UHS is the only nationally representative household data set in China that contains yearly information dating back to the early 1990s, which enables us to study the link between the historical changes in the housing market and rising household saving rates.¹⁴

The UHS underwent a major revision in 2002, during which more samples and survey questions were added. Thus, the number of households covered by the UHS increased from about 11,000 households per year in 1992–2001 to about 20,000 households per year in 2002–2009. To study the effects of housing reform and the resulting housing ownership status on household saving rates in the short and long run, we use both 1992–2001 and 2002–2009 data sets. We restrict the sample to households in which the household heads were non-agricultural workers between the ages of 20 and 60. Moreover, we exclude households in which household heads were enrolled in school, retired, or self-employed, as well as those with missing values for the key variables. For the 2002–2009 sample, we exclude households in which property types were not well defined.¹⁵ Our final sample

¹³ The UHS mainly covers households that are registered as urban residences (urban Hukou), so migrant populations are excluded from the sample.

¹⁴ The UHS samples urban households in all 31 provinces in China. But due to access restrictions, we only can access data of 15 provinces (Beijing, Shanxi, Liaoning, Heilongjiang, Jiangsu, Anhui, Jiangxi, Shandong, Henan, Hubei, Guangdong, Chongqing, Sichuan, Yunnan, and Gansu). The 15 provinces vary considerably in their geography and levels of economic development and, thus, roughly represent the country. At the city level, the UHS covers about 110 cities, which include first-tier cities, such as Beijing and Guangzhou; and about 20 second-tier cities, which include autonomous municipalities, provincial capitals, and vital industrial or commercial centers; and about 80 third-tier cities.

¹⁵ The 2002–2009 UHS categorizes property types as (1) own a privatized public house, (b) rent a private house and (c) own a commodity house. We define the first group as “public housing residents” and the latter two groups as “private housing residents.” Besides the three groups, the UHS defines a fourth group as households that rent a public housing unit. This group is 10% of the UHS sample between 2002 and 2009. After the nationwide abolishment of the employee-based public housing system, households still rented public houses for two reasons. First, most public universities kept the old housing system longer than other institutions, so their employees were likely to be renting public housing during 2002–2009. Moreover, after replacing the employee-based public housing system, the government started launching public housing for low-income people, especially after 2007. We cannot distinguish those two groups of households based on questions asked in the UHS. This issue makes it difficult to interpret the estimation results, so we excluded public housing

includes 71,325 households for 1992–2001 and 166,986 households for 2002–2009.

We calculate household savings as the difference between disposable income¹⁶ and consumption expenditure. Disposable income includes labor income, property income, transfers (both social and private, including gifts), and income from household sideline production. The consumption expenditure covers a broad range of categories including food, clothing and footwear, household appliances, goods and services, medical care and health, transportation and communication, recreational activities, and education expenditures. The household saving rate is then calculated as the ratio of household saving to disposable income. All monetary variables are deflated to 2009 yuan using national urban consumer price indices.

A basic summary of statistics for the sampled households is provided in Table 2. The 1992–2001 sample contains household heads with an average age of 41. Almost 51% of the household heads were men, about 23% had a college or higher degree, and 44% graduated from high school. The average household size was three persons, and about 71% of the household had one member employed in an SOE. On average, annual disposable income and consumption were 19,459 yuan and 15,736 yuan, respectively, with 18% used for savings. The 2002–2009 sample contains household heads who were slightly older, with an average age of 42. The education level was much higher with 34% of household heads holding a college or higher degree. Annual consumption and household income almost doubled between 2001 and 2009 (26,227 yuan and 36,030 yuan, respectively), while the saving rate increased to 22%.

In the UHS, housing tenure status can be consistently defined as a homeowner, public housing renter, and private housing renter. The upper panel in Fig. 2 describes the composition of the three types of housing tenure status from 1992 to 2009. One striking pattern is that home ownership experienced a rapid increase during this period. In the early 1990s before the housing market privatization took place, most of the housing stocks were owned by the government and the majority of urban residences were public housing renters (around 84%). Homeownership was less than 20% in 1992 and experienced a steady increase before the 1998 housing reform. With rapid privatization of the public housing stocks, during the reform (1998–2001), homeownership almost doubled, increasing from around 40% to 80%. At the same time, the proportion of households that rented public housing decreased dramatically from 50% to 20%. Beginning in 2002, the UHS added private housing market renter as a new category, and the data show that the proportion of private housing market renter was stable at around 3% from 2002 to 2009. In the lower panel of Fig. 2, we show that housing tenure status seems to be an important factor affecting household saving rates. Household saving rates increased during the sample period for all three types of housing tenures, which is consistent with Fig. 1, where the data comes from the National Bureau of Statistics. Meanwhile, homeowners always have higher saving rates than renters, probably because homeowners usually have a higher household income. Among renters, household saving rates are slightly higher for private market renters than for public housing renters.

5. Estimation

5.1. Estimation strategy

The 1998 housing reform created a sudden and unexpected housing demand shock among urban households, which enables a DID approach

(footnote continued)

renters from the regression sample. The main estimation results vary little after adding this group of households.

¹⁶ The UHS reports disposable income, which is household income after deduction of taxes and government transfers.

Table 2
Summary of statistics: 1992–2009.

	Mean	S.D.	Min	Max
1992–2001				
Household Head Age	41.34	8.08	20.00	64.00
Household Head Female	0.34	0.47	0.00	1.00
Household Head College	0.23	0.41	0.00	1.00
Household Head High School	0.44	0.49	0.00	1.00
Household Head Work in Public Sector	0.71	0.25	0.00	1.00
Household Size	3.62	0.66	1.00	8.00
Household Consumption (RMB)	15736	7929	4756	49019
Household Disposable Income (RMB)	19459	9720	5832	58924
Household Saving Rate	0.18	0.20	-0.52	0.59
Observations	99,325			
2002–2009				
Household Head Age	42.06	7.59	20.00	64.00
Household Head Female	0.29	0.45	0.00	1.00
Household Head College	0.34	0.47	0.00	1.00
Household Head High School	0.39	0.49	0.00	1.00
Household Head Work in Public Sector	0.71	0.28	0.00	1.00
Household Size	2.94	0.71	1.00	9.00
Household Consumption (RMB)	26227	15935	4488	109450
Household Disposable Income (RMB)	36030	21909	5678	134792
Household Saving Rate	0.22	0.27	-0.95	0.74
Observations	166,986			

Source: UHS 1992–2009. Note: The UHS went through a revision in 2002 when more variables and observations were added. We divide the data into a 1992–2001 sample and a 2002–2009 sample. We use the 1992–2001 sample to study the effect of the 1998 reform on the household saving rate, and the 2002–2009 sample to explore the long-term implications of the 1998 reform. All monetary variables are deflated to 2009 yuan using national urban consumer price indices.

to estimate the effect of housing-related motives on household saving rates. As discussed in Section 3, households reacted differently to the housing reform depending on their housing condition and employment status. First, households living in low-quality houses would have much more incentive to purchase new houses compared with households living in good-quality houses. The UHS reports housing conditions in four areas: bathroom, water supply, heating system, and cooking fuel. We define the living condition as poor if the residence lacked at least two of the four areas.¹⁷

Moreover, only public-sector employees are likely to experience changes in housing demand because private-sector employees would not expect to receive such housing benefits. Thus, we define the treatment and control groups based on a household’s housing condition and employment status. Specifically, we define the treatment group as households that lived in houses of poor condition and that had at least one member employed in the public sector (household head or spouse).¹⁸

We define two control groups. The first control group consists of households that lived in houses of good condition and that had at least one member who worked in the public sector. We call this group the “state-employed control group.” The second control group includes households with both adult members working in the private sector. We call this group the “privately-employed control group.” Table 3 presents

¹⁷ We also used three alternative ways to define poor housing conditions. The first one uses the presence of a private bathroom as the only criteria. The second one explores housing types and defines a collective dormitory as poor housing. The third one define housing conditions as poor if the per capita floor area lower than the 30 percentile city-level per capita floor area. More details about the three definitions are reported in the Appendix Table A3. Using the three alternative definitions, we derived similar estimation results to the baseline measurement (Appendix Table A4).

¹⁸ According to policies at that time, as long as one household member was a public-sector employee, the household was qualified for living in a public house. In 1998, the public sector covered about 80% of households.

the summary statistics for the treatment and the two control groups before the 1998 housing reform. The treatment group accounts for about 27% of the entire sample. The state-employed control group and the private-employed control group account for 50% and 24%, respectively. The treatment group is statistically similar to the control groups along several dimensions, including age, years of education, and family size. However, the treatment group is also different from the control groups in several ways. For example, treatment households have relatively lower consumption and income. The difference-in-difference models we adopt in this paper allows difference across treatment and control groups, but require the difference to follow a common trend.

To test the parallel trend assumption, we compare the changes in household saving rates between 1992 and 2001 among households in the treatment and control groups in Fig. 3. We found, the saving rates for all three groups were at a similar level and were relatively flat before 1998. Only after 1998, saving rates among households in the treatment group begun to increase rapidly, while households saving rates for the two control groups experienced just moderately increases during the same time period. The time-series patterns provide visual evidence for a break in saving rates trends following the start of the reform in 1998, suggesting a pre-parallel trend which is critical for the validation of our difference-in-difference estimation. That is, in the absence of the 1998 reform, the difference between the treatment and control groups should be constant over time. Moreover, in the Appendix Fig. A1, we decompose household saving rate into income and savings and present the trend of the two variables for the treatment and the two control groups before and after the 1998 reform (1992–2001). We find household incomes in the treatment group are always lower than household incomes in the two control groups and they follow parallel trends between 1992 and 2001. In the meantime, household savings in the treatment group are always lower than household incomes in the two control groups, but they experienced a jump in 1998. These results are consistent with Fig. 3 and provide further confirmation of the parallel trend assumption.

We implement the DID analysis in the following regression equation:

$$S_{it} = \alpha_0 + \alpha_1 Treatment_{it} * Post_t + \alpha_2 Treatment_{it} + \alpha_3 X_{it} + D_t + \epsilon_{it} \quad (1)$$

where S_{it} is the household saving rate, $Treatment_{it}$ identifies the treatment group, and $Post_t$ is a dummy variable that equals 1 for years after 1998 and 0 for years before 1998. The vector of covariances, X_{it} , include household income and household size. In addition, since the theory of precautionary savings implies that uncertainty regarding future income are relevant to household saving behavior, thus, we include in X_{it} a set of variables related to future household income including household head characteristics, such as age, education, and occupation dummies. D_t is the year dummy variable. The coefficient, α_1 , is the estimated effect of the housing reform. Throughout this paper, the standard errors are adjusted to allow for clustering at the city level to account for correlation in the city-level errors over time. We conduct the DID estimation using one of the control groups each time.

5.2. Estimation results

Table 4 summarizes the estimation results from Eq. (1) using the state-employed control group. We consider several different regression specifications. In column (1), we report the results for a preliminary DID estimation in which the coefficient of interests is that for the interaction term of *Treatment* and *Post*. Columns (2) and (3) control for unobserved local-level economic factors by including province or city dummies separately. The estimated coefficients for the interaction term are significantly positive across all specifications, suggesting that after the 1998 reform, household saving rates increased more among the treatment group compared with the control groups. We use the specification in column (3) as the baseline model. According to this

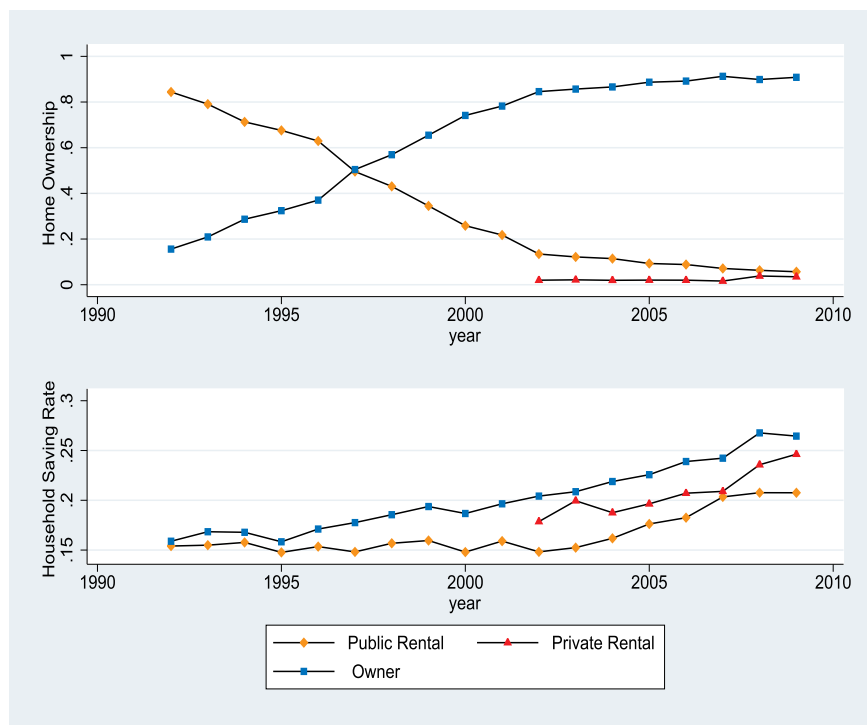


Fig. 2. Housing tenure status and household saving rates: 1992–2009. Note: In the UHS 1992–2009, housing tenure status can be consistently defined as homeowner, public housing renter, and private housing renter. The upper panel illustrates the percentages of the three types of housing tenure between 1992 and 2009. The lower panel illustrates household saving rates among the three types of households between 1992 and 2009.

Table 3
Summary statistics of the treatment and control groups: 1992–2001.

	Treatment Group		State-Employed		Private-Employed	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Household Head Age	40.26	(8.51)	42.02	(8.02)	46.99	(8.56)
Household Head Female	0.34	(0.45)	0.33	(0.44)	0.33	(0.49)
Household Head College	0.22	(0.39)	0.24	(0.41)	0.22	(0.44)
Household Head High School	0.41	(0.52)	0.44	(0.49)	0.40	(0.47)
Household Size	3.49	(0.71)	3.71	(0.63)	3.57	(0.63)
Household Consumption (RMB)	13541	(8081)	15332	(7640)	16081	(7755)
Household Disposable Income (RMB)	17667	(9032)	19571	(9703)	20629	(9320)
Household Saving Rate	0.17	(0.20)	0.18	(0.23)	0.18	(0.29)
Observations	26,817		49,662		22,844	
	27%		50%		23%	

Note: The treatment group includes households that lived in low-quality housing that have at least one member employed in the public sector. The first control group, state-employed control group, consists of households that live in good-quality housing that have at least one member employed in the public sector. The second control group, privately-employed control group, includes households with both members employed in the private sector. All monetary variables are deflated to 2009 yuan using national urban consumer price indices.

specification, the saving rates of the treatment households increased by about 2.1 percentage points more after the reform compared to households in the state-employed control group.

Our baseline results are robust to several different specifications. First, although we have controlled potential time-invariant local economic factors by including province or city dummies, the 1998 reform may be accompanied by certain time-varying macroeconomic shocks and labor market condition that affect our treated and control groups differently. To further control those characteristics, we include the province and year interaction dummies in column (4), and find that the estimate of interest hardly changes.

The accuracy of the DID estimate depends on the assumption that the composition of households in the different groups is unchanged over time. This assumption might be violated if, for example, only

households with limited financial resources lived in low-condition houses after the reform. In this case, the DID estimations may underestimate the effect of the reform. To deal with this problem, we use data from only one year before and after the 1998 reform and repeat the baseline DID regression in column (5). The idea is that, within a relatively short period, the composition of households in each group will be relatively stable. As expected, the estimated effects stay positive and statistically significant, with a slightly smaller magnitude.

Column (6) addresses a concern with the megacity effect. It is well known that the megacities in China, such as Beijing and Guangzhou, are significantly different from other cities regarding government policies and economic development. To ensure that our results are not driven by the unique features in megacities, we exclude those two cities from our sample and repeat our baseline regression. Column (6) reports the

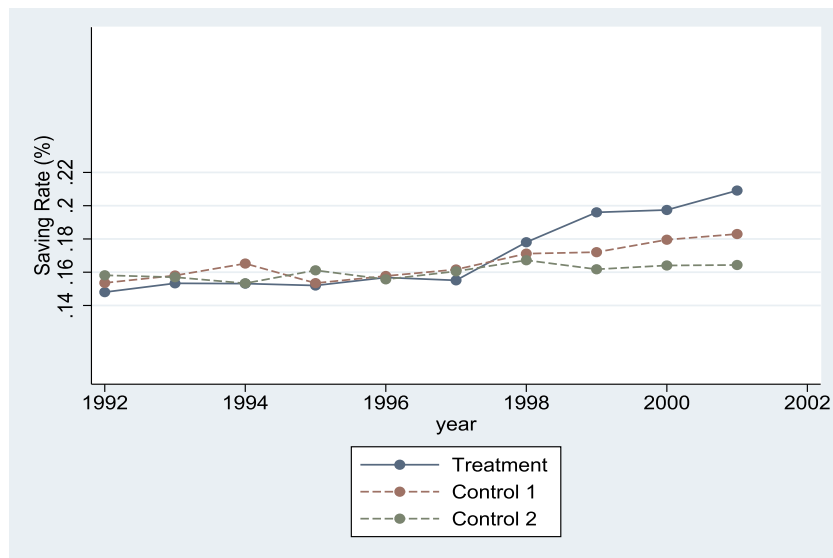


Fig. 3. Household saving rates for treatment and control groups: 1992–2001. Note: The treatment group includes households that lived in low-quality housing that have at least one member employed in the public sector. The first control group, state-employed control group, consists of households that live in good-quality housing that have at least one member employed in the public sector. The second control group, privately-employed control group, includes households with both members employed in the private sector.

Table 4
The 1998 Reform and Household Saving Rates (1992–2001) DID: State-Employed Control Group.

	(1)	(2)	(3)	(4)	(5)	(6)
Post98*Treatment	0.026*** (0.007)	0.022*** (0.007)	0.021*** (0.006)	0.013** (0.006)	0.018* (0.009)	0.014*** (0.005)
Treatment	0.019*** (0.004)	0.028*** (0.004)	0.013*** (0.003)	0.016*** (0.003)	0.017*** (0.006)	0.011*** (0.003)
log(Income)	0.140*** (0.005)	0.197*** (0.004)	0.218*** (0.003)	0.219*** (0.003)	0.172*** (0.005)	0.167*** (0.003)
Age	-0.020*** (0.001)	-0.016*** (0.001)	-0.011*** (0.001)	-0.011*** (0.001)	-0.012*** (0.001)	-0.009*** (0.001)
Age squared	0.024*** (0.001)	0.019*** (0.001)	0.014*** (0.001)	0.014*** (0.001)	0.014*** (0.002)	0.011*** (0.001)
Female	-0.001 (0.001)	-0.004*** (0.001)	-0.003*** (0.001)	-0.003*** (0.001)	-0.002* (0.001)	-0.002*** (0.001)
College	-0.008*** (0.003)	-0.018*** (0.003)	-0.014*** (0.003)	-0.014*** (0.003)	-0.011*** (0.004)	-0.012*** (0.002)
High School	-0.004* (0.002)	-0.011*** (0.002)	-0.008*** (0.002)	-0.008*** (0.002)	-0.005* (0.003)	-0.007*** (0.001)
Household Size	0.061*** (0.003)	0.086*** (0.002)	0.090*** (0.002)	0.091*** (0.002)	0.076*** (0.003)	0.070*** (0.002)
Constant	-0.445*** (0.082)	-1.017*** (0.077)	-1.273*** (0.073)	-1.238*** (0.072)	-0.954*** (0.114)	-0.900*** (0.069)
Occupation dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
City dummies	No	No	Yes	Yes	Yes	Yes
Prov dummies	No	Yes	No	No	No	No
Prov*Year dummies	No	No	No	Yes	No	No
Observations	76,479	76,479	76,479	76,479	24,855	67,853
R ²	0.073	0.111	0.147	0.152	0.141	0.130

Note: Column (3) is the baseline specification. Column (4) adds the interactions of province and year dummies. Column (5) uses the 1997–1999 sample instead of the 1992–2001 sample. Column (6) mitigates the mega city effect by excluding two mega cities (Beijing and Guangzhou). In the UHS, occupations are divided into seven categories: managers, technicians, public-sector workers, service-sector workers, business sector workers, manufacturing and transportation workers, and others. Standard errors in brackets are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

estimation results with megacities excluded from the estimation sample. The estimates we obtained are in line with the other specifications, with a slightly smaller coefficient. Overall, the DID estimates vary little across different specifications, which suggests that our baseline results are robust.

Table 5 reports the estimation results from Eq. (1) using the privately-employed control group. We consider a similar set of specifications as in Table 4. The estimates suggest that the saving rates of the treatment households increased by about 2.4 percentage points more after the reform, compared to households employed in the private sector. The results are robust across different specifications, which confirms our hypothesis that the housing reform increased household

saving rates. Because the two control groups differ substantially from each other in some characteristics, it is reassuring to derive similar estimates by using the two control groups.¹⁹

¹⁹ Fig. 3 shows that household saving rates are higher for households in the treatment group than households in other two control groups after 1998. In particular, the gap is greater when using the private-employed control group than using the public-employed control group. This pattern is consistent with our estimation results which find the effect of the 1998 housing reform is greater when we adopt privately-employed households instead of public-employed households as the control group.

Table 5
The 1998 reform and household saving rates (1992–2001) DID: privately-employed control group .

	(1)	(2)	(3)	(4)	(5)	(6)
Post98*Treatment	0.025*** (0.007)	0.024** (0.010)	0.024*** (0.009)	0.022*** (0.009)	0.022* (0.012)	0.019** (0.008)
Treatment	0.057*** (0.006)	0.055*** (0.007)	0.034*** (0.007)	0.034*** (0.007)	0.022** (0.010)	0.026*** (0.006)
log(Income)	0.152*** (0.005)	0.208*** (0.005)	0.226*** (0.005)	0.228*** (0.005)	0.176*** (0.008)	0.172*** (0.004)
Age	-0.014*** (0.002)	-0.012*** (0.002)	-0.009*** (0.002)	-0.009*** (0.002)	-0.009*** (0.003)	-0.008*** (0.001)
Age Squared	0.017*** (0.002)	0.013*** (0.002)	0.011*** (0.002)	0.011*** (0.002)	0.011*** (0.003)	0.009*** (0.001)
Female	-0.002 (0.002)	-0.004** (0.002)	-0.002 (0.001)	-0.002 (0.001)	-0.002 (0.002)	-0.002 (0.001)
College	-0.009 (0.006)	-0.020*** (0.005)	-0.014*** (0.005)	-0.015*** (0.005)	-0.006 (0.008)	-0.014*** (0.004)
High School	-0.007** (0.003)	-0.014*** (0.003)	-0.013*** (0.003)	-0.012*** (0.003)	-0.003 (0.005)	-0.011*** (0.003)
Household Size	0.064*** (0.003)	0.087*** (0.003)	0.090*** (0.003)	0.090*** (0.003)	0.077*** (0.005)	0.069*** (0.003)
Constant	-0.697*** (0.054)	-1.209*** (0.056)	-1.370*** (0.055)	-1.340*** (0.054)	-1.016*** (0.092)	-0.890*** (0.049)
Occupation dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	No	No
City dummies	No	No	Yes	Yes	Yes	Yes
Prov dummies	No	Yes	Yes	Yes	Yes	Yes
Prov*Year dummies	No	Yes	Yes	Yes	Yes	Yes
Observations	49,661	49,661	49,661	49,661	15,331	43,066
R ²	0.089	0.123	0.156	0.166	0.162	0.135

Note: Column (3) is the baseline specification. Column (4) adds the interactions of province and year dummies. Column (5) uses the 1997–1999 sample instead of the 1992–2001 sample. Column (6) mitigates the mega city effect by excluding two mega cities (Beijing and Guangzhou). In the UHS, occupations are divided into seven categories: managers, technicians, public-sector workers, service-sector workers, business sector workers, manufacturing and transportation workers, and others. Standard errors in brackets are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.3. Robustness checks

One concern about our main results is that the 1998 housing reform might affect household saving rates through household income, which makes the household income a classic "bad control."²⁰ To address this concern, we conduct several robustness checks. First, we repeat our main results excluding household income. The results are reported in column (1) Table 6. We find that the effect of housing reform is slightly smaller compared to our baseline results (Table 5 column (3)), but it remains positive and significant no matter which control group we use. These results help lessen the concerns about household income as bad control. Second, we decompose the effects of housing reform on household saving rates into effects on household income and saving in Appendix Table A5 where we repeat our main estimation adopting household income and saving as dependent variables. We find that the coefficients for the interaction of post 98 and treatment are positive but insignificant for household incomes, and these coefficients are positive and significant for household savings. These results suggest that the housing reform does not have a significant impact on household income. In other words, housing reform is less likely to affect household saving rates through its impacts on household income. Furthermore, Appendix Fig. A1 demonstrates a pattern that is consistent with these estimation results where we present trends of household incomes and savings for the treatment and the two control groups before and after the 1998 reform (1992–2001). We find household incomes for the treatment group experienced a similar increase compared with those for the two control groups after 1998. However, household savings for the treatment group experienced a much greater increase compared with

those for the two control groups after 1998. These results imply that the 1998 reform affects household saving rates mainly through the increasing of household savings instead of through the increase of household income.

Another concern about the estimated effect of housing reform is that it might be confounded by the competitive saving motive. As demonstrated in Wei and Zhang (2011), the unbalanced sex ratio in China may lead to competitive saving behaviors in the marriage market, which may significantly raise the aggregate household saving rate because men with adequate wealth accumulation (e.g., enough savings to buy a house) have a greater chance to attract marriage partners. Such competitive behavior can drive up housing prices and household saving rates at the same time. To make sure that our results are not driving by those competitive saving behaviors, we include two additional control variables to our main regression. The first variable is the number of unmarried sons (5–19) at the household level, and the second variable is the sex ratio in the pre-marital cohort at the city level. Table 6 column (2) reports the estimation results with the inclusion of the two additional control variables. Our main DID estimates remain positive and significant, which suggests that the effect of 1998 housing reform is not confounded by the competitive saving motives. Meanwhile, the number of unmarried sons and the city-level sex ratios are positively correlated with household saving rates, consistent with the observation in Wei and Zhang (2011) and Wei et al. (2017). However, these coefficients are not significant. This result is probably because our sample period covers 1992–2001, while the analysis in Wei and Zhang (2011) and Wei et al. (2017) cover a longer period of 1990–2007. Since the housing market boom in China started around 2002, it is possible that the competitive saving motives among households in China are relatively weak in our sample period and became stronger in more recent years.

Furthermore, one potential challenge to the DID strategy is that differential changes between treatment and control groups may be driven by pre-existing trends. To address this issue, we conduct placebo tests by pretending that the abolishment of public housing was enacted

²⁰ In Angrist and Pischke (2008), "bad controls are variables that are themselves outcome variables in the notional experiment at hand. That is, bad controls might just as well be dependent variables too. Good controls are variables that we can think of as having been fixed at the time the regressor of interest was determined."

Table 6
The 1998 reform and household saving rates: robustness checks.

	State-employed Control Group					
	Without		Add		Placebo	
	Income	Sex Ratio	1996	1997	Pseudo Panel	
	(1)	(2)	(3)	(4)	OLS	Cohort FE
Post98*Treatment	0.019*** (0.007)	0.020*** (0.006)			0.015** (0.005)	0.011** (0.006)
Post96*Treatment			0.021 (0.034)			
Post97*Treatment				0.012 (0.013)		
No. of boys 5–19		0.004 (0.031)	0.021 (0.034)			
Sex Ratio		0.057 (0.043)		0.012 (0.013)		
Observations	76,479	76,479	31,382	31,382	550	550
R ²	0.214	0.194	0.127	0.101	0.211	0.188
			State-employed Control Group			
	Without	Add	Placebo		Pseudo Panel	
	Income	Sex Ratio	1996	1997	OLS	Cohort FE
Post98*Treatment	0.021*** (0.009)	0.022*** (0.009)			0.023*** (0.006)	0.020*** (0.007)
Post96*Treatment			0.019 (0.015)			
Post97*Treatment				0.013 (0.011)		
No. of boys 5–19		0.003 (0.027)	0.021 (0.034)			
Sex Ratio		0.067 (0.055)		0.012 (0.013)		
Observations	76,479	76,479	20,377	20,377	530	530
R ²	0.156	0.138	0.114	0.133	0.214	0.194
Occupation dummies	Yes	Yes	Yes	Yes	Yes	Yes
City dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes

Note: Column (1) excludes household income from control variables. Column (2) adds the number of unmarried sons (5–19) and the other is the sex ratio in the pre-marital cohort (5–19) at the city level. Columns (3) and (4) conduct placebo tests that assume the reform happened in 1996 or 1997 instead of 1998. We use the 1995–1998 sample instead of the 1992–2001 sample to avoid including the effects of the 1994 and 1998 reforms. Columns (5) and (6) conduct DID estimation using cohort-level pseudo-panel data. Cohorts are defined based on six year-of-birth groups interacting with three treatment status and four regions. All regressions include control variables in the baseline specification (Table 5 column (3)). Standard errors in brackets are clustered at the city level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

in 1996 or 1997 instead of 1998. Specially, we consider the following estimation equation:

$$S_{it} = \alpha_0 + \alpha_1 Treatment_{it} * PostYear_t + \alpha_2 Treatment_{it} + \alpha_3 X_{it} + D_t + \epsilon_{it} \tag{2}$$

where we consider $PostYear_t$ equals 1 for years after 1996/1997 and equals 0 for years before 1996/1997. We restrict the sample period to 1995–1998 to avoid including the effects of the 1998 reform. The estimation results with two different control groups are presented in Table 6 columns (3) and (4). The DID estimates are positive probably because of the anticipation effects of the privatization reform. Indeed, starting in 1994, the government had begun to establish a framework to facilitate the privatization of public housing stocks. But only until 1998 did the government implement specific plans to abolish the public housing system completely. Nevertheless, these estimates are not significant in the placebo models, which reveal that there are no systematic differences in pre-existing trends across the treatment group and the two control groups. Thus, our baseline results are less likely to be driven by pre-existing trends.

Finally, one limitation of our repeated cross-sectional data is that it cannot employ the fixed-effect model to remove the potential bias caused by the unobserved individual effect.²¹ To deal with this issue, we generate a pseudo-panel data and estimate fixed-effects models

²¹ That is, if $\epsilon_{it} = v_i + \mu_{it}$, our main estimation results are likely to be biased because of the fixed effect u_i .

using this panel (Deaton, 1985; Verbeek, 2008). Specifically, we construct a pseudo panel based on our 10 years of repeated cross-sections from 1992 to 2001. We first define cohorts²² on the basis of six year-of-birth groups interacting with three treatment status and four regions. The six year-of-birth groups include those with a head of household born in 1940–4, 1945–9, 1950–5, 1955–9, 1960–5, after 1965 (the youngest cohort). We choose an interval of five years instead of shorter intervals in order to have enough samples to calculate cohort population means. The three treatment statuses are defined as in the previous section including treatment group, state-employed control group, and private-employed control group. The four regions include East, Center, West, and Northeast based on the breakdown provided by China’s National Bureau of Statistics. We then average the relevant variables over the year within the cohort. In this way, we construct a series of means from 1992 to 2001 for households that are members of the same cohort. Our choice of cohorts gives a balanced pseudo-panel of 72 cohorts over 10 years. The average sample size of each cohort is 197.²³

²² In some papers, the term “cohorts” specifically means year-of-birth groups, but in this paper, “cohorts” refer to groups of households sharing some common characteristics.

²³ As discussed in Verbeek (2008), the size of the cohorts results from a trade-off between bias and variance. It must be large enough to limit the extent of measurement error on cohort means. However, the increase in the size of the cohorts decreases the number of cohorts observed, which makes estimator less precise.

We implement the DID analysis in Eq. (1) using the cohort-level data and the estimation equation becomes the following:

$$\bar{S}_{ct} = \alpha_0 + \alpha_1 Treatment_c * Post_t + \alpha_2 Treatment_c + \alpha_4 \bar{X}_{ct} + D_t + \bar{\epsilon}_{ct} \quad (3)$$

where \bar{S}_{ct} is the average household saving rates of all households in cohort c in year t , $Treatment_t$ identifies the treatment cohort, and $Post_t$ is a dummy variable that equals 1 for years after 1998 and 0 for years before 1998. The vector of covariances, \bar{X}_{ct} is the average of relevant controls in cohort c in year t . As in our baseline model, D_t is the year dummy variable, and the coefficient α_1 is the estimated effect of the housing reform. Since the cohort-level data has a panel structure, we are able to conduct cohort-level fixed-effect models to get consistent estimates of α_1 .²⁴

The estimated DID and DID with cohort-fixed effects results are presented in columns (5) and (6) of Table 6. The upper panel uses state-employed cohorts as the control group and the lower panel uses private-employed cohorts as the control group. In column (5), we conduct DID without including cohort-fixed effects. We find that saving rates of the treatment cohorts increased by about 1.5 and 2.3 percentage points more after the reform compared to cohorts in the state-employed and private-employed control groups, respectively. Those results are consistent with our main results in Tables 4 and 5. In column (6), we conduct cohort-level DID and incorporate the cohort-fixed effects. We find that the effects of the 1998 housing reform remain positive and significant. After the reform, the saving rates of the treatment cohorts increased by about 1.1 and 2.0 percentage points more than cohorts in the state-employed and private-employed control groups, respectively. In other words, the estimated effects of the 1998 housing reform are robust to the inclusion of the cohort-fixed effect, which suggests that the bias caused by the unobservable individual effect can be small.

6. Discussion

In this section, we discuss the effect of housing reform from three more perspectives. First, considering that there is large geographic heterogeneity in terms of the speed of housing reform, we investigate whether the speed of housing reform matters in determining the magnitude of its effect on household saving rates. Second, since the housing reform took place at the same time as the SOE reform, we study whether the effect of the housing reform is confounded by the SOE reform. Finally, we discuss the long-term implications of the 1998 housing reform.

6.1. The speed of housing reform

The Chinese government adopted a decentralized approach of implementing the 1998 housing reform. That is, the central government laid out the framework in 1998, while the local governments implemented specific programs at their own pace (Huang, 2004). As a result, there has been considerable spatial heterogeneity in the timetable and degree of the reform (Wang et al., 2005). To measure the pace of the 1998 reform at the city level, we calculate the decrease in the proportion of public housing among urban households between 1998 and 2001. Places with rapid reform should experience larger decreases in public housing. The province-level decreases in public housing among urban households are listed in Appendix Table A6. It shows that the proportion of public housing decreased most rapidly in Guangdong and Shandong, which are both located in China's coastal areas, and less rapidly in Shaanxi, which is located in China's hinterlands.

The regional-level variation allows us to identify the effect of reform

²⁴ If $\bar{\epsilon}_{ct} = \bar{v}_t + \bar{\mu}_{ct}$, then $\bar{\epsilon}_{ct} = \bar{v}_t + \bar{\mu}_{ct}$ and the cohort-level fixed effect can help eliminate the bias caused by \bar{v}_t . See Deaton (1985); Verbeek (2008) for more detailed discussion of the procedures for constructing cohorts and for estimation using the resulting data.

on household saving rates at the city level. We expect cities that underwent rapid housing reforms faced a greater increase in housing demand and, therefore, would experience more significant increases in household saving rates. We investigate the effect of the reform pace based on the following triple-difference model:

$$S_{it} = \alpha_0 + \alpha_1 Treat_{it} * Post_t * RapidHousingReform_i + \alpha_2 Post_t * Treat_{it} + \alpha_3 Post_t * RapidHousingReform_i + \alpha_4 Treat_{it} * RapidHousingReform_i + \alpha_5 RapidHousingReform_i + \alpha_6 Post_t + \alpha_7 Treat_{it} + \alpha_4 X_{it} + D_t + \epsilon_{it}. \quad (4)$$

We calculate the average decrease in the proportion of public housing between 1998 and 2001 at the city level and define *RapidHousingReform* as 1 for observations in cities that experienced a higher than average decrease in the proportion of public housing and 0 in other cities. Columns (1) and (4) in Table 7 report the estimation results for the above model with the two different control groups. The estimated coefficient in front of $Treat_{it} * Post_t * RapidHousingReform_i$ is significantly positive (0.026 and 0.012, respectively), which suggests that the estimated effects of the housing reform are more evident in rapid-reform cities. These results confirm our hypothesis that a fast reform could intensify the effects of housing reform on household saving rates.

6.2. The effect of the SOE reform

The mid-1990s was a time of continued economic growth, during which the Chinese government introduced numerous policies to reform the socialist system. Besides the housing reform, the most important effort was the SOE reform, which led to a large-scale layoff of SOE employees. Household saving rates may have reacted differently to the SOE reform and potentially confounded our baseline results (Wang et al., 2005; He et al., 2017). For example, if households in the treatment group were more likely to be laid off in the SOE reform, they may experience higher saving rates after 1998 because of the growing uncertainties caused by the SOE reform, and not because of the housing reform. To ensure that the effect of the housing reform is not confounded by the SOE reform, we check the relation between the two reforms. Appendix Table A6 illustrates the pace of the SOE reform at the province level, measured by the decline in the proportion of SOE employees among the labor force in that province. It shows that provinces that experienced a rapid housing reform (for example, Guangdong and Shandong) may not necessarily have engaged in rapid SOE reform (for example, Zhejiang and Sichuan). In the meantime, the correlation between the speed of housing reform and the SOE reform is 0.067 at the city level and is only significant at the 5% level. The results indicate that, though correlated, the 1998 housing reform and the SOE reform were carried out at different paces at the local level.

While the weak correlation between the speed of housing and SOE reforms at the province- and city-level alleviates the confounding concern, we conduct further analysis to address this issue. Specially, we conduct a triple-difference analysis by making use of the pace of the SOE reform. The pace of the SOE reform is measured by the variable *RapidSOEReform*, which equals 1 when a city experienced a relatively larger decrease (higher than the sample average) in the proportion of SOE employees among the total workforce in the city and 0 otherwise. Columns (2) and (5) in Table 7 report the estimation results for the triple-difference estimation. Columns (3) and (6) report the estimation results for the triple-difference analysis of the pace of housing and SOE reforms at the same time. The coefficients for the interaction with the pace of the SOE reform are insignificant in both models, which indicate that the effects of the SOE reform on household saving rates are limited and cannot confound the effects of the housing reform we found so far.

Table 7
The speed of housing reform and SOE reform: triple differences (1995–2001).

	State-employed Control Group			Private-employed Control Group		
	(1)	(2)	(3)	(4)	(5)	(6)
Post98*Treat*RapidHR	0.027*** (0.010)		0.029*** (0.011)	0.012** (0.07)		0.014* (0.009)
Treat*RapidHR	0.003 (0.004)		0.002 (0.005)	0.004 (0.013)		0.006 (0.013)
Post98*RapidHR	0.018*** (0.005)		0.026*** (0.005)	0.034** (0.016)		0.036** (0.017)
Post98*Treat*RapidSOER		-0.004 (0.010)	-0.005 (0.009)		0.012 (0.014)	0.016 (0.014)
Treat*RapidSOER		0.006 (0.005)	0.006 (0.005)		-0.006 (0.010)	-0.003 (0.010)
Post98*RapidSOER		0.000 (0.003)	0.000 (0.004)		-0.019 (0.012)	-0.016 (0.012)
Post98*Treatment	0.013* (0.08)	0.017** (0.007)	0.015* (0.009)	0.022 (0.015)	0.012 (0.009)	0.015 (0.016)
Occupation dummies	Yes	Yes	Yes	Yes	Yes	Yes
City dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	76,479	76,479	76,479	49,661	49,661	49,661
R ²	0.133	0.127	0.125	0.143	0.141	0.137

Note: We define *RapidHousingReform* as 1 for cities that experienced a relatively larger decrease in the proportion of public housing between 1998 and 2001 and 0 for other cities. We define *RapidSOEReform* as 1 for cities that experienced a relatively larger decrease in the proportion of the SOE employees among the total workforce and 0 for other cities. All regressions include control variables in the baseline specification (Table 5 column (3)). Standard errors in brackets are clustered at the city level. $p < 0.10$, $** p < 0.05$, $*** p < 0.01$.

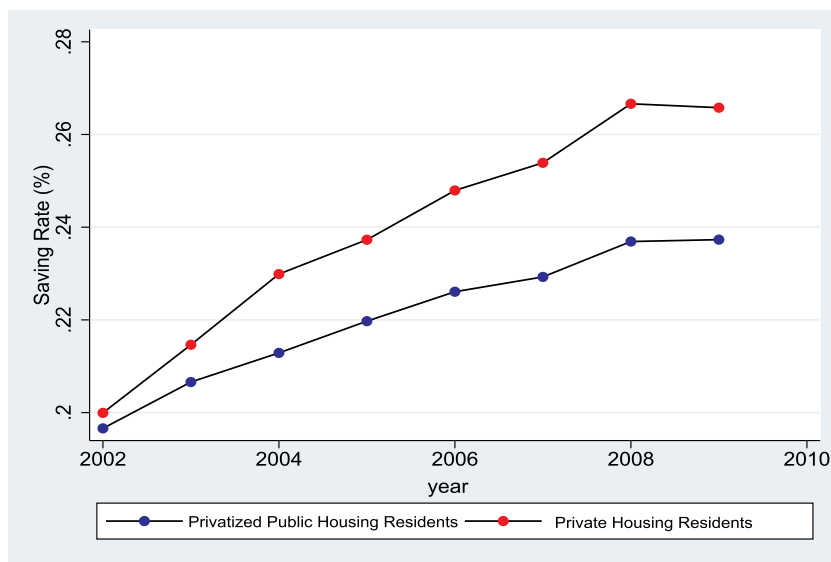


Fig. 4. Household saving rates for privatized public housing residents and private housing residents: 2002–2009. Note: Privatized public housing residents include households that own a house that used to be a public housing unit. Private housing residents include households that own or rent a commodity house.

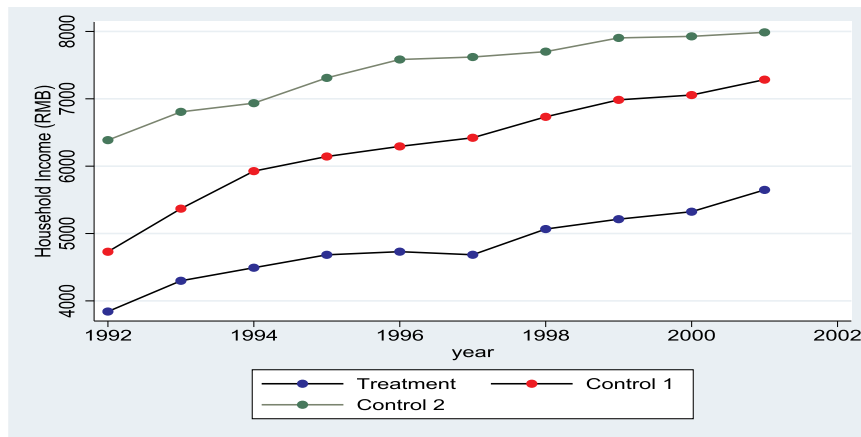
6.3. The long-term implications of the 1998 housing reform

The abolishment of public housing in 1998 resulted in an unequal distribution of original public housing stock among urban households, which created a divergent housing demand. In this section, we attempt to investigate the long-term implications of this reform using the UHS 2002–2009 data.

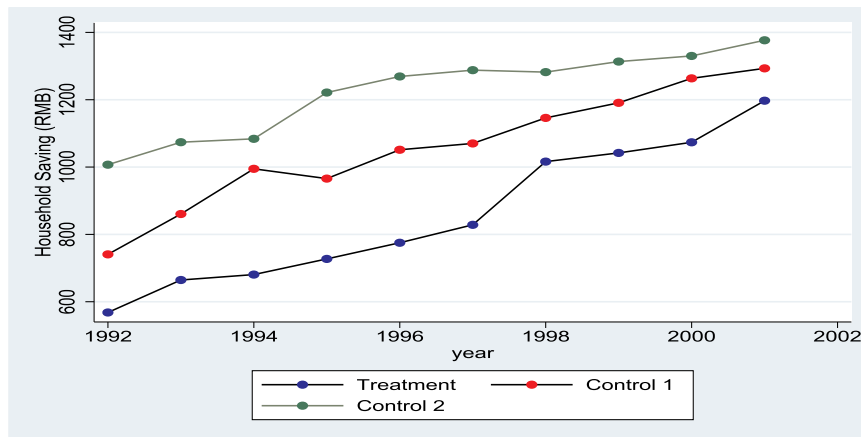
Ideally, if we could identify the groups of households that are and are not the beneficiaries of the old housing system, then we could estimate the long-term effect of the 1998 housing reform by comparing the two groups of households in the 2002–2009 period. However, this task is not easy considering that the 1992–2001 sample is not a panel, so we cannot track back exactly which households received the allocation of the public housing from the government and which households missed out on that opportunity. To work around this issue, we

divide our sample households into three groups depending on the types of houses they lived in. The first group owned and lived in a housing unit that used to be public housing but was privatized and purchased at subsidized prices during the reform period. This group was more likely to be beneficiaries of the old housing system and had relatively low incentive to save for home purchases compared with other households. We call this group “privatized public housing residents.”²⁵ The rest of the sample households include “private housing residents,” who live in commodity housing units and can be divided into “private housing homeowners” and “private housing renters” depending on their tenure

²⁵ We name this group of households as “privatized public housing residents” instead of “public housing residents” because they are different from households that live in public housing units that intend to serve low-income households.



(a) Household Income



(b) Household Saving

Fig. A1. Household Income and Saving for Treatment and Control Groups: 1992–2001 Note: The treatment group includes households that lived in poor-quality housing that have at least one member employed in the public sector. The first control group consists of households that live in good-quality housing that have at least one member employed in the public sector. We call this group the “state-employed control group.” The second control group includes households with both members employed in the private sector. We call this group the “privately-employed control group.”

Table 8
Private housing residents and household saving rates: 2002–2009.

	(1)	(2)	(3)	(4)
Private House Homeowner	0.013*** (0.003)	0.013*** (0.003)	0.012*** (0.003)	0.010*** (0.002)
Private House Renter	0.022*** (0.003)	0.023*** (0.003)	0.022*** (0.006)	0.017*** (0.002)
Log(GDP per Capita)		0.008 (0.007)		
Log (Housing Area)				0.015*** (0.002)
Constant	0.020*** (0.007)	0.033*** (0.001)	0.022*** (0.007)	0.040*** (0.001)
Occupation dummies	Yes	Yes	Yes	Yes
City dummies	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Prov*Year dummies	No	Yes	No	No
Observations	166,986	166,986	144,321	166,986
R ²	0.165	0.181	0.174	0.149

Note: The benchmark group for “private house homeowners” and “private house renters” are “privatized public housing residents” who own and live in a housing unit that used to be public housing but was privatized and purchased at subsidized prices during the reform period. Column (2) adds city-level log (GDP per capita) and the interactions of province and year dummies. Column (3) mitigates the mega city effect by excluding two mega cities (Beijing and Guangzhou). Column 4 adds housing size. All regressions include control variables in the baseline specification (Table 5 column [3]). Standard errors in brackets are clustered at the city level. $p < 0.10$, $** p < 0.05$, $*** p < 0.01$.

status.

Private housing residents were either not satisfied with the house they had received (public-sector employees) or had not received any housing (private-sector employees) under the old system and, therefore, had to accumulate a substantial amount of wealth to purchase housing from the private market. Their saving rates were high and increased more significantly compared with public housing residents (Fig. 4), which could potentially contribute to the high and rising household saving rates in China. The private and public housing status was largely determined around 1998, so the difference in the saving rates we found during 2002–2009 could more or less reflect the long-term effects of this historical event.

We compare household saving rates between privatized public housing residents and private housing residents using the following regression equation:

$$S_{it} = \beta_0 + \beta_1 PrivateHomeowner_{it} + \beta_2 PrivateRenter_{it} + \beta_3 X_i + \epsilon_{ijt} \quad (5)$$

where S_{it} is the saving rate of household i at time t , $PrivateHomeowner_{it}$ is a dummy variable that equals 1 for a private housing homeowner and 0 for other households, and $PrivateRenter_{it}$ is a dummy variable that equals 1 for a private housing renter and 0 for other households. The vector of covariances X_i includes a set of individual- and household-level variables as in Eq. (1). We are interested in the coefficients β_1 and β_2 which estimate whether households that missed out on the housing benefits during the 1998 reform save more than the beneficiaries no

Table A1
Floor area and housing structure over time: 1992–2009.

	Floor Area per capita(sq m)	Single-Family House	One-Bedroom Apartment	Two-Bedroom Apartment	Three-Bedroom Apartment	Four-Bedroom Apartment	Collective Dormitories
1992	12.822	0.005	0.089	0.320	0.144	0.018	0.423
1993	13.134	0.004	0.083	0.345	0.155	0.017	0.396
1994	13.829	0.009	0.084	0.361	0.164	0.019	0.363
1995	14.202	0.008	0.092	0.391	0.171	0.022	0.317
1996	14.645	0.008	0.094	0.408	0.181	0.021	0.287
1997	15.505	0.011	0.089	0.417	0.193	0.020	0.270
1998	16.049	0.009	0.089	0.430	0.203	0.022	0.247
1999	16.765	0.010	0.082	0.441	0.214	0.022	0.232
2000	17.516	0.011	0.078	0.457	0.218	0.022	0.213
2001	18.068	0.012	0.080	0.466	0.217	0.021	0.205
2002	25.168	0.019	0.062	0.457	0.265	0.032	0.165
2003	26.658	0.024	0.061	0.457	0.270	0.032	0.157
2004	27.096	0.024	0.058	0.466	0.275	0.030	0.147
2005	28.952	0.020	0.056	0.478	0.292	0.032	0.121
2006	29.342	0.020	0.058	0.474	0.301	0.034	0.113
2007	29.451	0.021	0.050	0.481	0.308	0.034	0.107
2008	31.944	0.028	0.054	0.449	0.326	0.039	0.105
2009	31.599	0.027	0.052	0.446	0.335	0.039	0.101
Total	21.040	0.015	0.073	0.429	0.233	0.026	0.225
Obs.	266,311						

Source: UHS 1992–2009.

matter what their housing tenure status.

Table 8 summarizes the ordinary least squares estimation results based on Eq. (5). Column (1) shows that private housing homeowners saved about 1.3 percentage points more than public housing residents, while private house renters save even more, about 2.2 percentage point. These results suggest that the 1998 reform may have a long-term impact on household saving behaviors by imposing different housing demands on households.

The estimation results in column (1) are robust to several different specifications. First, in column (2), we include gross domestic product (GDP) per capita, along with the interaction of province and year dummies as additional variables to control for the effect of local macroeconomic factors. The estimates on private house homeowners and renters remain positive and significant. In addition, Beijing and Guangzhou have the highest house prices and household income. To ensure that our results are not driven by the mega city effect, we estimate the baseline model excluding the two cities. These estimates are reported in column 3, where the estimated coefficient for private housing resident remains positive and significant. So, it does not appear that our baseline results are driven by high house prices in the mega cities alone.

Besides geographic differences, an alternative interpretation is that private and public housing residents may have different housing preferences, which results in different saving behaviors. For example, some households may be more willing to sacrifice their consumption for a larger living space, and this preference causes them to buy a larger house in the private market and forces them to save more at the same time. To mitigate the confounding effect, we include per capita living space as an additional control. The estimates are reported in column (4), which are qualitatively similar to those in column (1), though the magnitude drops slightly. The estimates for per capita living space and home ownership are significantly positive, which suggests better living conditions are associated with higher household saving rates.

Overall, the results in Table 8 suggest that the 1998 housing reform created long-term divergent saving behaviors among urban households. However, one caveat is important to remember. Analysis of the long-term effect of the housing reform is based on the assumption that the privatized public housing residents are more likely to be beneficiaries of the old housing system. This statement is largely true, but it is possible that those households are not the first owners of the house, therefore, they are not beneficiaries of the old housing system. In that case, our estimate will underestimate the long-term effect of the 1998 reform. We

leave this issue for future research to better identify the potential beneficiaries and the long-term effects of the 1998 reform.

7. Conclusion

This paper shows that the abolishment of public housing in China triggered a rise in the household saving rate. In particular, the reform distributed the original public housing stocks unequally and, thus, led to divergent saving behaviors among urban households. Compared with beneficiaries of the old housing system, households that missed the benefits experienced an increase of about 2.2% in annual saving rates during the reform period (1998–2001). After the reform, with rising housing costs and an underdeveloped mortgage market, the divergence in saving behaviors continued. We find that private housing residents saved about 1.3% more annually than privatized public housing residents.

Considering the ongoing nationwide urbanization, the housing-related saving motives are not likely to disappear anytime soon. A large number of rural migrants are moving into urban areas, especially into first- and second-tier cities. Between 1996 and 2005, China's urban population increased by about 50% from 373 million to 562 million (Fang et al., 2016). The growth of the urban population will possibly keep housing demand and, thus, household saving at a high level for a certain period under the current policies.²⁶

What are the policy implications of our findings for the debate about how to rebalance China's growth by boosting domestic consumption? Since the shift of the housing financial burden from the public to private contributes to the rising savings rates, government policies that promote housing affordability will help to stimulate consumption and maintain sustainable economic growth. From this perspective, government-subsidized housing, including affordable housing, low-cost housing, and public rental housing, may help relieve the financial burdens faced by low-income households, especially in certain expensive cities and help raise their non-housing consumption.

²⁶ Historically, the inter-province migration in China is largely regulated by the household registration system (Hukou). Under this system, households must have official registration to live in a specific city and to have access to health, education, and other public services. However, the restriction of this system has lessened in recent years (Garriga et al., 2017).

CRedit authorship contribution statement

Binkai Chen: Conceptualization, Formal analysis, Writing - review & editing. **Xi Yang:** Writing - review & editing, Formal analysis,

Conceptualization, Formal analysis, Writing - review & editing. **Ninghua Zhong:** Conceptualization, Formal analysis, Writing - review & editing.

Appendix A

A1. Improved living conditions

Along with housing privatization, urban households' living conditions have been dramatically improved. The UHS enables us to measure conditions of a housing unit in three areas: size, structure, and facilities. We measure size by total square meters of the house. Table A1 shows that the floor area has increased from 13 square meters in 1992 to 32 square meters in 2009. According to structure, our sample can be divided into the following categories: single-family house, one-bedroom apartment, two-bedroom apartment, three-bedroom apartment, four-bedroom apartment, and collective dormitory. Junior public-sector employees usually lived in the collective dormitories where they had to share the bathroom and kitchen with others for years before moving into an apartment with its own facilities. Single-family houses were not common and were usually reserved for high-status employees. Columns (2)–(6) of Table A1 show the change of housing structures over years. In 1992, 42.5% of urban households lived in the collective dormitories; this proportion decreased to 25% in 1998 and further to 11% in 2009. Concurrently, 57% of urban households lived in apartments in 1992; this proportion increased to 74% in 1998 and further to 86% in 2009.

We can also measure living conditions by facilities within the house: whether an individual bathroom or kitchen is included in the house, types of water supply, heating system, and cooking fuel. Table A2 shows the improvement of housing conditions over years. A larger value corresponds to better living conditions. For example, bathroom condition is coded as “no bathroom = 1,” “shared bathroom = 2,” “own bathroom without

Table A2
Housing facilities over time: 1992–2009.

	Bathroom	Water Supply	Cooking Fuel	Heating System
1992	2.528	2.836	1.618	1.753
1993	2.635	2.856	1.668	1.810
1994	2.768	2.868	1.721	1.861
1995	2.872	2.888	1.756	1.915
1996	2.968	2.906	1.785	1.983
1997	3.034	2.890	1.823	2.016
1998	3.082	2.898	1.848	2.041
1999	3.123	2.912	1.861	2.089
2000	3.178	2.926	1.872	2.179
2001	3.196	2.930	1.875	2.221
2002	3.392	2.938	1.893	2.377
2003	3.427	2.942	1.899	2.421
2004	3.469	2.941	1.911	2.503
2005	3.550	2.945	1.928	2.667
2006	3.574	2.949	1.931	2.707
2007	3.727	2.981	1.942	2.793
2008	3.751	2.988	1.945	2.780
2009	3.761	2.988	1.938	2.797
Total	3.207	2.919	1.837	2.259
Obs.	266,311			

Note: A larger value corresponds to better conditions. Bathroom condition is coded as no bathroom = 1, shared bathroom = 2, own bathroom without shower = 3, and own bathroom with shower = 4; water supply is coded as river water = 1, shared running water = 2, and own running water = 3; heating system is coded as no heating system = 1, stove = 2, heater = 3, and air conditioning = 4; and cooking fuel is coded as no cooking fuel = 1, coal = 2, and liquefied petroleum gas and pipeline gas = 3.

Table A3
Proportion of poor-condition housing: alternative definitions.

	Definition 1	Definition 2	Definition 3	Definition 4
1992	0.351	0.432	0.442	0.453
1993	0.291	0.395	0.389	0.433
1994	0.244	0.361	0.345	0.388
1995	0.219	0.316	0.307	0.358
1996	0.192	0.286	0.269	0.325
1997	0.159	0.269	0.229	0.285
1998	0.142	0.247	0.206	0.252
1999	0.129	0.231	0.190	0.223
2000	0.109	0.213	0.172	0.183
2001	0.104	0.203	0.161	0.166
Total	0.199	0.300	0.277	0.313
Observations	99,325			

Note: Definition 1 defines housing conditions as poor if the house lacks at least two of the four essential facilities. Definition 2 defines housing Conditions as poor if the house lacks a private bathroom. Definition 3 housing conditions as poor if the house is a collective dormitory. Definition 4 defines housing conditions as poor if the per capita floor area lower than the 30 percentile city-level per capita floor area.

Table A4
The 1998 reform and household saving rates (1992–2001): alternative definitions.

	State-employed Control Group			Private-employed Control Group		
	Def 2	Def 3	Def 4	Def 2	Def 3	Def 4
	(1)	(2)	(3)	(14)	(5)	(6)
Post98*Treatment	0.014* (0.009)	0.017** (0.009)	0.013** (0.005)	0.019*** (0.005)	0.023*** (0.007)	0.0017*** (0.006)
Occupation dummies	Yes	Yes	Yes	Yes	Yes	Yes
City dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	76,479	76,479	76,479	58,661	58,661	58,661
R ²	0.099	0.117	0.112	0.121	0.132	0.111

Note: Definition 1 defines housing conditions as poor if the house lacks at least two of the four essential facilities. Definition 2 defines housing conditions as poor if the house lacks a private bathroom. Definition 3 housing conditions as poor if the house is a collective dormitory. Definition 4 defines housing conditions as poor if the per capita floor area lower than the 30 percentile city-level per capita floor area. Our baseline regressions in Tables 4 and Table 5 use definition 1. All regressions include control variables in the baseline specification (Table 5 column [3]). Standard errors in brackets are clustered at the city level. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5
The effects of 1998 reform on household income and household saving (1992–2001).

	State-employed Control Group		State-employed Control Group	
	Income	Saving	Income	Saving
	(1)	(2)	(3)	(4)
Post98*Treatment	-0.057 (0.031)	0.089*** (0.018)	-0.043 (0.027)	0.108*** (0.024)
Treat	-0.142*** (0.006)	-0.210*** (0.016)	-0.195*** (0.015)	-0.267*** (0.036)
Occupation dummies	Yes	Yes	Yes	Yes
City dummies	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Observations	76,479	76,479	58,661	58,661
R ²	0.652	0.159	0.706	0.183

Note: All regressions include control variables in the baseline specification (Table 5 column [3]). Standard errors in brackets are clustered at the city level. $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6
The pace of the 1998 reform and the SOE reform at the province level: 1998–2001.

	Housing Reform		SOE Reform	
	1997	1998–2001	1997	1998–2001
Beijing	0.88	-0.20	0.47	-0.03
Shanxi	0.69	-0.08	0.53	-0.08
Liaoning	0.68	-0.37	0.51	-0.06
Heilongjiang	0.53	-0.30	0.46	-0.09
Jiangsu	0.59	-0.23	0.53	-0.08
Zhejiang	0.38	-0.25	0.51	-0.19
Anhui	0.62	-0.23	0.56	-0.15
Jiangxi	0.71	-0.23	0.56	-0.09
Shandong	0.76	-0.40	0.60	-0.09
Henan	0.61	-0.10	0.51	-0.10
Hubei	0.83	-0.13	0.54	-0.04
Guangdong	0.80	-0.52	0.46	-0.06
Chongqing	0.76	-0.37	0.58	-0.11
Sichuan	0.55	-0.29	0.49	-0.14
Yunnan	0.37	-0.19	0.57	-0.08
Shaanxi	0.84	-0.00	0.50	-0.09
Gansu	0.81	-0.25	0.49	-0.04
Total	0.67	-0.24	0.51	-0.09

Note: The pace of the 1998 reform is measured by the decrease in the proportion of public housing among urban households from 1998 to 2001. The pace of the SOE reform is measured by the decrease in the proportion of SOE employees among all urban workers from 1998 to 2001.

shower = 3,” and “own bathroom with shower = 4.”²⁷ Table A2 shows that only after 2002 did the average housing quality begin to improve, which implies that the large scale of new construction did not appear until 2002.

We consider four alternative definitions of poor housing conditions. The first definition defines a house as poor condition if it lacked at least two basic facilities among the four (bathroom, water supply, heating system, and cooking fuel). This is the baseline definition we use in the main body of the paper. The second one defines housing condition as poor if the house is a collective dormitory. The third one defines housing condition as poor if the house did not have a bathroom. The fourth one defines housing condition as poor if the house’s per capita floor area is lower than the 30 percentile city-level per capita floor area.

Table A3 presents the proportions of poor-condition housing stocks using the four alternative definitions. They are quite consistent. Table A4 replicates main DID regressions in Table 5 with alternative measurements of housing quality. The estimate of interest hardly changes, suggesting that our main conclusion is not sensitive to alternative definitions of poor housing conditions.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at [10.1016/j.jhe.2020.101693](https://doi.org/10.1016/j.jhe.2020.101693).

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²⁷ Water supply is coded as “river water = 1,” “shared running water = 2,” and “own running water = 3”; heating system is coded as “no heating system = 1,” “stove = 2,” “heater = 3,” and “air conditioning = 4”; and cooking fuel is coded as “no cooking fuel = 1,” “coal = 2, and “liquefied petroleum gas or pipeline gas = 3.”