The effect of homeownership on migrant household savings: Evidence from the removal of home purchase restrictions in China[☆]

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ARTICLE INFO

ABSTRACT

Internal migrants account for over one-fourth of China's population, and their saving behavior is important in explaining "the Chinese saving puzzle". The relation between household savings and house prices has been studied, but this paper utilizes a quasi-experiment that removed home purchase restrictions to examine the causal effect of homeownership on migrant household savings through a difference-in-differences approach and instrumental variable estimation. We find that the migrant saving rate significantly decreases after migrants buy houses, falling below that of the local *hukou* population. Further analysis reveals that this effect of homeownership can be explained by the consumption effect rather than housing purchase or increased household income effects. Our findings imply that *hukou*-based policies, including the allocation of public goods and the right to buy private goods such as houses, will harm both migrant welfare and sustained economic growth.

1. Introduction

China's high saving rate has drawn attention from around the world not only because it poses "the Chinese saving puzzle" (Modigliani and Cao, 2004) but also because it may worsen the global imbalance and harm China's sustained economic growth (Blanchard and Giavazzi, 2006) as China gradually transforms from a manufacturing-based economy to a globally important consumer market. According to the National Bureau of Statistics of China, the national saving rate soared from 38.1% in 1978 to 44.7% in 2017, and the aggregate household saving rate increased from 51% in 1978 to 61% in 2018. In addition, the share of aggregate household savings in national savings was 46.4% in 2018, making aggregate household savings a crucial component in the composition of the national savings of China. The cross-country comparisons in Fig. 1 reveal that China's gross savings are higher than those of other countries with the same level of gross national income (GNI) per capita.

As a result of China's *hukou* system, internal migration has emerged. The total number of migrants rose from 6.57 million in 1982 to 376 million in 2020, accounting for 26.62% of China's total population based on the seventh national census. Demographic dividends, mainly caused by migration, together with the reform dividend and the global dividend, were originally considered the causes of China's rapid and sustained economic growth following the reform and opening-up in 1978 (Cai, 2018). Currently, however, temporary migration is leading to social problems and hindering both sustained economic growth and smooth and effective urbanization. Migrants have been researched due to their high household saving rates (Zhu et al., 2012; Chen et al., 2015; Chen, 2018) because they are tightly connected with their hometown for reasons related to family function and the rural land system. Their mobility combined with the dual economy created by the *hukou* system affects consumption behaviors, leading migrants to consume less (Chen, 2018).

The existing literature emphasizes the role of migrants' saving behaviors. Migrants are motivated to adopt precautionary saving because they lack social insurance and engage in high-risk work (Su et al., 2018). From the perspective of China's *hukou* system, Chen et al. (2015) find that rural migrant workers consume less than the local *hukou* population. Such findings partly explain the low household consumption-to-GDP ratio in China (Kraay, 2000). Housing is an important factor in understanding the high saving rate in China. Since the abolition of the public housing system in 1998, the housing demand of households has increased and thus has become an important factor affecting household

savings (Chen et al., 2020). The soaring housing prices in China combined with the imbalanced gender ratio strengthen the precautionary saving motive of households (Modigliani and Cao, 2004; Wei and Zhang, 2011).

In theory, homeownership provides a sense of economic security and increases the ability of households to withstand risk, which may reduce the precautionary saving motive and household savings (Megbolugbe and Linneman, 1993; Xiang et al., 2021). However, most empirical studies verify the relationship between housing and savings in terms of housing demand or housing prices, and few articles examine how homeownership causally affects the saving behavior of households, especially migrant households. Chen (2018) examined homeownership as one factor influencing migrant consumption, thereby enriching our understanding of the consumption behavior of the migrant population. Based on Chen (2018), we address the endogeneity of homeownership.

In this paper, we use an exogenous policy shock, the removal of home

purchase restrictions (HPR) in China, combined with a difference-in-differences (DID) approach and instrumental variable (IV) estimation to address the endogeneity of homeownership; we establish the causal relationship between homeownership and migrant savings with a newly available survey called the China Migrants Dynamic Survey (CMDS); and we discuss the mechanisms through which homeownership affects household savings. In addition, we test the robustness of the results with a variety of identification strategies. The main finding of this paper is that homeownership has a causal effect on migrant household savings. The mechanism analysis shows that homeownership reduces migrant savings mainly by promoting migrants' consumption of high-elasticity goods.

This paper contributes to the literature in four ways. First, using an exogenous policy shock, we solve the endogeneity problem and identify the causal effect of homeownership on lowering savings, especially among migrants. In our paper, homeownership is defined not only as economic affordability but also as the right to purchase housing and to participate in the affordable public housing system. The existing literature mainly focuses on housing prices and savings, and some research has proven that housing prices lower savings through the expected wealth effect (Zang et al., 2015). Notably, however, the expected effect of homeownership cannot be clearly identified based on expected behaviors, and empirical evidence on causal effects is still lacking.

Second, this study contributes to the literature on the consequences of home purchase restrictions by evaluating relevant policy from the perspective of savings, and this makes our research different from the existing literature, which mainly focuses on the price effect of home purchase restrictions (Li et al., 2017; Sun et al., 2017). Our paper assesses the net effect of removing home purchase restrictions on household savings, capturing an unanticipated consequence of removing such restrictions. Our study deepens understanding of the impact of China's housing market policy. As shown below, local policy, including housing and social insurance, is still oriented toward the *hukou* population. Thus, the effect of ending this real estate policy on consumption is by no means

Fig. 1. The relation of GNI per capita with the national gross saving rate. Data source: WBI.

obvious.

Third, we present three mechanisms through which housing affects savings: the housing purchase effect, the consumption effect, and the increased household income effect. We find that the consumption effect is the most prominent and that the increased household income effect is weak, while the housing purchase effect is unlikely to appear.

Finally, we use the 2010, 2013, 2015, and 2017 waves of the CMDS, which are the most representative, comprehensive, and up-to-date datasets for conducting migrant research. As the most well-established migrant population dataset, CMDS provides the conditions needed to accurately identify the causal effect of homeownership on household savings; other datasets lack this advantage. One challenge in identifying the causal effect of homeownership on household savings using the removal of HPR as an exogenous policy shock is excluding the effect of inherited housing in the focal city. Since migrants are far less likely to own inheritable houses locally than the *hukou* population, they are more sensitive to the removal of HPR, and thus the accuracy of identification is further enhanced by using the migrant sample.

The remainder of this paper is organized as follows. Section 2 reviews the related literature. Section 3 introduces the housing-related policy background. Section 4 shows the datasets and the econometric specification. Sections 5 and 6 present the empirical results and offer further discussion. Section 7 concludes the paper.

2. Literature review

Household savings have contributed more to the increase in national savings in China than enterprises and government (Chu and Wen, 2017); this constitutes "the Chinese saving puzzle", that is, the contradiction between China's continuous high growth rate and the pessimism implied by high savings (Modigliani and Cao, 2004). The reasons for such high savings have consistently been classified into three categories. The first category consists of the economic and social factors related to financial constraints and financial underdevelopment (Chamon and Prasad, 2010), wealth and income inequality (Gu and Tam, 2013), a lack of public services such as health services (Blanchard and Giavazzi, 2006), and internal and external imbalances (Blanchard and Giavazzi, 2006; Yang, 2012). The second category consists of demographic and family factors. As proposed by Modigliani and Cao (2004), problems related to aging and demographic transformation have helped to raise the saving rate. Ge et al. (2018) found that the changes in the demographic structure caused by the one-child policy have an age-specific effect on household savings. In addition to the age structure, the gender ratio combined with soaring housing prices has increased the saving rate for reasons related to competitiveness in the marriage market (Wei and Zhang, 2011). Family size and the number of children affect household savings, and the one-child policy is consistently blamed for causing high savings (Huang et al., 2021). Tan and Liu (2020) discovered that the two-child policy will raise the saving rate of both migrant and *hukou* population groups, although this effect is more prominent for the *hukou* population. Educational expenditure and the differences in saving behaviors between people with different educational levels could also be a factor in the high saving rate (Lugauer et al., 2019). The third category consists of institutional factors characterized by the dual economy and *hukou* systems. For the majority of public goods or services, such as the affordable housing system, education, and social insurance, there is discriminatory treatment and an unfair distribution based on individuals' *hukou* status. Imposed in 1951 in urban areas and in 1953 in rural areas, the *hukou* system prevents people from migrating freely to other cities because of the rationing system, which ultimately influences savings through the precautionary saving motive. Regarding social insurance, Feng et al. (2011) found that decreased pension wealth results in increased household savings, especially for the 25–29 age group. Additionally, people will consume more if they are covered by the public pension system (Huang and Zhang, 2021).

The high saving rate of migrants could be explained by institutional factors (Chen et al., 2015). Since the theoretical work of Galor and Stark (1990), migrant savings have been treated as a decision made by the whole family, including those in local cities and those in the hometown, to share risk, diversify risk, and avoid income fluctuations. In China, internal migrant workers without local *hukou* usually save more because they are not entitled to the same rights as the *hukou* population (Zhu et al., 2012; Chen et al., 2015; Wang et al., 2021). Using the 2007 and 2008 waves of the Chinese Household Income Project (CHIP) survey, Chen (2018) argued that disentitlement promotes temporary migration and encourages the saving of perceived transitory income. Similar results have also been found in other countries. Using the case of Germany, Piracha and Zhu (2011) proved that international migrants do not reduce their savings until they obtain citizenship.

Generally, three motives are presented for why migrant save more. The first is the precautionary saving motive. Using a dynamic stochastic optimization framework, Vinogradova (2014) found that the precautionary saving motive is the key to understanding the saving behavior of migrants. Giles and Yoo (2007) discovered that uncertainty regarding agricultural production affects saving behaviors in rural China and that 10% of savings can be attributed to a precautionary motive. Zheng and Zhong (2016) proved that social pensions play an important role in reducing financial risks, resulting in lower savings. Therefore, experiences of disentitlement, such as the lack of social insurance and barriers to employment, makes migrants save more (Chen, 2018). The second motive is lower incomes. Islam et al. (2013) found that there is no significant savings differential between immigrant households and native-born households in Australia, and further decomposition analysis revealed that household income, rather than a precautionary saving motive, could explain the saving behavior of immigrant households. The third way consists of settlement intentions and abilities. Using CHIP survey waves, Chen (2018) proved that migrant households with a long duration of migration and higher settlement intentions intend to save less. Settlement intentions could be indicated by the housing purchase plan (Cao et al., 2015), and housing prices affect the housing purchase plan and settlement intentions through the welfare dissipation effect and expected wealth effect (Zang et al., 2015). As explained below in section 3, there is a distinction between the *hukou* population and migrants regarding home purchase rights and the affordable housing system. Housing is an important factor explaining the high saving rate (Zang et al., 2015). Using a simulation model, Skinner (1989) was the first to show that savings decline if homeowners spend down their housing windfalls; additionally, with a bequest motive, the saving rate will rise to some extent. Wang and Wen (2012) concluded that housing prices matter in theory and that rising mortgage costs can increase the aggregate saving rate in China by 2–4 percentage points at most.

In summary, the picture drawn by the existing literature indicates that migrants will save more due to disentitlement and the economic decisions made by all family members. Although some studies discuss the effect of housing prices on savings, discussion of homeownership is scarce since there may be some endogeneity that prevents it from being clearly identified. In this paper, we focus on the effect of homeownership on savings as well as the savings gap between the *hukou* population and migrants, using the removal of home purchase restrictions as an exogenous policy shock. Additionally, we reveal the mechanisms through which homeownership affects savings.

3. Institutional background

In China, public goods, social services, and social welfare are allocated by the *hukou* system (Song, 2014). *Hukou is* an identity record in the household registration system that is generated by the public security sector when a child is born, and this system has been blamed for the dual economy in China (Gersovitz, 2016). Migrants without local *hukou* cannot access the majority of public goods or services, such as the affordable housing system, education for their children, and subsistence allowances (Vendryes, 2011). Thus, the affordable housing system,

including housing purchase rights, inexpensive rental housing, and economical and comfortable housing, is oriented toward the *hukou* population (Deng et al., 2011).

Since 1998, China has experienced market-oriented reform; this included the abolition of the public housing system, which led to an increase in the housing needs of households, thus affecting their saving behavior (Chen et al., 2020). Many cities, especially large cities, have experienced soaring housing prices, which, when combined with enormous construction booms, may cause economic bubbles (Zhao et al., 2017). Thus, city governments have frequently intervened and regulated the rapid price appreciation. In early 2010, following instructions from the central government, 39 major cities in China imposed home purchase restrictions to prevent speculation in the real estate market and stabilize housing prices; generally, under these restrictions, people were restricted in their housing purchases (Fang et al., 2016), although the details of these local government policies differ. However, in general, the local *hukou* population is restricted to buying at most two houses or apartments unconditionally, while migrants are usually not eligible to purchase housing, even an apartment, unless they meet certain conditions, such as paying taxes for more than one year. If such varied conditions are met, then migrants are allowed to purchase at most one house or apartment. Thus, for the *hukou* population, investment demand is constrained by HPR, while for migrant workers, rigid demand is restrained by HPR, which can be considered *hukou*-based discrimination under HPR. The set of home purchase restrictions launched in 2010 was lifted in late 2013 and from 2014 to 2016 in most major cities, providing us with DID design opportunities. In theory, the removal of home purchase restrictions will push the demand curve outward, and the right shift of the demand curve will lead to an increase in both price and quantity demanded, which can also be regarded as the net result of the removal of the restrictions.

In addition to the discriminatory home purchase restrictions and housing purchase rights that accompany *hukou*, social welfare with respect to housing is provided based on individuals' *hukou* status. For low-income households, in recent years, the central government has tried to improve the construction of government-subsidized housing, including affordable housing and housing provident funds (Deng et al., 2011). Migrants are not entitled to affordable housing programs, such as inexpensive rental housing and economical and comfortable housing, and they have very limited access to housing provident funds because of poor labor rights protection and their informal employment (Gao et al., 2012).

4. Data and empirical strategy

4.1. Data

The data we used are from the CMDS, which was conducted by the National Health Commission of China from 2009 to 2017. The CMDS program adopts the probability-proportionate-to-size sampling method, and the survey is conducted around May during the sampling years. According to the official documentation, only people aged 16 to 59 are randomly surveyed to capture both personal information and household information, but migrants who temporarily reside in stations, docks, airports, hotels, or hospitals are not investigated.

The data employed in this paper have two particularities. First, the 2010, 2013, 2015, and 2017 survey waves are used because in these waves, information on both the local *hukou* population and migrants is included. Second, there are two types of samples in these years: one sample covers all cities around China but has data on fewer variables, and it has information only for migrants. The other sample has a smaller sample size and covers some cities, but the data on the variables include information for both migrants and the *hukou* population. We use the latter for our research purposes. Whether a respondent is the household head is unclear. According to the suggestions made by Zhou (2014), respondents who live with siblings or working-age parents are more likely not to be household heads, and they account for only 3.71% of the total

Table 1
Variable description.

Variable	Definition	Observations
Panel A: 2010, 2013, 2015 and 2017		
Saving rate	1-consumption/disposable income	112,283
Migrant	= 1 if migrant	112,736
Farmer	= 1 if agricultural *hukou* status	105,236
Ln (real income)	Ln (household disposable income/ CPI + 1), monthly, household	112,559
Family scale	Number of family members	112,736
Living with spouse	= 1 if living with spouse	112,736
Sex	= 1 if male	112,736
Age	Age in survey year	112,736
Education	Years of education in survey year	112,736
Married	= 1 if married	112,736
Blue-collar	= 1 if blue-collar worker	112,736
Employer	= 1 if employer; = 0 if employee or self-employed	112,736
Medical insurance	= 1 if covered by medical insurance	112,736
Children number	Number of children in the family	112,736
Children in local place	Number of children in the family in local city	112,736
Settlement intention	= 1 if want to stay for more than five years	112,736
Panel B: 2010 and 2013		
Homeownership*	= 1 if has an apartment or house in local place	68,776
Housing provident fund	= 1 if has housing provident fund	39,010
Hometown homeownership	= 1 if has an apartment or house in hometown	25,078
Ln (children's educational expense)	Ln (children's educational expense/ CPI + 1), monthly	30,218
Children's educational expense ratio	Children's educational expenses/total expenditure	33,009
Panel C: 2010		
Ln (clothing expense)	Ln (clothing expense/CPI + 1), monthly, per capita	16,370
Clothing expense ratio	Clothing expenses/total expenditure	16,351

Note: *, for the migrant sample, the data from 2010, 2013, and 2017 waves of the CMDS are used; for the local *hukou* population, the data from 2010 and 2013 waves of the CMDS are used.

sample we used, which means that our analytical sample is representative. Overall, we obtained detailed information on approximately 112, 736 individuals and their households and basic information on approximately 352,222 family members. There are missing values; therefore, not all the variables are the same across the four sample years. The definitions of the main variables used in this paper and their observations are listed in Table 1.

4.2. Empirical strategy and descriptive statistics

The empirical strategy is implemented in four steps. First, we use OLS to estimate the effect of homeownership on the savings gap between migrants and the local *hukou* population, in this way demonstrating the savings gap and thus the role of housing. Second, we use DID to identify the causal effect of homeownership using the removal of home purchase restrictions in approximately 2014. Third, we use the policy effect, that is, the interaction term of the policy DID approach, and the homeownership of others as IVs to test the local average treatment effect (LATE) of homeownership, and endogeneity is thus addressed. Finally, we try to identify the mechanisms driving the housing effect, including the consumption effect, economic security effect, and increased household income effect. From steps two to four, the analysis focuses on explaining the saving rate and homeownership of migrants, and it does not consider the local *hukou* population for three reasons. The first is that for houses or apartments for migrants, there is more likely to be rigid demand, which we focus on in our paper, while for the local *hukou* population, housing can constitute a certain kind of investment or even speculative demand. From the questions asked by the surveys, we could only obtain

information on "whether you live in a home through ownership or through other conditions, for example, you just rent it", and we could not observe the number of houses or apartments that a household owns. In particular, the policy that we are concerned with, that is, home purchase restrictions, treats migrants and the local *hukou* population differently, as explicated in section 3. The *hukou* population is restricted to buying at most two houses or apartments, while migrants are restricted to buying one and must meet extra requirements to do so.

The second reason is in the design of the DID approach. We use migrant year cohorts to identify the treatment group to capture the policy effect on homeownership, and we assume that a migrant household is more likely to purchase housing after migration occurs,[1] while this assumption is not applicable to the local *hukou* population. The third reason for only considering migrants is data availability. The home purchase restrictions were lifted in approximately 2014, and in the 2015 sample, we could not obtain information about the homeownership of a migrant or the families of the *hukou* population; only the homeownership of migrant households was available in the 2017 sample.

4.2.1. Benchmark model specification

To identify the effect of homeownership on savings and the savings gap between migrants and their local urban counterparts, we use equations (1) and (2), respectively:

$$Saving\ rate_{ipt} = \beta_0 + \alpha_1 Homeownership_{ipt} + X'_{ipt}\beta + \lambda_t + \mu_p + \varepsilon_{ipt} \quad (1)$$

where subscript i identifies household i or individual i; p refers to city p; t is the year investigated in the model; β_0 is the constant term; λ_t and μ_p are included to control for year and city fixed effects; and ε_{ipt} is an error term. The main dependent variable is *Saving rate$_{ipt}$*, which is calculated as one minus the ratio of expenditure to household disposable income. We also use the log form of the size of a family's savings and winsorize the saving rate at the 5% level as a robustness check. *Homeownership$_{ipt}$* denotes homeownership, and it takes the value of one if an individual has an apartment or a house in the local city and zero otherwise. α_1 measures the average effect of homeownership on savings. X_{ipt} is a vector containing the sets of control variables.

$$Saving\ rate_{ipt} = \beta_0 + \alpha_1 Migrant_{ipt} \times Homeownership_{ipt} + \alpha_2 Migrant_{ipt}$$
$$+ \alpha_3 Homeownership_{ipt} + X'_{ipt}\beta + \lambda_t + \mu_p + \varepsilon_{ipt} \quad (2)$$

where *Migrant$_{ipt}$* takes the value of one if the respondent is a migrant and the value of zero if the respondent is a member of the local *hukou* population. Given all the control variables X_{ipt}, we show the effects for different groups in Table 3, where the coefficient α_1 of the interaction term between *Migrant$_{ipt}$* and *Homeownership$_{ipt}$* is the difference in the homeownership effect on the saving rate of migrants and on that of the local *hukou* population.

X_{ipt} is a vector that includes a set of household information and individual characteristics. All the nominal variables are deflated by the consumer price index (CPI) to generate real values in 2010 prices. In the

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[1] If a migrant household purchases housing locally prior to migration, our assumption may not hold. However, on the one hand, only a small percentage of migrant households purchase housing locally prior to migration. In the 2010 sample (where no purchase restrictions were imposed), only 1.9% of migrant households who migrated in 2010 (the survey year) owned a local home. This percentage should be interpreted as an upper limit on migrant households who purchase housing locally prior to migration. On the other hand, since in Table 9, we give the empirical evidence with DID approach by survey year, which does not hinge on this assumption, our identification strategy relies on this assumption only to a limited extent, and the estimates remain robust when our assumption is violated as shown in Table 9. Additionally, in a subsequent analysis, when we use an identification strategy not based on this assumption, the results remain robust.

Table 2
Data description.

Samples	All		Migrants	*Hukou* Population
Variable	(Mean)	(Standard deviation)	(Mean)	(Mean)
Panel A: 2010, 2013, 2015 and 2017				
Saving rate	0.4584	0.3031	0.4857	0.3990
Migrant	0.6844	0.4648		
Ln (real income)	8.5750	0.7572	8.5842	8.5550
Family members	3.1490	1.1501	3.1122	3.2290
Sex	0.5046	0.5000	0.5059	0.5019
Age	36.9085	11.8141	35.2011	40.6102
Education	10.5502	2.9611	10.0303	11.6776
Married	0.8032	0.3976	0.8159	0.7757
Blue-collar	0.5242	0.4994	0.6076	0.3434
Employer	0.0460	0.2096	0.0577	0.0208
Medical insurance	0.4963	0.5000	0.3438	0.8269
Children number	0.6653	0.7420	0.7496	0.4825
Children in local place			0.4773	/
Settlement intention			0.6124	/
Panel B: 2010 and 2013				
Homeownership	0.3827	0.4860	0.0967*	0.8394*
Housing provident fund	0.1573	0.3640	0.1087	0.2348
Hometown homeownership			0.2230	/
Ln (children's educational expense)	3.8498	3.3823	3.4874	4.3872
Children's educational expense ratio	0.0611	0.2476	0.0486	0.0793
Panel C: 2010				
Ln (clothing expense)	6.2672	1.2814	6.2361	6.2984
Clothing expense ratio	0.1178	0.1377	0.1077	0.1279

Note: *, for the migrant sample, the data from 2010, 2013, and 2017 waves of the CMDS are used.

full sample, the control variables for households contain the log form of real household income based on equation $ln(disposable\ income\ /CPI + 1)$ [2] and family size. To control for the individual-level conditions, we include a dummy variable for gender (with 1 denoting male and 0 denoting female), age and age squared, educational level,[3] a dummy variable for marriage (indicating marital status, with 1 denoting married and 0 denoting unmarried), a dummy for blue-collar workers (with a value of one denoting blue-collar workers and zero otherwise), a dummy variable for employers (with one denoting an employer and zero denoting employees and self-employed persons), and a dummy variable for medical insurance (with a value of one denoting individuals covered by medical insurance and zero otherwise). In the 2010 and 2013 samples, we also control for a dummy variable for housing provident funds, which takes the value of one for those who are covered by this affordable housing policy and zero for those who have no housing provident funds. In addition, we control for the conditions of medical insurance, settlement intentions, and hometown homeownership as dummy variables, which may affect the saving motives of migrant families.

Table 2 presents the descriptive statistics of the full sample and the different groups. As shown in Panel A of Table 2, in our full sample, migrants account for 68.44% of all respondents. The average saving rate is 45.84%; for migrants, the saving rate is 48.57%, while for the local *hukou* population, it is 39.90%. Since we have the homeownership

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[2] One is added before the log form is taken to prevent disposable income from having a 0 value.

[3] In China, we generally determine the educational level as follows: 6 years for a primary school level of education, 9 years for a junior high school level of education, 12 years for a senior high school level of education, 13 years for an educational level equivalent to technical secondary school, 15 years for an educational level equivalent to junior college, 16 years for a university level of education, and 19 years for a graduate school level of education or higher.

Table 3
The implication of the coefficients of equation (2).

	Owning a house	Not owning a house	Difference between (1) and (2)	Difference-in-differences
	(1)	(2)	(3)	(4)
Migrants	$\beta_0 + \alpha_1 + \alpha_2 + \alpha_3$	$\beta_0 + \alpha_2$	$\alpha_1 + \alpha_3$	α_1
Local *hukou* population	$\beta_0 + \alpha_3$	β_0	α_3	

Table 4
Different city groups of removal of home purchase restrictions.

	Group	Cities
Treatment group	Cities with home purchase restrictions removed	Urumqi, Hefei, Dalian, Chengdu, Wuxi, Hangzhou, Suzhou, Xi'an, Zhengzhou, Qingdao, Guizhou
Control group	Cities with consistent home purchase restriction	Beijing, Shanghai, Guangzhou
Placebo Test Group	Cities never issuing home purchase restrictions	Zhongshan, Xianyang, Wuhan, Quanzhou, Xishuang Banna, Chongqing, Changsha, Hancheng

Note: Only the cities surveyed in CMDS are contained in the three groups, which means not all cities that lifted the home purchase restrictions in approximately 2014 are included here.

condition for the *hukou* population in 2010 and 2013 and for migrants in 2010, 2013, and 2017, we find that in 2010 and 2013, a large number of migrants lived in a rented house, and only 9.67% of migrants owned a house, compared with 83.94% of the local *hukou* population. While saving more, migrants spend less on clothing and their children's education. As shown in Panels B and C, migrants use 4.86% and 10.77% of their monthly income for their children's education and clothing, respectively, while for the *hukou* population, the proportion of monthly expenditure on children's education and clothing is 7.93% and 12.79%. Regarding family background and individual characteristics, migrants are different from their local urban counterparts. Migrants have fewer family members and are more likely to be married, while in terms of individual characteristics, migrants are more than five years younger and more than one year less educated. A total of 60.76% of migrants are blue-collar workers, while 34.34% of their local urban counterparts are blue-collar workers. For the migrant population, the medical insurance coverage rate is 34.38%, which is much lower than that for the *hukou* population (82.69%). Panel B of Table 2 shows that for migrants, the average rate of coverage by the housing provident fund system is 10.87%, which is significantly lower than that for the local *hukou* population (23.48%). Since there are no significant differences in real household income between the *hukou* population and migrants,[4] their saving motives are probably related to social and policy conditions.

4.2.2. Migrant cohort specification for the removal of home purchase restrictions

To more precisely capture the effect of housing on savings, we consider the removal of home purchase restrictions in approximately 2014 and introduce the DID approach by migrant year cohorts with equation (3) following Qian (2008) and Isaksson and Kotsadam (2018).

$$Y_{icpt} = \beta_0 + \alpha_1 DID_{cp} + X'_{icpt}\beta + \upsilon_c + \mu_p + \lambda_t + \varepsilon_{icpt} \qquad (3)$$

where subscript *c* refers to the migrant year cohort, that is, migrants who move to cities in the same year. In this part, we employ only the 2010, 2013, 2015, and 2017 migrant samples and use *Saving rate*$_{icpt}$ as the dependent variable. When we use the 2010, 2013, and 2017 samples, *Homeownership*$_{icpt}$ is the dependent variable. In addition, for both dependent variables, we also use the probit model, and the results are similar to the OLS estimation results.[5]

Since home purchase restrictions were first imposed in approximately 2010, lifted in approximately 2014, and reimplemented starting in 2017, we restrict our sample to migrants who immigrated between 2010 and 2016. The home purchase restrictions state that migrants who have resided in cities for less than a year cannot buy an apartment since they have not paid taxes or paid for social insurance locally for more than one year. Thus, the actual treated cohorts are migrants who immigrated after 2013 due to the removal of the home purchase restrictions in 2014, and as shown in Table 4, the treatment group consists of the cities among our

sample cities that lifted these restrictions, such as Hefei, Hangzhou, and Suzhou. Therefore, the interaction term *Treated × Post* denoted as DID_{cp} takes the value of one when a cohort migrates after 2013 to a city that has lifted the home purchase restrictions. As shown in Table 4, the control group consists of cities where home purchase restrictions were in place until 2017, such as Guangzhou, Beijing, and Shanghai. For robustness checks, we also take into account the cities that never imposed home purchase restrictions, such as Zhongshan and Wuhan, during our analysis years; these cities constitute the placebo test group.

This setting is valid for migrants in terms of homeownership, as explained previously in section 4.1, since it is rational to assume that home purchases occur after migration. Notably, the treated cohorts are more likely to be affected by the removal of home purchase restrictions, which means that they are not necessarily affected. Therefore, we discuss the possibility of buying housing. Another assumption is the persistent effect of home purchase restrictions. We suppose that the actual possibility of purchasing housing will be persistently influenced for the cohorts affected by the home purchase restrictions even if such restrictions were lifted. This assumption will be verified below in section 5.5. In addition, to avoid the assumption of a persistent effect of home purchase restrictions, we incorporate the survey year into the design of the DID approach.

There is another concern about the validity of the control group, which includes cities such as Beijing, Shanghai, and Guangzhou. To observe the ratio of migrant households with local homeownership, we divide our sample into two categories: the first includes migrant cohorts in treatment group cities, and the second includes migrant cohorts in control group cities. Fig. 2 shows that the basic trends in the ratio of migrant households with homeownership in the treated group and
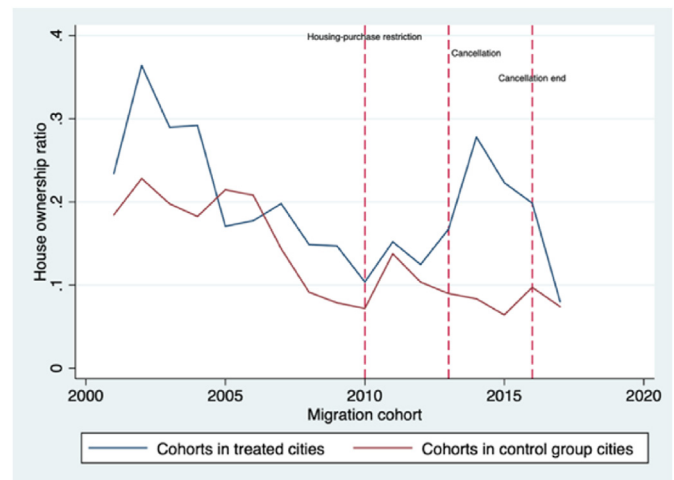

Fig. 2. The trend and differences in the ratio of migrant households with homeownership.

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[4] In the analyses of variance and covariance in Panel A of Table 2, the F value is 0.81, and P value is 0.3696.

[5] Due to space limitations, the probit estimations are omitted but are available upon request.
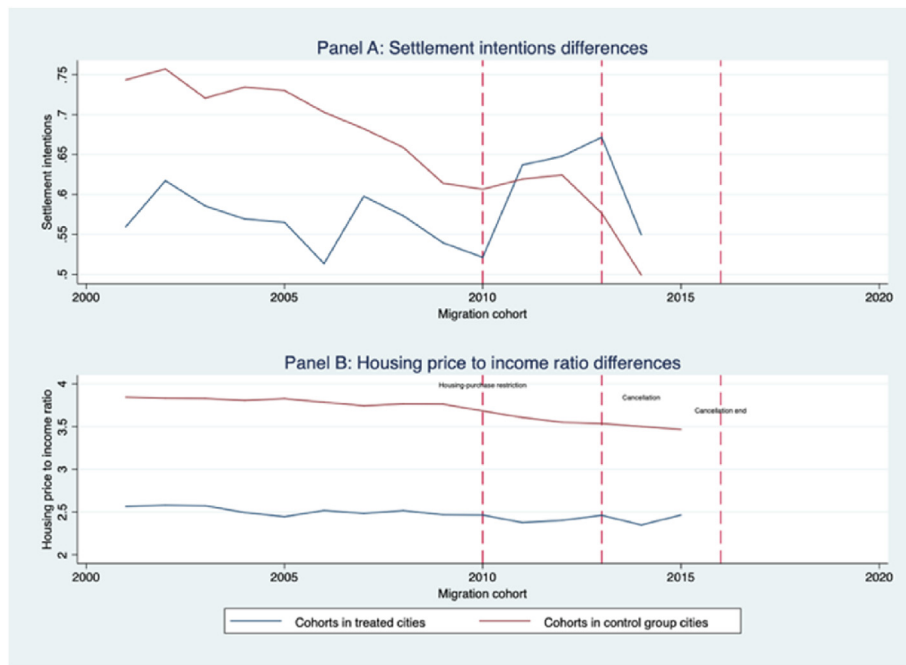
Fig. 3. The trend and differences in the ratio of settlement intentions and housing Price-to-Income ratio.

control group look similar for the 2000–2013 migrant cohorts. From 2010 to 2013, both groups of cities imposed home purchase restrictions, which means that the two groups are comparable. For the 2013–2015 migrant cohorts, the treated group witnessed a sharp increase in the ratio of migrant households with homeownership, since the removal of restrictions increased the possibility of migrants buying housing.

To further justify the parallel trend in the ratio of migrant households with local homeownership between cohorts in the treatment group cities and cohorts in the control group cities before the removal of home purchase restrictions, we take two factors into account: settlement intentions and the housing-to-income ratio. From Panel A and Panel B of Fig. 3, the cohorts in treatment group cities consistently show lower settlement intentions and a smaller housing price-to-income ratio than their counterparts in the control group cities, which means that migrants always want to settle in the control group cities, which are almost all Chinese megacities; however, the higher housing price-to-income ratio pushes them out. We also provide more empirical evidence and checks in section 5.3.

4.2.3. Parallel trend test specification for removing home purchase restrictions

For the parallel trend test, we estimate equation (4) through OLS estimation.

$$Y_{icpt} = \beta_0 + \sum_{j=2011}^{5} \alpha_j DID_{jcp} + X'_{icpt}\beta + \upsilon_c + \mu_p + \lambda_t + \varepsilon_{icpt} \qquad (4)$$

where j refers to the cohort group migrating in year j. For the duration of the removal of the restrictions explained in section 3, we test the cohorts that migrated from 2010 to 2016, and we use a set of DID_{jcp}, which takes the value of one if an individual migrates to a treated city in year j and zero otherwise. Therefore, individuals who migrated to a treated city in 2010 are taken as the base. Thus, to pass the parallel trend test, it is better for α_j to be not significantly different from zero when $j < 2013$ and significantly different from zero when $j \geq 2013$.

4.2.4. Specification by survey year

Beyond the assumption we have made that the migrant cohorts in 2012 and earlier will be persistently affected by home purchase re-

strictions even after such restrictions are lifted, we now assume that all the migrant cohorts in 2015 and 2017 entitled to purchase housing have the same possibility of buying housing. Therefore, in addition to the cohort setting, we have the chance to depict the effect of the removal of home purchase restrictions on savings through the DID approach incorporating survey years using equation (5). Then, we employ the difference-in-difference-in-difference (DDD) approach to capture the heterogeneous effects of homeownership on the saving behaviors of migrants and the *hukou* population using equation (6).

$$Saving\ rate_{ipt} = \beta_0 + \alpha_1 Treated_p \times Post_t + \alpha_2 Treated_p + \alpha_3 Post_t + X'_{ipt}\beta + \varepsilon_{ipt} \qquad (5)$$

where $Post_t$ is a dummy variable denoting the periods during and after the removal of the restrictions. In our sample, for 2015, it takes the value of one, and for 2010 and 2013, it takes the value of zero.[6] For $Treated_c$, when the variable takes the value of one, it represents that the treatment city group that has lifted the restrictions; when it takes the value of zero, it represents the control city group. Therefore, α_1 indicates the effect of the removal of restrictions on savings with a DID approach by survey year.

$$\begin{aligned} Saving\ rate_{ipt} = & \beta_0 + \alpha_1 Treated_p \times Post_t \times Migrant_{ipt} + \alpha_2 Treated_p \times Post_t \\ & + \alpha_3 Treated_p \times Migrant_{ipt} + \alpha_4 Post_t \times Migrant_{ipt} \\ & + \alpha_5 Treated_p + \alpha_6 Post_t + \alpha_7 Migrant_{ipt} + X'_{ipt}\beta + \varepsilon_{ipt} \end{aligned} \qquad (6)$$

where α_1 indicates the effect of the removal of home purchase restrictions on different saving behaviors of migrants and the *hukou* population. In equations (5) and (6), we consider *Saving rate* instead of homeownership as the dependent variable, since for 2015, we could not obtain information on homeownership from the questionnaire, and for 2017, we could obtain only the homeownership condition of migrants.

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[6] We do not incorporate 2017 because all the cities in our sample removing home purchase restrictions in approximately 2014 reimplemented these restrictions in 2016. With policy implementation, it is necessary to observe that the first affected cohort was the 2013 migrant cohort, and for the analysis by year, the cutoff point is 2014, which is when the policy was actually removed.

Table 5

The effects of housing on savings and the savings gap between migrants and the local *hukou* population.

Sample	All	All	Migrants with agricultural *hukou* and local *hukou* population	All	Migrants	Migrants	Local *hukou* population	All	Homeownership = 1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Migrant	0.0487***	0.0623***	0.0731***	0.0441***				0.1185***	−0.0302***
	(0.0027)	(0.0051)	(0.0057)	(0.0071)				(0.0105)	(0.0098)
Homeownership				−0.0273***	−0.0886***	−0.0868***	0.0422***	0.0447***	
				(0.0070)	(0.0090)	(0.0089)	(0.0105)	(0.0104)	
Housing provident fund					−0.0147*	−0.0146*	−0.0248***	−0.0188***	
					(0.0077)	(0.0077)	(0.0082)	(0.0054)	
Migrant × Homeownership								−0.1428***	
								(0.0130)	
Ln (real income)	0.1894***	0.2221***	0.2289***	0.2241***	0.1876***	0.1883***	0.2740***	0.2265***	0.2594***
	(0.0051)	(0.0109)	(0.0120)	(0.0110)	(0.0086)	(0.0086)	(0.0216)	(0.0110)	(0.0207)
Family members	−0.0266***	−0.0324***	−0.0335***	−0.0317***	−0.0290***	−0.0289***	−0.0331***	−0.0322***	−0.0273***
	(0.0015)	(0.0041)	(0.0044)	(0.0040)	(0.0028)	(0.0028)	(0.0083)	(0.0040)	(0.0078)
Sex	0.0085***	−0.0025	−0.0054	−0.0027	−0.0018	−0.0017	−0.0031	−0.0030	−0.0020
	(0.0021)	(0.0039)	(0.0041)	(0.0039)	(0.0040)	(0.0040)	(0.0077)	(0.0039)	(0.0074)
Age	−0.0022***	−0.0055***	−0.0046***	−0.0055***	−0.0050**	−0.0049**	−0.0006	−0.0048***	−0.0007
	(0.0005)	(0.0016)	(0.0016)	(0.0016)	(0.0024)	(0.0024)	(0.0026)	(0.0016)	(0.0027)
Age^2	0.0000***	0.0001***	0.0001***	0.0001***	0.0001**	0.0001**	0.0000	0.0001***	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Education	−0.0101***	−0.0110***	−0.0098***	−0.0107***	−0.0071***	−0.0070***	−0.0140***	−0.0093***	−0.0141***
	(0.0005)	(0.0008)	(0.0009)	(0.0008)	(0.0008)	(0.0008)	(0.0018)	(0.0008)	(0.0017)
Married	−0.0347***	−0.0210***	−0.0258***	−0.0206***	−0.0033	−0.0038	−0.0395***	−0.0198***	−0.0328***
	(0.0034)	(0.0072)	(0.0074)	(0.0072)	(0.0089)	(0.0090)	(0.0119)	(0.0072)	(0.0123)
Blue-collar	0.0648***	0.0764***	0.0780***	0.0754***	0.0744***	0.0737***	0.0595***	0.0713***	0.0603***
	(0.0024)	(0.0045)	(0.0048)	(0.0045)	(0.0049)	(0.0049)	(0.0082)	(0.0044)	(0.0080)
Employer	−0.0655***	−0.0683***	−0.0699***	−0.0669***	−0.0600***	−0.0596***	−0.0636***	−0.0659***	−0.0587***
	(0.0049)	(0.0096)	(0.0103)	(0.0096)	(0.0097)	(0.0097)	(0.0231)	(0.0096)	(0.0196)
Medical insurance	−0.0104***	−0.0065	−0.0051	−0.0049	0.0102**	0.0103**	−0.0316***	0.0047	−0.0216**
	(0.0025)	(0.0043)	(0.0045)	(0.0043)	(0.0051)	(0.0051)	(0.0107)	(0.0044)	(0.0093)
Settlement intention					0.0055	0.0058			
					(0.0041)	(0.0041)			
Hometown homeownership						0.0238***			
						(0.0068)			
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample of years included	2010, 2013, 2015, 2017	2010, 2013	2010, 2013	2010, 2013	2010, 2013	2010, 2013	2010, 2013	2010, 2013	2010, 2013
R^2	0.1477	0.1682	0.1728	0.1687	0.1537	0.1545	0.1638	0.1729	0.1558
Observations	112,283	40,428	37,376	40,428	24,844	24,844	15,584	40,428	15,486

Note: Robust standard errors are presented in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively. Constant is included in all regression settings.

4.2.5. Instrumental variable estimation

For equation (1), there is an endogeneity problem in homeownership caused by omitted variables or reverse causality, since those who plan to purchase housing or invest in real estate will have a greater incentive to save, while those who have housing and thus no incentive to purchase housing will save less. Therefore, we will use DID_{cp} in equation (3) as an IV and adopt the two-stage least squares (TSLS) method to overcome this problem, since home purchase restrictions are lifted by city governments. In addition, to conduct the overidentification test to check whether the IVs meet the exclusion restriction assumption, we use the peer effect as an IV. In this paper, we define peers as groups of people who meet the following conditions: they come from the same city or province, have the same *hukou* status, such as agricultural *hukou* or nonagricultural *hukou*, and have the same employment status, such as blue-collar workers or white-collar workers, in the same year and in the same cities of residence. Then, we calculate the ratio of local homeownership among these migrant peers.

5. Empirical results

5.1. Benchmark model

Columns (1) to (3) in Table 5 present the general picture of how

migrant savings are distinct from the savings of the local *hukou* population on average based on OLS estimation. The migrant saving rate is 4.87 percentage points higher than that of the *hukou* population based on the full sample, and the figure is 6.23 percentage points based on the 2010 and 2013 samples. If we directly compare the savings of migrants with agricultural *hukou* with those of the local *hukou* population, which is the comparison made by Chen et al. (2015), the savings gap increases to 7.31 percentage points. In column (4), when we take homeownership into account, the savings gap decreases from 6.23 percentage points to 4.41 percentage points, from which we infer that homeownership plays an important role in explaining the savings gap. In addition, the estimation results in column (4) show that people with housing save 2.73 percentage points less than those with no housing, and when the sample is limited to the migrant sample, the coefficient of *homeownership* is even larger in column (5), that is, 8.86 percentage points. Column (8) reports the estimation results of equation (2). The coefficient of the interaction term of homeownership and migrants means that the effect of homeownership on savings is different between migrants and the local *hukou* population. The difference in savings between migrants with homeownership and the local *hukou* population with homeownership is 14.28 percentage points, and homeownership plays a role in reducing the savings gap. Notably, in column (7), homeownership increases the saving rate for the *hukou* population, and the reason may be that the local *hukou* population has

more speculation incentives or replacement housing incentives. However, these issues fall beyond the scope of our study, and our sample cannot take them into account. In column (9), we use only the sample of households with local housing, and the results indicate that homeownership helps to reduce the savings gap between the *hukou* population and migrants since, in the sample with homeownership, migrants save even less than the *hukou* population.

In column (1) of Table 5 with the full sample, on average, a one percent increase in real household income leads to a 0.1894 percentage point increase in the saving rate, which is in line with the law of diminishing marginal propensity to consume. Each additional member in a family significantly lowers the household saving rate by 2.66 percentage points, and married individuals save 3.47 percentage points less than unmarried individuals. Males tend to save more than females, and age has a U-shaped relationship with the saving rate. A one-year increase in educational level decreases the saving rate by 1.01 percentage points, meaning that human capital investment could spread risk. Blue-collar workers save 6.48 percentage points more than white-collar workers, for two potential reasons. First, the greater proportion of blue-collar workers in high-risk occupations with lower social and economic status and greater income risk leads to greater incentives for precautionary saving. Second, their lower social and economic status may increase their incentives to accumulate wealth through savings to improve their social status (Jin et al., 2011). Employers save significantly less, 6.55 percentage points, than other groups, which may be because employers have higher expected incomes and spend more on durable goods. In addition, a recent study shows that reducing the household debt burden can significantly increase household consumption (Zhao et al., 2020). Households that own housing will probably have a mortgage, which will decrease disposable income and further lower savings, thus leading to an estimation bias in the coefficient of *homeownership*. For this reason, we also use a housing provident fund dummy as a control variable[7] in columns (5) to (8). The results show that housing provident funds have a nonsignificant effect on migrant savings and a negative and significant effect on the savings of the local *hukou* population. Thus, it can be deduced that the affordable housing system is more inclined toward the *hukou* population. In column (6), we further control for the hometown homeownership of the migrant population, which has a very small impact on the estimated coefficient of homeownership (from −0.0886 to −0.0868). This result indicates that controlling for the hometown homeownership of the migrant population provides little help in identifying the effect of homeownership on migrant household savings. However, data on the hometown homeownership of the migrant population are available only in the 2010 and 2013 samples, resulting in a large number of samples lost. Therefore, we did not add the hometown homeownership of the migrant population as a control variable in the subsequent analysis.

Furthermore, we replace the dependent variable *Saving rate* with the saving rate winsorized at the 5% level and with the log form of savings, calculated as $\ln\left(\frac{\text{household disposable income}-\text{expenditure}}{\text{CPI}} + 1\right)$. Additionally, we eliminate the respondents who, as mentioned above, live with siblings or working-age parents. All of these changes in our specifications prove that our results are robust. That is, migrants save significantly more than their local urban counterparts, and homeownership exerts an effect on reducing the saving rate, especially the migrant saving rate. Therefore, homeownership also narrows the savings gap between the two groups.

The endogeneity problem caused by omitted variables and reverse causality will introduce bias in the OLS estimation and lead to overestimation of the homeownership coefficient. To solve this problem, we employ the TSLS method with IVs constructed from the DID approach with the removal of home purchase restrictions. Sections 5.2 and 5.3 test

the effect of removing these restrictions and the validity of the DID approach, and section 5.4 presents the IV estimation. For the reasons mentioned in section 4.2.2, we employ only the migrant sample in sections 5.2 to 5.4.

5.2. The effect of removing home purchase restrictions

Table 6 presents the OLS estimation results based on equation (3). Columns (1), (3), and (5) use homeownership as the dependent dummy variable; thus, they use a linear probability model. The other columns utilize the saving rate as the dependent variable, and column (7) employs the sample that has housing under the assumption that households buying housing are affected by the removal of home purchase restrictions. After controlling for year fixed effects, cohort fixed effects, and city fixed effects in columns (5) and (6), we note that the removal of the restrictions increases the probability of a household buying housing and reduces the saving rate by 1.47 percentage points on average. If we take into account only the households that have bought housing, the decrease is 6.44 percentage points in column (7). As mentioned above in section 4.2.3, in section 5.3, we further demonstrate the rationality and effectiveness of our results based on the DID approach.

5.3. Robustness checks of the difference-in-differences specification

5.3.1. Parallel trend test

We test the parallel trend using equation (4), as displayed in Fig. 4, in which Panel A presents the estimations with homeownership as the dependent variable, Panels B and C present the estimations with the saving rate as the dependent variable, and Panel C restricts the sample to migrant households with homeownership. Furthermore, the left column does not control for year fixed effects, but the middle and right columns do. After the removal of home purchase restrictions, migrant cohorts are more likely to buy houses or apartments in local cities and thus save less.[8]

5.3.2. Changing the treatment group

We are concerned that other policy or random factors in addition to the removal of home purchase restrictions may contribute to this difference in homeownership and saving rate, thus threatening our conclusions. To address this concern, we conduct a placebo test in which we replace the treatment group cities with placebo cities. If at this point, the coefficient on the *Treated* × *Post* interaction term is significant, then our conclusions are influenced by other policy or random factors. However, if the coefficient is nonsignificant, then it indicates that our conclusions are not threatened by such factors.

As shown in Table 4, for the placebo test, we change the cities in our treatment group and replace them with those in the placebo test group, that is, the cities that never imposed home purchase restrictions. Based on the DID approach, the results in Table 7 reveal that in the regression on homeownership and the saving rate, the average treated effects are no longer significant. This result indicates that our estimation results are robust.

5.3.3. The impact of property tax reform

In January 2011, only two cities in China, Shanghai and Chongqing, instituted a property tax reform. This reform exerted a positive effect on housing prices in Chongqing and a negative effect on housing prices in Shanghai, as proven by Bai et al. (2014) through the synthetic control method. In the reform, not all housing was taxed. For the local *hukou* population, taxes were levied on newly purchased housing but not on

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[7] We could not obtain the mortgage information of households from all of the questionnaires.

[8] We also replace the dependent variables with disposable household income, family size, and so on to conduct a balance test to observe whether the households in the treatment group are comparable to those in the control group. The DID coefficient is nonsignificant, indicating that the result passes the balance test. Due to space limitations, these results are not presented but are available upon request.

Table 6

The effect of removing of home purchase restrictions on homeownership and the saving rate.

	Homeownership	Saving rate	Homeownership	Saving rate	Homeownership	Saving rate	Saving rate Homeownership = 1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
DID (Treated × Post)	0.0570*** (0.0160)	−0.0298*** (0.0070)	0.0687*** (0.0101)	−0.0257*** (0.0075)	0.0708*** (0.0102)	−0.0147** (0.0074)	−0.0644* (0.0359)
Treated	0.0634*** (0.0110)	0.0309*** (0.0049)					
Post	−0.0436*** (0.0118)	0.0051 (0.0048)					
Migrant year dummies	No	No	Yes	Yes	Yes	Yes	Yes
Year dummies	No	No	No	No	Yes	Yes	Yes
Sample of years included	2010, 2013, 2017	2010, 2013, 2015, 2017	2010, 2013, 2017	2010, 2013, 2015, 2017	2010, 2013, 2017	2010, 2013, 2015, 2017	2010, 2013, 2017
Observation	12,062	29,597	12,062	29,597	12,062	29,597	1799
R^2	0.1866	0.0968	0.1754	0.1085	0.1772	0.1135	0.1937

Note: Robust standard errors are presented in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively. With saving rate as the dependent variable, the other control variables presented in Table 5 column (1) are all included, that is, log of real household income, family members, sex, age, age squared, education, marital status, working status, medical insurance, and employment status; we also include settlement intention. For the dependent variable homeownership, in addition to all the control variables in Table 5 column (1), we also include the square of educational level and *hukou* status, which takes a value of one for agricultural *hukou* and zero for nonagricultural *hukou*. Constant is included in all regression settings.
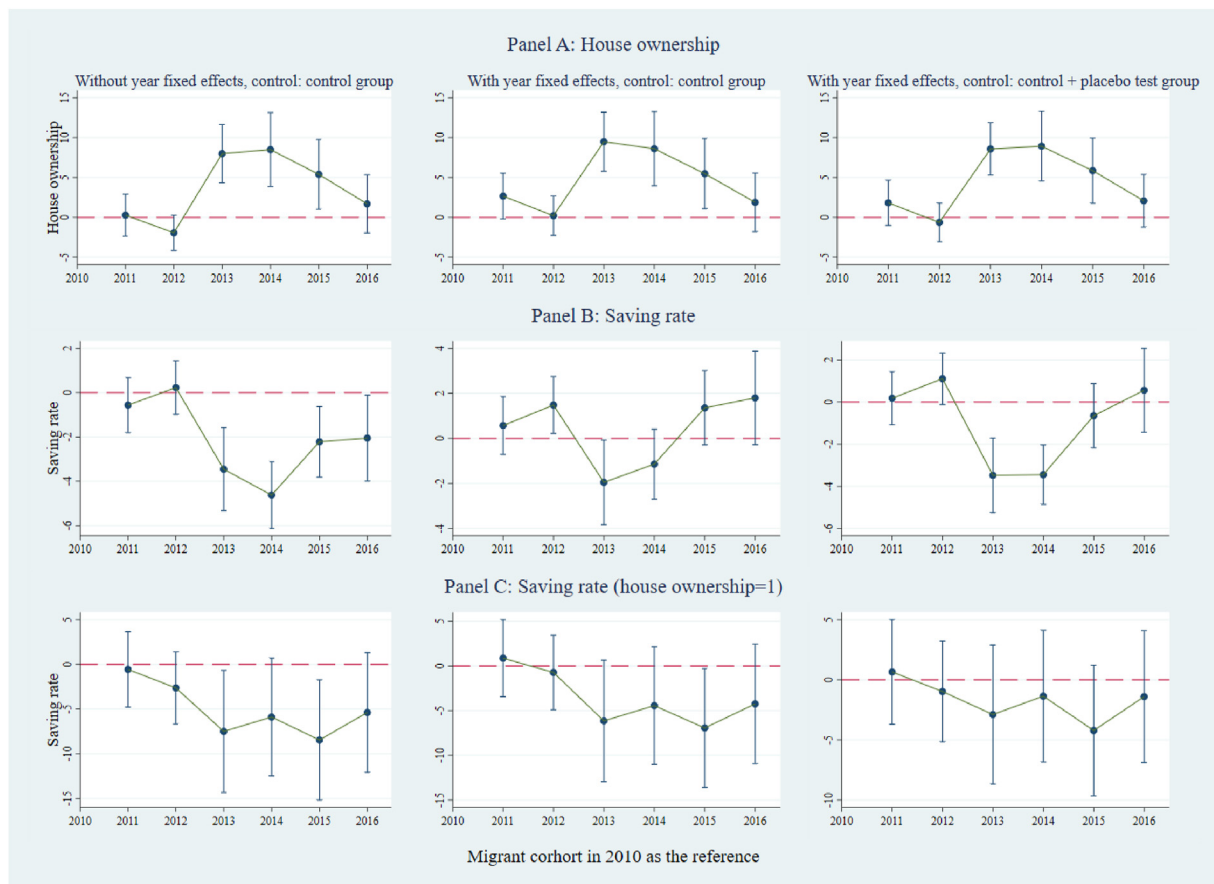

Fig. 4. Parallel trend test with homeownership and saving rate. Note: The migrant cohort in 2010 is the base. The affected migrant cohorts are the cohorts migrating in 2013 and after.

housing bought by first-time home buyers. Thus, migrants are not directly affected by the property tax reform but are indirectly affected by the housing price effect. Therefore, we eliminate the respondents from these two cities to re-estimate the effect of home purchase restrictions, and Table 8 shows the results. The policy effect is still significant, and the results also pass the parallel trend test, which is presented as Panels A and B in Fig. 5.

As mentioned in section 3, removing home purchase restrictions has a direct and positive effect on homeownership by entitling migrants to more flexible purchasing rights. At the same time, there is an indirect and negative effect on homeownership due to the increase in housing prices, and this effect is similar to the property tax reform. Therefore, we capture the net positive effect of removing the home purchase restrictions on homeownership, and it can also be inferred that there are significant

Table 7
DID of migrant cohorts with placebo test group as the treated.

	Homeownership	Saving rate
	(1)	(2)
Treated × Post	0.0017 (0.0180)	−0.0176* (0.0104)
Migrant year dummies	Yes	Yes
Year dummies	Yes	Yes
City dummies	Yes	Yes
Sample of years included	2010, 2013, 2017	2010, 2013, 2015, 2017
Observations	9458	20,864
R²	0.1560	0.1056

Note: Robust standard errors are presented in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively. Other control variables presented in Table 5 column (1) are all included, that is, log of real household income, family members, sex, age, age squared, education, marital status, working status, medical insurance, and employment status; we also include settlement intention. Constant is included in all regression settings.

direct and positive effects.

5.3.4. Difference-in-differences estimation by year

Another way to design the DID approach is by survey year. In this respect, the results based on equation (5) are shown in columns (1), (2), (4) and (5) of Table 9, and the results based on equation (6) are shown in column (3). In columns (1) and (3)–(5), the control group consists of cities that did not lift the home purchase restrictions. Column (1) illustrates that the removal of these restrictions in approximately 2014 entitles more households to be involved in housing-purchasing activities, which further reduces the saving rate by 6.84 percentage points. If we take the cities that never imposed such restrictions as well as the cities that did not lift them as the control group, the reduction is 6.15 percentage points.

To determine the extent to which removing the home purchase restrictions affects the savings gap between migrants and the local *hukou* population, from columns (4) and (5) in Table 9, we see that the negative effect on savings is larger for the local *hukou* population and that the removal of these restrictions reduces the saving rate by 4.43 percentage points for migrants and by 10.55 percentage points for the local *hukou* population. Furthermore, we estimate equation (6) using the DDD setting and consider only the control group of cities that did not lift their home purchase restrictions. We see that removing these restrictions is more effective in reducing the savings of the local *hukou* population, which can be easily understood because these restrictions were implemented differently for the two groups. Hence, it can naturally be concluded that removing the home purchase restrictions stimulates consumption and exerts a more prominent impact on reducing the savings of the local *hukou* population.

5.4. IV regression

To address the endogeneity of homeownership in equation (1), we

introduce the exogenous policy shock, as discussed in sections 5.2 and 5.3. As mentioned in section 4.2.5, we use DID_{cp}, the interaction term of the migrant cohort multiplied by the treated city dummies, as our IV, and we employ the peer effect as a second IV to utilize the overidentification test.

As shown in Table 10, columns (1) to (4) employ the full sample with only the 2010–2016 migrant cohorts, and columns (5) and (6) exclude the respondents in the cities that never imposed home purchase restrictions. Columns (1) and (2) use only DID_{cp} as the IV, while the other columns use the two IVs mentioned above. Columns (1), (3), and (5) give the first-stage estimation with IV regressions, while the others show the second-stage estimation. The IVs pass the overidentification test or meet the exclusion restriction assumption, as shown in columns (3) and (5). The F values range from 7.25 in column (1) to 23.03 in column (3), and the coefficients in the first stage are significant. We believe that the IVs also pass the weak instrument test.

The IV estimations of homeownership are significant and larger than the OLS estimation results in Table 5, which implies that there is an endogeneity problem because the IV estimation measures a certain kind of LATE. Furthermore, DID_{cp} and the peer effect positively and significantly affect homeownership.

5.5. Further discussion

5.5.1. How does removing the restrictions affect the saving rate?

Does removing home purchase restrictions affect the saving rate through homeownership only? To answer this question, we include homeownership in the estimations in Panel B of Fig. 4. From the results shown in Table 11, the coefficients in and after 2013 are no longer significantly negative, which reveals that removing the home purchase restrictions affects the saving rate through homeownership.

5.5.2. Verification of the persistent effect of home purchase restrictions

The DID specification by migrant cohorts is based on the assumption that the effect of home purchase restrictions persists. That is, the probability of home purchase will be persistently reduced in cohorts affected by home purchase restrictions even if these restrictions have been lifted. The underlying reasons may be that because of the limitations on home purchases, migrants will buy housing in their hometown instead, as proposed by Wu and Zhang (2018) and Wang et al. (2020a), or that they will make capital investments (Wang et al., 2020b), such as founding an enterprise, both of which will permanently affect the average housing-purchasing power of these cohorts. We try to empirically test and verify this effect from the perspective of hometown home purchases and entrepreneurial behaviors.

Regarding the first assumption that migrants will buy housing in their hometown if they are restricted from buying housing in local cities, which will permanently reduce their possibility of buying housing locally even after such restrictions are lifted, we adopt OLS estimation based on equation (7).

Table 8
Eliminating the samples of Shanghai and Chongqing.

	Homeownership	Saving rate	Homeownership	Saving rate
	(1)	(2)	(3)	(4)
Treated × Post	0.0437** (0.0176)	−0.0120 (0.0082)	0.0398* (0.0205)	−0.0194** (0.0085)
Treated	0.0865*** (0.0146)	0.0120* (0.0065)		
Post	−0.0427*** (0.0130)	0.0164** (0.0065)		
Migrant year dummies	No	No	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
City dummies	No	No	Yes	Yes
Sample of years included	2010, 2013, 2017	2010, 2013, 2015, 2017	2010, 2013, 2017	2010, 2013, 2015, 2017
Observations	11,216	24,464	11,216	24,464
R²	0.1921	0.1023	0.2006	0.1113

Note: Robust standard errors are presented in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively. Other control variables presented in Table 5 column (1) are all included, that is, log of real household income, family members, sex, age, age squared, education, marital status, working status, medical insurance, and employment status; we also include settlement intention. Constant is included in all regression settings.
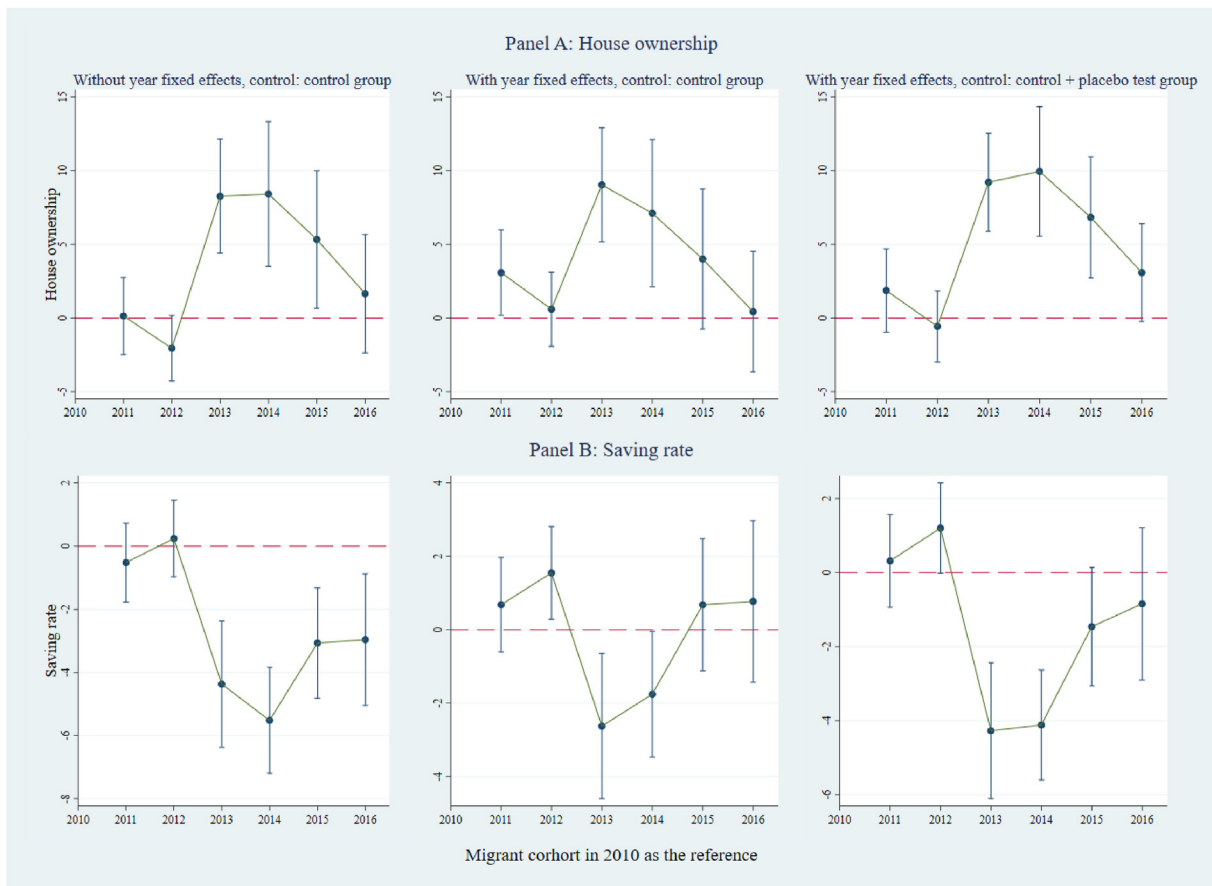
Fig. 5. Parallel trend test with homeownership and saving rate eliminating Shanghai, Chongqing samples. Note: The migrant cohort in 2010 as the base. The affected migrant cohorts are the cohorts migrating in 2013 and after.

Table 9
The effects of removing home purchase restrictions on saving rates.

Control group	Cities with home purchase restriction	All	Cities with home purchase restriction	Cities with home purchase restriction	Cities with home purchase restriction
Sample	All	All	All	Migrant	*Hukou* population
	(1)	(2)	(3)	(4)	(5)
Treated × Post × Migrant			0.0758***		
			(0.0145)		
Treated × Post	−0.0684***	−0.0615***	−0.1176***	−0.0443***	−0.1055***
	(0.0065)	(0.0046)	(0.0128)	(0.0070)	(0.0130)
Treated × Migrant			−0.0723***		
			(0.0133)		
Post × Migrant			−0.0786***		
			(0.0128)		
Treated	0.0637***	0.0542***	0.1097***	0.0375***	0.1123***
	(0.0059)	(0.0037)	(0.0118)	(0.0061)	(0.0117)
Post	−0.0203***	−0.0308***	0.0321***	−0.0299***	−0.0182
	(0.0057)	(0.0035)	(0.0115)	(0.0060)	(0.0128)
Migrant	0.0537***	0.0504***	0.1218***		
	(0.0032)	(0.0029)	(0.0121)		
Sample of years included	2010, 2013, 2015	2010, 2013, 2015	2010, 2013, 2015	2010, 2013, 2015	2010, 2013, 2015
Observations	67,663	84,347	67,663	42,789	24,874
R²	0.1527	0.1508	0.1539	0.1248	0.1753

Note: Robust standard errors are presented in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively. Other control variables presented in Table 5 column (1) are all included, that is, log of real household income, family members, sex, age, age squared, education, marital status, working status, medical insurance, and employment status; we also include settlement intention in column (4). Constant is included in all regression settings.

$$Hometown\ house\ ownership_{icp} = \beta_0 + \alpha_1 Marital\ cohort_c \times Restriction\ cities_p$$
$$+ \alpha_2 Marital\ cohort_c + \alpha_3 Restriction\ cities_p + X'_{icp}\beta + \varepsilon_{icp}$$

(7)

where subscript i means household i, p refers to city p, and c refers to cohort c. The dependent variable *Hometown house ownership$_{icp}$* takes the value of one if migrants buy housing in their place of *hukou* registration

Table 10
The IV regression.

Sample	All				Cities never issuing home purchase restrictions excluded	
Dependent variable	Homeownership	Saving rate	Homeownership	Saving rate	Homeownership	Saving rate
		TSLS		TSLS		TSLS
	(1)	(2)	(3)	(4)	(5)	(6)
DID	0.0392***		0.0400***		0.0351*	
	(0.0145)		(0.0145)		(0.0193)	
Others' homeownership			0.2440***		0.2219***	
			(0.0391)		(0.0428)	
Homeownership		−0.6977*		−0.5554***		−0.5930***
		(0.3639)		(0.1103)		(0.1374)
Migrant year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
City dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	21,482	21,482	21,270	21,270	13,548	13,548
F-value	7.25		23.03		14.99	
Wooldridge's robust score test (p-value)			0.5715 (0.4496)		0.0198 (0.8880)	
R^2	0.2010		0.2048		0.2152	

Note: Robust standard errors are presented in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively. Other control variables presented in Table 5 column (1) are all included, that is, log of real household income, family members, sex, age, age squared, education, marital status, working status, medical insurance, and employment status; we also include settlement intention. Constant is included in all regression settings. Data from the 2010, 2013, and 2017 waves of the CMDS are used.

Table 11
Removing the restriction affecting saving rate.

Sample	All		Excluding cities never issuing home purchase restrictions	
	(1)	(2)	(3)	(4)
DID	−0.0124		0.0075	
	(0.0105)		(0.0132)	
DID2011		0.0090		0.0114
		(0.0100)		(0.0099)
DID2012		0.0257***		0.0293***
		(0.0092)		(0.0091)
DID2013		−0.0244		−0.0096
		(0.0198)		(0.0216)
DID2014		0.0004		0.0192
		(0.0164)		(0.0183)
DID2015		−0.0171		0.0027
		(0.0143)		(0.0162)
DID2016		−0.0052		0.0140
		(0.0135)		(0.0157)
Homeownership	−0.1145***	−0.1132***	−0.1085***	−0.1083***
	(0.0077)	(0.0077)	(0.0087)	(0.0086)
Treated	0.0443***	0.0461***	0.0198**	0.0232**
	(0.0061)	(0.0084)	(0.0085)	(0.0103)
Post	0.0051	0.0266***	−0.0076	0.0168
	(0.0080)	(0.0080)	(0.0117)	(0.0122)
Year dummies	Yes	Yes	Yes	Yes
Sample of years included	2010, 2013, 2017	2010, 2013, 2017	2010, 2013, 2017	2010, 2013, 2017
Observations	21,482	20,232	13,656	12,811
R^2	0.1240	0.1369	0.1221	0.1377

Note: Robust standard errors are presented in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively. Other control variables presented in Table 5 column (1) are all included, that is, log of real household income, family members, sex, age, age squared, education, marital status, working status, medical insurance, and employment status; we also include settlement intention. Constant is included in all regression settings.

and otherwise zero. *Marital cohort$_c$* is a dummy variable that takes the value of one when marital cohorts married before 2014 or after 2009 and otherwise zero. In the datasets, only the 2010 and 2013 samples contain information on hometown homeownership. Thus, the marital cohorts included are cohorts that married before 2014, and our sample is also limited to migrants who are married. For *Restriction cities$_p$*, the variable takes the value of one for cities that imposed home purchase restrictions, and it takes the value of zero for cities that never imposed such restrictions before 2014. Due to China's special marriage customs, owning a home at the time of marriage has become the new social norm in China, with approximately 70% of women believing that it is necessary to own a home at the time of marriage and 80% of Chinese mothers expressing opposition to their daughters marrying men without homes (Wei et al., 2017; Wrenn et al., 2019). Therefore, we assume that people are likely to own a home when they marry, and the coefficient of the interaction term *Marital cohort$_c$ × Restriction cities$_p$* indicates the effect of home purchase restrictions in local cities on home purchases in the hometown. The control variables are generally similar to those in equation (1), and *hukou* provincial fixed effects are included. We also consider the effect of homeownership in local cities. The results are listed in columns (1) to (3) of Table 12, and columns (1) and (2) suggest that the cohorts that married after 2010 and that were restricted from buying housing in local cities are inclined to buy housing in their hometown. In column (3), when we take local homeownership into account, the coefficient of home-ownership implies that when migrants buy local housing, the possibility of buying housing in their hometown decreases, and the interaction term is nonsignificant, which to some extent illustrates that home purchase restrictions affect housing-purchasing behaviors in their hometown by restricting local housing-purchasing behaviors.

Regarding the second assumption, migrants may start a business if they are restricted from buying housing locally, which may lower the possibility of buying housing locally even after such restrictions are lifted. Equation (3) and equation (8) are employed to test this assumption. In equation (3), the dependent variable is *Entrepreneurship$_{icp}$*, which takes the value of one if a migrant starts a business locally and otherwise zero. The estimation results are shown in columns (4) to (6) of Table 12, and the coefficients of the interaction terms are significant, which indicates that compared to cohorts migrating from 2010 to 2012, those migrating after 2013 are less likely to start a business.

$$Entrepreneurship_{icp} = \beta_0 + \alpha_1 Migrant\ cohort\ (<2010)_c \times Restriction\ cities_p + \alpha_2 Migrant\ cohort\ (>2013)_c \times Restriction\ cities_p$$
$$+ X'_{icp}\beta + \upsilon_c + \lambda_t + \mu_p + \varepsilon_{icp} \tag{8}$$

Table 12
Verification of the persistent effect of the home purchase restrictions policy.

Dependent variable	Homeownership in hometown			Entrepreneurial behavior			
Control group	Placebo test group			Control group		Control + placebo test group	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Marital cohort × Restriction cities	0.0411**	0.0236*	0.0109				
	(0.0189)	(0.0128)	(0.0183)				
Treated × Post				−0.0503***	−0.0765***	−0.0234**	
				(0.0133)	(0.0219)	(0.0118)	
Migrant cohort (<2010) × Restriction cities							−0.0345*
							(0.0189)
Migrant cohort (>2013) × Restriction cities							−0.0250*
							(0.0130)
Marital cohort	−0.0590***	−0.0201	−0.0141				
	(0.0182)	(0.0128)	(0.0169)				
Restriction cities	−0.4595***	−0.0371***	0.0324***				
	(0.0077)	(0.0065)	(0.0097)				
Treated				−0.0032	−0.0337**	−0.0367***	
				(0.0104)	(0.0148)	(0.0085)	
Post				0.0035	0.0306	−0.0033	
				(0.0093)	(0.0196)	(0.0081)	
Homeownership			−0.8421***		−0.0526***		−0.0293***
			(0.0063)		(0.0166)		(0.0099)
Control variables	No	Yes	Yes	Yes	Yes	Yes	Yes
Hukou provincial dummies	No	Yes	Yes	No	No	No	No
Year dummies	No	Yes	Yes	Yes	Yes	Yes	Yes
City dummies	No	No	No	No	No	No	Yes
Sample of years included	2010, 2013	2010, 2013	2010, 2013	2010, 2013, 2015, 2017	2010, 2013, 2017	2010, 2013, 2015, 2017	2010, 2013, 2017
Observations	33,104	33,029	13,046	29,660	13,702	39,047	38,946
R²	0.1234	0.7069	0.4385	0.0906	0.1123	0.0923	0.1571

Note: Robust standard errors are presented in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively. Other control variables include log of real household income, family members, sex, age, age squared, and education. Constant is included in all regression settings.

where the subscript setting is similar to equation (7). For $Restriction\ cities_p$, the variable takes the value of one for cities that impose home purchase restrictions, and it takes the value of zero for cities that never imposed such restrictions before 2017. There are two interaction terms: $Migrant\ cohort\ (<2010)_c \times Restriction\ cities_p$ and $Migrant\ cohort\ (>2013)_c \times Restriction\ cities_p$. $Migrant\ cohort\ (<2010)_c$ refers to migration before home purchase restrictions were imposed, and $Migrant\ cohort\ (>2013)_c$ refers to migration after such restrictions were lifted; therefore, the migrant cohorts migrating from 2010 to 2013 are the base group. Column (7) in Table 12 shows the estimation results, and the interaction terms demonstrate that the base group of migrant cohorts migrating from 2010 to 2013 is more likely to be involved in entrepreneurship. Additionally, in columns (5) and (7), the coefficients of homeownership are negatively significant. Based on the above analysis, the estimation we obtain from equation (3) is the lower bound of the actual policy effect.

5.5.3. Migration self-selection

The remaining problem we need to consider is self-selection. Migrants who do not have the ability to purchase houses or apartments subjectively or objectively probably left their local cities. As shown in Fig. 6, among the group of people with homeownership, 72.09% have settlement intentions, while among their counterparts, the figure is only 59.72%. If those who have no housing and who have emigrated are included in our sample, the estimated effect is even larger, which means that the effect of homeownership on savings is underestimated in our paper.

6. The mechanisms of housing effects

In their research analyzing the way housing prices affect savings, Zang et al. (2015) discuss the welfare dissipation effect and the expected wealth effect. Therefore, in this paper, we propose two possible hypotheses to understand the negative effects of homeownership on savings. The first hypothesis concerns the consumption effect. As wealth,
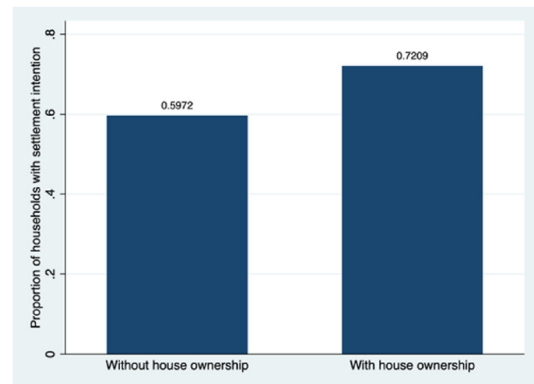

Fig. 6. Differences in settlement intention between groups with or without homeownership.

homeownership can provide economic security, which will lower the saving motive and increase the consumption of high-elasticity goods. Migrants have an even stronger precautionary saving motive because their income is more uncertain and they lack the social security provided by the local government (Chen et al., 2015); this economic security effect should thus affect migrants more. The second hypothesis concerns the housing purchase effect. People always save to purchase housing; therefore, people owning a house or an apartment will not have this saving motive and will save less.

We test the first hypothesis by analyzing the effect of homeownership on the consumption of high-elasticity goods. It is reasonable to believe that people will consume more high-elasticity goods when their precautionary saving motive is lower. Columns (1) and (4) in Table 13 show that people owning housing in local cities will increase their per capita expenditure on clothing and education by 41.04% and 28.98%, respectively, and these results are robust when we use the proportion of related

Table 13

The consumption effects of housing on saving rates.

	Ln (clothing expense)	Ln (clothing expense)	Clothing expense ratio	Ln (children's educational expense)	Ln (children's educational expense)	children's educational expense ratio
	(1)	(2)	(3)	(4)	(5)	(6)
Homeownership × Migrant		0.2635***			0.4016***	
		(0.0851)			(0.1307)	
Homeownership	0.4104***	0.3087***	1.5313***	0.2898***	0.0760	0.2405***
	(0.0414)	(0.0527)	(0.1375)	(0.0670)	(0.0914)	(0.0803)
Migrant	0.1463***	0.0345	0.0616	−0.5509***	−0.7793***	0.1364**
	(0.0433)	(0.0579)	(0.1545)	(0.0761)	(0.1017)	(0.0681)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
City dummies	Yes	Yes	Yes	Yes	Yes	Yes
Sample of years included	2010	2010	2010	2010, 2013	2010, 2013	2010, 2013
Observations	16,368	16,368	16,350	30,119	30,119	31,582
R^2	0.2138	0.2147	0.1495	0.2432	0.2436	0.0297

Note: Robust standard errors are presented in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively. Other control variables presented in Table 5 column (1) are all included, that is, log of real household income, family members, sex, age, age squared, education, marital status, working status, medical insurance, and employment status. In addition, we control the total number of children in the family and the number of their children in the local city. Constant is included in all regression settings.

expenditure in total expenditure as the independent variable. Moreover, the homeownership effect on consumption is even stronger in the migrant group, as shown in columns (2) and (5).

Regarding the second hypothesis, given the condition of homeownership, if people have strong motives to save for a home purchase or mortgage, then housing provident funds, which aim to increase the affordability of housing for households, should affect those who own housing. Thus, these homeowners probably have mortgages that are covered in part by housing provident funds. To be explicit, housing provident funds should have heterogeneous effects on those who have housing and those who do not. As shown by the evidence in column (3) in Table 14 and the nonsignificant coefficient of the interaction term between homeownership and housing provident funds, there is no significant difference between those with homeownership and those without homeownership. From another perspective, the group without homeownership but with the willingness to settle in local cities may save more for home purchases. We investigate the group of migrants without homeownership. The estimation results in column (4) show that willingness to stay has a nonsignificant effect on the saving rate. Therefore, it is unlikely that homeownership negatively affects savings through housing purchase pressure.

In addition, homeownership has a positive effect on savings. Married migrant couples always live separately in different places due to migration. Such couples can be called split households, and this situation is exacerbated by renting housing (Fan et al., 2011). Therefore, owning housing may promote family migration, which in turn increases local household income and, according to the law of diminishing marginal propensity to consume, further increases the saving rate. We test this hypothesis by conducting a mediating effect analysis with only married samples by adopting the mediation analysis framework proposed by (Baron and Kenny, 1986). To capture the mechanism of the increased household income effect, we use a dummy variable for whether a certain migrant lives locally with his or her spouse as the mediator and estimate the following equations to conduct mediation analysis.

$$Ln(real\ income)_{ict} = \beta_0 + \alpha_1 Homeownership_{ict} + X'_{ict}\beta + \lambda_t + \mu_c + \varepsilon_{ict} \quad (9)$$

$$Living\ with\ spouse_{ict} = \beta'_0 + \alpha'_1 Homeownership_{ict} + X'_{ict}\beta' + \lambda'_t + \mu'_c + \varepsilon'_{ict} \quad (10)$$

$$Ln(real\ income)_{ict} = \beta''_0 + \alpha''_1 Homeownership_{ict} + \alpha''_2 Living\ with\ spouse_{ict} + X'_{ict}\beta'' + \lambda''_t + \mu''_c + \varepsilon''_{ict} \quad (11)$$

Plugging equation (10) into equation (11), we obtain equation (12):

$$Ln(real\ income)_{ict} = (\beta''_0 + \alpha''_2\beta'_0) + (\alpha''_1 + \alpha''_2\alpha'_1)Homeownership_{ict} + X'_{ict}(\beta'' + \alpha''_2\beta') + \lambda'''_t + \mu'''_c + \varepsilon'''_{ict} \quad (12)$$

The overall effect of housing on household income is α_1 in equation (9), while the direct effect and the indirect effect through living with a spouse are α''_1 and $\alpha''_2\alpha'_1$ in equation (12), respectively. To identify the indirect effect, we employ the Sobel-Goodman test.

The estimation results in Table 15 show that homeownership increases household income indirectly through family migration or living with a spouse, and the indirect effect accounts for 4.87% (0.025*0.389/(0.190 + 0.025*0.389)) of the total effect on average, 6.60% (0.0425*0.469/(0.2822 + 0.0425*0.469)) of the total effect for migrants and 1.35% (0.0096*0.184/(0.128 + 0.0096*0.184)) of the total effect for the local *hukou* population. These results also indicate that the economic significance of this mechanism is lower.

7. Conclusion

There have been several explanations for China's high savings; this paper focuses on the effect of homeownership on migrant savings and the savings gap between the *hukou* population and migrants. First, using the 2010, 2013, 2015, and 2017 waves of the CMDS, we find that migrants save at least 4.87 percentage points more than the *hukou* population; however, when we focus on households that have bought housing, the saving rate of migrants with homeownership drops below that of the local *hukou* population. In addition, homeownership lowers the saving rate of migrants and raises the saving rate of the local *hukou* population, with endogeneity addressed. We also use IVs to solve the problems stemming from the availability of data for the migrant sample and the characteristics of the policy shock of HPR. Second, we consider the removal of HPR from 2014 to 2016 using a DID approach that incorporates migrant cohorts into the design. We demonstrate that removing these restrictions effectively increases the probability of migrants buying housing locally, thus lowering the saving rate of migrant households, and this result is robust. Therefore, housing and enhancing the right to purchase houses help to stimulate consumption. Third, using the policy shock and the peer effect as IVs, we prove that the homeownership effect is biased in OLS estimation and that it should be even larger based on IV estimation. Finally, regarding the mechanisms of the effect of homeownership on savings, we propose two negative hypotheses. The first hypothesis is that housing, as wealth, provides economic security and will lower the precautionary saving motive; in turn, people will buy more goods or services with higher price elasticity, such as clothing and children's education. The second hypothesis concerns the housing purchase motive; since people will save to buy housing, for families with homeownership, this motive is lower or even disappears.

Table 14

The effect of housing purchase on saving rates.

Sample	Homeownership = 1	Homeownership = 0	All	Homeownership = 0 & migrants
	(1)	(2)	(3)	(4)
Homeownership × Housing provident fund			−0.0031	
			(0.0093)	
Homeownership			−0.0263***	
			(0.0074)	
Housing provident fund	−0.0145**	−0.0229***	−0.0176**	−0.0170**
	(0.0067)	(0.0087)	(0.0082)	(0.0083)
Residence willingness				−0.0004
				(0.0043)
Migrant	−0.0303***	0.1172***	0.0444***	
	(0.0098)	(0.0113)	(0.0071)	
Year dummies	Yes	Yes	Yes	Yes
City dummies	Yes	Yes	Yes	Yes
Sample of years included	2010, 2013	2010, 2013	2010, 2013	2010, 2013
Observations	15,486	24,942	40,428	22,801
R^2	0.1559	0.1870	0.1690	0.1523

Note: Robust standard errors are presented in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively. Other control variables presented in Table 5 column (1) are all included, that is, log of real household income, family members, sex, age, age squared, education, marital status, working status, medical insurance, and employment status. Constant is included in all regression settings.

Table 15

The household income-increasing effect of housing on saving rates with living with spouse as the mediator.

Sample	All married		Married migrants		Married local *hukou* population	
	Living with spouse	Ln (real income)	Living with spouse	Ln (real income)	Living with spouse	Ln (real income)
	Equation (10)	Equation (11)	Equation (10)	Equation (11)	Equation (10)	Equation (11)
	(1)	(2)	(3)	(4)	(5)	(6)
Homeownership	0.0251***	0.1902***	0.0425***	0.2822***	0.0096**	0.1282***
	(0.0049)	(0.0112)	(0.0077)	(0.0140)	(0.0045)	(0.0188)
Living with spouse		0.3893***		0.4692***		0.1839***
		(0.0128)		(0.0131)		(0.0367)
Migrant	−0.0845***	0.2558***				
	(0.0050)	(0.0116)				
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
City dummies	Yes	Yes	Yes	Yes	Yes	Yes
Sample of years included	2010, 2013	2010, 2013	2010, 2013	2010, 2013	2010, 2013	2010, 2013
Observations	32,084	32,084	19,417	19,417	12,667	12,667
R^2	0.1148	0.2762	0.1606	0.2672	0.0265	0.3257
Sobel test						
Indirect effect		0.0098***		0.0199***		0.0018***
		(0.0019)		(0.0037)		(0.0009)
Proportion of mediated total effect		4.87%		6.60%		1.35%

Note: Robust standard errors are presented in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% levels, respectively. Other control variables are all included, that is, log of real household income, family members, sex, age, age squared, education, marital status, working status, and employment status. Constant is included in all regression settings.

We prove that homeownership likely affects savings through the former mechanism. Additionally, homeownership can increase the saving rate by increasing the possibility of couples living together, which in turn increases household income per capita; however, this mechanism does not have a significant effect.

Our results contribute to the debate over "the Chinese saving puzzle" from the perspective of housing market policy. First, *hukou* discrimination due to real estate market policy will lead to an increase in savings. That is, *hukou*-based policy, not only the system of public goods allocation but also the right to buy private goods in the market, such as houses, will harm both migrant welfare and sustained economic growth by raising savings. Migrants have become an inseparable part of urbanization, and their saving motive could make an important contribution to the national saving rate in China. To decrease the precautionary saving motive, housing plays a more important role for migrants than for the local *hukou* population. Second, policy uncertainty in the real estate market will exacerbate the increase in savings. Removing home purchase restrictions helps to boost the real estate market and enhance consumption, and this effect is long lasting. Finally, while many reforms implemented in China have aimed to boost domestic consumption, such as the removal of home purchase restrictions, these reforms are to some extent characterized by *hukou*-based discrimination. To be more effective and harmonious, such reforms need to be further deepened and improved.

Declaration of competing interest

We declare that we have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.econmod.2021.105679.

Funding

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