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Housing wealth and labor supply: Evidence from a regression discontinuity design☆

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ABSTRACT

This paper uses the discontinuity in house size generated by Chinese housing policies to identify the effect of housing wealth on labor supply. The analysis finds a substantial deterrent effect of housing wealth on labor supply. Much of the housing wealth effect comes from the labor participation decision, with the effect on employed workers being ambiguous. Females, young generation and households with high repayment capacity are more responsive to gains in housing wealth.

1. Introduction

Housing is the most important household asset in many economies (Badarinza et al., 2016), and in particular, a dominant asset for middle-class homeowners (Campbell, 2006). Understanding how housing wealth affects household behavior is then of great importance to economists and policymakers. For example, some prominent policymakers such as former Fed governors Frederic Mishkin and Ben Bernanke point out that the housing wealth effect may play a vital role in the monetary transmission mechanism, and needs to be taken into consideration when making policies (Calomiris et al., 2009).[1] In this paper, we study the effect of unexpected gains in housing wealth on labor supply.[2] Such an evaluation is often a critical consideration when designing a wide variety of policies, such as taxes and welfare programs, as the primary goal of these policy reforms is to encourage worker efforts (Blundell and MaCurdy, 1999). Meanwhile, the response of labor supply to housing wealth helps to explain employment patterns and the consequences of economic

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[1] The 2013 Economic Report of the President stated that "A total of $16 trillion in wealth was erased by the financial and housing crisis, causing families to pull back on spending plans, reduce personal debt and increase savings, in turn leading companies to cut back hiring, lay off valued employees, and halt investment plans."
[2] Literature has examined the impacts of housing wealth on: consumption (e.g., Bostic et al., 2009; Disney et al., 2010; Attanasio et al., 2011; Browning et al., 2013; Cooper, 2013), entrepreneurship (e.g., Adelino et al., 2015; Corradin and Popov, 2015), fertility (e.g., Dettling and Kearney, 2014; Lovenheim and Mumford, 2013), education (e.g., Lovenheim, 2011; Lovenheim and Reynolds, 2013), financial investment decisions (e.g., Chetty et al., 2017), and child development (e.g., Goux and Maurin, 2005; Cooper and Luengo-Prado, 2015; Jacob et al., 2014).

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fluctuations, especially after the largest-ever housing bust in the mid-2000s.

Any investigation of the effect of housing wealth on labor supply encounters an inherent identification issue: households do not randomly purchase housing units. As a result, changes in housing wealth could be accompanied by heterogeneity in household characteristics, which would confound the estimation. One solution widely used in the literature is to explore variations in local housing markets, which are arguably exogenous to households (e.g., Disney and Gathergood, 2018; Johnson, 2014; Klein, 2014; Zhao and Burge, 2017). However, whether households randomly select the location of residence is questionable. Hence, it is difficult to separate changes in local housing prices from changes in other local residential characteristics.

We implement a regression discontinuity (RD) design to address the identification problem. Chinese housing policies enacted in the mid-2000s generated a discontinuity in the size of housing units. Specifically, the State Council, China's cabinet, and its seven ministries issued a document in May 2006 that reduced the percent down payment for units with floor areas less than or equal to 90 m^2, from 30 to 20%. Later, on November 1, 2008, the central government further lowered the property deed taxes for individuals purchasing housing units with floor areas less than or equal to 90 m^2, from 3 to 1%. Hence, houses just below and just above the threshold of 90 m^2 may share similar characteristics but differential growth rates in housing prices due to the favorable terms given to small houses. This discontinuity in housing value with respect to house size is used to identify the effect of housing wealth on household labor supply.

Moreover, China provides an ideal setting to investigate the housing wealth effect. Housing assets represent the dominant component in Chinese families' asset portfolios. Fig. 1 depicts the shares of different assets in total family wealth for 10 quantiles of family wealth in 2017 from the China Household Finance Survey (CHFS), a data set that we describe below. We observe an overwhelming dominance of housing assets, i.e., housing assets account for more than 60% of total family wealth in China. Changes in housing values are a major factor driving fluctuations in household wealth for Chinese families. Moreover, there has been a tremendous boom in the Chinese real estate market in the past decade. According to the National Bureau of Statistics of China, housing prices in 35 major Chinese cities increased by 12.68% annually on average from 2002 to 2010. Such dramatic changes in housing wealth allow us to identify the effect on labor supply response.

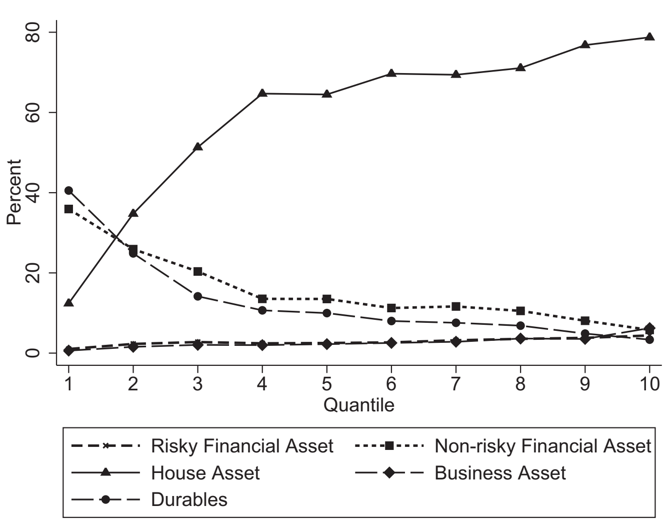

Fig. 1. Shares of different assets in total family wealth. Note: 1. X axis represents ten quantiles of family wealth in the data.
Data source: CHFS 2017.

We obtain five sets of results. First, anecdotal evidence and quantitative analyses following the suggestions by Lee and Lemieux (2010) confirm the validity of our RD framework; that is, there is no full manipulation of the assignment variable (i.e., house size). Second, we find a substantial deterrent effect of housing wealth on labor supply. Specifically, the preferential housing policies cause households with floor areas of 90 m^2 to work 0.567 fewer hours or 12.57% less than the control group in the past week, 12.59% less than the control group in the past month and 13.12% less than the control group in the past year. Prices for homes with floor areas smaller than 90 m^2 increased 1.3% faster than those with floor areas larger than 90 m^2. In conjunction, these results imply that a one-percent increase in the annual growth rate of the price of housing reduces hours worked by 118.09 per year, which corresponds to approximately 10% of the mean annual hours worked for the control group. We also calculate that a one-percent increase in housing wealth per year reduces annual labor earnings by 182.67 RMB (USD 26.92). This implies that a gain of USD 10,000 in housing wealth reduces annual labor earnings by USD 203.06, corresponding to 5.97% of the control mean. To link to the employment dynamics in China, we find that our estimates can explain approximately 5.4% of the macro employment changes during the period. And the percent change in employment induced by the growth of housing wealth and the percent change in employment from the China Statistical Yearbook are significantly correlated with a degree of 0.274.

Third, we find that much of the housing wealth effect comes from the labor participation decision; that is, the increase in housing wealth leads individuals to withdraw from the labor market. However, the effect on the labor supply of employed workers is ambiguous. Fourth, using a sample of households that purchased houses after the regulatory policies were introduced (hence, the identifying assumption underlying the RD framework may not hold), we examine the severity of the omitted variable bias arising from the nonrandom selection of house size, which frequently contaminates the results of previous studies. We obtain dramatically different results (i.e., different signs and substantial differences in magnitude) in this sample with problematic identification, suggesting that biases from the nonrandom selection of house size are significant.

Fifth, we investigate heterogeneous effects across gender, across age and across repayment capacity groups. We find that females are more sensitive in their labor supply response, which is consistent with the view of gender identity in many societies; that is, males should earn more than females (Bertrand et al., 2015). Across age groups, we find that elderly people (defined as individuals aged 60 and older) exhibit a small response, in absolute terms, to gains in housing wealth, which is consistent with the findings by Imbens et al. (2001). The young population (i.e., those below age 45) has a similar response to the cohorts near retirement (i.e., those aged between 45 and 60). Across repayment capacity, we find that households with lower debt-to-asset ratios exhibit much stronger labor supply responses to changes in housing wealth than low-repayment-capacity households.

Our paper connects to several strands of literature. It is closely related to studies on housing wealth and labor supply. Henley (2004) uses British panel data on individual workers from 1992 to 2001 to investigate the adjustment of working hours to housing wealth gains and financial windfalls and finds significant reductions in working hours in response to housing gains. Jacob and Ludwig (2012) study the labor supply response to a mean-tested housing program in Chicago. Using the variations in local housing prices for identification, Disney and Gathergood (2018) find significant housing wealth effects on labor supply in the United Kingdom. Johnson (2014) considers the effect of housing prices on female labor force participation and finds little evidence for the US. Zhao and Burge (2017) examine the effect of housing wealth on the labor supply of the elderly, given the unprecedented growth in the number of elderly-headed households in the past decade in the US. Klein (2014) uses unexpected changes in housing prices to identify labor supply responses for a broad range of housing types and other characteristics, such as gender, marital status, and demographic groups. Our study's contribution to this literature lies in its novel identification strategy. Instead of using variations in the local housing market as the instrument, we implement an RD specification and carefully address various identifying conditions underlying the RD framework (e.g., the no-full-manipulation assumption and the treatment effect away from the cutoff point). Our analyses show that the nonrandom selection of house size can introduce substantial estimation bias.

Our study is also related to studies on the labor supply response to other types of financial wealth. In a seminal work, Holtz-Eakin et al. (1993) examine the labor supply response to inheritances and confirm the Carnegie conjecture that large inheritances reduce labor force participation. However, using data from the Michigan Panel Study, Joulfaian and Wilhelm (1994) do not find a large inheritance effect on labor supply. In another seminal work, Imbens et al. (2001) explore the unexpected earning gains from the lottery and find that lottery prizes significantly reduce labor earnings, especially for individuals aged between 55 and 65. Similarly, Cesarini et al. (2016) examine a large sample of Swedish lottery players and find that winning a lottery prize modestly reduces labor earnings. The departure of our study from this line of work is that we focus on changes in housing wealth, which is the most important component of family wealth. Moreover, appreciations in housing wealth may differ in nature from other types of wealth gains (e.g., lottery prizes, inheritance income); hence, estimates of their impact could be very different. Inheritances and lottery winnings usually involve windfalls paid in lump sums, which has been shown to boost the consumption of goods, services and leisure because of lifetime wealth effects (Cesarini et al., 2017). However, for households planning to live in their house forever, housing price appreciations increase housing equity, which in turn affects household behavior (e.g., consumption, labor supply, portfolio choices). First, rising home equity loosens a household's lifetime budget constraint, otherwise known as the pure wealth effect (e.g., Cesarini et al., 2017).[3] Second, rising house prices relax households' borrowing constraints, so that they can re-mortgage and withdraw equity for spending and investment, otherwise known as the borrowing collateral effect (e.g., Cooper, 2013).

In addition to the researches on the housing wealth effect, our work belongs to a recent literature investigating the effect of family wealth on economic behavior. Recent studies have examined the effect on, for example, consumption (e.g., Lettau and Ludvigson, 2004; Campbell, 2006; Kuhn et al., 2011; Mian and Sufi, 2011; Mian et al., 2013), entrepreneurship (e.g., Hurst and Lusardi, 2004; Wang, 2012), fertility (e.g., Lovenheim and Mumford, 2013), financial investment decisions (e.g., Briggs et al., 2015), and child development (e.g., Akee et al., 2010; Cesarini et al., 2017; Akee et al., 2018).

The remainder of this paper is organized as follows. Section 2 presents the estimation strategy, including a description of housing policies in China, data and variables, and our RD estimation framework. Section 3 describes our empirical findings, including validity checks on the identification strategy, main results, robustness checks, extensive and intensive margin analyses, investigations of omitted variable bias and the treatment effect away from the cutoff point, and heterogeneous effects. The paper concludes with Section 4.

[3] However, in contrast to wealth shocks generated by prizes and inheritances, housing values are perceived wealth. That is, households feel richer as house prices appreciate, which then increases their confidence to spend, borrow and take risks.

2. Estimation strategy

2.1. Housing policies in China

In the late 1990s, China began to abandon its Soviet Union style housing distribution system and embraced market-oriented housing reform. In a milestone document issued by the State Council in 1998, "The Resolution on Continuing Urban Housing System Reform and Accelerating Housing Development," the central government formally abolished the previous welfare housing system based on residents' work units and urged individuals to purchase commercial housing from the market. To promote privatization, the government established a new housing finance system to help individuals obtain mortgages. The People's Bank of China, China's central bank, further lowered the mortgage interest rates five times between 1998 and 2002. These marketization policies effectively stimulated the development of China's housing market, and the real estate sector became the new engine of China's economic growth.

In the early 2000s, housing market development was accompanied by massive urbanization throughout China, as millions of rural migrants moved into cities each year, which created huge demand for urban residential housing. The price of housing began to grow at a dazzling speed. According to the National Bureau of Statistics of China, from 2002 to 2010, housing prices in 35 major Chinese cities increased by 12.68% annually on average.

These soaring housing prices generated strong public sentiment on the issue of housing affordability and attracted considerable attention from policy makers. Some worried that the housing affordability problem could jeopardize social stability and create social turmoil. Others feared that the "overheated" housing market could put the financial market at risk if there was a slump in the market. Therefore, the government launched a series of regulatory policies beginning in 2005, in an effort to stabilize the growth of housing prices. Enhancing the affordability of housing, especially for low-to-medium income households, became the priority of public policies in this era (Yang et al., 2014).

In May 2006, the State Council and its seven ministries issued a document, "Suggestions on Adjusting Housing Supply Structure to Stabilize Housing Price," also known as *National Article Six*. This milestone document provided the basic framework for housing market regulation in China, where the main idea was to encourage the demand and supply of smaller and more affordable housing units. Specifically, the new policy lowered the percentage of the down payment for purchasing units with floor areas less than or equal to 90 m^2 to 20%, while the down payment for units with floor space greater than 90 m^2 remained at 30%. The discontinuous down payment ratio at 90 m^2 was further emphasized in the new housing finance policy issued in September 2007 (also known as the *9.27 Housing Finance Policy*) and the new housing regulation issued in April 2010 (also known as the *New National Article Ten*).

Moreover, the Ministry of Finance and the State Administration of Taxation jointly issued a document indicating that from November 1, 2008, the property deed taxes for individuals purchasing housing units with floor areas less than or equal to 90 m^2 would decrease from 3 to 1%. This discontinuity in the tax rate for different house sizes was further introduced in the document jointly issued by the Ministry of Finance, the State Administration of Taxation, and the Ministry of Housing and Urban-Rural Development in 2010.

Note that in late 2000s and early 2010s, there were many other restrictive housing market cooling measures (such as *National Article Eleven* and the *New National Article Eight*) and other convoluted major macroeconomic and monetary policies. However, none of these regulations involved discontinuity in terms of house size, which is essential for our identification discussed in Section 2.3.

2.2. Data and variables

Our empirical analysis draws on data from the CHFS, the China Family Panel Studies (CFPS), and two waves of population censuses (i.e., the Fifth National Population Census of China in 2000 (the 2000 Census) and the 2005 One Percent Population Survey of China (the 2005 Mini Census), four nationally representative surveys in China. We summarize these data sets in Table A1 in the Appendix. Because the population censuses were conducted before our focal housing policies were implemented and did not record current house value, we combine the CHFS and CFPS data in our main analysis and use the 2000 Census and the 2005 Mini Census in examining the validity of our identification and the robustness of our findings.

2.2.1. CHFS

We use all four waves of the CHFS, which were jointly conducted by the People's Bank of China and the Southwestern University of Finance and Economics in 2011, 2013, 2015 and 2017. The CHFS is a nationally representative survey in China that has detailed information on household finance and assets, including housing, business assets, financial assets, and other household assets. It also contains many other household demographics and variables, such as expenditure, employment, social security, and many others. The first wave of the survey was conducted in summer 2011, with a sample size of 8438 households located in 320 communities across 25 of China's provinces. The second wave of the survey was conducted in 2013, with 28,228 households covering 29 provinces, 262 counties, and 1048 communities. The third wave, conducted in 2015, sampled 37,399 households in 1430 communities across 29 provinces. The last wave was conducted in 2017, with 40,011 households sampled from 29 provinces, 363 counties, and 1550 communities. The final CHFS sample includes 25,215 houses and 49,610 individuals, among which 4482 individuals come from the 2011 wave, 13,409 individuals from the 2013 wave, 11,911 individuals from the 2015 wave, and 19,808 individuals from the 2017 wave. Since the release of the data, the CHFS has attracted considerable attention from policy makers and media outlets, such as CNN, The New York Times, the Financial Times, Forbes, and PBS.

The CHFS employs a stratified three-stage probability proportion to size (PPS) random sample design. According to its documentation, the primary sampling units include 2585 counties from all provinces in China except Tibet, Xinjiang, Hong Kong, Macau, and Taiwan (The first and second waves do not include Inner Mongolia). The second stage of sampling involves selecting residential committees/villages from the counties/cities selected in the first stage. The last stage is to select households from the residential committees/villages chosen in the second stage. Every stage of sampling is conducted using the PPS method and weighted by its population size. During the interview, each respondent receives a small gift (valued at approximately $15) for completing the questionnaire. The average time to interview a household is 2 h.

2.2.2. CFPS

The CFPS is a nationally representative survey covering 25 of 31 province-level regions that accounted for 95% of the total population of China in 2010. This longitudinal survey was launched in 2010 by the Institute of Social Science Survey of Peking University, China. The first wave was administered in 2010, and subsequent waves were conducted in 2012, 2014, and 2016. Samples through three-stage (county or equivalent, village or equivalent, household) probability were drawn with implicit stratification. In the 2010 baseline survey, the CFPS successfully interviewed nearly 15,000 families and 33,600 individuals within these families, for an approximate response rate of 79%.

The CFPS contains four sets of questionnaires (i.e., community, family, adolescent, and adult), which include most questions covered in four U.S. counterpart data sets (i.e., the Panel Study of Income Dynamics, the Child Development Supplement, the Health and Retirement Study, and the New York Longitudinal Study). The family questionnaire collects information on the family structure, daily life, social interactions, and economy of the sampled families. In particular, it records home ownership, house size (in square meters), years lived in the home, and the initial and current market values. The individual questionnaires contain rich information on demographic and socioeconomic characteristics, such as gender, date of birth (month and year), ethnicity, marital status, educational attainments, family background, registered residency (or hukou in Chinese), type of residency (rural or urban) at different ages, employment details, and health details.

2.2.3. Two censuses

We use two waves of population surveys of China; that is, the 2000 Census and the 2005 Mini Census. Enumerations were taken between 1 and 15 November in 2000 and 2005, respectively, by the National Bureau of Statistics of China. The 2000 Census is the fifth national population census in China, covering 1,265,830,000 individuals from 31 provinces. We have the 0.95 thousandth sample of the 2000 Census, consisting of 1,180,111 individuals from 345,167 households. The 2005 One Percent Population Survey of China is also known as the Mini Census, covering a population of 17 million, or 1.31% of the total population of China residing in all 31 provinces. The Mini Census used geographical districts as its sampling frame and interviewed all households in a sampled district. We have a 15.2% sample of the 2005 One Percent Population Survey, consisting of 2,585,481 individuals from 996,588 households. These two population surveys contain detailed demographic information on the respondents, for instance, home ownership, housing cost, housing age, housing floor and structure, gender, place and date of birth (i.e., month and year), educational attainment, the type of hukou (i.e., rural or urban), province of hukou, employment status, monthly income (only in the 2005 Mini Census), weekly working days (in the 2000 Census), weekly working hours (in the 2005 Mini Census), marital status, number of children (enumerated for women only), and health status (only in the 2005 Mini Census).

2.2.4. Analysis sample

In our main analyses, we pool four waves of the CHFS and CFPS and keep the variables consistently measured across the data sets. To address the concern of full manipulation of house size, we restrict the sample to urban houses purchased before 2006. Our final sample includes 31,587 houses and 61,849 individuals. Of these, 25,215 houses and 49,610 individuals come from the CHFS, whereas 6372 houses and 12,239 individuals come from the CFPS.

2.2.5. Labor supply measures

Our outcome variable concerns households' labor supply. Specifically, our regression unit is at the individual level, measuring labor supply of each household member surveyed (e.g., the household head and his/her spouse). There are three questions related to labor supply in the CHFS. Specifically, there is a question asking interviewees aged 16 and above, "In the last week, what were your average daily working hours?". An outcome variable called *Daily Labor Supply* is constructed accordingly. There is another question asking interviewees, "In the last month, how many days per week did you work on average?". An outcome variable called *Monthly Labor Supply* is constructed accordingly. Finally, there is a question asking interviewees, "In the last year, how many months did you work in your current job?". An outcome variable called *Yearly Labor Supply* is constructed accordingly. Three similar questions related to labor supply are used in the CFPS. Specifically, household members aged 16 and above were asked "How many months did you work last year?", "How many days per month on average did you work during the working months in the last year?" and "How many hours per day did you work during the working days in the last year?". We constructed *Yearly Labor Supply*, *Monthly Labor Supply* and *Daily Labor Supply* in CFPS accordingly. To make the units comparable across the three measures, we convert days worked and months worked into hours worked. Specifically, we use the information on reported average hours worked per day and average days worked per week to calculate the average hours worked per month by multiplying these two numbers by 4. This method is applied to back out the average hours worked per year. In the 2000 Census data, only weekly days worked are included; that is, the census asks the following: "In the last week, how many days did you work?". An outcome variable called 2000 Weekly Labor Supply is constructed accordingly. In the 2005 Mini Census data, only weekly hours worked are included; that is, the Mini Census asks the following: "In the last week, how many hours did you work on average?". An outcome variable called 2005 Weekly Labor Supply is constructed accordingly. To match with the labor supply information in the CFPS and CHFS data, we convert weekly days/hours worked in censuses to monthly values and construct the measure of monthly hours worked.[4]

Meanwhile, in constructing the labor supply measures, we add a value of 0 for the unemployed, except for disabled individuals and students. We further disentangle the effect of housing wealth on labor supply at the extensive and intensive margins. Specifically, we construct three dummy variables indicating whether the household supplied labor in the last week, in the last month, and in the last year to examine the extensive margin effect. To study the intensive margin effect, we focus on the group of employed households to examine how housing wealth affects hours worked.

2.2.6. Predetermined characteristics

To check the validity of our RD framework, we construct a series of predetermined variables, i.e., the values of which were determined before the housing purchases. Specifically, we have the following: 1) *Education*: years of schooling; 2) *Gender*: a dummy variable taking value 1 if the interviewee is male and 0 otherwise; 3) *Age*: age of the interviewee; 4) *Marriage Status*: whether the interviewee was married before the housing purchase; 5) *Ethnicity*: a dummy variable taking value 1 if the interviewee is Han Chinese (China's majority ethnicity) and 0 otherwise; 6) *Number of siblings*: the interviewee's number of siblings; 7) *Father education*: the interviewee's father's years of schooling; 8) *Mother education*: the interviewee's mother's years of schooling; 9) *Father party membership*: a dummy variable taking value 1 if the interviewee's father is a communist party member and 0 otherwise; 10) *Mother party membership*: a dummy variable taking value 1 if the interviewee's mother is a communist party member and 0 otherwise; 11) *Housing cost*: housing price per m^2 when the house was acquired; and 12) *Housing age*: years since the house was purchased.

Table 1 reports variable definitions (Table 1a) and summary statistics for the variables used in our analysis. The mean and standard deviation for each variable for the full analysis sample are given in the left panel of Table 1b. We also present subsample statistics separately for the households with housing units 90 m^2 and smaller (treatment group) and with housing units larger than 90 m^2 (control group) in the middle and right panels, respectively. The average household head is 55 years old and has lived in his current house for 16 years. Respondents report average housing assets valued at approximately 1,221,600 RMB (180,435 USD),[5] which constitute a sizable portion of households' mean total wealth (1,707,200 RMB or 252,160 USD). On average, 9.1% of sample households report housing debt, and the average outstanding mortgage is 22,300 RMB (3294 USD), accounting for 7.8% of housing wealth[6].

2.3. Estimation framework

To identify the response of labor supply to housing wealth, we use the differential down payment ratios and property deed taxes for different house sizes introduced in a series of Chinese housing market cooling measures since 2006 and 2008. Specifically, the discontinuities in house size allow us to apply an RD framework, which is arguably the closest approach in observational data analysis to experimental designs (Lee and Lemieux, 2010).

As an illustration of our estimation framework, consider the case of two states: $T_i = 1$ indicates a state of high housing wealth, while $T_i = 0$ is the state of low housing wealth. Let Y_{i1} be the outcome (labor supply; see Section 4 for details of the construction of variables) of household i with $T_i = 1$ and Y_{i0} be the outcome with $T_i = 0$. Hence, the average treatment effect can be calculated as

$$\beta = E\left[Y_{i1} - Y_{i0}\right]. \tag{1}$$

However, in any observational data, we cannot observe both Y_{i1} and Y_{i0} for household i. Hence, the comparison of households from the two states could be biased due to the nonrandom selection issue. Specifically, $E[Y_i|T_i = 1] - E[Y_i|T_i = 0] = E[Y_{i1} - Y_{i0}|T_i = 1] + (E[Y_{i0}|T_i = 1] - E[Y_{i0}|T_i = 0]) \neq E[Y_{i1} - Y_{i0}|T_i = 1]$, as $E[Y_{i0}|T_i = 1] \neq E[Y_{i0}|T_i = 0]$.

To address this identification issue, we use housing regulations implemented in China in 2006–2008, which created a discontinuity in the house size for down payment ratios and property deed taxes, and the differential growth in housing wealth across the cutoff size of housing units. Specifically, denote $D_i \equiv I[c_i \le c_0]$ as the treatment status (i.e., policy-affected households), where $I[.]$ is an identity function; c_i is the size of housing unit purchased by household i; and $c_0 = 90$. Assuming that $E[Y_{i0}|c_i = c]$ is continuous in c at c_0, Hahn et al. (2001) show that the treatment effect (specifically, the intent-to-treat (ITT) effect in our setting) can be identified as

$$\hat{\beta} = \lim_{c \downarrow c_0} E\left[Y_i|c_i = c\right] - \lim_{c \uparrow c_0} E\left[Y_i|c_i = c\right]. \tag{2}$$

We estimate $\hat{\beta}$ using a nonparametric approach, i.e., local linear regression as suggested by Hahn et al. (2001). Specifically, $\hat{\beta}_{RD}$ is estimated from

$$\min_{\beta,\gamma,\tau,\delta} \sum_{i=1}^{N} K\left(\frac{c_i - c_0}{h}\right)\left[Y_i - \delta - \gamma\left(c_i - c_0\right) - \beta D_i - \tau D_i\left(c_i - c_0\right)\right]^2, \tag{3}$$

where D_i takes a value of 1 if $c_i \le c_0$ and 0 otherwise; h is the bandwidth; and $K(.)$ is a rectangle kernel function. We calculate the optimal bandwidth $h*$ using Imbens and Kalyanaraman's (2012) approach. To assess whether our findings are sensitive to the selected optimal bandwidth $h*$, we experiment with alternative bandwidths

[4] Specifically, the days worked per week in the 2000 Census data are multiplied by 8 (the mandatory daily working hours in China) and then by 4 to obtain monthly hours worked; whereas the weekly hours worked in the 2005 Mini Census data are multiplied by 4 to obtain the monthly hours worked.

[5] We converted the RMB value to US dollars using the official nominal exchange rate from the Word Bank in 2010.

[6] The mortgage rate declines in the age of the homeowners in our analysis: the mortgage rate is 26.3% for owners below age 45, 3.7% for owners aged between 45 and 60, and only 0.8% for those aged 60 and above. These rates become much higher when we also consider the houses bought after 2006. The corresponding mortgage rates for three age groups are 42.1%, 31.3%, and 5.5%.

Table 1a
Variable definitions.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

Variables	Definition
Outcome variables	
Daily labor supply	Daily hours worked for all
Monthly labor supply	Monthly hours worked for all
Yearly labor supply	Yearly hours worked for all
Daily hours worked	Daily hours worked for labor participants
Monthly hours worked	Monthly hours worked for labor participants
Yearly hours worked	Yearly hours worked for labor participants
Annualized growth rate of housing prices	Annualized growth rate of housing prices
Regressor of interests	
Housing size	House size
D = I [Housing size ≤ 90]	1 if housing size is smaller than or equal to 90; and 0 otherwise
Predetermined variables of the household interviewee	
Education	Years of schooling
Gender	1 if male, 0 otherwise
Age	Age of the interviewee
Marriage status	1 if married before purchasing the house, 0 otherwise
Ethnicity	1 if the majority, 0 otherwise
Number of siblings	Interviewee's number of siblings
Father education	The interviewee's father's years of schooling
Mother education	The interviewee's mother's years of schooling
Father party member	1 if the interviewee's father is a party member
Mother party member	1 if the interviewee's mother is a party member
Housing cost	Housing price per m^2 when the house was purchased
Housing age	Years since the house was purchased
Household asset and debt	
Housasset	Self-reported housing value (unit: 10 thousand)
Non-housasset	Self-reported non-housing value (unit: 10 thousand)
Housdebt	Housing debt (unit: 10 thousand)
Non-housdebt	Non-housing debt (unit: 10 thousand)
Debt ratio	Total debt divided by total assets

around h^* as a robustness check (for the same exercise, see, e.g., Carneiro et al., 2015).

As a robustness check, we also calculate $\hat{\beta}$ using a parametric approach. We compute clustered standard errors at the assignment variable level, which allows us to capture some random sampling errors and obtain conservative statistical inference (see Lee and Card, 2008)[7].

2.4. Estimation particulars

In this subsection, we provide some particulars of our RD estimations—specifically, the estimation sample and construction of the assignment variable. We also discuss estimation issues—specifically, bunching at multiples of 10.

2.4.1. Estimation sample

After the *National Article Six* policy was issued in May 2006, households could endogenously choose whether to purchase a house with a floor area larger or smaller than 90 m^2. This full manipulation would then invalidate our RD framework, i.e., $E[Y_{i0}|c_i = c]$ is discontinuous in c at c_0. To address this identification concern, we focus on the group of households whose current houses were purchased before 2006 (or six months before the first housing cooling measures with a cutoff house size) in the analyses. In the next section, we will provide anecdotal and quantitative evidence to support the assumption that there is no full manipulation of house size by households who purchased houses before 2006 at the cutoff point.

2.4.2. Assignment variable—house size

The assignment variable in our RD estimation is house size. To simplify the interpretation, we normalize the assignment variable with respect to the cutoff point, i.e., $\tilde{c} = c - 90$. Most of the households in the data report floor area as an integer, with sporadic observations at the decimal point (i.e., approximately 7% of the full sample), which could generate a rounding error problem in the assignment variable (e.g., Barreca et al., 2011; Barreca et al., 2016). To assess whether rounding in the assignment variable biases our estimates, we apply a method developed by Dong (2015), which corrects for rounding in the assignment variable in the RD framework.

2.4.3. Bunching at multiples of 10

Our assignment variable, the reported house size, has a clear pattern of bunching at multiples of 10. As the cutoff point of the assignment is 90, there may be a concern that the estimated treatment effect could be caused by the bunching rather than the real effect of housing wealth. To address this concern, we include a dummy variable indicating the multiples of 10 in the analyses; hence, the effects at other multiples of 10 are used to difference out the bunching effect at the cutoff point of 90, which then isolates the effect of housing wealth.

[7] When the small number of clusters is less than 50, we use the Wild cluster-bootstrap percentile-t procedure developed by Cameron et al. (2008) to address the issue of a small number of clusters.

Table 1b
Summary statistics.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

Variables	Analysis sample			Treatment group			Control group		
	# obs	Mean	S.D.	# obs	Mean	S.D.	# obs	Mean	S.D.
Outcome variables									
Daily labor supply	61,849	3.85	4.58	39,951	3.53	4.54	21,898	4.45	4.58
Monthly labor supply	61,789	86.85	105.97	39,916	79.75	105.23	21,873	99.82	106.10
Yearly labor supply	61,649	964.66	1213.39	39,818	882.47	1199.06	21,831	1114.56	1225.01
Daily hours worked	27,713	8.60	2.43	16,281	8.65	2.49	11,432	8.53	2.33
Monthly hours worked	27,594	194.49	64.92	16,205	196.44	65.98	11,389	191.70	63.28
Yearly hours worked	27,267	2181.03	822.08	15,994	2196.96	831.49	11,273	2158.42	808.03
Annualized growth rate of housing prices	28,950	0.16	0.11	18,535	0.17	0.11	10,415	0.14	0.09
Annual percent change in house value	28,405	0.97	1.66	18,129	1.22	1.88	10,276	0.53	1.03
Regressor of interests									
Housing size	31,587	87.66	56.67	20,684	63.69	16.82	10,903	133.14	74.89
D = I [Housing size ≤ 90]	31,587	0.66	0.48	20,684	1.00	0.00	10,903	0.00	0.00
Predetermined variables of the household interviewee									
Education	31,119	11.01	4.02	20,385	10.70	3.87	10,734	11.60	4.24
Gender	31,145	0.48	0.50	20,401	0.46	0.50	10,744	0.50	0.50
Age	31,145	54.94	14.50	20,401	56.54	14.37	10,744	51.88	14.25
Marriage status	31,128	0.57	0.50	20,390	0.57	0.50	10,738	0.58	0.49
Ethnicity	22,429	0.96	0.19	14,630	0.97	0.18	7799	0.96	0.21
Number of siblings	27,632	2.51	1.90	18,045	2.57	1.91	9587	2.41	1.87
Father education	24,800	6.30	4.91	16,078	6.05	4.87	8722	6.76	4.96
Mother education	25,409	4.50	4.78	16,488	4.23	4.70	8921	5.01	4.89
Father party member	24,276	0.25	0.43	15,905	0.24	0.42	8371	0.28	0.45
Mother party member	24,057	0.08	0.27	15,748	0.07	0.25	8309	0.09	0.29
Housing cost	29,675	1390.49	5226.67	19,093	1248.84	6352.86	10,582	1646.08	1920.61
Housing age	31,587	16.22	7.78	20,684	17.48	8.04	10,903	13.84	6.61
Household asset and debt									
Housasset	31,587	122.16	205.09	20,684	103.60	161.30	10,903	157.37	265.72
Non-housasset	31,587	48.56	1009.26	20,684	30.30	717.43	10,903	83.20	1404.58
Housdebt	31,569	2.23	12.78	20,679	1.63	9.83	10,890	3.37	16.98
Non-housdebt	31,569	1.91	68.88	20,679	0.70	9.41	10,890	4.21	116.53
Debt ratio	31,548	0.15	8.90	20,661	0.17	10.87	10,887	0.11	2.27

3. Empirical findings

3.1. Threats to identification

The key identifying assumption of our RD estimation is that $E[Y_{i0}|c_i = c]$ is continuous in c at c_0. As discussed in Lee (2008), this means that our assignment variable (i.e., house size) cannot be fully manipulated. In this subsection, we discuss and examine two potential threats to our identifying assumption: the manipulation of house size and sample selection due to endogenous house moving.

3.1.1. Manipulation of house size

3.1.1.1. Anecdotal evidence. Since the country's reform and opening in 1978, Chinese governments have taken an experimental approach; that is, reforms were conducted without a clear blueprint, and policies were launched contingent on past progress and current situations. The introduction of *National Article Six* in May 2006 was a response to the unexpected overheated housing market of the mid-2000s and was widely considered "unexpected." For example, after the introduction of *National Article Six*, Yicai Daily (the leading financial media outlet in China) reported the following:

> "Just two months ago, Shen Jianzhong (the Head of the Real Estate Division at the Ministry of Housing and Urban-Rural Development) at a meeting gave an affirmative appraisal of the *National Article Eight* (government housing policies) launched last year, saying that 'the basic principle is correct, and it captures the principle to maintain pressures.' Because of this conclusion, the industry believed that new control policies would not be introduced at this moment. However, after only 60 days, a new round of regulations started."[8]

Given this unexpected change in housing policies, it was difficult for people to precisely select a house size of 90 m^2. To attenuate any possibility of an expectation effect, we focus on a group of households that purchased houses before 2006, or six months before *National Article Six* was introduced in China. This further helps in excluding the possible endogenous selection of house size at the cutoff point (i.e., 90 m^2).

3.1.1.2. Density check. To lend further support to our identifying assumption, we provide a quantitative analysis of density distribution, as suggested by Lee and Lemieux (2010). Specifically, if there were full manipulation of house size to obtain a lower down payment ratio and/or a lower property deed tax, the distribution of household characteristics on the two sides of the cutoff points would be different. A mixture of discontinuities in household characteristics would further imply that the density distribution of the assignment variable is discontinuous at the cutoff point. To conduct this exercise, we first use the 2000 Census and the 2005 Mini Census, in which all houses were purchased before our focal housing policies. The histogram of the density of the assignment variable is presented in Fig. 2a. There is no visible discontinuity in the density distribution of the assignment variable at the cutoff point. Also, we conduct a formal analysis of

[8] See http://bj.house.sina.com.cn/news/2006-06-29/1006134898.html (accessed on March 27, 2019).

density discontinuity, following McCrary (2008). Estimation results are reported in column 1 of Table A2 in the Appendix. We do not find any statistically and economically significant discontinuity in the density of the assignment variable at the cutoff point.

Next, we re-conduct these quantitative analyses of the density distribution using our estimation sample (i.e., households that purchased houses before 2006 in the combined CHFS and CFPS data). Results are reported in Fig. 2b and in column 1 of Table A3 in the Appendix, respectively. Consistently, we do not find any significant jumps at the cutoff in our estimation sample, which helps confirm the no-full-manipulation of house purchasing.

To further shed light on our identifying assumption, we use the panel feature of our data to illustrate the dynamics of the density distribution. Specifically, we first separate the censuses and the combined CHFS and CFPS (unconditional on the purchase year) into six waves; that is, the 2000 wave from the 2000 Census data, the 2005 wave from the 2005 Mini Census data, and the 2010–2011 wave, the 2012–2013 wave, the 2014–2015 wave, and the 2016–2017 wave from the combined CHFS and CFPS data. We draw six density distributions in Appendix Fig. A1a–f. No density peaks or discontinuities at 90 were found in 2000 and 2005. But from the 2010s, the peak of the density distribution gradually moved to 90, despite the lack of significant discontinuities being found by the McCrary test. Second, we investigate the changes in density distributions over purchase years. Specifically, we divide the data into four groups: houses purchased before 2000 (from the 2000 Census and the combined CHFS and CFPS data), houses purchased between 2001 and 2005 (from the 2005 Mini Census and the combined CHFS and CFPS data), houses purchased between 2006 and 2010 (from the combined CHFS and CFPS data), and houses purchased after 2010 (from the combined CHFS and CFPS data). Density distribution figures are reported in Appendix Fig. A2a–d. We find that before 2006 (the implementation of our focal housing policies), the policy cutoff point (i.e., 90 m^2) was never a density peak and had no statistical discontinuity in density. After 2006, 90 m^2 became a peak in the density distribution, despite the lack of significant discontinuities being identified by the McCrary test. Combined, these dynamic changes in density distributions further imply no full manipulation of housing size in our analysis sample (i.e., houses purchased before 2006).

3.1.1.3. Composition check. A second quantitative check on the manipulation of assignment variable suggested by Lee and Lemieux (2010) is to directly examine whether households' predetermined socioeconomic characteristics are smooth at the cutoff point. If there were full manipulation in our research setting, we would find discontinuities in these predetermined characteristics at the cutoff point. To this end, we first go through 12 predetermined variables that can be identified in the 2000 Census and the 2005 Mini Census: *Education*, *Gender*, *Age*, *Marriage status*, *Number of siblings*, *Health*, *Ethnicity*, *Welfare house*, *Housing cost*, *Housing age*, *Housing floors*, and *Housing structure*. Fig. 3a–l show that none of the predetermined variables exhibit discontinuities at the cutoff point. The regression results are reported in columns 2–13 of Table A2 in the Appendix, further confirming that there are no statistically and economically significant discontinuities.

We also use our estimation sample to examine whether there are differences in the composition of households across the cutoff point. Specifically, we examine 12 predetermined variables that can be identified in the CHFS and CFPS data: *Education*, *Gender*, *Age*, *Marriage status*, *Ethnicity*, *Number of siblings*, *Father education*, *Mother education*, *Father party membership*, *Mother party membership*, *Housing cost*, and *Housing age*. These estimation results are reported in Fig. 4a–l and in columns 2–13 of Table A3 in the Appendix. Consistently, none of the predetermined variables exhibit discontinuities at the cutoff point in our estimation sample.

In summary, the exercises in this subsection suggest that there is no full manipulation of the assignment variable in our RD framework, lending support to our estimation strategy.

3.1.2. House moving

Our regression analysis uses information on houses purchased before 2006 from surveys in the 2010s (i.e., CHFS 2011, 2013, 2015, and 2017 and CFPS 2010, 2012, 2014, and 2016). Implicitly, this data structure restricts our analysis to a sample of households that did not move houses from 2006 through the survey years. If our focal housing policies changed households' house moving behavior at the cutoff point (i.e., 90 m^2), the groups just below and just above the cutoff would be different in our analysis sample even if they were similar at the time, before 2006, that they purchased a house. Ideally, if we had the panel data dating back to the pre-2006 period, we could then track the movement of households from 2006, and directly examine whether our focal housing policy created differential incentives for households to move houses on the two sides of the cutoff point. Unfortunately, our household survey data (i.e., CHFS and CFPS) begin in the 2010s. Alternatively, we provide several threads of indirect evidence to shed light on the potential estimation bias from endogenous house moving.

First, our anecdotal evidence and density and composition checks using the 2000 Census and the 2005 Mini Census both indicate no full manipulation of house size around the policy cutoff point at the time of house purchases before 2006. Moreover, our quantitative analysis using our regression sample (i.e., the combined CHFS and CFPS data) also show no discontinuity at the policy cutoff point in the 2010s. While the smoothness at the cutoff in the 2010s data could be caused by the exact opposite effects from the manipulation of house purchasing and endogenous house moving, the combined findings of no discontinuity in density and the predetermined covariates in both the pre-reform data (i.e., the 2000 Census and 2005 Mini Census) and our main regression data (i.e., the CHFS and CFPS in the 2010s) suggest that there is no significant sample selection bias for house moving.

Second, we provide information on houses in our main regression data (such as location and year of purchase) and examine whether houses on the two sides of the policy cutoff differ along these characteristics. Specifically, in Table A4 in the Appendix, we examine the locations of houses across the cutoff. We find that our RD treatment and control groups are balanced in their house location, that is, city development (proxied by city GDP per capita) and city size (proxied by city population, an indicator for mega cities and an indicator for first-tier cities[9]). Moreover, in Fig. 4l and column 13 of Table A3, we find that the years when the current house was purchased are balanced at the policy cutoff point. We also find in Fig. 4k and column 12 of Table A3 that the costs of purchasing the current house are similar on the two sides of the cutoff point. To further attenuate the potential estimation biases from the purchase dates and locations of houses, we include purchase-year fixed effects and city fixed effects in all specifications.

Third, our main analyses draw on the CHFS and CFPS surveys, in which the CHFS tracked 72% of households over the survey waves, while the CFPS contains panel data on households from 2010 to 2016. In China, in the 2010s, housing prices also experienced rapid growth, echoing the experience of the housing boom in the late 2000s. To shed light on the house moving issue, we examine whether there is

[9] Mega cities are defined as cities with at least 3 million population, and first-tier cities include Beijing, Shanghai, Guangzhou and Shenzhen.

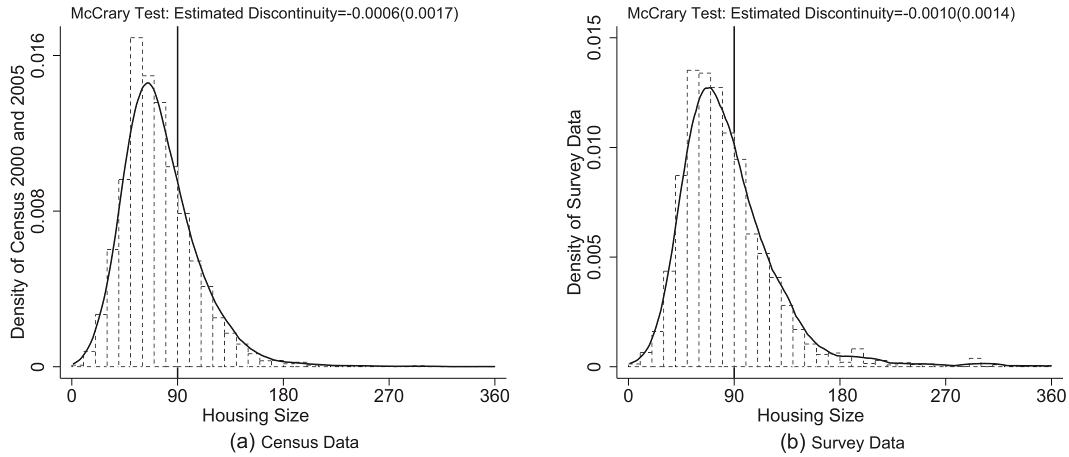

Fig. 2. Distribution of housing size. Notes: 1. Panel a (left) is the distribution of housing size in census data (2000 Census and 2005 Mini Census) and panel b (right) is the distribution of houses purchased before 2006 in survey data (CFPS and CHFS); 2. The solid line is the kernel density distribution, while kernel = triangular.
Data source: urban houses in the 2000 Census and 2005 Mini Census; urban houses purchased before 2006 in CHFS and CFPS.

a discontinuity in house moving at the cutoff in the CHFS and CFPS data in the 2010s. Observing no differential degrees of house moving at the cutoff in the 2010s may indicate that the issue of endogenous house moving from 2006 to the survey years is not severe in our research setting. The estimation results are presented in Appendix Fig. A3. Clearly, we do not observe a discontinuity in house moving in our main regression data at the cutoff point.

Combined, these pieces of indirect evidence suggest that there may not be significant sample selection bias due to endogenous house moving in our research setting.

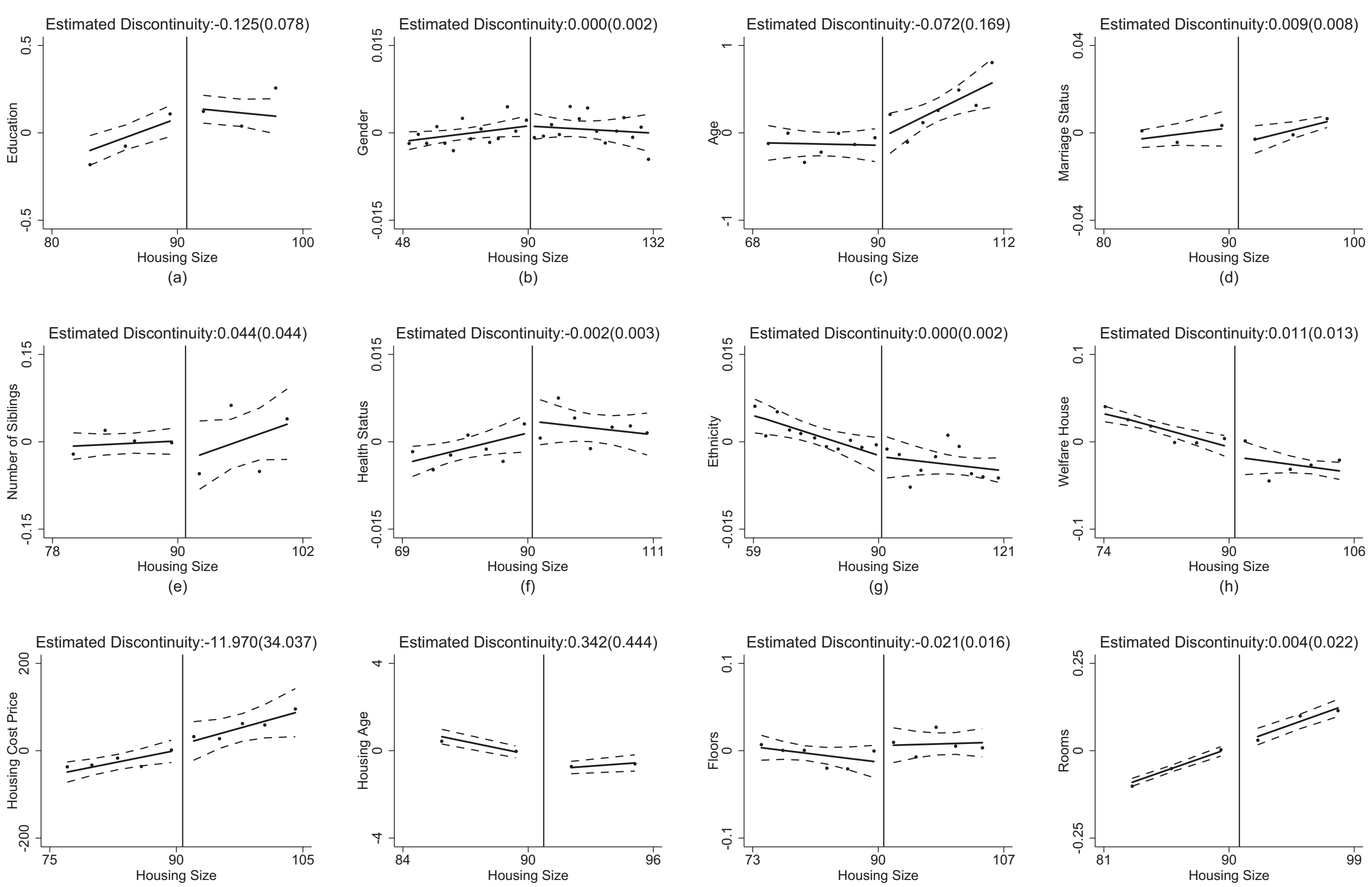

Fig. 3. Predetermined variables from the census data. Notes: 1. Circles represent conditional mean values of the respective variable for each bin with a size of 3 m^2 after controlling for year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten; 2. Solid lines are the fitted values from the local linear regression with the optimal bandwidth calculated using Imbens and Kalyanaraman's (2012) approach, and the dashed lines are the 95% confidence intervals; 3. The vertical line is the cutoff point (i.e., 90) in the assignment variable.
Data source: urban houses in the 2000 Census and 2005 Mini Census.

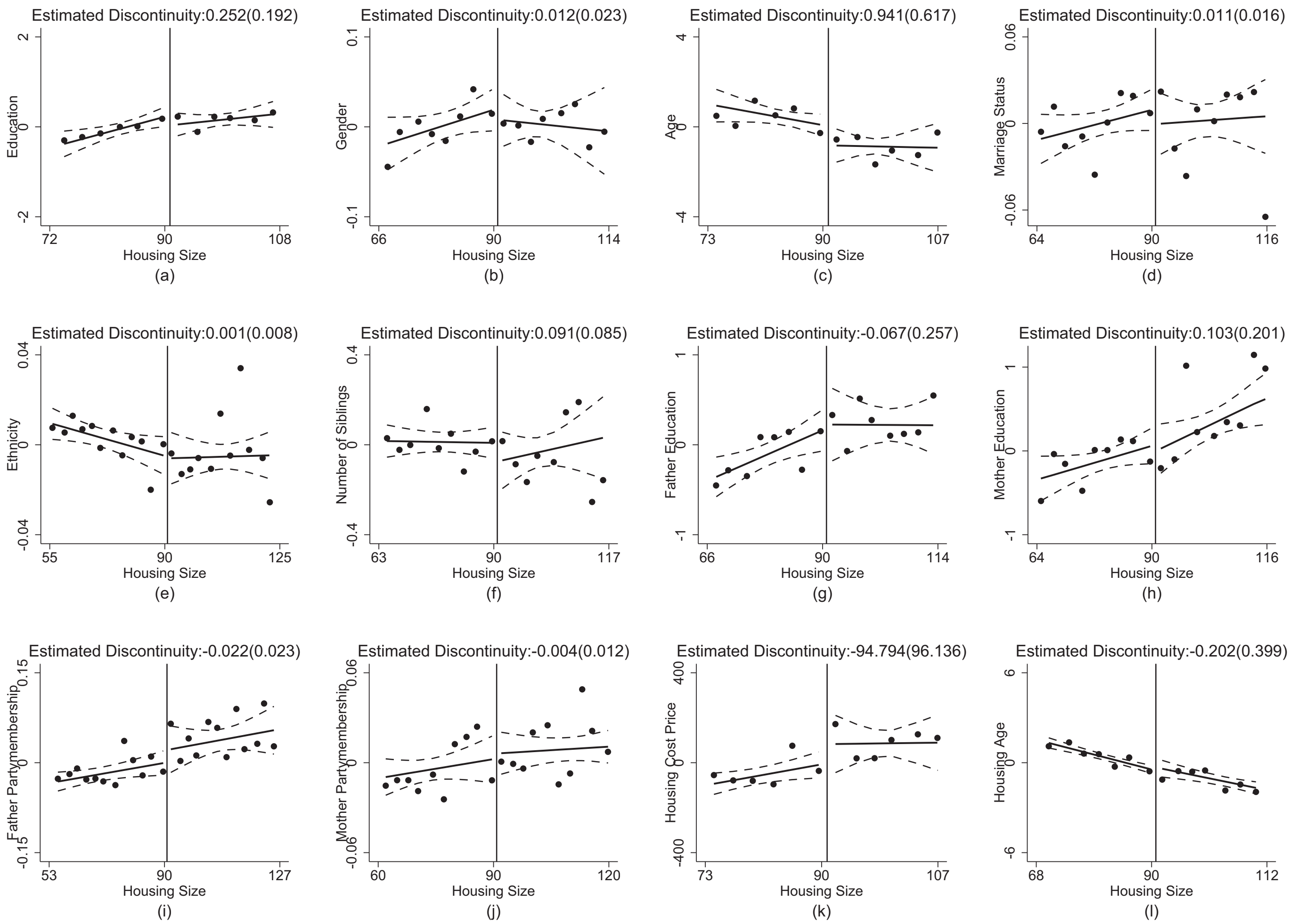

Fig. 4. Predetermined variables from survey data. Notes: 1. Circles represent conditional mean values of the respective variable for each bin with a size of 3 m^2 after controlling for year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten; 2. Solid lines are the fitted values from the local linear regression with the optimal bandwidth calculated using Imbens and Kalyanaram's (2012) approach, and the dashed lines are the 95% confidence intervals; 3.The vertical line is the cutoff point (i.e., 90) in the assignment variable.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

3.2. Main results

Panels a–c in Fig. 5 plot the relation between the size of the housing unit (our assignment variable) and each of our three measures of labor supply. Circles represent the conditional mean values of labor supply for each bin with a size of 3 m^2,[10] lines are the fitted values from the local linear regression with optimal bandwidth calculated using Imbens and Kalyanaraman's (2012) approach, the dashed lines capture the 95% confidence intervals, and the vertical line denotes the cutoff point for eligibility for our focal housing policies.[11]

We consistently find a clear decline in hours worked at the cutoff in all the figures, indicating that households purchasing housing units with floor areas just below 90 m^2 work less than those with floor areas just above 90 m^2.

Table 2 reports the nonparametric RD estimates, with daily labor supply, monthly labor supply, and yearly labor supply in columns 1–3, respectively. All regressions control for a linear term of the normalized assignment variable $\tilde{c}$, an interaction between $\tilde{c}$ and treatment status D_i, a dummy for multiples of 10, year fixed effects, purchase-year fixed effects, and city fixed effects, the coefficients of which are suppressed to save space. We find consistent evidence, as conveyed in Fig. 5a–c; that is, households with housing units smaller than 90 m^2 supply fewer hours worked than their counterparts with larger housing units.[12]

Given the preferential government policies given to housing units with floor areas less than or equal to 90 m^2 (i.e., lower down payment ratios and lower property deed taxes), these results suggest that this increase in housing wealth reduces labor supply. To further corroborate this argument, we investigate the differential growth rate of housing prices on the two sides of the cutoff point of 90. Specifically, the data contain household-reported housing prices at the time of

[10] Regarding the selection of bin size, Imbens and Lemieux (2008) and Lee and Lemieux (2010) suggest that there is a trade-off between the precision in calculating average outcome values and the proximity to the cutoff point. To this end, we use a bin size of 3 m^2. For robustness, we report the figures using bin sizes of 1–5 m^2 in Appendix Figs. A4a–A4c for each of the three measures of labor supply.

[11] We control for year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten.

[12] Note that we calculate the optimal bandwidth for the three labor supply measures separately, and hence, the observations differ across columns. In a robustness check reported in Appendix Table A5, we use the same bandwidth (that is, the smallest among the three optimals) and the same observations. The results remain nearly unchanged, suggesting that the change in observations does not bias our findings.

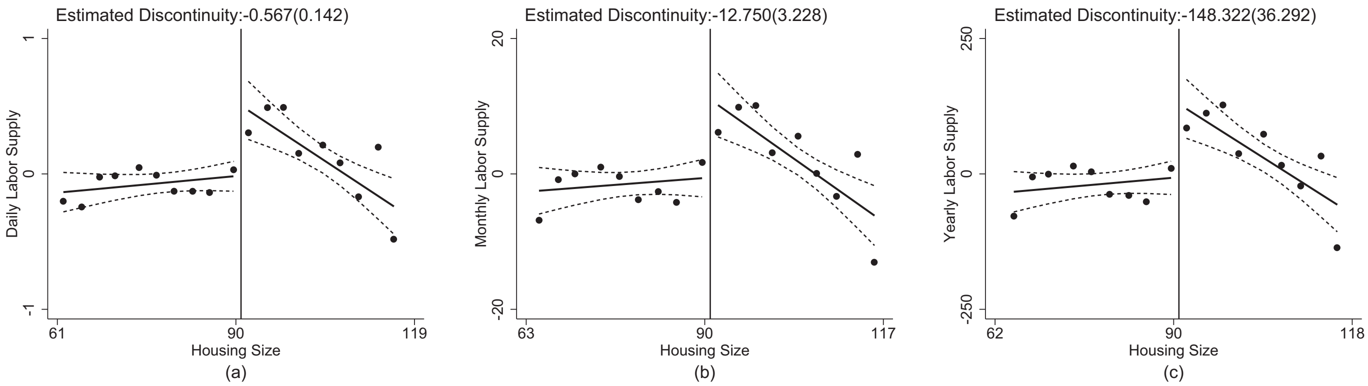

Fig. 5. Labor supply. Notes: 1. Circles represent conditional mean values of the respective variable for each bin with a size of 3 m^2 after controlling for year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten; 2. Solid lines are the fitted values from the local linear regression with the optimal bandwidth calculated using Imbens and Kalyanaraman's (2012) approach, and the dashed lines are the 95% confidence intervals; 3. The vertical line is the cutoff point (i.e., 90) in the assignment variable. Data source: urban houses purchased before 2006 in CHFS and CFPS.

Table 2
Main results.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

	(1)	(2)	(3)	(4)	(5)
	Daily labor supply	Monthly labor supply	Yearly labor supply	Annualized growth rate of housing prices	Annual percent change in house value
D = I [Housing size ≤ 90]	−0.567***	−12.750***	−148.322***	0.013**	0.164***
	(0.142)	(3.228)	(36.292)	(0.005)	(0.057)
Mean value of the control group	4.511	101.236	1130.616	0.139	0.576
Optimal bandwidth	29	27	28	20	27
No. of observations	33,599	32,269	32,910	12,694	15,699

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

purchase and in the current period; hence, we are able to back out the average annual growth rate of housing prices.[13] Fig. 6 reports the relation between house size and the annualized growth rate of housing prices. There is a clear jump in the annualized growth rate at the cutoff point, indicating that housing prices with floor areas smaller than 90 m^2 increased faster than those with floor areas larger than 90 m^2 in the past decade.[14] The estimation results are shown in column 4 of Table 2 and further confirm a positive and statistically significant coefficient of the treatment variable. Specifically, over the past decade, prices for homes with floor areas smaller than 90 m^2 increased 1.3% faster than those with floor areas larger than 90 m^2. Given that the average housing value in the control group (i.e., households with floor areas between 91 and 100 m^2) over the sample period (2010–2017) is RMB 899,563.4 (USD 132,869.1), this number translates to an annual housing wealth increase of RMB 11,694.3 (USD 1727.3).

The difference in labor supply on the two sides of the 90 square-meter cutoff could be explained by a reduction in labor supply by the households on the left side of the cutoff, which experienced gains in housing wealth, or an increase in labor supply by households on the right side, which experienced losses in housing wealth. To shed light on these possibilities, we compare labor supply in our main analysis (i.e., housing purchased before 2006 in the combined CHFS and CFPS data) with that in the 2000 and 2005 censuses (before the focal housing policies). The results are reported in Appendix Fig. A5. The

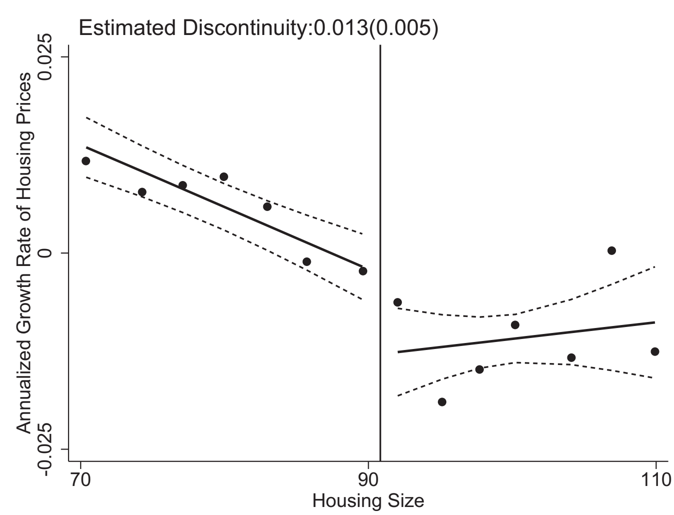

Fig. 6. Annualized growth rate of housing prices. Notes: 1. Circles represent conditional mean values of the respective variable for each bin with a size of 3 m^2 after controlling for year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten; 2. Solid lines are the fitted values from the local linear regression with the optimal bandwidth calculated using Imbens and Kalyanaraman's (2012) approach, and the dashed lines are the 95% confidence intervals; 3. The vertical line is the cutoff point (i.e., 90) in the assignment variable.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

[13] Specifically, let hp_0 be the housing price at purchase and hp_t be the housing price in the current period t. Then, the average annual growth rate $r \simeq \left(\frac{hp_t}{hp_0}\right)^{\frac{1}{t}} - 1$.

[14] There is a negative relationship between house size and the annualized growth rate of housing prices from 50 to 90 m^2, which stabilizes thereafter. In our local linear RD estimation, we include a linear term for house size and an interaction between house size and the post-90-square-meter indicator, which controls for these differential local linear effects of house size on the growth rate of housing prices on the two sides of the cutoff threshold.

blue triangles represent the numbers from the censuses, whereas the black dots represent the CHFS and CFPS data. Over time, all labor supply falls, regardless of housing size. This is consistent with our story because the prices for all houses increased significantly during this period. Interestingly, the reduction in labor supply is larger on the left side of the 90 square-meter cutoff than on the right side of it. These results suggest that the estimated labor supply effects may largely come from gains (rather than losses) in housing wealth.

Note that our two focal housing policies, a reduction of the down payment ratio and a decrease in property deed tax, may have differential effects on price growth for houses smaller than 90 m^2. Specifically, the decrease in the property deed tax may lead to a one-off increase in housing prices because housing costs are decreased, whereas the reduction in the down payment may relax financing and borrowing constraints for home buyers, which in turn increases demand and affects housing prices. In China, the second policy is expected to have a greater influence than the first on housing prices. On the one hand, the 2% reduction in the property deed tax is small relative to the total home price and is marginal when compared with housing price appreciation (e.g., an average of 12.68% annual growth between 2002 and 2010 in 35 major Chinese cities). On the other hand, given the high housing prices, the down payment constitutes a significant financial constraint on home buyers. Specifically, home values are 13 times larger, on average, than total household annual incomes in our data. To further confirm the effect of the down payment policy on housing prices, we divide the sample in two, based on the median ratio of home value over total household annual income. The estimation results are reported in Appendix Table A6, columns 1 and 2. We find a significant and positive effect in the sample where the ratio of home value over total household annual income is above the median but do not obtain this result in the sample where the ratio is below the median. As the financial and borrowing constraints are more binding for home buyers in the former sample than those in the latter sample, these results indicate that the reduced down payment has a significant policy effect.

These results indicate that unexpected gains in housing wealth significantly reduce individuals' daily, monthly and yearly hours worked in our sample. Since housing wealth is the dominant component of Chinese households' asset portfolios, our findings shed light on the literature examining the effect of wealth on household labor supply. Specifically, the life-cycle theory predicts that unexpected wealth gains can boost the consumption of both goods and leisure, leading to reduced labor supply (Poterba, 2000; Jappelli and Pistaferri, 2010). Empirical studies confirm that labor supply decisions are responsive to changes in financial wealth and housing wealth. For example, Imbens et al. (2001) find that lottery winnings significantly reduce households' labor earnings and marginal propensity to earn; Zhao and Burge (2017) find a negative relationship between the housing wealth gains and labor supply of the elderly. Our findings are in line with these studies.

3.3. Economic magnitude and comparison with the literature

To gauge the economic magnitude of our estimates, we report the control mean, i.e., the mean values of labor supply for the households with floor areas between 91 and 100 m^2 in Table 2. For the variable Daily Labor Supply, we find that the control group worked, on average, 4.51 h in the last week. The preferential housing policies cause households with floor areas of 90 m^2 to work 0.567 fewer hours or 12.57% less than the control group in the past week. As a point of reference, the average hourly wage during our sample period (2010–2017) was approximately RMB 24.7 (USD 3.65) in our sample; hence, a reduction of 0.567 in daily hours worked is equivalent to a wage loss of RMB 14.01 (USD 2.06) per day. Similarly, our estimates suggest that the preferential housing policies cause households with floor areas of 90 m^2 to work 12.59% less than the control group in the past month and 13.12% less in the past year, which are equivalent to wage losses of RMB 314.94 (USD 46.52) per month and RMB 3022.56 (USD 446.45) per year[15], respectively.

Note that the RD estimator in Eq. (2) essentially represents the ITT effect at the cutoff point; that is, policy-affected households work less than those in the control group. To calculate the effect of housing wealth on labor supply (or the average treatment effect at the cutoff point), we further conduct an analysis using the treatment status (D_i) as an instrument for the growth in housing prices in the regression of labor supply on housing prices.[16] These estimation results are reported in columns 1–3 of Table 3 and yield negative and statistically significant estimates. These results further confirm that compared with the policy-unaffected households just above the policy threshold, policy-affected households just below the threshold experienced faster growth in housing prices over the past decade, which leads to less labor supply. In terms of economic magnitude, our RD estimates suggest that experiencing a one-percent increase in the annual growth rate of the price of housing leads to a 0.47 decline in daily hours worked, a 9.93 decline in monthly hours worked, and a 118.09 decline in hours worked annually. These numbers correspond to approximately 10% of the mean hours worked for the control group.

There are relatively few causal analyses of housing wealth on labor supply. Disney and Gathergood (2018), using household panel data from the UK, find that a 10% increase in housing prices reduces annual hours worked by 27 h for married/cohabiting young female homeowners, approximately 1.8% of the sample average non-zero annual hours for this group. Also using UK panel data, Henley (2004) finds that a 10% decline in housing prices is associated with an 18-minute increase in weekly hours worked, with the mean value being 39.7 h for males. Our estimated magnitude is larger than those reported in the literature. One possible explanation for this discrepancy is that the size of our housing shock was larger and more consistent than those investigated in the literature. Specifically, the housing price in our sample continued to grow at an average annual rate of 16%, but it fluctuated in the study period considered by Henley (2004), exhibiting ups and downs and growing at low rate. Another possible reason is differences in specification; that is, we investigate the effect of individuals' housing prices on their labor supply, while the literature studies the effect of local average housing prices. Such aggregation might mute estimated magnitude; for example, given the fluctuations in housing prices in Henley (2004), the negative and positive effects may cancel out at the aggregate level.

It is also possible that the nonrandom selection of housing purchases (in particular with respect to house size) contaminates the effect of housing wealth on labor supply. To shed light on this possibility, we use data on housing purchases after the housing policies were implemented, whereby households could capitalize on the differential treatments in terms of house size by intentionally selecting housing that is larger or smaller than 90 m^2. This full manipulation would then imply that households on the two sides of the cutoff point could differ, which in turn would bias the estimates.[17] We re-estimate the RD analyses using the sample of housing purchases after 2006 when the preferential treatment on the down payment ratio was in

[15] The average monthly wage during 2010 to 2017 is approximately RMB 3164.38 or USD 467.39, and the average annual wage income is approximately RMB 23,040.14 (USD 3403.12).

[16] The instrumental variable (IV) estimation is conducted using the individual data (via STATA package, ivreg2). However, the results of regressing housing price growth on D_i reported in column 4 of Table 2 are obtained using the household level data. For consistency and to facilitate comparison, we also report the first stage of our IV estimation using the individual data in Appendix Table A7. The effect of the housing policies on housing price growth in the individual data is similar to that in the household data.

[17] The density distribution of house size using the post-2006 sample is reported in Appendix Fig. A6. It clearly shows that the distribution peaks at 90 m^2, despite the statistically insignificant discontinuity reported by the McCrary test.

Table 3
IV estimation.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

	(1)	(2)	(3)	(4)	(5)
	Daily labor supply	Monthly labor supply	Yearly labor supply	Annual labor income	Labor participation
Annualized growth rate of housing prices	−47.204**	−992.930**	−11,808.777**		
	(22.088)	(481.763)	(5743.232)		
Annual percent change in house value				−18,266.957**	−0.443**
				(7956.696)	(0.202)
Mean value of the control group	4.565	102.442	1145.714	23,649.774	0.529
Optimal bandwidth	20	20	20	27	20
No. of observations	25,135	25,103	25,025	33,033	24,641

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. Columns (1)–(3) use the indicator D=I [Housing size≤90] as the instrument for the annualized growth rate of housing prices, while columns (4)–(5) use the indicator D=I [Housing size≤90] as the instrument for annual percent change in house value, and all regressions use 2SLS estimation; 3. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 4. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

place and report the estimation results in Appendix Table A8. Interestingly, we find that all three estimates (i.e., for the three measures of labor supply) are positive, which is in sharp contrast to the unbiased estimates (that are negative) in Table 2 and suggests significant biases from the nonrandom selection of house size.[18]

We next compare our results with the literature studying the effect of unearned income on labor supply outcomes, including the lottery studies by Imbens et al. (2001) and Cesarini et al. (2017), and the work by Gelber et al. (2017) and Giupponi (2019) on labor supply responses to changes in social security wealth. To do so, we estimate the effect of annual growth in housing wealth on labor earnings using our focal housing policies as the instrument. The estimation results, reported in column 4 of Table 3, reveal a significantly negative effect of a change in housing wealth on labor earnings. Specifically, a one-percent increase in housing wealth per year reduces annual labor earnings by 182.67 RMB (USD 26.98), corresponding to 0.79% of the average labor income in the control group. To facilitate comparison with the literature, we obtain the effect size per USD 10,000 gain in unearned wealth. The average housing wealth in the control group over our sample period (2010–2017) was RMB 899,563.4 (USD 132,869.1). Hence, our coefficient implies that a gain of USD 10,000 in housing wealth reduces annual labor earnings by USD 203.06, corresponding to 5.97% of the control mean.

Our estimated impact of housing wealth is comparable to the wealth effect form social security benefits but larger than that from an one-time lottery gain. Specifically, Gelber et al. (2017) show that a USD 10,000 increase in lifetime discounted Old Age and Survivors Insurance (OASI) benefits causes annual earnings in old age to decrease by USD 4600 to 6100, which corresponds to approximately 5.3%-7.1% of the sample mean. Cesarini et al. (2017) show that lottery winners reduce their annual earnings by approximately 1% of the prize amount per year, implying that a USD 10,000 yearly prize reduces labor earnings by approximately USD 100, which corresponds to 0.39% of the annual average earnings in their sample. One possible explanation for these findings is that winning the lottery is a one-time income shock, whereas housing wealth and social security benefits are lifetime income changes. Meanwhile, the underlying variation in terms of lifetime wealth is bigger in our paper compared to the lottery gains used in Cesarini et al. (2017). Specifically, less than 9% of the 247,275 lottery players in Cesarini et al. (2017) won more than USD 1400, [19] but the average change of housing wealth in our sample is around USD 283,260, corresponding to USD 17,464 per owning year.

In a closely related work, Imbens et al. (2001) analyze the effects of Megabucks lottery in Massachusetts in the mid-1980s on economic behavior. Unlike the one-time prize, the winner sample of this study are paid out in yearly installments over 20 years, with the mean yearly prize equal to USD 55,200 (or USD 1,104,000 in total). They estimate the income elasticity to the lottery payments to be approximately −0.11 over the six post-lottery years, implying that an annual prize of USD 10,000 decreases labor earnings by USD 1100, or 6.37% of the average Social Security earnings for non-winners for the year of winning and the six subsequent years. This estimate is comparable to ours and to that by Gelber et al. (2017). Giupponi (2019) estimates the long-run effect of welfare transfers from Italian survivor insurance scheme and also find a large income effect. Specifically, widowers facing a benefit loss of 2000 Euro per year due to the benefit reform increase their average annual taxable income by approximately the same amount. This estimate is larger than ours, and one potential explanation is that individuals' responses to benefit losses are larger than responses to wealth gains.

To further gauge the economic significance, we link our estimates to the employment dynamics in China following the housing boom during our sample period. To this end, we first estimate the effect of the annual growth in housing wealth on labor participation using our focal housing policies as instruments. The results, reported in column 5 of Table 3, show that a one-percent increase in housing wealth per year reduces yearly labor participation by 0.0044 percentage points. From 2010 to 2017, the percent change in average housing wealth is 17.8% per year, generating a 0.08 percentage-point reduction in yearly employment. During the same period, the percent change in the number of labor participants in China is −1.45% per year[20] Hence, our estimates can explain approximately 5.4% of the macro employment changes during the period. Moreover, given the biennial structure of our survey data, we also calculate the percent change in employment induced by the growth of housing wealth and the percent change in employment from the China Statistical Yearbook on a biennial basis. We find a significant correlation of 0.274 for the two series of employment dynamics.

[18] One potential explanation is that households that purchased smaller houses after 2006 were those with financial constraints, and hence even with a lower down payment ratio and property deed tax, they would not reduce their labor supply or still need to work more to repay their mortgage. To corroborate this argument, we test whether, after 2006, households purchasing smaller houses were more credit constrained (proxied by the ratio of household debt to assets) than those buying larger houses. The estimation results are reported in Appendix Table A9. The positive coefficient indicates that households with house size just below 90 m^2 had higher debt-to-asset ratios than those just above 90 m^2, lending support to our argument.

[19] The dollar amount is converted from SEK using the January 2010 exchange rate (7.153 SEK per USD).

[20] The percent change in the number of labor participants per year is calculated using data from the 2011–2018 China Statistical Yearbook.

3.4. Robustness checks

In this subsection, we present a battery of robustness checks on our above-reported results. Specifically, we check their sensitivity to different bandwidths, use a parametric approach to calculate the RD estimators, include predetermined characteristics, conduct a permutation test with placebo cutoff points, examine the issue of rounding errors, use pre-policies data as a placebo test, and experiment with an alternative estimation strategy.

3.4.1. Alternative bandwidths

To check whether our findings are sensitive to the optimal bandwidth (h^*) that we choose based on Imbens and Kalyanaraman's (2012) approach, we experiment with alternative bandwidths from ($h^* - 17$) to ($h^* + 17$) for three measures of labor supply. Fig. 7 reports the RD estimates corresponding to each bandwidth, together with the 95% confidence interval. We find that while the confidence interval becomes relatively larger when we use a smaller bandwidth (which is consistent with the small sample situation), our estimates are stable across all bandwidths, suggesting that our results are not driven by any particular bandwidth.

3.4.2. Inclusion of covariates

As a second robustness check, we follow Lee and Lemieux (2010)'s suggestion by including predetermined household characteristics as additional controls. Provided that the research design is valid, including these controls should have little effect on our estimates. The regression results are reported in column 1 in Table 4. We find that our results remain robust to these additional controls.[21]

3.4.3. Parametric estimation

In the baseline estimations, the RD estimators are calculated using the nonparametric approach. To assess whether the results are sensitive to the use of the nonparametric approach, we employ a parametric approach to calculate the RD estimates. Specifically, we estimate the RD using polynomial functions from the first order to seventh order and plot the coefficients, 95% confidence intervals, and AIC values against the corresponding polynomial orders in Fig. 8. Clearly, the third order is optimally chosen, as it has the lowest AIC value. The parametric RD estimates using the third-order polynomial function are reported in column 2 in Table 4. We find a consistent pattern and similar magnitudes compared with the nonparametric RD estimates, suggesting the robustness of our findings.

3.4.4. Rounding errors

When reporting their house size, people tend to round up the values to the nearest integer. However, this practice may introduce rounding errors into the assignment variable, which could lead to inconsistent RD estimates (e.g., Barreca et al., 2011; Barreca et al., 2016). To assess this concern, we adopt the method developed by Dong (2015), which corrects the rounding error problem in the RD framework. Specifically, we apply Dong's bias correction formula by assuming a uniform distribution of house size within the integers, as the true distribution is unknown. The estimates are reported in column 3 of Table 4 and are consistent with our previous results, suggesting that our estimates are not biased by rounding in the assignment variable.

3.4.5. A permutation test with placebo cutoff points

As a further validity check on our identification framework, we conduct an analysis using fake cutoff points (for a similar exercise, see, for example, Kane, 2003; Gelman and Imbens, 2019; Gelber et al., 2017). Specifically, instead of using the true cutoff point of 90, 500 times we randomly select a single point from the empirical distribution of housing size that will serve as a pseudo threshold[22]. Using the same specification as our main estimates, we then estimate the discontinuities at these randomly selected cutoff points and plot the histogram of the placebo RD estimates in Fig. 9. The figure clearly shows that the point from the true cutoff point (90), represented by the vertical red line, is located well below the significant majority of the distribution of placebo estimates, lending further support to the validity of our estimation.

3.4.6. Placebo tests of using pre-policies data

We have access to two waves of population censuses (i.e., the 2000 Census and the 2005 Mini Census) before our focal housing policies. As there were no housing policies in effect, we should not observe any discontinuity of labor supply at the cutoff of 90 m^2. To this end, we plot the relation between house size and 2000 Monthly Labor Supply in Fig. 10a and the relation between house size and 2005 Monthly Labor Supply in Fig. 10b. There is clearly no discontinuity at the cutoff point, leading support to our identification strategy.

3.4.7. Difference-in-discontinuities

With two year of data (i.e., the 2000 Census and the 2005 Mini Census) before and several years of data (i.e., the combined CHFS and CFPS data) after the housing policies were implemented in mid-2000s, we are able to conduct an alternative estimation strategy; that is, a difference-in-discontinuities design (D-DD, see in Grembi et al., 2016). Specifically, we conduct the RD estimation (3) using the 2000 Census and the 2005 Mini Census and subtract the obtained coefficient from our focal estimation coefficient obtained from CFPS and CHFS as the D-DD estimator. The D-DD strategy can help control for effects at the cutoff point that are unrelated to our housing policy changes, such as preferences or building structure. The D-DD estimation coefficients are reported in Table 5. We continue to find a negative and statistically significant coefficient. Moreover, the coefficient is close to the RD coefficient in column 2 of Table 2, primarily because the RD coefficient from the census data is small and statistically insignificant (i.e., −0.900). These results further confirm our previous findings.

To shed further light on our identification strategy, we report a series of RD estimates for both before and after the housing policies in Fig. 11. We find that the RD estimates are close to zero in years before the housing policies were implemented and become negative and statistically significant after the policies were in place. These findings lend further support to our identification framework.

3.5. Extensive and intensive margin effects

While we document a negative effect of housing wealth on labor supply, it would be interesting to understand whether the effect comes from the labor participation decision (i.e., whether to work, namely, the extensive margin effect) or behavior changes by employed workers (the intensive margin effect). To this end, in panel A of Table 6, we first investigate whether housing wealth affects the labor participation decision. The estimation results show that there is a significant decline in the labor participation rate for the treatment group. Specifically, the treatment group is approximately 7

[21] The magnitudes drop quite significantly. We adopt a stepwise approach to examine which predetermined covariates cause the reduction in estimated magnitude and identify *Age*. However, age does not show significant discontinuity at the cutoff, i.e., the discontinuity is 0.94 with a control mean of 17. Note that $\beta = \beta_{aug} + \sigma_{age}\alpha$, where β_{aug} is the RD estimate with a control for age; σ_{age} is the coefficient of age on labor supply; and α is the discontinuity of age at the cutoff. With a small α, the decline in magnitude (i.e., $\beta - \beta_{aug}$) is mainly driven by the large age effect on labor supply, i.e., the large σ_{age} that is identified in our analyses.

[22] To ensure that we have a sufficient number of observations on both sides of the threshold, we restrict the random draw of thresholds to be between the 10th and 90th percentiles of the distribution.

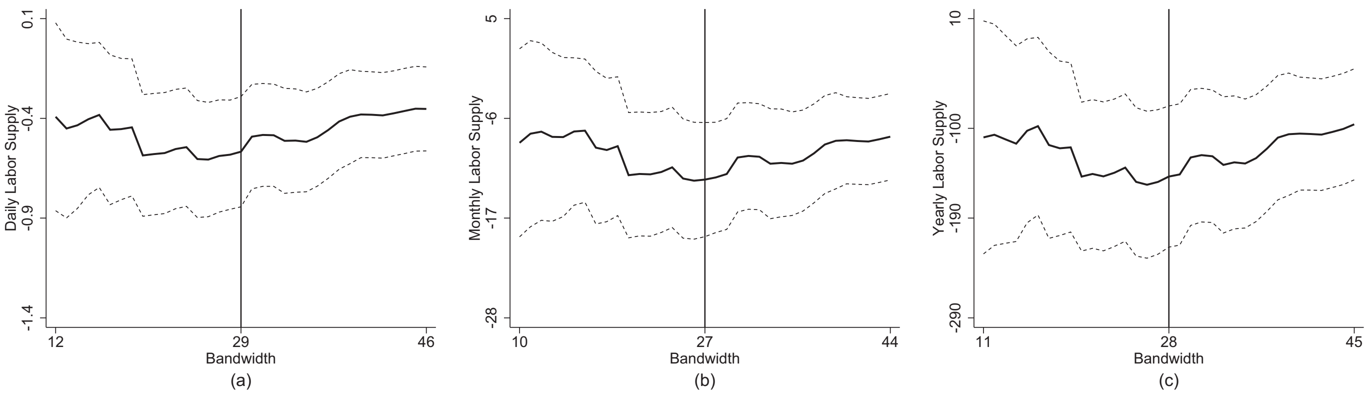

Fig. 7. Estimates with different bandwidths. Notes: 1. The solid line depicts the RD estimates from the local linear regression with different bandwidths (displayed on the X-axis), and the dashed lines are the 95% confidence intervals; 2. The vertical line is the optimal bandwidth used in our baseline analysis.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

percentage points less willing to participate in the labor market than is the control group.

The intensive margin effects (i.e., the effect on hours worked for employed individuals) are reported in panel B. None of the estimates has any statistical significance, and the estimates are small in magnitude, indicating that housing wealth barely affects labor supply for employed people. However, a concern in analyses of the intensive margin effect is that there could be sample selection bias. Specifically, the analyses focus on the sample of employed workers, whose labor force participation decision is significantly affected by housing wealth.

To address this sample selection concern, we adopt a trimming procedure developed by Lee (2009). The premise of this methodology is to consider the best- and worst-case scenarios caused by sample selection and then bound the estimated effects. Specifically, consider first the case in which the 7-percentage-point decrease in the employment rate in the treatment group caused by the treatment effect reflects those individuals with the most hours worked. Then, the comparison of hours worked between the remaining individuals in the treatment group and the entire population in the control group constitutes the worst-case scenario and largest bias. To restore a balanced comparison between the treatment and control groups in this setting, we then drop the top 7% of individuals in terms of hours worked from the control group, and the resulting estimates constitute the lower bound of the true effect. Similarly, if we assume that in the treatment group, the 7-percentage-point decline in employment in the treatment group comes from individuals with the least hours worked, we can then exclude the bottom 7% of individuals in terms of hours worked in the control group from the estimations, and the estimates from this refined sample represent the upper bound of the true effect. Appendix Table A10 reports the lower and upper bounds of the intensive margin effects. We find mixed results, with the lower and upper bounds having the opposite signs, indicating an undetermined effect of housing wealth at the intensive margin.

Table 4
Robustness checks.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

	(1) Inclusion of covariates	(2) Parametric estimation	(3) Bias-corrected estimates via Dong (2015)
Dependent variable: Daily labor supply			
D = I[Housing size ≤ 90]	−0.530***	−0.477***	−0.460***
	(0.161)	(0.124)	(0.125)
Mean value of the control group	4.681	4.511	4.511
Optimal bandwidth	26	–	–
No. of observations	17,717	61,416	61,416
Dependent variable: Monthly labor supply			
D = I[Housing size ≤ 90]	−6.753**	−9.940***	−9.467***
	(2.903)	(2.812)	(2.840)
Mean value of the control group	104.731	101.236	101.236
Optimal bandwidth	32	–	–
No. of observations	21,956	61,357	61,357
Dependent variable: Yearly labor supply			
D = I[Housing size ≤ 90]	−86.877**	−127.830***	−123.161***
	(34.004)	(32.815)	(33.104)
Mean value of the control group	1162.598	1130.616	1130.616
Optimal bandwidth	34	–	–
No. of observations	22,846	61,217	61,217

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten. In column (1), all predetermined covariates, a linear term of the normalized assignment variable (i.e., housing size) and its interaction with treatment status D are included; whereas in columns (2) and (3), a third-order polynomial function of the assignment variable and its interaction with D are included. The coefficients of all the controls are suppressed to save space.; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

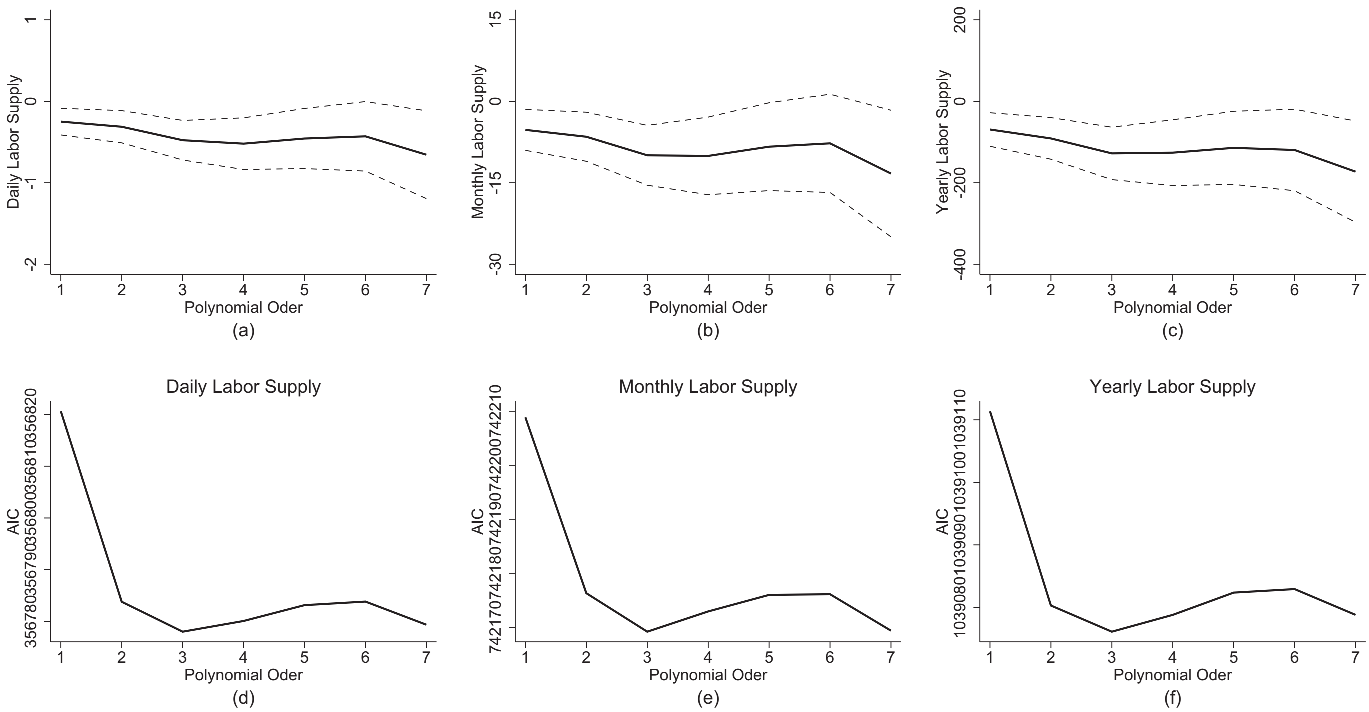

Fig. 8. Estimates with different orders of polynomial functions. Notes: 1. The solid line in the upper panel depicts the RD estimates from parametric estimation using different orders of polynomial functions (displayed on the X-axis), and the dashed lines are the 95% confidence intervals; 2. The solid line in the lower panel displays the AIC values of different polynomial orders.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

Combined, these analyses suggest that housing wealth significantly reduces the likelihood of participating in the labor market, but its effect on the labor supply of employed workers is ambiguous. These results are consistent with the findings in the literature. For example, Klein (2014) finds that the negative effect of housing prices on the labor supply of married women is only significant at the extensive margin and not at the intensive margin.

3.6. *Treatment effect away from the cutoff*

Our RD estimator in Eq. (2) essentially identifies the effect in the neighborhood of the cutoff point, i.e., the population with a house size of 90 m^2. For academia and policy makers, it is important to understand what the treatment effects are for other house sizes (and hence other populations in the economy). To this end, we apply a method developed by Dong and Lewbel (2015). Specifically, denote $\beta(c)$ as the treatment effect at c. Hence, $\hat{\beta}_{RD}$ in Eq. (2) is $\beta(c_0)$. Dong and Lewbel (2015) argue that $\beta'(c_0) \equiv \left.\frac{\partial\beta(c)}{\partial c}\right|_{c=c_0}$ can shed light on the treatment effect away from the cutoff point c_0. Specifically, they show that the existence assumption for $\beta'(c_0)$ is satisfied for the local linear regression in Eq. (2), and $\beta'(c_0) = \hat{\tau}$, the estimated coefficient for the interaction between normalized house size $\tilde{c}_i$ and treatment status D_i.

We report $\hat{\tau}$ in columns 1–3 of Appendix Table A11. We find positive and statistically significant coefficients for all three measures of labor supply. These results imply that the treatment

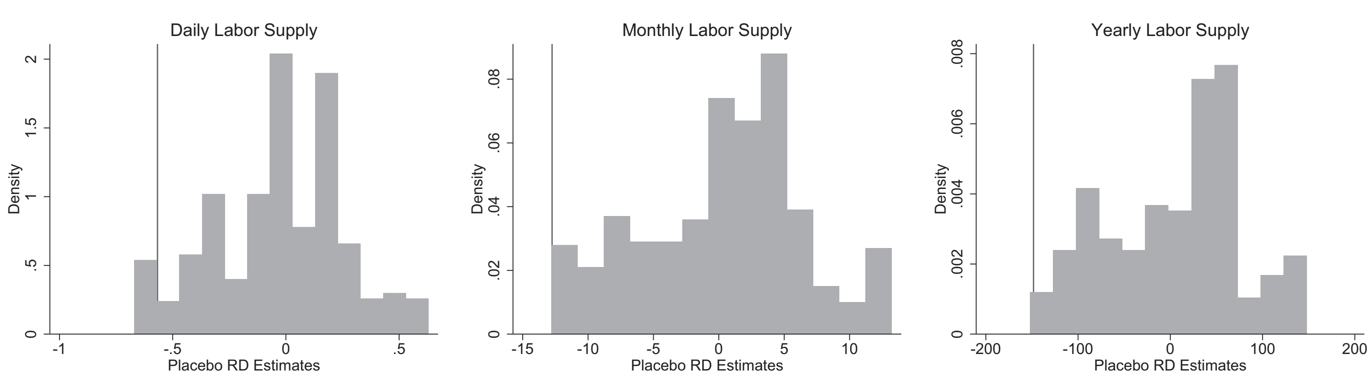

Fig. 9. Placebo test: Density of 500 runs of estimates with different cutoffs. Note: The vertical solid line is the real effect of the cutoff (90), while the bandwidths for each cutoff are calculated using Imbens and Kalyanaraman's (2012) approach.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

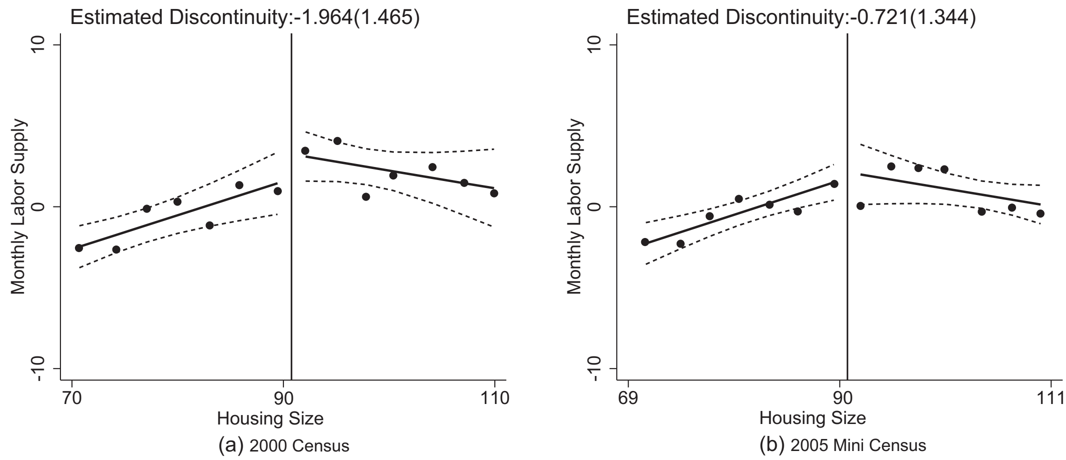

Fig. 10. (a–b) Monthly labor supply and housing size: Census data. Notes: 1. Circles represent conditional mean values of the respective variable for each bin with a size of 3 m^2 after controlling for purchase-year fixed effects, city fixed effects and a dummy for multiples of ten; 2. Solid lines are the fitted values from the local linear regression with the optimal bandwidth calculated using Imbens and Kalyanaraman's (2012) approach, and the dashed lines are the 95% confidence intervals; 3. The vertical line is the cutoff point (i.e., 90) in the assignment variable.
Data source: urban houses in 2000 Census and 2005 Mini Census.

effect positively depends on house size. Specifically, in response to the policy shocks, households with housing units of 80 m^2 reduce labor supply 60% more than those living in housing units of 90 m^2. This heterogeneous treatment effect could be due to the larger effect of housing policies on housing prices for smaller housing units or greater responsiveness to changes in housing wealth by households with smaller housing units. While we are unable to test the latter explanation due to the identification issue, we examine the possibility of the former explanation and shed light on the underlying mechanisms. Specifically, we report the coefficient of the interaction between normalized house size $\tilde{c}_i$ and treatment status D_i for the regression using growth in housing prices as the outcome variable in column 4 of Appendix Table A11. The negative coefficient indicates that housing prices grow faster in smaller units, qualitatively supporting the argument that our focal housing policies have larger effects on housing prices for smaller housing units. Quantitatively, the prices of housing units of 80 m^2 grow 77% faster than those of 90 m^2. These results may suggest that the heterogeneous treatment effects across housing size largely come from the differential effects of housing policies on growth in housing prices across size.

3.7. Heterogeneous effects

We explore differential treatment effects across households in this subsection, specifically, male versus female and the young versus the elderly population.

3.7.1. Heterogeneous effects with respect to gender

Recent studies from behavioral and experimental economics show that men and women differ in a wide range of economic preferences (for reviews, see, Croson and Gneezy, 2009; Bertrand, 2011). It is interesting to investigate whether the labor supply response to housing prices differs by gender. To this end, we report the estimation results for the male and female subsamples in columns 1 and 2 of Table 7, respectively.

Although we find negative and statistically significant estimates for both samples, the magnitude is larger for the female population than the male one, suggesting that females are more sensitive to the housing policies in their labor supply response. The gender difference in response could be due to the larger effect of housing policies on

Table 5
D-DD estimation.
Data source: urban houses purchased before 2006 in CHFS and CFPS, and urban houses in 2000 Census and 2005 Mini Census.

	(1)	(2)
	Monthly labor supply	Labor participation
RD from CHFS + CFPS	−12.750***	−0.069***
	(3.214)	(0.016)
RD from 2000 and 2005 Mini Census	−0.874	−0.006
	(1.060)	(0.005)
D-DD	−11.876***	−0.063***
	(3.329)	(0.016)
Mean value of the control group	114.210	0.630
Optimal bandwidth in survey data	27	23
Optimal bandwidth in census	20	25
No. of observations	221,945	254,860

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

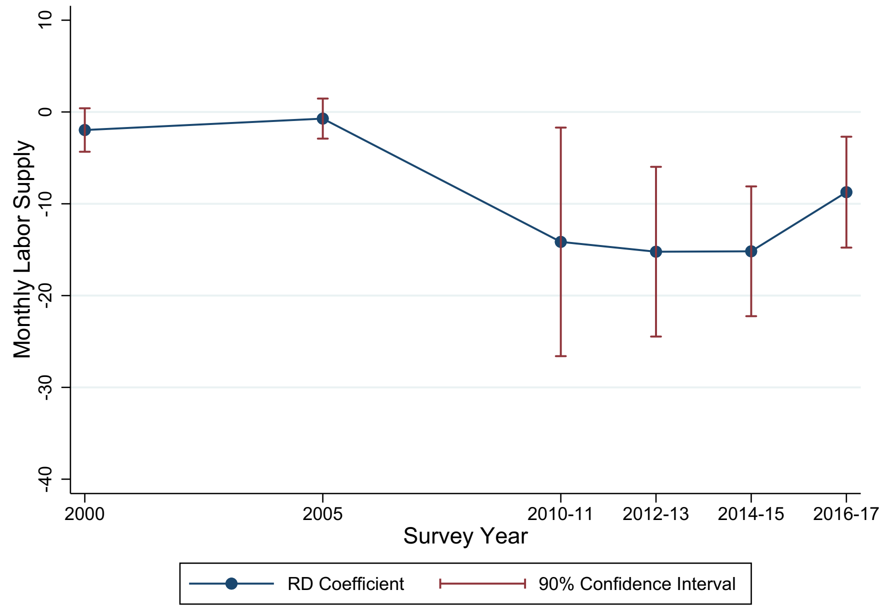

Fig. 11. RD estimates before and after the reform. Note: Circles represent the RD estimates for different waves of data from the local linear regression with the optimal bandwidth calculated using Imbens and Kalyanaraman's (2012) approach, and the capped spikes are the 90% confidence intervals.
Data source: urban houses in 2000 Census and 2005 Mini Census, and urban houses purchased after 2006 in CFPS and CHFS.

Table 6
Extensive and intensive margin effects.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

	(1)	(2)	(3)
	Daily labor supply	Monthly labor supply	Yearly labor supply
Panel A: Extensive margin effect			
D = I[Housing size ≤ 90]	−0.069***	−0.070***	−0.068***
	(0.016)	(0.016)	(0.017)
Mean value of the control group	0.530	0.529	0.523
Optimal bandwidth	23	23	20
No. of observations	28,950	28,915	26,655
Panel B: Intensive margin effect			
D = I[Housing size ≤ 90]	−0.003	1.427	−5.957
	(0.118)	(2.945)	(33.133)
Mean value of the control group	8.507	191.289	2161.342
Optimal bandwidth	19	18	15
No. of observations	10,998	10,734	9490

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

housing prices for smaller housing units or greater responsiveness to the changes in housing wealth by smaller housing units between genders. To shed further light on these two explanations, we investigate the heterogeneous effects of housing policies on housing prices between males and females. The results are reported in columns 1–2 of Appendix Table A12. We find quite similar housing price effects with respect to gender (albeit with a slightly larger magnitude for females than males). Hence, this result implies the difference in labor supply response between genders largely comes from the heterogeneous reaction to changes in housing wealth. These results are consistent with the view of gender identity in many societies; that is, males should earn more than females (Bertrand et al., 2015).[23] Hence, in response to shocks to housing prices, females exhibit more resilience than males, as gender identity makes the labor supply of females more adjustable. Alternatively, our findings could also be due to the differences in constraints or abilities to alter labor supply across gender (e.g., differences in working histories).

3.7.2. Heterogeneous effects with respect to age

The standard life-cycle model predicts that elderly individuals respond more strongly to unanticipated wealth shocks because they have fewer years to live. Zhao and Burge (2017) examine the effect of housing wealth on the labor supply of elderly individuals, given the unprecedented growth in the number of households headed by elderly individuals in the past decade in the U.S. In light of this, we also investigate whether there are differential effects across age groups in China. Note that the official retirement age in China ranges from 55 to 60. Hence, we divide sample into three groups; that is, the young generation (defined as individuals aged below 45), the near-retirement cohort (defined as individuals aged between 45 and

[23] Akerlof and Kranton (2000) argue that economic outcomes are influenced by individual identity because deviating from the prescribed behavior is costly.

Table 7
Heterogeneous effects.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Female	Male	Age < 45	45 < Age < 60	60 < Age	(Debt/asset) < median	(Debt/asset) > median
Dependent variable: Daily labor supply							
D = I[Housing size ≤ 90]	−0.656***	−0.504***	−0.335*	−0.381*	−0.122	−0.577***	0.724
	(0.158)	(0.177)	(0.184)	(0.210)	(0.095)	(0.142)	(0.494)
Mean value of the control group	3.683	5.357	6.766	5.102	0.646	4.324	5.534
Optimal bandwidth	22	24	27	36	32	29	13
No. of observations	14,319	14,553	10,916	14,441	12,992	29,372	2418
Dependent variable: Weekly labor supply							
D = I[Housing size ≤ 90]	−12.395***	−10.134***	−6.813*	−7.978	−2.575	−11.391***	13.604
	(3.544)	(3.414)	(3.888)	(5.208)	(2.182)	(3.240)	(8.545)
Mean value of the control group	82.475	120.421	152.567	114.233	14.039	97.423	122.090
Optimal bandwidth	24	31	30	35	27	34	15
No. of observations	15,058	19,031	12,807	14,172	10,462	36,128	2724
Dependent variable: Yearly labor supply							
D = I[Housing size ≤ 90]	−150.859***	−126.206***	−117.612**	−70.754	−30.647	−138.797***	109.410
	(40.595)	(38.652)	(55.505)	(58.854)	(23.413)	(35.841)	(103.512)
Mean value of the control group	916.330	1349.967	1705.973	1277.480	156.040	1085.843	1375.199
Optimal bandwidth	24	31	25	38	33	35	15
No. of observations	15.024	18,960	10,477	14,868	13,341	37,081	2717

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

60) and the elderly population (defined as individuals aged 60 and above).

The estimation results are presented in columns 3–5 in Table 7. Although the coefficients are less precisely estimated, we find heterogeneous effects across age groups. The estimated coefficients for individuals aged 60 and older are statistically insignificant and small in absolute magnitude (despite the large in relative change due to the small control means). These findings are largely in line with the results of Imbens et al. (2001), who do not find a significant response by the elderly group. Workers below age 45 exhibit significant decline in their labor supply, with magnitudes comparable to those aged between 45 and 60 (approximately 4.5–7.5% of the mean). These results differ from the findings of Zhao and Burge (2017) that the magnitude of the elderly group's response is comparable to that of the near-retirement cohorts.

To further understand the heterogeneous labor supply responses to housing policies across age groups, we examine whether they are due to the larger effect of housing policies on housing prices or greater responsiveness to changes in housing wealth across ages. To this end, we report the heterogeneous effects of housing policies on housing prices across the three age groups in columns 3–5 of Appendix Table A12. We find large and significant housing price effects for the elderly population, relatively smaller (statistically not significant) effects for near-retirement cohorts but almost no effect for the young generation. Combined with the results in Table 7, these findings suggest that the labor supply response to the housing policies by the young generation mostly comes from their sensitivity to changes in housing wealth, whereas the elderly population is less responsive to changes in housing wealth despite the significant increase in housing wealth generated by the housing policies.

3.7.3. Heterogeneous effects with respect to repayment capacity

Household debt levels increased rapidly in China in recent years, raising concerns about households' repayment capacity and the risk of financial instability. It is interesting to investigate whether the labor supply response to housing prices differs by a household's repayment capacity. In our study, we use the ratio of household debt to assets to measure repayment capacity. Specifically, we group our sample based on the sample median of the debt-to-assets ratio and report the labor supply responses for two subsamples in columns 6 and 7 of Table 7. The estimation results show that the negative effects of housing policies on labor supply are concentrated among households with low debt ratios. The estimation results in columns 6 and 7 of Appendix Table A12 further show that households with lower debt ratios have smaller growth in housing prices than those with higher debt ratios, suggesting that their negative labor supply response mainly comes from their sensitivity to housing wealth appreciation. While the labor supply of low-repayment-capacity (i.e., high debt-to-asset ratio) households exhibits positive, if any, response to housing wealth appreciation. If changes in housing wealth affect borrowing collateral, then one would expect housing wealth increases to have a positive impact on borrowing-constrained households' consumption of goods and services as well as leisure (as shown in Cooper, 2013). Our estimation results may then suggest that housing wealth does not primarily affect labor supply through the repayment capacity channel.

4. Conclusion

In this paper, we implement an RD design to estimate the labor supply response to housing wealth. Specifically, in the mid-2000s, the Chinese government imposed a series of policies to regulate the Chinese housing market. These unanticipated policies caused the value of housing units with floor areas less than or equal to 90 m^2 to grow faster than that of larger units. This setting allows us to address potential identification issues, for example, nonrandom selection of the housing unit, including the size and location.

Our results suggest a significant reduction in hours worked in response to unanticipated gains in housing wealth. Specifically, a one-percent increase in the annual growth rate of the price of housing leads to a 118.09 decline in hours worked per year, which corresponds to approximately 10% of the mean annual hours worked for the control group. We also find heterogeneous effects across age groups, gender and repayment capacity. Young individuals are more sensitive to the changes in wealth than are elderly cohorts. Female labor supply is more responsive to changes in wealth than male labor supply. Households with high repayment capacity exhibit a much stronger response in labor supply to changes in housing wealth than low-repayment-capacity households. Further investigation indicates that changes in housing wealth affect labor supply primarily through the extensive margin effect, i.e., labor market participation, whereas the intensive margin effect is ambiguous.

Declaration of competing interest

The authors declare that there is no conflict of interest.

Appendix A. Figures and tables

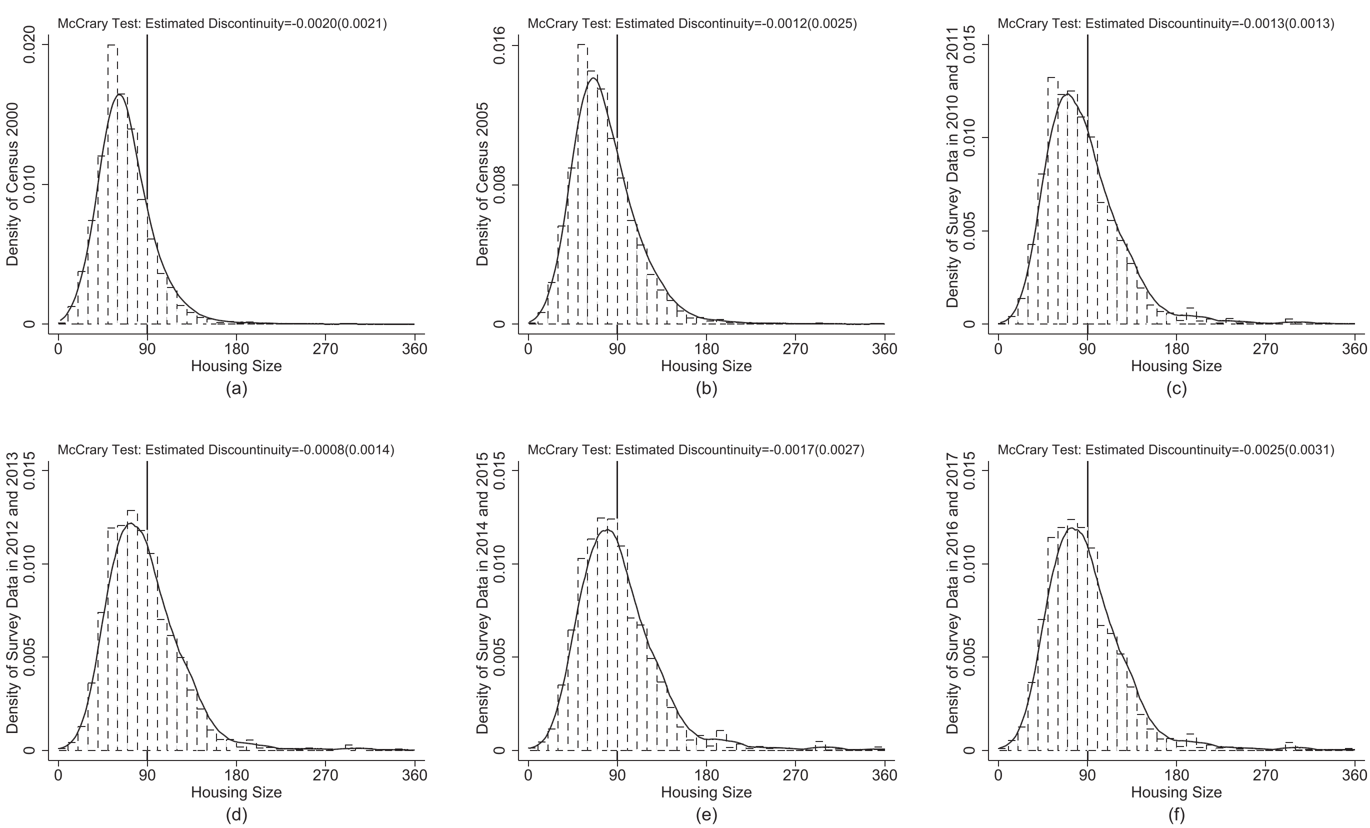

Fig. A1. Distribution of house size over time. Note: The solid line is the kernel density distribution, while kernel = epanechnikov.
Data source: urban houses in 2000 Census, urban houses in 2005 Mini Census, and urban houses in CHFS and CFPS.

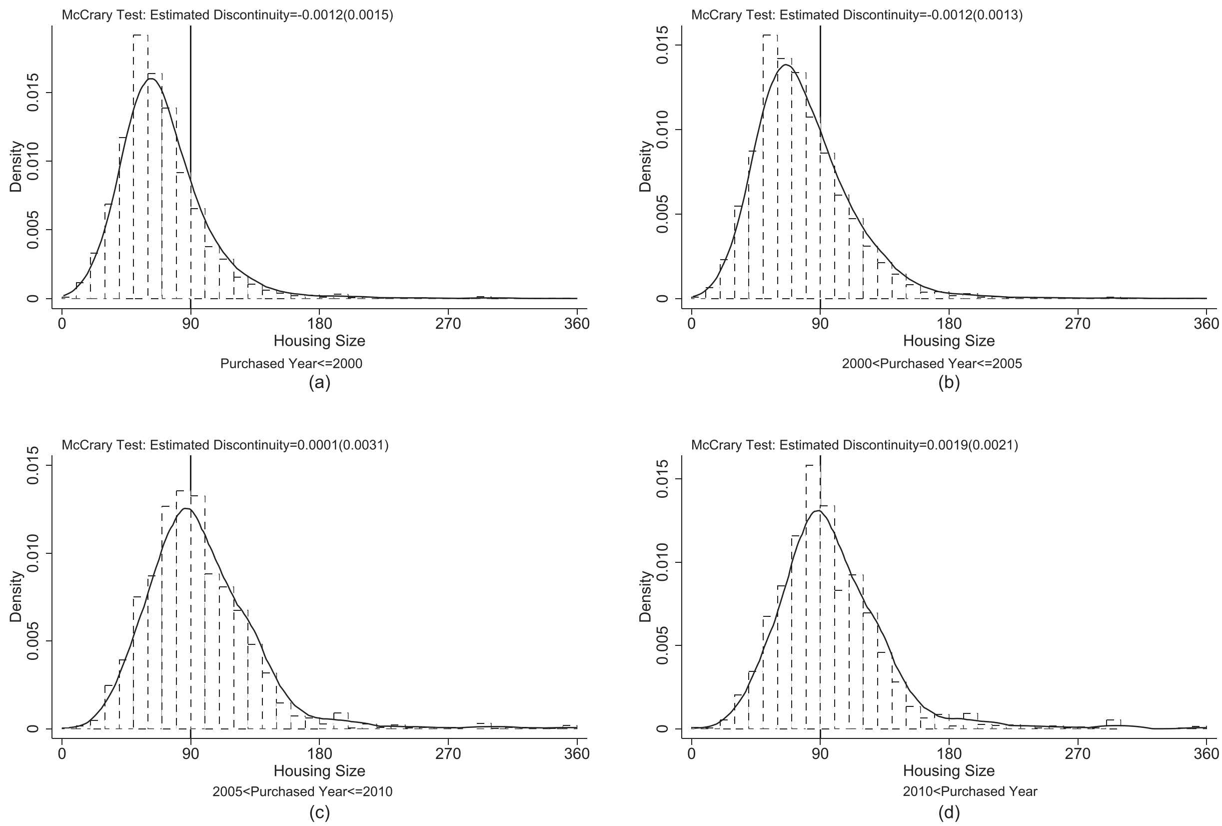

Fig. A2. Distribution of house size over purchase year. Note: The solid line is the kernel density distribution, while kernel = epanechnikov.
Data source: urban houses in 2000 Census, urban houses in 2005 Mini Census, and urban houses in CHFS and CFPS.

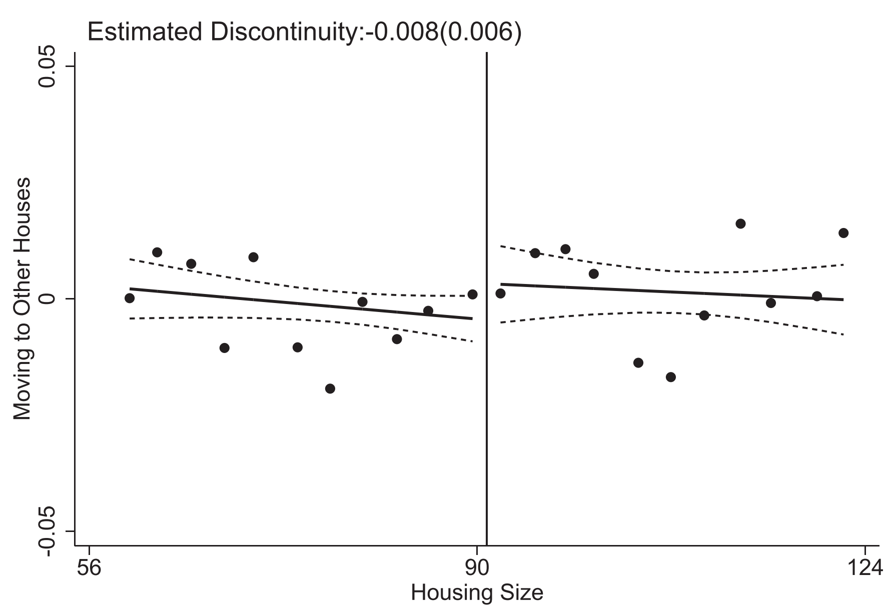

Fig. A3. Whether move to another house after 2010. Notes: 1. Circles represent conditional mean values of the respective variable for each bin with a size of 3 m^2; 2. Solid lines are the fitted values from the local linear regression with the optimal bandwidth calculated using Imbens and Kalyanaraman's (2012) approach, and the dashed lines are the 95% confidence intervals; 3. The vertical line is the cutoff point (i.e., 90) in the assignment variable.
Data source: all urban houses in CHFS and CFPS.

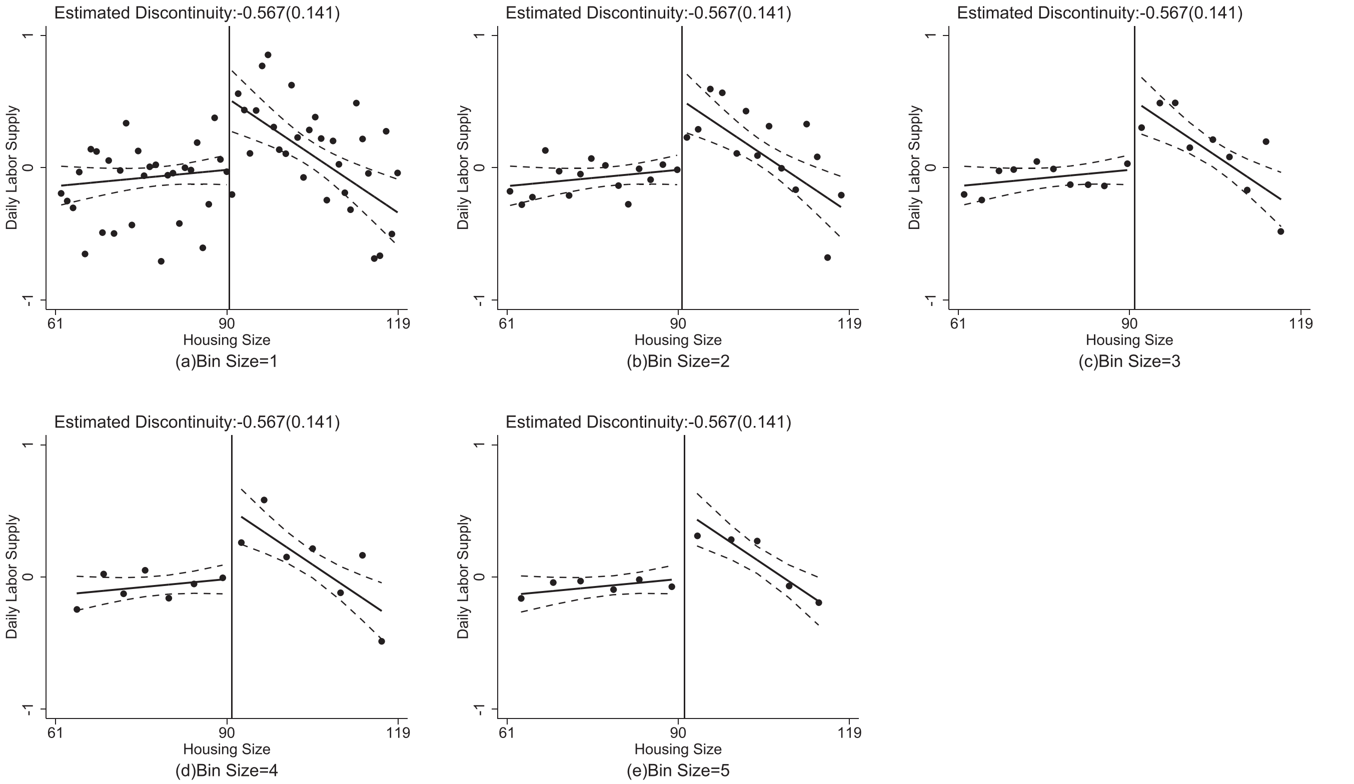

Fig. A4a. Different bin sizes for daily labor supply. Notes: 1. Circles represent conditional mean values of the respective variable for each bin with bin sizes of 1 to 5 m^2; 2. Solid lines are the fitted values from the local linear regression with the optimal bandwidth calculated using Imbens and Kalyanaraman's (2012) approach, and the dashed lines are the 95% confidence intervals; 3. The vertical line is the cutoff point (i.e., 90) in the assignment variable.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

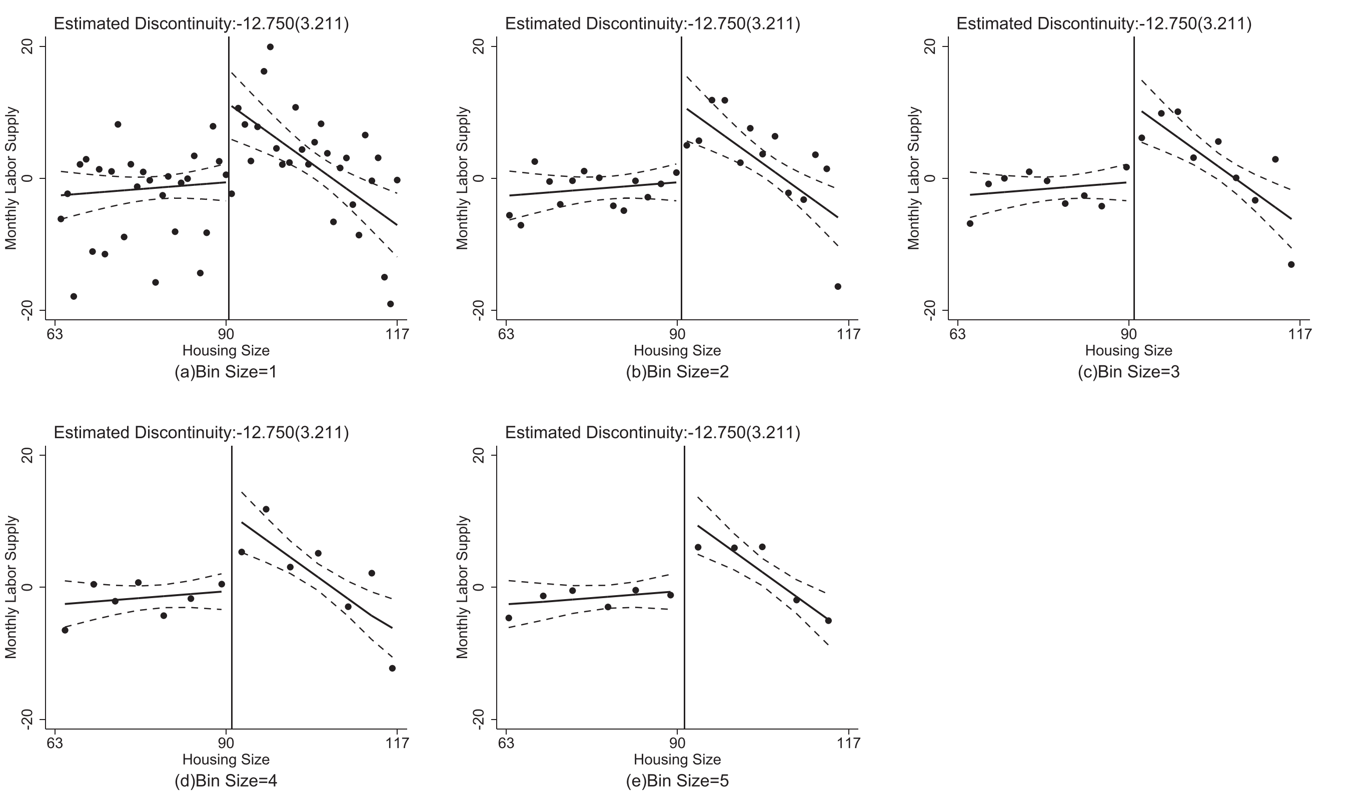

Fig. A4b. Different bin sizes for monthly labor supply. Notes: 1. Circles represent conditional mean values of the respective variable for each bin with bin sizes of 1 to 5 m^2; 2. Solid lines are the fitted values from the local linear regression with the optimal bandwidth calculated using Imbens and Kalyanaraman's (2012) approach, and the dashed lines are the 95% confidence intervals; 3. The vertical line is the cutoff point (i.e., 90) in the assignment variable.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

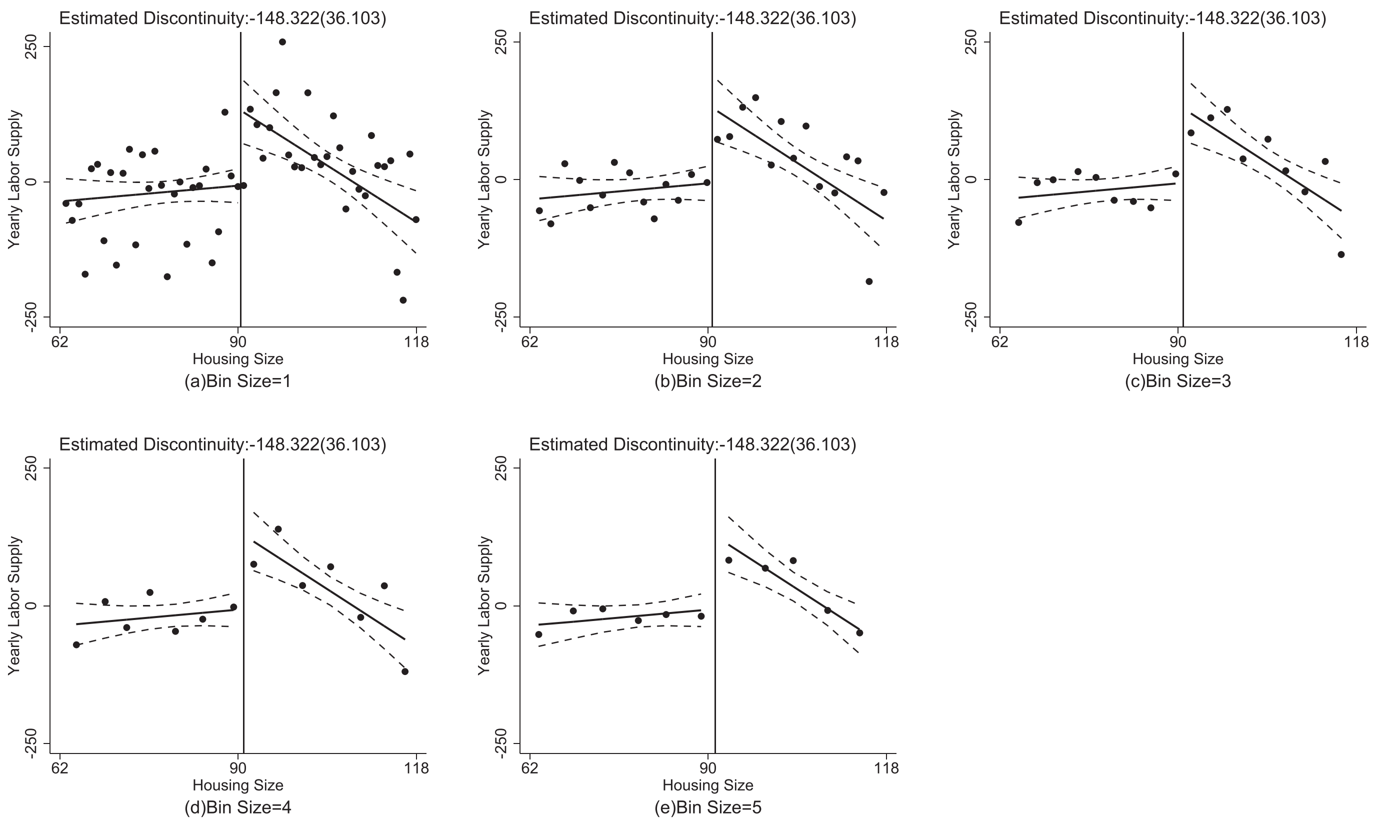

Fig. A4c. Different bin sizes for yearly labor supply. Notes: 1. Circles represent conditional mean values of the respective variable for each bin with bin sizes of 1 to 5 m^2; 2. Solid lines are the fitted values from the local linear regression with the optimal bandwidth calculated using Imbens and Kalyanaraman's (2012) approach, and the dashed lines are the 95% confidence intervals; 3. The vertical line is the cutoff point (i.e., 90) in the assignment variable.
Data source: urban houses purchased before 2006 in CHFS and CFPS.

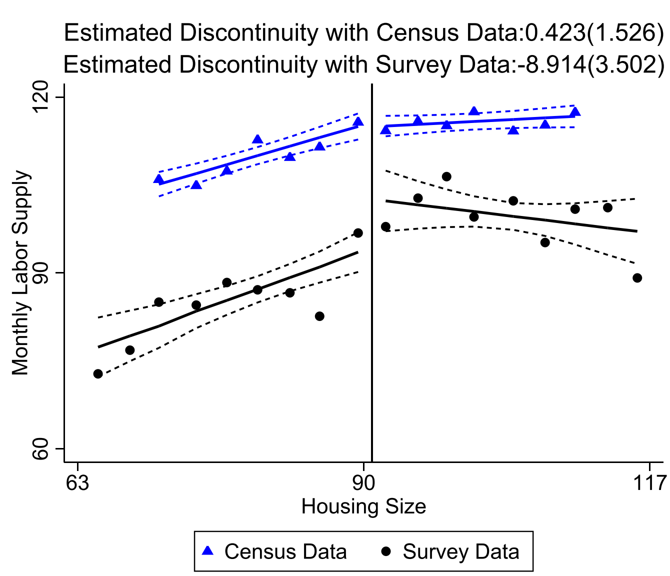

Fig. A5. Monthly labor supply and housing size: census data vs. survey data. Notes: 1. Blue triangles represent mean values of the respective variable for each bin with a size of 3 m^2 using urban houses in 2000 Census and 2005 Mini Census; 2. Black circles represent mean values of the respective variable for each bin with a size of 3 m^2 using urban houses purchased before 2006 in CHFS and CFPS; 3. Solid lines are the fitted values from the local linear regression with the optimal bandwidth calculated using Imbens and Kalyanaraman's (2012) approach, and the dashed lines are the 95% confidence intervals; 4. The vertical line is the cutoff point (i.e., 90) in the assignment variable.

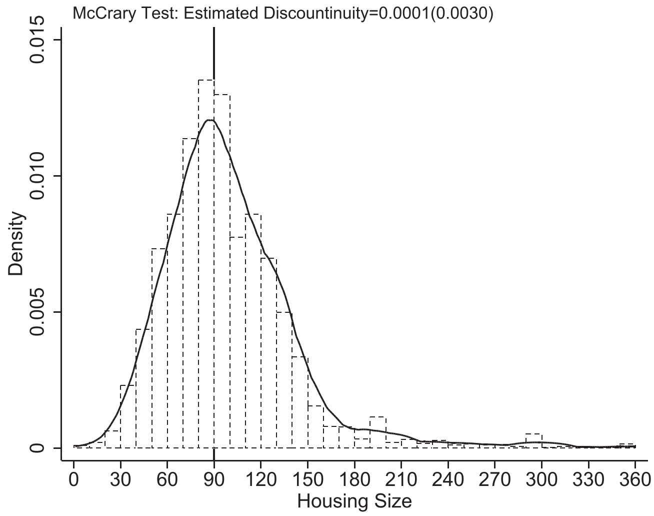

Fig. A6. Distribution of housing size for houses purchased after 2006. Notes: 1. The solid line is the kernel density distribution, while kernel = epanechnikov. Data source: urban houses purchased after 2006 in CHFS and CFPS.

Table A1
Data source.

Data source	Years	Sample	Primary variables	Notes
China Household Finance Survey (CHFS)	2011, 2013, 2015, 2017	Households owned houses before 2006 in urban areas	House size, annualized growth rate of housing prices, characteristics of houses, family member's labor participation and hours worked, characteristics of the household interviewee	Individual-level and household-level panel data
China Family Panel Studies (CFPS)	2010, 2012, 2014, 2016	Households owned houses before 2006 in urban areas	House size, annualized growth rate of housing prices, characteristics of houses, family member's labor participation and hours worked, characteristics of the household interviewee	Individual-level and household-level panel data
2005 One Percent Population Survey (Mini Census)	2005	Households owned houses in urban areas	House size, characteristics of houses, family member's labor participation and hours worked, characteristics of the family members	15.2% sample

Table A2
House size density and predetermined variable continuity test from 2000 and 2005 Mini Census.

	(1)	(2)	(3)	(4)	(5)	(6)	
	Density	Education	Gender	Age	Marriage status	Number of siblings	
D = I[Housing size ≤ 90]	−0.0006	−0.1253	0.0000	−0.0718	0.0092	0.0437	
	(0.0017)	(0.0781)	(0.0015)	(0.1692)	(0.0076)	(0.0443)	
Mean value of the control group	0.010	11.626	0.484	42.509	0.754	1.209	
Optimal bandwidth	22	10	42	22	10	12	
No. of observations	105,042	1012,91	377,458	202,911	99,050	17,081	
	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Health	Ethnicity	Welfare house	Housing cost	Housing age	Housing floors	Rooms
D = I[Housing size ≤ 90]	−0.0023	0.0003	0.0108	−11.9703	0.3420	−0.0210	0.0039
	(0.0030)	(0.0025)	(0.0130)	(34.0368)	(0.4441)	(0.0155)	(0.0216)
Mean value of the control group	0.965	0.948	0.296	1083.952	8.124	0.328	2.787
Optimal bandwidth	21	31	16	15	6	17	9
No. of observations	148,417	290,567	75,776	72,403	28,755	79,114	39,525

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects (except Housing Age), city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

Table A3
House size density and predetermined variable continuity test from CFPS and CHFS.

	(1)	(2)	(3)	(4)	(5)	(6)	
	Density	Education	Gender	Age	Marriage status	Ethnicity	
D = I[Housing size ≤ 90]	−0.0010	0.2525	0.0117	0.9405	0.0111	0.0011	
	(0.0014)	(0.1924)	(0.0232)	(0.6172)	(0.0156)	(0.0080)	
Mean value of the control group	0.020	11.478	0.491	52.122	0.577	0.952	
Optimal bandwidth	14	18	24	17	26	35	
No. of observations	9270	11,168	14,756	10,744	15,822	15,145	
	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Number of siblings	Father education	Mother education	Father party member	Mother party member	Housing cost	Housing age
D = I[Housing size ≤ 90]	0.0913	−0.0667	0.1028	−0.0217	−0.0040	−94.7937	−0.2019
	(0.0853)	(0.2570)	(0.2007)	(0.0235)	(0.0119)	(96.1363)	(0.3985)
Mean value of the control group	2.359	6.753	4.924	0.277	0.087	1608.826	14.472
Optimal bandwidth	27	24	26	37	30	17	22
No. of observations	14,353	11,864	13,013	17,173	14,893	10,444	14,216

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects (except Housing Age), city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

Table A4
House location.

	(1)	(2)	(3)	(4)
	Log(GDP per capita in survey year)	City population in survey year (in 10,000)	Mega city (city population ≥ 3 million) in survey year	Big city (first tier cities) in survey year
D = I[Housing size ≤ 90]	0.002	0.625	−0.002	0.007
	(0.005)	(0.575)	(0.005)	(0.018)
Mean value of the control group	11.128	793.390	0.515	0.587
bandwidth	15	25	16	14
Observations	9926	15,344	10,095	9270

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1; 4. First tier cities include Beijing, Shanghai, Guangzhou and Shenzhen.

Table A5
Homogenize sample size.

	(1)	(2)	(3)
	Daily labor supply	Monthly labor supply	Yearly labor supply
D = I [Housing size ≤ 90]	−0.597***	−12.980***	−151.797***
	(0.142)	(3.211)	(37.224)
Mean value of the control group	4.495	100.896	1130.286
Optimal bandwidth	27	27	27
No. of observations	32,139	32,139	32,139

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

Table A6
Test budget constraint.

	(1)	(2)
	(House value * 0.1)/annual income > median annualized growth rate of housing prices	(House value * 0.1)/annual income < median annualized growth rate of housing prices
D = I [Housing size ≤ 90]	0.022**	0.005
	(0.010)	(0.004)
Mean value of the control group	0.166	0.109
Optimal bandwidth	19	23
No. of observations	5532	6847

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

Table A7
IV first stage using individual data.

	(1) First stage of IV estimate for daily labor supply	(2) First stage of IV estimate for monthly labor supply	(3) First stage of IV estimate for yearly labor supply
Dependent variable: Annualized growth rate of housing prices			
D = I [Housing size ≤ 90]	0.012**	0.012**	0.012**
	(0.006)	(0.006)	(0.006)
Mean value of the control group	0.134	0.134	0.134
Optimal bandwidth	20	20	20
No. of observations	25,135	25,103	25,025

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

Table A8
Investigation of omitted variables bias.

	(1) Daily labor supply	(2) Monthly labor supply	(3) Yearly labor supply
Housing purchased after 2006			
D = I[Housing size ≤ 90]	0.021	0.317	0.168
	(0.167)	(4.268)	(50.184)
Mean value of the control group	5.366	123.702	1346.153
Optimal bandwidth	21	21	20
No. of observations	20,065	20,035	19,734

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

Table A9
Test for credit constrained using houses purchased after 2006.

	(1) Houses purchased after 2006 debt ratio
D = I[Housing size ≤ 90]	0.129*
	(0.077)
Mean value of the control group	0.162
Optimal bandwidth	16
No. of observations	9661

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

Table A10
Extensive and intensive margin effects robustness checks.

	(1)	(2)
	Drop highest 6.9% of control group	Drop lowest 6.9% of control group
Panel A: Daily labor supply		
D = I[Housing size ≤ 90]	0.276***	−0.215*
	(0.085)	(0.112)
Mean value of the control group	8.210	8.702
Optimal bandwidth	19	19
No. of observations	10,838	10,827
	Drop highest 7.0% of control group	Drop lowest 7.0% of control group
Panel B: Monthly labor supply		
D = I[Housing size ≤ 90]	11.928***	−6.682**
	(2.056)	(3.031)
Mean value of the control group	179.713	199.340
Optimal bandwidth	18	18
No. of observations	10,445	10,439
	Drop highest 6.8% of control group	Drop lowest 6.8% of control group
Panel C: Yearly labor supply		
D = I[Housing size ≤ 90]	103.812***	108.072***
	(24.424)	(34.149)
Mean value of the control group	2031.897	2277.182
Optimal bandwidth	15	15
No. of observations	9263	9230

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

Table A11
Coefficients of interaction between assignment variable and treatment status D.

	(1)	(2)	(3)	(4)
	Daily labor supply	Monthly labor supply	Yearly labor supply	Annualized growth rate of housing prices
(Housing size − 90) * D = I[Housing size ≤ 90]	0.036***	0.807***	9.030***	−0.001***
	(0.009)	(0.209)	(2.324)	(0.000)
Mean value of the control group	4.511	101.236	1130.616	0.139
Optimal bandwidth	29	27	28	20
No. of observations	33,599	32,269	32,910	12,694

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

Table A12
Heterogeneous first stage estimates.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Female	Male	Age < 45	45 < age < 60	60 < age	(Debt/asset) < median	(Debt/asset) > median
Dependent variable: Annualized growth rate of housing prices							
D = I[Housing size ≤ 90]	0.013**	0.010*	0.003	0.012	0.019**	0.015**	0.026**
	(0.006)	(0.006)	(0.006)	(0.007)	(0.008)	(0.007)	(0.013)
Mean value of the control group	0.141	0.139	0.133	0.137	0.152	0.140	0.137
Optimal bandwidth	21	22	22	25	27	17	13
No. of observations	13042	12970	9188	9702	9821	17,337	2332

Notes: 1. Local linear regressions are used with the optimal bandwidth calculated based on Imbens and Kalyanaraman's (2012) approach; 2. All regressions control for a linear term of the normalized assignment variable (i.e., housing size), an interaction between the assignment variable and treatment status D, year fixed effects, purchase-year fixed effects, city fixed effects and a dummy for multiples of ten, the coefficients of which are suppressed to save space; 3. Standard errors in parentheses are clustered at the housing size level: ***p<0.01, **p<0.05, *p<0.1.

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