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Tax evasion, capital gains taxes, and the housing market

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ABSTRACT

In this study, we exploit a policy shock that differentially increased capital gains taxes for housing units with holding period less than 5 years, and document tax avoidance and tax evasion in the residential resale market in China. We show suggestive evidence that after the capital gains tax increase, resale transactions exhibit more bunching above 5 years of holding period, but the responses are small and imprecisely estimated. More importantly, using *precise* information of both the actual transaction price and the reported price to the tax authority, we find that tax evasion, measured by the difference between the two prices, becomes 23.3% higher. We also document that the policy has strong heterogeneous effects, whereby cash buyers are 8.4% more likely to buy a house than buyers who need financing after the policy. This is mainly because financing buyers prefer a higher reported price (and so less tax evasion) to secure higher bank loans but cash buyers do not have such concern.

1. Introduction

Tax evasion is illegal and incurs large social costs, e.g., lower tax revenues. Many government programs and services cannot be supported, and it also has implications on redistributive policies of a country. According to IMF estimates, total tax losses due to tax havens are over $400 billion for OECD member states and around $200 billion for lower-income countries.[1] Despite the growing literature documenting tax evasion through hidden wealth/income or underreporting of sales revenues, detecting tax evasion is very difficult and so these are conservative estimates.[2] The vast majority of tax evasion occurs in plain sight but we do not have precise data to document it. In this study, we use a policy change on capital gains taxes for home sellers with less than 5 years of holding period and take advantage of an administrative dataset that can *precisely* measure the house selling price and house registered price with the tax authority to causally document tax evasion in China.

Tax instruments are frequently used to cool down an overheated real estate market. The literature has focused on the effect of housing-related taxes, including both property tax and transfer tax, on the transaction price and volume in the housing market.[3] However, the real estate sector is also a market with prevalent tax evasion due to the high transaction value and the ensuing tax burden. As noted by officials in New York City, "It was impossible to know how many of the city's one million property owners were evading taxes by filing false income and expense statements with the Finance Department of the Tax Commission," and "even a small amount of fraud can result in real money lost.[4]" According to a

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[1] The US Internal Revenue Service reports tax gap for some tax years. The tax gap provides a rough gauge of the level of overall noncompliance and voluntary compliance. For 2008 to 2010, the average annual tax liability is $2496 billion with $2038 billion paid voluntarily and on time. This results in a gross tax gap is of $458 billion. However, within this tax gap, corporate income tax comprises only $44 billion, which is less than 10% of the total tax gap. By contrast, the individual income tax gap comprises $319 billion (https://www.irs.gov/newsroom/the-tax-gap). These facts suggest that the estimated tax loss reported in the literature due to tax havens (mainly applicable to corporate income tax) is conservative in terms of the magnitude of tax loss.

[2] Zucman (2013) and Alstadsæter et al. (2019) estimate that $5.6 trillion is held in tax havens and indicate approximately $252.9 billion tax losses due to hidden wealth. Fisman and Wei (2004) infer tax evasion on tariff from the difference between Hong Kong's reported exports to mainland China and mainland China's reported imports from Hong Kong. Fisman et al. (2008) infer tariff evasion based on mainland China's indirect export via Hong Kong. Artavanis et al. (2016) estimate that 43–45% of self-employed income goes unreported and thus untaxed by inferring from bank's underwriting model. Kleven et al. (2011) conduct a field experiment in Denmark and find that underreporting of income is substantial without third party audit. Marion and Muehlegger (2008) find that sales of taxed diesel fuel significantly rose and the sales of untaxed heating oil, a perfect substitute for diesel fuel, fell by a similar amount after regulators used dyeing technology to identify untaxed fuel.

[3] See Dachis et al. (2012), Fu et al. (2016), Slemrod et al. (2017), Best and Kleven (2018) and Deng et al. (2018).

[4] Please see full report available at the *New York Times* https://www.nytimes.com/2012/08/02/nyregion/property-tax-evasion-in-city-is-widespread-report-suggests.html.

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survey of 18 countries by the OECD,[5] the real estate sector has been identified as an important sector being used to facilitate tax fraud and money laundering. One of the most reported vulnerabilities that facilitate tax evasion is that the correct value of real estate can be easily under or over declared, as documented in countries including but not limited to Australia, Canada, Germany, Ireland, Spain, and Sweden. Therefore, it is important to document tax evasion in the real estate market, as well as its consequences.

To investigate the role of tax changes on tax evasion, we take advantage of an administrative dataset in China's real estate market, which records both the actual transaction price and the reported price registered with the tax authorities. The taxes, including stamp duty,[6] deed tax, sales tax, and capital gains tax, are calculated based on the reported price registered in the system of the tax bureau. To evade taxes, buyers and sellers can report lower registered prices to the local housing authorities, who share the information with the local tax bureaus. Both the registered price and the actual price are known to us in the dataset, and thus we can calculate the amount of tax evasion of each housing resale transaction. To study the impact of tax evasion on the outcome of tax policies in the housing market, we exploit a policy shock that was aimed at attacking housing speculators in China. Specifically, in 2013 the government sustainably increased the capital gains tax[7] of housing resale transactions with a holding period less than 5 years. In addition, given that the sellers may postpone the sale of housing units to avoid taxes after this policy change, we also study such tax avoidance behavior using a bunching design, and apply the findings in such design to improve the identification strategy on tax evasion.

With such a policy experiment and the existence of tax avoidance and tax evasion in the housing market, we aim to answer the following three questions. First, how does an increase in capital gains tax affect the tax avoidance of market participants by changing the timing of housing transactions? Second, how does an increase in capital gains tax affect the tax evasion of market participants? Third, has the policy generated any unintended consequences due to the leeway on tax reporting? Specifically, how does the policy affect tax avoidance/evasion of different types of buyers, such as loan buyers and cash buyers?

We examine tax evasion in the housing markets in 35 major Chinese cities. However, our main analysis uses the housing transaction data from one brokerage firm in one major city in China given the strength of the data: 1) we have precise information on the actual price and registered price of each transaction to study the tax evasion behavior; 2) we have the holding period information for the transacted units to study the tax avoidance behavior; 3) we can clearly identify the affected units by the tax increase; and 4) we have some information on the buyers, such as gender, age, hometown, income, occupation, as well as the financing of the house. We first use bunching estimation to identify the tax avoidance behavior of market participants, i.e., whether the sellers and buyers postpone the transactions to avoid higher taxes if the holding period of a house is approaching 5 years. We then apply a difference-in-differences (DID) framework to study the tax evasion behavior of market participants. Specifically, we define the treated units as houses with holding period less than 5 years since the last transaction dates, which are subject to higher capital gains tax rate after the policy. We define the control group as houses being held for more than 5 years since the last transaction dates, which are exempted from capital gains tax both before and after the policy. The policy was announced on February 20, 2013 and allowed the market an announcement period of slightly over a month, then implemented on March 31, 2013. Therefore, we define the "before period" being the period prior to the announcement and define the "after period" being the period after the policy implementation. To mitigate the sorting into the treatment and control groups due to tax avoidance behavior, we exclude the transactions with holding period more than 4 years and less than 6 years for a conservative estimate.

We find that transactions before the policy announcement have little bunching above 5 years of holding period, and increased but still limited bunching after the implementation period. As suggested by the raw data, the bunching mostly happens within 6 months after the 5-year holding period threshold. We then use bunching estimation to determine the corresponding hole and show the evidence of tax avoidance behavior, though the excess bunching is statistically insignificant. For the DID analysis on tax evasion, we find that tax evasion in the treatment group is increased by 23.3% after the implementation, relative to the control group. If we take the tax evasion level before the policy as a benchmark, our estimate indicates that the capital gains tax increase leads to an increase in the actual-registered price gap by approximately 192,453 *yuan* (about 30,890 USD) in the treatment group after the implementation. Our results survive from the test of the parallel trend of the treatment and control group, different sets of fixed effects, and the falsification test using the announcement period as the treatment period. We also address the selection into transaction problem using a Heckman two-stage procedure, and deal with after-implementation composition change of the treatment and control groups using the Lee bounds estimate (Lee, 2009). The results remain consistent. Our results also hold for other major cities.

How does this leeway of tax reporting affect different types of buyers? In particular, lower registered price (i.e., more tax evasion) would restrict the capability of mortgage financing because mortgage issuance is based on the valuation of the house, which is usually highly correlated with the *registered price* (instead of the actual price). As a result, the marginal cost of mortgage increases after the policy because buyers need to pay higher taxes for each additional *yuan* that they report to the tax authority. Thus, buyers who can seek alternative funding sources (e.g., borrowing from family members or friends or credit loans) would choose to do so and apply for smaller mortgage loans from the formal financial institutions. Hence, the policy unintendedly reduces the financing capacity of loan buyers from formal financial institutions. In fact, we find that the share of loan buyers buying the treated units is reduced by about 8.4 percentage points after the policy, and the loan to actual price ratio (as a proxy for the loan to value ratio) is reduced by 8.1 percentage points for the loan buyers buying the treated units after the policy. For cash buyers, they do not have to borrow from the bank, thus always try to minimize the registered price before and after the policy; so that the policy would not change their registered price, other things being equal. Consistently, we find that the causal impact of the capital gains tax increase on tax evasion is most pronounced for buyers who need mortgages and almost zero for cash buyers.

Our research makes three important contributions. First, we study an economically consequential market in China - the housing market, which accounted for 35.4% of total national wealth in 2015 (Piketty et al., 2019), and 11.4% of total tax revenue in 2017.[8] Second, we have precise information regarding the actual price, the reported price to the tax authorities, and the housing units' holding period to measure both tax avoidance and tax evasion behaviors *directly*, instead of using imputed values. With the aid of the holding period information, we can consider tax avoidance. Then, these data, along with the policy shock that differentially increased capital gains taxes for some residential property sellers, allow us to have a clean identification strategy to study the causal impact of the capital

[5] Please see full report available at http://www.oecd.org/ctp/crime/realestatesectortaxfraudandmoneylaunderingvulnerabilities.htm.

[6] The stamp duty for housing resale transaction was exempted during our sample period. Please see http://www.chinatax.gov.cn/n810341/n810765/n812171/n812685/c1191154/content.html.

[7] Strictly, the government implemented more stringent enforcement of the capital gains tax, which then led to a higher tax rate, because the parties involved in the transaction can no longer use an alternative method to calculate the tax amount, which, in most cases, results in a much lower amount. We discuss the details in the policy background section.

[8] Please see the full report available at http://gks.mof.gov.cn/tongjishuju/201801/t20180125_2800116.htm.

gains tax increase on tax evasion behavior. Third, our results show that cash buyers in the housing market, who are typically richer, evade more taxes than loan buyers do, and are less impacted by anti-speculation tax policies. Our study has direct implications for the discussion on growing wealth inequality (Piketty et al., 2019) and intergenerational mobility (e.g., Chetty et al., 2017). Incorporating the fact that the rich are more capable of evading more taxes due to their lower financial constraints may further exacerbate the estimated wealth inequality.

Our research contributes to two strands of literature. First, our study sheds light on the tax evasion literature. Previous works empirically document behavioral responses, especially tax evasion, to tax policy changes (Chetty, 2009; Merriman, 2010; Kleven et al., 2011; Balafoutas et al., 2015; Hanlon et al., 2015; Feldman et al., 2016; Rees-Jones, 2018; Waseem, 2018). A novelty of our study is that we simultaneously consider tax avoidance and tax evasion responses to a tax policy change and the interaction between these two responses. The fact that we find limited engagement in tax avoidance could be explained by the low (if not zero) cost of evading taxes. Based on our knowledge, Alstadsæter et al. (2018) is the only empirical paper that considers the interaction of tax avoidance and tax evasion and finds limited substitution between these two.

Second, our study is related to the growing literature on the outcome of government interventions in the housing market (Gervais, 2002; Chambers et al., 2009; Brogaard and Roshak, 2011; Agarwal et al., 2011; Cho and Francis, 2011; Dynan et al., 2013; Agarwal et al., 2015; Floetotto et al., 2016; Fu et al., 2016; Hembre, 2016; Agarwal et al., 2017; Agarwal and Qian, 2017; Agarwal et al., 2018; Best and Kleven, 2018; Deng et al., 2018; Sommer and Sullivan, 2018; Zhou, 2018; Berger et al., 2020). Our study adds to this strand of research by documenting the impact of tax evasion on the outcome of tax policy instruments. Specifically, we are among the first to document the extent of tax evasion in the real estate market and to show how tax evasion affects the outcome of market interventions.

The rest of the paper is structured as follows. Section 2 introduces the policy background. Section 3 discusses our unique dataset and identification strategies in detail. Section 4 presents the results and Section 5 concludes.

2. Policy background

2.1. Transaction taxes for housing resales in China

The major Chinese cities, especially the superstar cities, such as Beijing, Shanghai, and Shenzhen, have experienced a remarkable housing price surge in the past decade (Fang et al., 2016; Wu et al., 2016). Taking our sample city as an example, Fig. 1 adopts the constant-quality index by Wu et al. (2014) to depict the monthly housing price change between 2006 and 2015. The accumulative nominal housing price growth during this decade reached 550%, or an average compound monthly increase rate of 1.6%. There were several peculiar boom periods. In particular, as a result of the Chinese government's stimulus package after the Global Financial Crisis, the housing price more than doubled in 2009 and 2010, as depicted in Fig. 1. Such a dramatic housing price surge leads to concerns about the potential consequences, such as affordability problems and bubbles (Song and Xiong, 2018; Li et al., 2020).

Accordingly, the Chinese government has frequently implemented interventions in the housing market (Fang et al., 2016; Song and Xiong, 2018), in most cases with the explicit goal of "reining housing price surge." The housing transaction tax for housing resales has played a key role in the interventions. As listed in Table 1, four types of taxes are applied to housing resales in urban China, namely, stamp duty, deed tax, sales tax, and capital gains tax. Except for stamp duty, which is exempted for our sample period, these tax rates vary with the holding period (i.e., duration between the current transaction and the previous transaction of the unit), unit size, or the number of dwelling units owned by the seller. To curb housing speculations, the tax rate is substantially higher for frequent resales with short holding periods: if a unit is resold within 5 years, the resale transaction is subject to an additional 5.6% of the total price as the sales tax, and 20% of the realized capital gains as the capital gains tax. Meanwhile, larger units are subject to a higher deed tax rate and even a higher sales tax rate.

It is important to note that, as a common practice in China's housing resale markets, all the transaction taxes of a resale, even are legally levied on the seller according to Table 1 (e.g., the capital gains tax and the sales tax), are in practice out-of-pocket expenses by the *buyer*.[9] The seller would not "reimburse" the buyer via cash or explicit deductions in the transaction price. However, such an additional tax burden for the buyer, including its change, should have been considered in the bargaining process and thus reasonably reflected in the transaction price, because there is prior knowledge of such an arrangement by both the seller and buyer, especially via the advice of professional brokers.

For a specific housing resale transaction, all these four types of transaction taxes are calculated based on the total price of the unit (or the realized capital gains, i.e., the increment in total price between the previous and current transactions) registered with the local housing authority. Therefore, underreporting the total price of the current transaction becomes the most direct way for the transaction parties to partially evade these transaction taxes. This practice introduces the so-called "dual contract" phenomenon into most housing resale transactions in urban China. Specifically, the buyer and seller first sign an actual contract with the help of a broker, which records the actual total price, or the so-called *yin* (translated as "under the table") price in Chinese, of this transaction. Then the buyer and seller need to register this transaction in the online system of the local housing authority. In most cases, the buyer and seller choose to report a substantially lower total price, or the so-called *yang* (registered) price, on this official registration. The buyer makes the payment to the seller according to the actual price, but the transaction taxes are calculated and paid based on the registered price. By adopting this "dual contract" strategy, the transaction parties (specifically, the buyer) can evade the transaction taxes on the gap between the registered and actual total prices of this transaction.[10]

The local tax authorities are aware of the "dual contract" phenomenon and have made some efforts to reduce such tax evasion. Typically, the local tax authority of a city divides the urban area into several submarkets and sets a minimum unit price (i.e., *yuan* per square meter) for each submarket. For any resale transaction, if the registered price submitted is lower than the minimum required price in the corresponding submarket, this registration application is rejected, and the transaction parties need to submit a new registration application with a revised

[9] For instance, supposing that the total transaction price for a resale unit is 5 million *yuan* RMB or 0.8 million USD (100 square meters, with the holding period less than 5 years and realized capital gains of 2 million *yuan* or 0.3 million USD), without any tax evasion, the total transaction tax would be 75,000 *yuan* (about 12,038 USD) for the buyer and 680,000 *yuan* (about 109,144 USD) for the seller, respectively. Besides transferring 5 million *yuan* of cash (or via mortgage loan) to the seller and submitting 75,000 *yuan* of the buyer-side transaction tax to the local tax authority, the buyer also needs to submit 680,000 *yuan* of the seller-side transaction tax to the local tax authority on behalf of the seller. There is no conclusive evidence on why transaction participants in China's resale housing market choose to adopt this convention. One widespread explanation is that, controlling for other factors, transaction participants have the incentive to minimize the transaction price that appears on the (actual) contract because the brokerage fee is calculated as a given percentage (1–2.7%) of the total transaction price. At the same time, unlike in many other countries, such as the US, where the capital gains tax is filed at the end of the tax year by the party that receives the gains, the capital gains tax associated with a housing resale transaction is paid simultaneously with the completion of the transaction in China.

[10] More specifically, in the under-the-table contract, the buyer and seller would claim that this transaction includes two parts: the transaction of the residential unit, with the total price equaling the registered price, and the transaction of the affiliated furniture, with the total price equaling the gap between the actual and registered prices. In this way, the seller (or buyer) can still use the under-the-table contract to protect her rights if the transaction parties have any disputes during or after the transaction. This is also why the buyer and seller need to decide the registered price when they sign the actual contract.

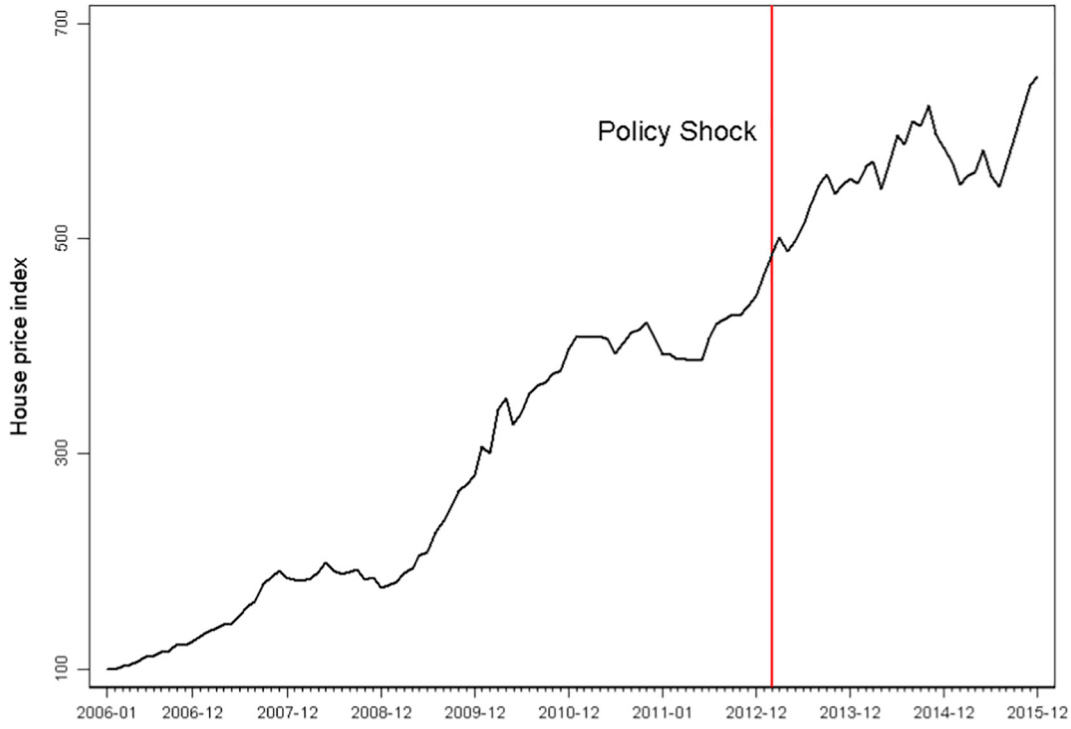


Fig. 1. Monthly constant-quality housing price index in the sample city, 2006–2015. The methodology for constructing the constant-quality housing price index is available in Wu et al. (2014).

price. Three points are worth noting here. First, in order to avoid too many disputes and complaints, typically, the local tax authority chooses initially to set the minimum required prices below the market level. In addition, these criteria are not frequently updated, even during boom periods. Thus, in most cases, the minimum required prices are far below the market level. Second, the local tax authority does not publicly release the submarket-level minimum required prices. However, the brokers, based on their experiences on registering resale transactions, are able to obtain estimates on these criteria and advise buyers/sellers accordingly. Finally, at least in our sample city, even if a registered price submitted was believed to be too low and thereby rejected, there would be *no* additional penalties for its seller, buyer, or broker. One may ask why local

Table 1
Summary of transaction taxes for housing resales in the sample city.
Information collected by the authors from official sources.

	Before Mar 30, 2013		Since Mar 31, 2013	
	Buyer	Seller	Buyer	Seller
Stamp duty (exempted)	0.05% of the total price	0.05% of the total price	0.05% of the total price	0.05% of the total price
Deed tax	• 1% of the total price for the first home under 90 square meters. • 1.5% of the total price for the first home between 90 and 140 square meters. • 3% of the total price for all other cases.	N.A.	• 1% of the total price for the first home under 90 square meters. • 1.5% of the total price for the first home between 90 and 140 square meters. • 3% of the total price for all other cases.	N.A.
Sales tax	N.A.	• 5.6% of the total price if the holding period since the previous transaction was less than 5 years. • 5.6% of the realized capital gains since the previous transaction if: 1) the holding period since the previous transaction exceeded 5 years, and 2) the unit was larger than 140 square meters. • 0% for all other cases.	N.A.	• 5.6% of the total price if the holding period since the previous transaction was less than 5 years. • 5.6% of the realized capital gains since the previous transaction if: 1) the holding period since the previous transaction exceeded 5 years, and 2) the unit was larger than 140 square meters. • 0% for all other cases.
Capital gains tax	N.A.	• **20% of the realized capital gains since the previous transaction, or 1% of the total price**, if the holding period since the previous transaction was less than 5 years, or the seller owned more than one unit in the city. • 0% for all other cases.	N.A.	• **20% of the realized capital gains since the previous transaction** if the holding period since the previous transaction was less than 5 years, or the seller owned more than one unit in the city. • 0% for all other cases.

governments do not choose to make more efforts in enforcing transaction tax and reducing such tax evasion. While this interesting question is well beyond the scope of the current study, a possible explanation is, as revealed in several existing studies (Pan et al., 2015; Wang and Hui, 2017), local governments (*not* the central government) are reluctant to cool down the housing market, because they heavily rely on land sales revenue as a major off-budget funding source.

Given that buyers/sellers are extremely unlikely to be punished for evading the transaction taxes, the best practice is to report the lowest possible price (i.e., submarket-level minimum required price) to the tax authority. However, other constraints still exist for buyers/sellers. The registered prices not only determine the amounts of transaction taxes, but also greatly affect buyers' capability to receive mortgage financing. In China, many home buyers need to take mortgage loans from the housing provident fund (HPF) or a commercial bank, or the combination of the two, and the amount of home mortgage loan that a home purchaser can receive is quantitatively determined by three factors. First, the Peoples' Bank of China, China's central bank, has always maintained a ceiling requirement of 70% on the loan-to-value (LTV) ratio for home mortgages; that is, the amount of mortgage loan cannot exceed 70% of the total price registered.[11] Second, the monthly service, calculated based on the mortgage amount and mortgage terms, cannot exceed 50% of reported household income. Third, the home purchaser's credit score evaluated based on her characteristics and previous credit history matters. This means, in a housing resale transaction, if the buyer's mortgage application is bound by the first condition above (i.e., LTV ratio), the amount of mortgage loan that she can obtain would change proportionally with the total registered price. Therefore, the loan buyers need to make a tradeoff between lower transaction taxes and larger mortgage loans in determining the registered prices. However, cash buyers are not subject to such tradeoff and are likely to report the lowest possible registered price bound by the submarket-level minimum required price.

2.2. Policy shocks in February 2013

The Chinese government has made several adjustments to the transaction tax policies in the past decade according to housing market condition changes. In almost all cases, such adjustments were determined by the central government. In this study, we focus on a policy adjustment in February 2013.

As depicted in Fig. 1, after a stagnant period between early 2011 and mid-2012, housing prices in major cities started to increase rapidly again in the second half of 2012. As a result, the central government gradually tightened the housing market intervention policies again in early 2013. The first policy signal appeared on February 20, 2013, when the State Council held an executive meeting to discuss the housing market condition. On the same day, in a gazette of this meeting, the State Council expressed concerns about potential housing market risks and expressed strong willingness to tighten market intervention policies, including the transaction tax policy, in order to keep the housing price relatively stable, although no details about the policy adjustments were reported.

On February 26, 2013, the State Council issued the "Circular of the State Council on Further Improving Regulations of the Real Estate Market" (Decree No. 2013–17). In the decree, the State Council explicitly stated that a new round of market intervention would serve as a response to increasing expectations on housing price change, with the key policy target to "stabilize the housing price." The decree contained a bundle of more than 10 policy measures, including the specific requirements on the enforcement of the capital gains tax for housing resales, and the principle of tightened credit policy for multiple home purchase ("tightening mortgage financing for multiple home buyers") and tightened home purchase restrictions ("tightening home purchase restriction").[12] In particular (see Table 1), before this enforcement, a resale transaction, if subject to the capital gains tax due to either a short holding period or seller's multiple homeownership, could choose to calculate the amount of capital gains tax either as 20% of the realized capital gains, or 1% of the total price of the current transaction. However, after the enforcement, the capital gains tax, if applicable, can be calculated only as 20% of the realized capital gains.[13] Following this requirement, on March 30, 2013, the local housing and tax authorities in our sample city announced that the capital gains tax enforcement would be implemented from March 31, 2013.[14]

In the context of our sample city, which experienced huge housing price appreciation in the years before as shown in Fig. 1, this tax enforcement led to a striking increase in the tax rate of capital gains tax, and thus, the total tax burden of housing resales. Assume that a household purchased a dwelling unit of 100 square meters in this city for 2 million *yuan* (about 0.3 million USD) in March 2009 and resold it in March 2013 (i.e., with a holding period of 4 years). According to the constant-quality housing price index in Fig. 1, the accumulative housing price growth in the sample city during this interval reached 165.2%; that is, the unit could be expected to sell for approximately 5.3 million *yuan* (about 0.9 million USD) in March 2013. Since the holding period was less than 5 years, this resale transaction was subject to the capital gains tax. According to the taxation code before March 30, 2013, the buyer could choose to calculate the capital gains tax as 1% of the total price of the current transaction (i.e., about 53,000 *yuan* or 8507 USD). However, following the new code effective since March 31, 2013, the capital gains tax can be calculated only as 20% of the realized capital gains, which would reach as high as 660,000 ((5.30–2)*20%*1,000,000 = 660,000) *yuan* (about 105,934 USD). As a result, the total tax burden of this resale transaction would sharply increase from about 429,300 *yuan* (about 8.1% of the total price; 68,905 USD) to about 1,036,300 *yuan* (about 19.6% of the total price; 166,332 USD). Obviously, such a dramatic increase in transaction tax rate provides strong tax avoidance/evasion incentives for participants of resale transactions with holding periods less than 5 years. By contrast, housing resales with holding periods exceeding 5 years are exempted from the capital gains tax and thus are not affected by this policy change.

More specifically, home buyers and sellers of resale transactions with holding periods less than 5 years may respond to the tax rate increase in two ways. First, buyers and sellers may respond to the threshold of tax rate change by postponing their transactions (i.e., seller waits for the holding period of her house to exceed 5 years before she sells the house or completes the transaction with the buyer). We label such behavior as "tax avoidance." Second, buyers may report a lower registered price to evade more taxes after the tax rate increase, and we label such behavior as "tax evasion."

[11] More specifically, the HPF center or commercial bank would hire a professional real estate appraiser to provide an appraisal report for the dwelling unit, and then adopt the lower value of the registered total price and the appraised total price to calculate the amount of the mortgage loan. With the existence of a "dual contract," in most cases, the registered total price is lower than the appraised total price of the unit, and thus the loan value is determined by the registered price. By contrast, if the buyer overreported the registered price, the loan value would be determined by the appraised price. Thus, in most cases, buyers cannot get more loans simply by reporting higher registered prices.

[12] See http://www.gov.cn/zwgk/2013-03/01/content_2342885.htm for more details of the decree. Besides the enforcement of the capital gains tax, none of the other measures affected the transaction taxes of housing resales, or imposed other different treatments on resale transactions with different holding periods. The decree did not provide requirements on the implementation date of the capital gains tax enforcement, or the policy details or the implementation date of the tightened credit policy or the home purchase restriction policy.

[13] Legally, before this policy change, using 1% of the registered total price as the capital gains tax was allowed only when the registered price of the previous transaction could not be found. However, sellers usually lied to the tax authority by stating they could not find the original invoice—even if the buyer and seller involved in a transaction did not realize this, the broker would advise them to do so. However, after this policy change, if a seller fails to provide the original invoice of the residential unit, the tax authority directly adopts the transaction price of the previous transaction recorded in the official registration system of the local housing authority. Please see http://www.chinatax.gov.cn/chinatax/n363/c1339/content.html for details.

[14] The same announcement also includes details on the tightened credit policy (higher downpayment requirement for buyers purchasing a second unit) and tightened home purchase restrictions (prohibiting unmarried local buyers purchasing the second unit) in our sample city.

3. Data and empirical strategy

3.1. Data

While we will also generalize the analysis to all the 35 major cities in China in Section 4.4, in the main analysis we adopt one of these major cities as the sample, only because we are able to get access to several proprietary datasets in this sample city. As the major data source, we collect micro-level housing resale transaction data from one of the largest housing brokerage firms in our sample city,[15] covering all the 35,428 resale transactions within this brokerage company between January 1 and September 26 (i.e., 180 days after the effectiveness of the policy change on March 31), 2013.[16] For each resale transaction, we have access to both its actual price and the registered price that the buyer and seller agree upon when signing the contract. Thus, we can directly calculate the magnitude of tax evasion for each transaction.

Besides the actual and registered prices, the dataset provides detailed information on the housing attributes, mortgage usage, and a few demographic variables of the buyers and sellers, including their age, gender, and birth place. We can also observe the dates when the buyers and sellers signed the actual contracts. In addition, we merge the data with the local Housing Provident Fund contribution data and acquire information on buyers' and sellers' employer type and reported income for some observations. Moreover, we manually merge the above resale transaction dataset with the official housing transaction registration data according to the addresses of the transacted units, enabling us to check the registered price data in our dataset against the prices finally recorded in the official system. The definitions of the variables are listed in detail in Appendix Table C.1.

Additionally, we have information on whether the holding period of a unit exceeded 5 years before the current transaction, which helps us to identify resale transactions subject to the capital gains taxes.[17] In order to pin down the exact holding period information of each transaction, we manually collect the date of the previous transaction of the unit from the local housing registry office. Among 35,428 transaction records, we are able to obtain the exact dates of the last transaction for 7614 records and thus calculate the length of their holding periods. For 20,736 records, we can infer that their holding periods are longer than 7 years but are unable to calculate the exact length of their holding period.[18] For the rest 7078 transactions, we have no information on their holding period and thus exclude them from our analysis. In the actual analyses, we also drop the transactions with non-positive actual-registered price gaps (846 observations) to do the log transformation, and drop the units with extreme price gaps (top and bottom 1%) and missing hedonic characteristics. Moreover, we drop 59 observations in week 6 of the year 2013 due to the Chinese New Year holiday as the transactions completed in the week could be unusual. Finally, the "Full Sample" contains 26,578 housing resale transactions. Details about our sample selection are described in Appendix Table C.2.

3.2. Empirical strategies

As mentioned in Section 2.2, the policy shock may induce "tax avoidance" and "tax evasion" behaviors. We use bunching estimation to identify "tax avoidance" behavior and use a DID design to identify "tax evasion" behavior. For the purpose of these two different analyses, we also make use of different subsets of our sample.

The bunching analysis is used to identify the tax avoidance behavior of market participants. In other words, we could see a hole below the notch and a bunching above the notch because waiting for the holding period to exceed 5 years could save the buyer on tax payment. When the tax policy changes, sellers' and buyers' incentive to respond to such threshold also changes, so we expect to see more avoidance when the capital gains tax rate increases at the threshold. Following Best and Kleven (2018) and Kleven and Waseem (2013) and grouping transactions into holding period bin of one month, we have

$$vol_i = \sum_{j=0}^{q} \beta_j (z_j)^j + \sum_{k=v_-}^{v_+} \gamma_k I\{i = k\} + \mu_i \tag{1}$$

where vol_i is the transaction volume in holding period bin i; z_j refers to the distance between holding period bin i and the cutoff v (i.e., 60 months); q is the order of the polynomial (in our context, $q = 7$); we exclude a region (v_-, v_+) around the notch; μ_i represents the error term. Following Kleven and Waseem (2013)'s strategy, the upper bound v_+ is set at where the excess bunching ends, while the lower bound v_- is set to ensure the excess bunching is close to the missing mass as much as possible. The counterfactual distribution is predicted based on Eq. (1) without the term $\sum_{k=v_-}^{v_+} \gamma_k I\{i = k\}$. We further calculate the excess bunching above the notch as $B = \sum_{i=v}^{v_+} (vol_i - \widehat{vol}_i)$. Moreover, we calculate the excess bunching scaled by the average counterfactual frequency in the excluded range as $b = B / \left(\sum_{i=v_-}^{v_+} \widehat{vol}_i / (v_+ - v_-) \right)$. As a robustness check, we also estimate the counterfactual distribution using bins of two months wide. Following Chetty et al. (2011), we obtain the standard error by bootstrapping the above procedures 200 times.

The bunching estimation requires information on the exact holding period of each unit to calculate the distance to the 5-year threshold. Therefore, we can only make use of the transactions with accurate holding period information (i.e., housing units with holding periods less than 7 years). To estimate the higher-order polynomial, we keep the same lengths before and after the threshold by including all transactions with a holding period between 3 and 7 years. Appendix Table C.2 explains the definition of the subsample used for the bunching analysis, i.e., the "Tax Avoidance Sample."

We then conduct the DID analysis to study the impact of the tax rate increase on tax evasion behavior in the housing resale market, using the change in tax policy enforcement in February 2013 as a quasi-natural experiment. In principal, for our DID analysis, the treatment group should include all the resale transactions with holding periods less than 5 years, which were directly affected by the capital gains tax increase; the resales with holding periods over 5 years, which were exempted from the capital gains tax and thus were not affected by the tax policy change, serve as the control group. However, instead of using the whole sample for the DID analysis, we use those with holding periods shorter than 4 years or longer than 6 years because we need to ensure that the assignments to the treatment and control groups are not subject to manipulation by delaying transactions due to tax avoidance behavior. Appendix Table C.2 explains the definition of the subsample used for the DID analysis, i.e., the "Tax Evasion Sample."

As introduced in Section 2, the information on this capital gains tax enforcement was first released to the media on February 20, 2013, formally announced by the central government on February 26, and finally implemented in our sample city on March 31. Accordingly, we define the days on and before February 19 as the before-announcement period, the days between February 20 and March 30 as the announcement

[15] The market share of the brokerage firm in our dataset is 37.2% at the beginning of 2013.

[16] In part of the following empirical analysis, we also adopt an extended sample period between January 2013 and December 2015, including totally 115,853 resale transactions.

[17] As listed in Table 1, besides units with holding periods less than 5 years, units whose sellers own multiple homes are also subject to the capital gains tax. Unfortunately, we have no information on sellers' ownership in the dataset and thus cannot fully identify the treatment group according to the length of holding period; that is, some units currently classified in the control group actually belong to the treatment group. Therefore, we would have a more conservative estimate in the DID models. As a reference, the city-level statistics on the share of sellers owning multiple homes is available in our sample city in 2014, and multiple homeowners accounted for 30.85% in all the sellers in the resale housing market.

[18] The local housing registry office in our sample city only started to digitalize the records of housing transaction registrations since the beginning of 2006. For a transaction occurred before 2006, we can only know the year (instead of the exact month or date) of the transaction; therefore, for these 20,736 records, we can infer that their holding periods are longer than 7 years in our sample period of 2013, but cannot accurately infer the exact holding periods.

period, and the days after March 31 as the after-implementation (after) period. It is worth noting that the actual implementation date of the new policy was not released to the public until March 30, even though the content was released before that. Also, it usually takes a few weeks to register with the tax bureau and pay taxes after the transaction date (which is the date available to us in our sample). Therefore, even if the transactions took place in the announcement period, it is also possible that such transactions might be subject to the new policy if the time that the buyer reported to the tax bureau was after March 31. Hence, in the DID analysis, we exclude all observations in the announcement period because it is hard to clearly classify these transactions as affected or unaffected by the policy. Following this identification strategy, we have

$$Y_{i,j,t} = \beta_1 \times Treat_i \times After_t + \beta_2 \times Treat_i + \beta_3 \times After_t + X_i + \alpha_j + \delta_t + \varepsilon_{i,j,t} \quad (2)$$

where $Y_{i,j,t}$ refers to the outcome variable associated with unit i in complex j transacted on date t; specifically, here we focus on the actual-registered price gap, as a measure of tax evasion, as well as the registered total price and actual total price, all of which are in log form; $Treat_i$ is the dummy variable for the treatment group; $After_t$ is the dummy for the after period; X_i refers to a set of unit-level hedonic attributes; we also control for complex-level fixed effects, α_j, and transaction time fixed effects, δ_t, including year by month fixed effects, day of week fixed effects, and holiday fixed effects; and $\varepsilon_{i,j,t}$ is the error term. The parameter of interest is β_1, which represents the effect of the capital gains tax increase on the outcomes of the treatment group relative to the control group. Given that the transactions are correlated within the same complex or the same date, the standard errors are two way clustered at the complex-day level (Cameron and Miller, 2015).

Similarly, we study the impact of the tax rate change on transaction volume in the market. Specifically, we have two daily-frequency time series for the number of units sold (i.e., signing the actual contacts) in the treatment and control groups, respectively, and then, we apply the DID model to the daily aggregated data:

$$Vol_{k,t} = \gamma_1 \times Treat_k \times After_t + \gamma_2 \times Treat_k + \gamma_3 \times After_t + \delta_t + \varepsilon_{k,t} \quad (3)$$

where $Vol_{k,t}$ refers to the logged number of units sold (i.e., signing the actual contacts) on day t in the treatment ($k = 1$) or control ($k = 0$) group; $Treat_k$ refers to the transaction volume in the treatment group; and the other variables are the same as for Eq. (2). The robust standard error is used for this analysis.

4. Results

In this section, we first provide stylized facts to show the existence of tax avoidance and tax evasion in the housing market of our sample city. We then demonstrate how the policy change (i.e., capital gains tax rate increase) affects the tax evasion using both graphical evidence and regression analysis, followed by a set of robustness checks and discussion of the external validity of our main results. The last part of the section focuses on heterogeneity analyses, especially the different ways of financing (i.e., cash vs. mortgage loan).

4.1. Motivating facts

Fig. 2 shows the prevalence of tax evasion and tax avoidance in our sample city. Panel A of the figure plots the density of the actual-registered price gap (i.e., the absolute gap between the registered total price and actual total price; in thousand *yuan*) of all the transacted units during the sample period between January 1, 2013 and September 26, 2013. It is evident that the registered price is lower than the actual price in almost all transactions. Panel B of the same figure plots the volume of each holding period of transacted units with holding periods more than 3 years but less than 7 years. In the graph, it can be seen that the transaction volume with holding period just above 5 years is slightly higher than the volume with holding period just below 5 years, which provides weak evidence of bunching close to the 5 years cutoff. Panel B of Fig. 2 also plots the actual-registered price gap (bars, in thousand *yuan*). Overall, there does not seem to be a discontinuous change in tax evasion near the 5 years cutoff. However, please note that Panel B of Fig. 2 plots the full sample instead of the subsamples for whom the bunching incentives are the strongest. We will plot similar figures for these subgroups in Section 4.2 and Section 4.5.

Our rich dataset also allows us to calculate the tax evasion amounts for different groups of buyers or units. Appendix Table A.1 provides such information before the policy announcement. On average, cash buyers evade 1,199,190 *yuan* per transaction in contrast to 696,160 *yuan* for loan buyers. In terms of the income groups, we divide the before policy sample by the average of income. The high-income group evades a bit more at 887,420 *yuan* versus the low-income group at 730,650 *yuan*.[19] Lastly, dividing by the median of unit size, buyers purchasing larger units evade more at 1,054,200 *yuan* versus those buying smaller units at 714,640 *yuan*.

Table 2 provides the summary statistics of the housing transactions in the Full Sample (from January 1, 2013 to September 26, 2013). The actual-registered price gap (962,700 *yuan* or 154,519 USD) and register/actual price ratio (63%) both show the prevalence of the tax evasion during our sample period. Table 2 also provides summary statistics for other important variables. Among all the transactions in our sample, 23% transacted housing units with holding periods less than 5 years. The average unit size is 80.21 square meters with about 2 bedrooms in each unit and an average building age of 14 years. In terms of the buyers' characteristics, 36% of all buyers pay the full amount using cash. About 55% of the buyers are males and 37% are local buyers. The average buyers' age in our sample is 34.76. For sellers, about 56% are male sellers and about 63% are local sellers. The sellers' average age is 46.85 in our sample.

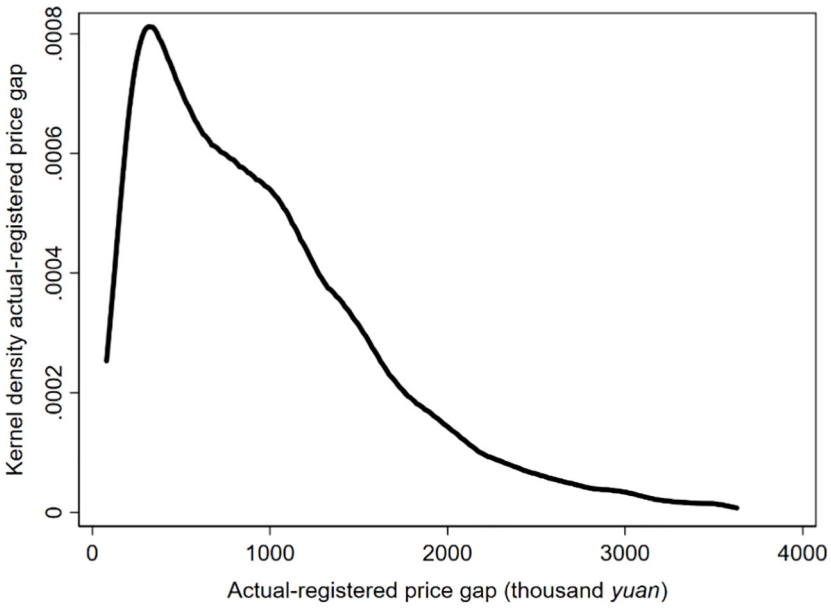
4.2. Policy impact

To identify the policy effect, we employ the bunching and DID research design as previously discussed. First, we analyze the changes in the holding period in response to the policy change. Fig. 3 plots the density of holding periods of transacted units for three different groups—before announcement, announcement period, and after implementation—using the timeline of the policy change. We use the Epanechnikov kernel to smooth the distribution for better visualization. The figure shows little bunching above 5 years for the transactions before the policy announcement and during the announcement period, and increased bunching after the implementation of the policy.[20] Besides, we also replicate Panel B of Fig. 2 using three different subsamples in Appendix Fig. A.1, and confirm the above findings using the raw data. Using a bunching estimation with the after-implementation subsample, we present the actual and counterfactual distributions using different bin sizes in Appendix Fig. A.2. There are two major observations. First, it can be visualized from the figure that the hole happens within 6 months below the threshold, which is of the same length of the time span as the bunching. Second, using bins of one month wide to estimate the counterfactual distribution, the excess bunching above the threshold is 0.50 times the height of counterfactual distribution in the excluded range (59.54) but is statistically insignificant (with a standard error of 0.85) after the policy implementation.[21] There are two possible major reasons for this lack of bunching in a broader range. Firstly, sellers and buyers can easily evade tax by reporting much lower registered price without any risk of being caught. Secondly, the housing price in our sample city was changing rapidly during our sample

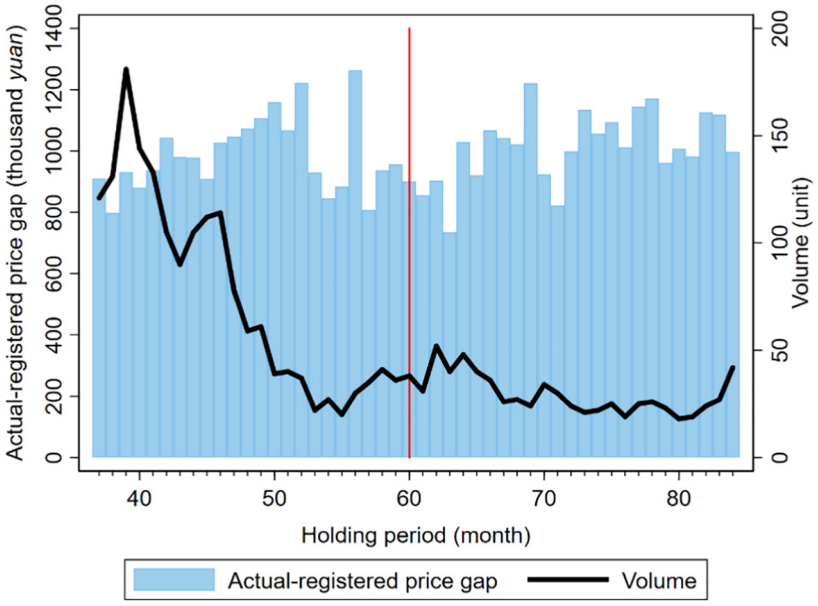
[19] Note that we only have income data for a subset of the total observations.

[20] It is less clear to interpret the results in the announcement subsample, because the transactions took place in the announcement period were also possible to subject to the new policy as discussed in Section 3.2.

[21] Appendix Table C.3 provides summary statistics for the Tax Avoidance Sample. We cannot find a significant bunching here possibly due to the small sample size, while bunching estimate requires large sample size (Kleven, 2016).



A: Distribution of the actual-registered price gap



B: Average actual-registered price gap and daily transaction volume by holding period

Fig. 2. Tax evasion and tax avoidance in the sample city. Panel A is based on the Full Sample, while Panel B comes from the Tax Avoidance Sample. The Epanechnikov Kernel is applied with optimal bandwidth in Panel A. The vertical line in Panel B denotes the 5-year holding period.

period, so the sellers and buyers were unwilling to delay their transactions for too long to avoid too much exposure to price volatilities.

We then move to the policy impact on tax evasion using the DID estimate. As discussed above, unlike typical DID research designs, the treatment and control groups in our study can change due to increased bunching incentives when the housing unit's holding period falls short of 5 years but is close enough to it. To address this issue, we take a conservative method to use the transactions with holding period less than 4 years or more than 6 years for the DID analysis, excluding those transactions with holding period close to 5 years because the bunching and hole are locally concentrated around holding period of 5 years as described above.[22]

Before moving to the regression analysis, we first describe the summary statistics of the Tax Evasion Sample in Table 3. To understand the dynamics of the policy announcement and implementation better, we divide our sample period into three sub-periods. The first sub-period (before-announcement period) has 43 days between January 1, 2013, the date that our sample starts, and February 19, right before the preliminary information of the policy change was released to the public.[23] The second sub-period (announcement period) has 39 days between February 20, when the tax enforcement was announced, and March 30, the date before it was implemented in the sample city. The third sub-

[22] We also employ other methods to address this concern in Section 4.3.

[23] We exclude the 59 observations in week 6 due to the Chinese New Year holiday in the following analysis.

Table 2
Summary statistics of the Full Sample.
The summary statistics are calculated by the authors from the Full Sample. See Table C.1 for the definitions of the variables. We exclude the 59 observations in week 6 due to the Chinese New Year holiday. We do not report the summary statistic of exact holding period information because 73% of transactions only have partial information on holding period.

Variable	Obs	Mean	Std. dev.	Min	Max
Panel A: transaction information					
a_totalprice	26,578	2535.62	1147.15	200	23,000
r_totalprice	26,578	1572.93	765.76	68.40	20,000
Agap	26,578	962.70	668.29	80	3630
Ratio	26,578	0.63	0.16	0.03	0.97
Lastprice	7402	800.86	1265.53	0.00	97,483.34
Panel B: housing attributes					
Treat	26,578	0.23	0.42	0	1
Unitsize	26,578	80.21	32.69	12	464.38
Floor	26,578	7.03	5.81	1	37
Bedroom	26,578	1.97	0.74	0	7
Builtyear	26,578	1999.09	7.59	1922	2013
Center	16,684	13,589.65	7288.44	1109	70,147
Subway	16,684	1226.02	1442.97	54	37,740
Panel C: buyer characteristics					
cashbuyer	26,578	0.36	0.48	0	1
b_gender	26,578	0.55	0.50	0	1
b_local	26,578	0.37	0.48	0	1
b_age	26,578	34.76	10.27	18	90
b_income	12,296	51,956.77	31,009.75	2425	584,100
b_public	12,296	0.21	0.41	0	1
b_period	24,876	20.80	36.21	0	534
b_hpf	26,578	0.46	0.50	0	1
Panel D: seller characteristics					
s_gender	26,578	0.56	0.50	0	1
s_local	26,578	0.63	0.48	0	1
s_age	26,527	46.85	13.85	18	90
s_income	6898	55,918.96	44,481.84	1625	1,054,850
s_public	6897	0.22	0.41	0	1
s_hpf	26,578	0.26	0.44	0	1

period is 180 days right after the policy implementation on March 31. In all but one of the analyses, we focus on the policy effect in the long run and report the same results for the short run (50 days right after the policy implementation) in Appendix B. On top of the three periods, we further divide the sample into treatment and control groups to examine different dynamics in the two groups.

For the treatment group (i.e., sellers' holding period less than 5 years), the actual total price of the treated transactions increase from 2.16 million *yuan* (about 0.35 million USD) in the before announcement period to 2.38 million *yuan* (about 0.38 million USD) in the after implementation period leading to an increase of 10.37%. The registered total price, on the other hand, remains largely unaffected (1.33 million *yuan*; around 0.2 million USD). In terms of the transaction volume, we see a huge drop of 45.00% from a daily mean of 28.60 units to 15.73 units. While for the control group, the actual total price increased from 2.40 million *yuan* (about 0.39 million USD) to 2.67 million *yuan* (about 0.43 million USD) which is an 11.16% increase. The registered total price increased from 1.53 million *yuan* (about 0.25 million USD) to 1.70 million *yuan* (about 0.27 million USD). Comparing the registered price between the treatment and control groups, it is evident that the relative decrease in registered price in the treatment group is substantially larger than the control group. In addition to prices, the volume of the control units decreases from 73.40 units to 64.79 units after the policy implementation, which is a much more moderate reduction of 11.73%. The actual-registered price gap and registered/actual price ratio both show more tax evasion with the gap increased and ratio decreased much more in the treatment group than the control group in response to the policy change.

We also provide summary statistics on other variables for the treatment and control groups separately for the three different periods. In general, buyers cope with the policy change by purchasing housing units with lower quality (i.e., smaller in size, fewer bedrooms, older buildings) and are more likely to pay the full amount with cash, especially in the treatment group. In terms of the buyers' and sellers' characteristics, we do not observe much change except that the percentage of local buyers decreases and buyers have slightly higher incomes after the policy change.

In addition to summary statistics, Panel A of Fig. 4 plots the distributions of the actual-registered price gap, where we divide the Tax Evasion Sample further into four different groups – treatment & before, treatment & after, control & before, and control & after. As shown in Panel A, the treatment & after group is to the right of the other three groups, suggesting that the policy changes the tax evasion in the treatment group substantially but leaves the control group largely unaffected. In Panel B of Fig. 4, we plot the weekly average actual-registered

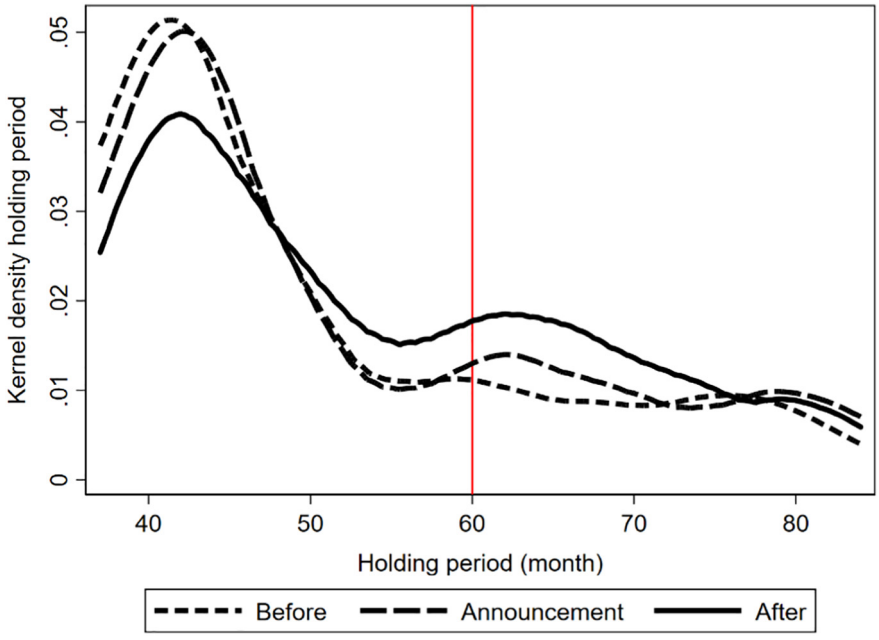


Fig. 3. Distribution of the holding period: By different periods. We define the days between January 1 and February 19 as the before-announcement period, the days between February 20 and March 30 as the announcement period, and the days between March 31 and September 26 (180 days) as the after-implementation period (long-run). The vertical line denotes the 5-year holding period. Authors' calculation from the Tax Avoidance Sample. The Epanechnikov Kernel is applied with optimal bandwidth.

Table 3
Summary statistics of the Tax Evasion Sample: By treatment and control groups and by different periods.
We define the days between January 1 and February 19 as the before-announcement period, the days between February 20 and March 30 as the announcement period, and the days since March 31 as the after-implementation period. For the short-run analysis, we include 50 days after March 31 as the after-implementation period; for the long-run analysis, we include 180 days after March 31 as the after period. The summary statistics are calculated by the authors using the Full Sample. See Table C.1 for the definitions of the variables. The treatment group includes the resale transactions with holding periods less than 4 years; the resales with holding periods over 6 years serve as the control group. We do not report the summary statistic of the exact holding period information because 73% of transactions only have partial information on holding period.

		Treatment				Control			
		Before	Announcement	After (long run)	After (short run)	Before	Announcement	After (long run)	After (short run)
a_totalprice	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	2160.88	2300.22	2384.93	2262.32	2401.39	2565.45	2669.32	2483.23
	Std. dev.	1082.62	1152.77	1187.70	1165.44	1097.48	1178.39	1114.61	1066.78
r_totalprice	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	1334.91	1348.25	1331.42	1230.84	1526.14	1577.12	1702.15	1560.66
	Std. dev.	678.96	721.60	701.89	627.60	726.99	804.07	751.30	704.56
Volume	Obs	43	39	180	50	43	39	180	50
	Mean	28.60	42.85	15.73	11.02	73.40	133.03	64.79	47.56
	Std. dev.	15.86	21.32	8.60	6.87	40.07	70.37	32.67	25.52
Agap	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	825.98	951.97	1053.51	1031.49	875.25	988.33	967.18	922.58
	Std. dev.	612.00	679.21	649.87	671.44	623.54	694.06	674.35	678.33
Ratio	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	0.64	0.60	0.57	0.56	0.65	0.63	0.65	0.65
	Std. dev.	0.16	0.17	0.14	0.14	0.15	0.17	0.16	0.17
Lastprice	Obs	1230	1671	2831	551	105	202	525	76
	Mean	778.77	812.88	955.02	946.69	578.27	612.97	551.79	566.44
	Std. dev.	582.96	2432.70	653.88	611.63	241.21	320.18	226.34	239.87
Unitsize	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	82.24	80.62	78.76	81.35	83.90	81.45	78.78	79.06
	Std. dev.	36.88	35.85	34.40	36.58	33.28	34.00	30.10	30.33
Floor	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	8.10	8.51	8.44	7.82	6.64	6.72	6.53	6.44
	Std. dev.	6.60	6.59	6.60	6.60	5.51	5.61	5.39	5.43
Bedroom	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	1.83	1.80	1.77	1.85	2.07	2.03	2.00	2.02
	Std. dev.	0.82	0.78	0.79	0.81	0.75	0.74	0.70	0.70
Builtyear	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	2002.92	2003.56	2004.35	2004.05	1998.18	1997.99	1997.36	1997.58
	Std. dev.	6.54	6.38	6.74	7.16	7.21	7.17	7.39	7.42
Center	Obs	720	920	1529	307	2041	3393	7563	1568
	Mean	14,942.81	14,735.26	14,830.97	15,445.43	13,282.88	13,188.28	13,331.36	14,237.66
	Std. dev.	7839.85	7130.74	7022.06	7612.41	7456.96	7198.13	7285.98	7625.04
Subway	Obs	720	920	1529	307	2041	3393	7563	1568
	Mean	1411.50	1390.89	1360.84	1540.29	1175.10	1158.91	1198.41	1202.14
	Std. Dev.	1880.45	1394.53	1388.07	1669.62	1270.62	1321.28	1461.19	1116.31
Cashbuyer	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	0.37	0.42	0.51	0.54	0.31	0.35	0.33	0.33
	Std. dev.	0.48	0.49	0.50	0.50	0.46	0.48	0.47	0.47
b_gender	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	0.53	0.54	0.52	0.52	0.55	0.57	0.56	0.58
	Std. dev.	0.50	0.50	0.50	0.50	0.50	0.49	0.50	0.49
b_local	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	0.42	0.37	0.37	0.40	0.37	0.38	0.35	0.35
	Std. dev.	0.49	0.48	0.48	0.49	0.48	0.49	0.48	0.48
b_age	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	35.30	34.86	35.58	35.79	35.08	34.36	34.59	34.61
	Std. dev.	10.16	10.45	11.09	11.21	9.97	9.78	10.32	10.41
b_income	Obs	559	804	1189	246	1516	2507	5336	1157
	Mean	47,630.68	48,475.50	51,190.90	44,969.11	51,952.79	51,021.19	53,520.04	50,625.35
	Std. dev.	27,374.13	29,490.03	31,680.06	26,661.87	30,125.08	28,586.53	32,514.55	33,918.99
b_public	Obs	559	804	1189	246	1516	2507	5336	1157
	Mean	0.21	0.20	0.19	0.24	0.20	0.24	0.21	0.22
	Std. dev.	0.41	0.40	0.40	0.43	0.40	0.43	0.41	0.42
b_period	Obs	1155	1546	2670	516	2965	4791	10,970	2234
	Mean	14.99	15.44	21.76	17.57	18.08	18.46	23.67	21.59
	Std. dev.	28.15	30.21	34.71	26.75	34.65	36.07	37.90	37.60
b_hpf	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	0.45	0.48	0.42	0.45	0.48	0.48	0.46	0.49
	Std. dev.	0.50	0.50	0.49	0.50	0.50	0.50	0.50	0.50
s_gender	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	0.50	0.54	0.52	0.51	0.57	0.57	0.57	0.58
	Std. dev.	0.50	0.50	0.50	0.50	0.50	0.50	0.50	0.49
s_local	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	0.43	0.42	0.42	0.41	0.70	0.70	0.70	0.69
	Std. dev.	0.50	0.49	0.49	0.49	0.46	0.46	0.46	0.46
s_age	Obs	1224	1670	2827	551	3149	5179	11,640	2375
	Mean	38.41	39.50	38.42	38.67	48.84	49.22	49.88	49.43

Table 3 (*continued*)

		Treatment				Control			
		Before	Announcement	After (long run)	After (short run)	Before	Announcement	After (long run)	After (short run)
	Std. dev.	10.61	11.82	11.26	11.44	13.11	13.14	13.89	13.80
s_income	Obs	394	507	841	164	795	1341	2731	600
	Mean	50,089.40	53,687.33	56,367.39	55,805.49	54,287.58	56,927.07	56,512.28	48,623.37
	Std. dev.	31,122.48	36,412.28	46,795.92	69,595.29	33,892.10	47,870.57	47,735.23	32,193.03
s_public	Obs	394	507	841	164	795	1341	2730	600
	Mean	0.19	0.21	0.19	0.24	0.26	0.25	0.21	0.21
	Std. dev.	0.39	0.41	0.39	0.43	0.44	0.43	0.41	0.40
s_hpf	Obs	1230	1671	2832	551	3156	5188	11,663	2378
	Mean	0.32	0.30	0.30	0.30	0.25	0.26	0.23	0.25
	Std. dev.	0.47	0.46	0.46	0.46	0.43	0.44	0.42	0.43

price gap in the treatment and control groups using line graphs, as well as 95% confidence intervals, so one can better visualize the timing of the change. The two vertical lines denote the announcement of the policy and the implementation of the policy. Panel B shows that the actual-registered price gap increases more for the treatment group after the policy implementation when compared with the control group. We also plot the same figures of other outcome variables, including registered total price, actual total price, and daily transaction volume in Appendix Fig. A.3. In terms of the registered total price, Panel A shows a steady increase in the control group while a dip in the treatment group before a more moderate increase. Regarding the actual total price, Panel B shows little difference between the treatment and control groups in terms of their trends. The results suggest that the increase in the actual-registered price gap is largely driven by the registered total price decrease in the treatment group after the policy implementation. We also examine the policy effects on transaction volumes. As shown in Panel C of Fig. A.3, the weekly transaction volume decreases after the policy implementation for both the treatment and control groups, with the treatment experiencing a larger decrease.[24] The gap between the two groups in terms of the transaction volume also enlarges after the policy change.

Before presenting the DID regression results, we verify the parallel trend assumption of the DID specification. We conduct a dynamic event study with the following specification as shown in Eq. (4), where the first 2 weeks are used as the benchmark period. The rest of the notations are the same as in Eq. (2).

$$Y_{i,j,t} = \sum_{k=3}^{39} \beta_k \times Treat_i \times 1\{Week_t = \mathrm{k}\} + \sum_{k=2}^{39} \pi_k \times 1\{Week_t = \mathrm{k}\} + \theta \times Treat_i + X_i + \alpha_j + \delta_t + \varepsilon_{i,j,t} \quad (4)$$

Fig. 5 plots the coefficients as well as the 95% confidence intervals of the coefficients β_k, where the actual-registered price gap is the outcome variable. Relative to the control group, the actual-registered price gap for the treatment group significantly increases after the policy implementation, with no significant change before the policy implementation.[25]

We also conduct the same event study for the registered total price, actual total price, and transaction volume. The graphs are shown in Appendix Fig. A.4, with Panel A, B, and C showing corresponding results. Similar to the price gap, we observe a decline in the registered price in the treatment group after the policy implementation while the actual price remains largely unaffected.[26] All three outcomes exhibit a parallel trend in the control and treatment groups before the announcement of the policy.

Now, we move on to our DID regression results reported in Table 4 using the specification of Eq. (2) and (3). In Panel A, the dependent variable is the actual-registered price gap of the transacted unit. Starting from column (1), we include complex fixed effects, hedonic attributes, month fixed effects, day of the week fixed effects, and holiday fixed effects one at a time with column (5) being the most complete specification. The estimated coefficients from column (1) to column (5) change little, suggesting the robustness of the results. In all the specifications, the reported coefficients of the DID term are positive and statistically significant at the 1% level. With the most complete set of control variables, the regression shows that tax evasion is increased by 23.3% in the treatment group after the implementation, relative to the control group. Given the baseline underreporting level of 825,980 *yuan* (i.e., the baseline actual-registered price gap) in the treatment group before the announcement, the tax policy change leads to an increase in the actual-registered price gap by about 192,453 *yuan* ($825{,}980 \times 23.3\% = 192{,}453$; about 30,890 USD) in the intensive margin of underreporting after the policy change. This result also confirms the observations in Fig. 4, suggesting that, as a response to the tax increase, the transaction parties of the treatment group further lower the registered price that they report to the tax authority relative to the actual price.

Panel B and Panel C of Table 4 then examine the relative change of the registered total price and actual total price. Column (5) of both panels again show the most complete specification. If we compare the estimated coefficients on registered and actual total prices, it shows that the actual total price decrease in the treatment group is only about 1.44% (1-exp $(-0.0145) = 0.0144$) when compared with the control group.[27] The decrease in the registered total price, however, is more than 10.31% (1-exp $(-0.1088) = 0.1031$). This, again, confirms our observation from Fig. 4. Mechanically, we know the relatively increased tax evasion in the treatment group is owing to its diversion from the control group in terms of the registered price.

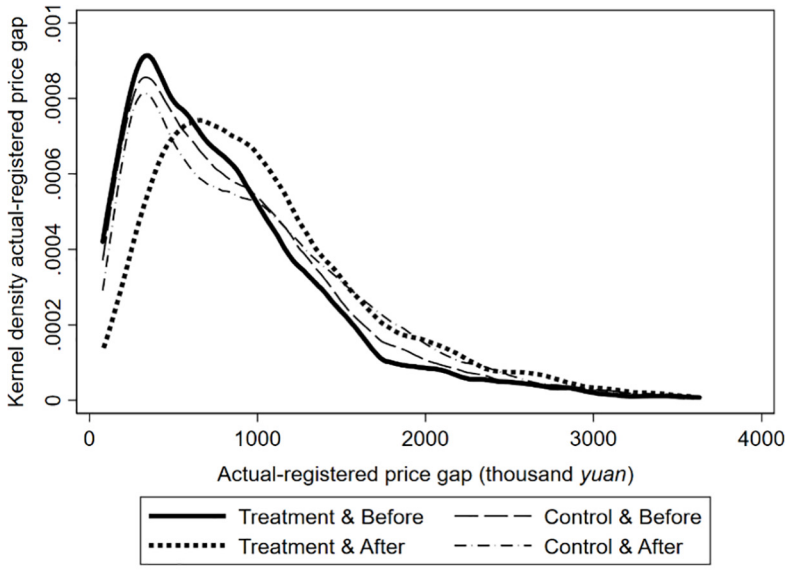
Panel D of Table 4 suggests that the policy enforcement reduces the number of transactions in the treatment group by 47.32% (1-exp

[24] Some may worry that the transaction volumes of both the treatment and control units increased during the announcement period, which may bias our estimate because many transactions have moved forward in time. As discussed in Section 2.2, there were a lot of uncertainties in the announcement period because the details of the policies and the implementation date in our sample city was not released to the public during the announcement period. Consequently, the market participants might rush into the market to close the deal during the announcement period, regardless of whether the unit had a holding period of more than 5 years or not, because possible purchase restrictions and credit tightening policies were likely to target all the units. As shown in Fig. A.4 Panel C, the treatment and control units share similar trend in terms of transaction volume in the announcement period.

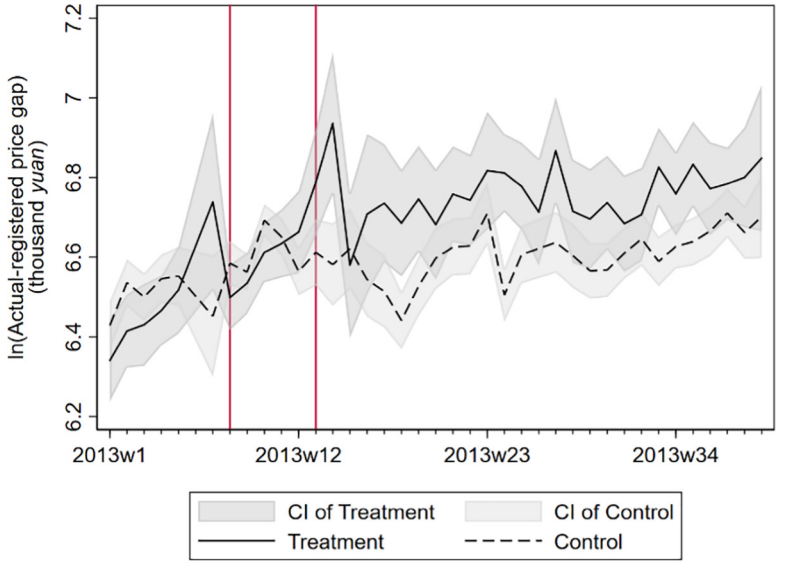
[25] The only exception is that the coefficient on the week right before implementation is significantly positive. It is possible that the realtors might have insider information about the implementation date of the new policy and informed the buyers. The transactions in this week (one week before implementation) are very likely to be subject to the policy change because the buyers usually need to wait for at least two weeks after the transaction date to pay taxes. The news in the following link supports this possibility: http://www.bjnews.com.cn/news/2013/03/18/253837.html.

[26] Two coefficients in the announcement period are significantly negative for the registered price, which is likely because that some transactions in the announcement period are also subject to the new tax rate if the date that the buyer registers the transaction in the tax bureau is after March 31.

[27] Besley et al. (2014) conduct a tax incidence analysis in the context of property tax changes. However, we cannot use their framework to analyze the tax incidence in our context because we have to factor in the composition change of cash buyers and loan buyers, the loan to value ratio decision of loan buyers, and the tax evasion of buyers which are not present in the model of Besley et al. (2014). Therefore, the discussion on tax incidence is beyond the scope of this paper and left for future research.



A: Distribution of the actual-registered price gap



B: Weekly average actual-registered price gap with 95% confidence interval

Fig. 4. Actual-registered price gap distribution: By treatment and control groups and by different periods. We define the days between January 1 and February 19 as the before-announcement period, the days between February 20 and March 30 as the announcement period, and the days between March 31 and September 26 (180 days) as the after-implementation period (long-run). The treatment group includes the resale transactions with holding periods less than 4 years; the resales with holding periods over 6 years serve as the control group. Authors' calculation from the Tax Evasion Sample. Week 6 (the Chinese New Year holiday) is excluded. The Epanechnikov Kernel is applied with optimal bandwidth in Panel A. The two vertical lines in Panel B denote the announcement of the policy and the implementation of the policy.

$(-0.6409) = 0.4732$) relative to the control group. It is not surprising that the enforcement reduces the number of transactions in the treatment group, because buyers would seek either to substitute their targeted houses with similar ones in the control group to avoid paying the high tax amount or simply to give up buying a house owing to budget constraint. It is also likely that some transactions shifted from the after-implementation period to the announcement period to avoid paying higher taxes. However, this volume change also imposes challenges on the validity of our DID design. We will address these issues in Section 4.3.

Before ending this subsection, we show two calculations that might be of interest to some. First, we show a back-of-the-envelope calculation to decompose the tax revenue change induced by the policy change into four components: the market response (i.e., tax revenue change due to the change in transaction volume and actual total price in the absence of tax avoidance and tax evasion), the tax rate (i.e., tax revenue change due to the increased tax rate), the tax evasion (i.e., tax revenue change due to the increased actual-registered price gap), and the tax avoidance (i.e., tax revenue change due to the delayed transactions).

Appendix D provides the detailed procedure to conduct this decomposition. Using the average prices and volumes from the summary statistics in Table 3, the estimated policy impact in Table 4, and the estimated $vol_{bunching}$ for Panel A of Fig. A.2, we can calculate the respective daily amounts for the four different components as −183,080 *yuan*

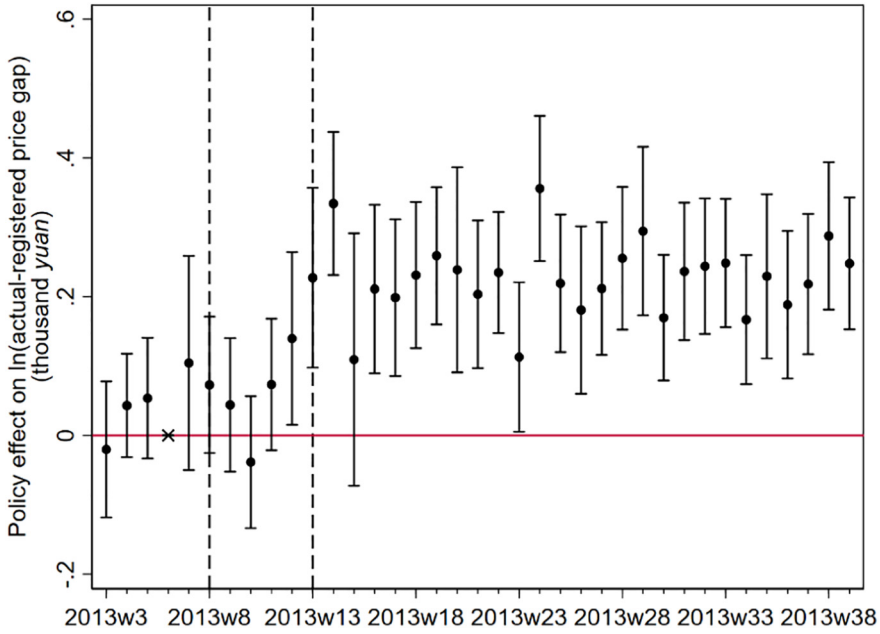


Fig. 5. Effect of the tax enforcement on the actual-registered price gap: Event study. The figure visualizes the coefficients estimated in Eq. (4) based on the Tax Evasion Sample, with both the coefficients and 95% confidence intervals reported. The treatment group includes the resale transactions with holding periods less than 4 years; the resales with holding periods over 6 years serve as the control group. Weeks 1 and 2 are the benchmark weeks. Week 6 (the Chinese New Year holiday) is excluded. The two vertical lines denote the announcement of the policy and the implementation of the policy, respectively.

Table 4
Effects of the tax enforcement on housing market outcomes (long-run).
This table reports the coefficient β_1 in Eq. (2) in Panel A, B, and C, where the logged actual-registered price gap, logged registered total price, and logged actual total price are the outcome variables in turn. Panel D reports the coefficient β_1 in Eq. (3), where the logged daily transaction volume is the outcome variable. The treatment group includes the resale transactions with holding periods less than 4 years; the resales with holding periods over 6 years serve as the control group. The analysis is based on the Tax Evasion Sample without observations in the announcement period. The short-run results are shown in Table B.1. The standard errors are two way clustered at the complex-day level in Panel A, B, and C, while the robust standard errors are used in Panel D. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

	(1)	(2)	(3)	(4)	(5)
Panel A: actual-registered price gap					
Variables	ln(Agap)	ln(Agap)	ln(Agap)	ln(Agap)	ln(Agap)
Treat × after	0.2071***	0.2077***	0.2087***	0.2094***	0.2094***
	(0.0247)	(0.0217)	(0.0213)	(0.0214)	(0.0214)
Observations	17,939	17,939	17,939	17,939	17,939
R-squared	0.5199	0.5887	0.5898	0.5900	0.5902
Panel B: registered total price					
Variables	ln(RTP)	ln(RTP)	ln(RTP)	ln(RTP)	ln(RTP)
Treat × after	−0.1111***	−0.1078***	−0.1086***	−0.1088***	−0.1088***
	(0.0160)	(0.0122)	(0.0121)	(0.0121)	(0.0121)
Observations	17,939	17,939	17,939	17,939	17,939
R-squared	0.5402	0.7042	0.7124	0.7125	0.7126
Panel C: actual total price					
Variables	ln(ATP)	ln(ATP)	ln(ATP)	ln(ATP)	ln(ATP)
Treat × after	−0.0169	−0.0144**	−0.0146***	−0.0145**	−0.0145**
	(0.0119)	(0.0057)	(0.0056)	(0.0056)	(0.0056)
Observations	17,939	17,939	17,939	17,939	17,939
R-squared	0.7368	0.9380	0.9426	0.9426	0.9427
Complex FE	Yes	Yes	Yes	Yes	Yes
Hedonic attributes	No	Yes	Yes	Yes	Yes
Month FE	No	No	Yes	Yes	Yes
Dow FE	No	No	No	Yes	Yes
Holiday FE	No	No	No	No	Yes

Panel D: volume	(1)	(2)	(3)
Variables	ln(Volume)	ln(Volume)	ln(Volume)
Treat × after	−0.6538***	−0.6393***	−0.6409***
	(0.1725)	(0.1549)	(0.1537)
Observations	442	442	442
R-squared	0.6784	0.7919	0.7921
Month FE	Yes	Yes	Yes
Dow FE	No	Yes	Yes
Holiday FE	No	No	Yes

(market response), 864,350 *yuan* (tax rate change), −586,440 *yuan* (tax evasion), and − 5320 *yuan* (tax avoidance), respectively. The total amount of tax revenue change is the sum of the four numbers, which is 89,510 *yuan* (about 14,367 USD). From this simple calculation, tax evasion is the largest negative component, meaning that tax evasion causes the most tax revenue loss. The tax loss due to market response is approximately 31% of the tax evasion amount. The magnitude of tax avoidance is much smaller than the other two negative components because our estimates suggest limited bunching above the 5-year holding period threshold. If one considers the 180 days after the policy implementation, the total tax revenue loss due to tax evasion would be around 106 million *yuan* (about 17 million USD) in our sample city, which is an economically significant amount.

Second, we estimate the impact of the tax rate increase on the effective capital gains tax rate of the transacted units using Eq. (2) based on the Tax Evasion Sample. Appendix Table A.2 shows the detailed results. The effective capital gains tax rate is calculated by dividing the *actual* capital gains tax payment (*after* tax evasion) by the *actual* total price. The five columns in Table A.2 control for different sets of control variables as Table 4. Column (5) shows the estimate with the most comprehensive specification. Compared to the control group, the effective capital gains tax rate in the treatment group increased by 3.37 percentage points after the policy change.

4.3. Robustness checks

In this subsection, we conduct robustness checks to ensure the validity of our research design. There are three major challenges to our DID strategy – selection into the treatment (i.e., sellers may decide not to sell the affected units after the policy change), manipulation of the treatment assignment (i.e., sellers and buyers may choose to delay transactions to avoid part of the tax payment), and composition change in the treatment and control groups (i.e., transacted housing units in the treatment and control groups may be different after the policy change).

We first address the concern on selection: certain transactions may not happen because of the policy change (capital gains tax rate increase); in other words, these transactions would have been completed in the absence of the policy change. To mitigate this concern, we conduct the Heckman two-stage procedure by obtaining the transaction data between September 27, 2013 and December 31, 2015 from the same brokerage firm. We assume that the observed transactions in the supplementary sample are potential transactions that could have been transacted during our main sample period. We can then use this supplementary sample to conduct the first step regression for the Heckman procedure. The result is shown in column (1) of Table 5. We include buyer's and seller's gender, age, and locality in the regression in addition to the control variables as in column (5) of Table 4, the inverse Mills ratio (IMR) can then be calculated from this regression. Column (2) of Table 5 shows the regression result from the second step of the Heckman procedure by including the IMR value in our main regression. The obtained coefficient is 0.2114, which is very similar to 0.2094 as we obtained from our main regression. This result suggests that selection in terms of transaction is not a major concern.

Second, some may still be concerned that the holding period is endogenously chosen by the sellers and buyers even after we eliminate those transacted units with holding periods longer than 4 years and shorter than 6 years. To address this concern, we use the holding period information before the policy announcement (so that the holding period calculation is totally determined by the policy announcement date) to redefine our treatment and control groups excluding those units with holding periods between 4 and 6 years. We then use this redefined treatment and control groups to estimate the same regressions as in Panel A of Table 4. Columns (3) of Table 5 shows the result. As expected, the estimated coefficient is similar to the one obtained from our main regression in Table 4.

An equally important threat to our identification strategy is sorting between the treatment and control groups. One may worry that the control group may be contaminated after the policy because people may switch from buying the treated units to buying the control units to save tax costs. We address this concern using a conservative method to bound our estimates. If the observables do not experience significant change after the policy implementation, we would have faith that such contamination is very limited. If the observables do show significant differences after the policy change, we follow Lee (2009) which corrects for the composition change of the treatment and control groups due to different attrition rates to bound our estimates. This practice requires comparing the housing characteristics and the buyer and the seller characteristics in the treatment and control groups before and after the policy. Specifically, we conduct DID regressions using the housing attributes and the buyer's/seller's characteristics as outcome variables, controlling for month fixed effects, day-of-the-week fixed effects, and a dummy for holidays. These regressions help to capture the differences in terms of the housing characteristics traded in the treatment group and the types of transaction parties in the treated units that may be caused by the policy change.

Panel A of Table 6 shows the DID regressions on housing attributes of the transacted units, including unit size, floor level, number of bedrooms, built year of the unit, facing of the unit, the distance to the center of the city, and the distance to the nearest subway station. The results suggest that housing units in the treatment group are more likely to be on higher floor level, older buildings, and having less favorable facings after the policy change, compared with the control group. Panel B and Panel C of the same table report the same results for buyer's and seller's characteristics. It shows that most characteristics (including gender, local residents or not, working in the public sector or not, income, the time since buyer's registration with the brokerage firm, and whether the buyer contributes to housing provident fund (HPF) or not) of the buyers buying the treated units, relative to the buyers buying the control units, do not differ significantly after the policy change. Buyers' average age increases slightly in the treatment after the policy change, which is marginally significant. The seller's characteristics follow a similar pattern with a significant difference in age and employer type in the public sector.[28]

In our context, housing transactions in the treatment group are reduced more after the policy than the control group. Therefore, we will correct for the composition differences in the control and treatment groups after the policy by trimming some of the observations in the "control & after" group to estimate the upper and lower bounds of the treatment effect. The intuition is that we assume the reduction in the transaction volume (47.3% in the long run) of the treated units after the policy shock is bounded by two extreme cases: 1) all the reduction in volume is from the top 47.3% of the transacted units in the control group after the policy; and 2) all the reduction in volume is from the bottom 47.3% of the transacted units in the control group after the policy. We use the six variables that show significant sorting in Table 6 to act as the sorting variables. For each variable, we drop the top and bottom 47.3% in the "control & after" group and re-estimate the main results, respectively. The bias due to sorting should be bounded by these two estimates.

Table 7 shows the estimation results. Panel A of the table reports the upper and lower bounds using the three housing attributes – floor level, building age, and unit facing direction – as sorting variables. Columns (1) and (2) are lower and upper bounds for floor level, columns (3) and (4) are for building age, while columns (5) and (6) are for unit facing direction. For all sorting variables, we obtain reasonable

[28] For all the six outcome variables with significant sorting as suggested in Table 6, we also conduct a comparison between the "control & before" and "control & after" groups. The results suggest no significant difference between these two groups; in other words, the sorting results in Table 6 are not driven by the change of the control units. The results are available upon request.

Table 5
Robustness check: Heckman two-stage model and alternative treatment definition (long-run).
The Heckman two-stage model uses the transactions from 2013 to 2015 to estimate the first stage. The second stage uses the Tax Evasion Sample. The alternative treatment group definition uses the holding period information on February 19, 2013 (i.e., the day before the announcement) based on the Full Sample. The treatment group includes the resale transactions with holding periods less than 4 years; the resales with holding periods over 6 years serve as the control group. The short-run results are shown in Table B.2. The standard errors are two way clustered at the complex-day level. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

Variables	(1)	(2)	(3)
	Heckman two-stage model		Redefine treatment
	Transacted	ln(Agap)	ln(Agap)
Treat × after		0.2114***	0.2051***
		(0.0217)	(0.0215)
IMR		0.3578***	
		(0.0685)	
B_gender	0.0124		
	(0.0094)		
B_age	−0.0043***		
	(0.0005)		
B_local	0.0559***		
	(0.0102)		
S_gender	0.0190**		
	(0.0094)		
S_age	−0.0012***		
	(0.0004)		
S_local	0.1838***		
	(0.0109)		
Observations	84,207	17,898	18,058
R-squared	0.0579	0.5908	0.5900
Complex FE	No	Yes	Yes
Hedonic attributes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Dow FE	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes

gaps between the upper and lower bounds, with both bounds still being statistically significant. Similarly, Panel B reports the same results using the buyer's and seller's characteristics – buyer's age, seller's age, and seller's employment segment (public or not). The results are also similar to Panel A with all upper bounds and lower bounds being positively significant at 1% level. Therefore, we conclude that sorting is not likely to significantly bias our main results.

Aside from the three major concerns, we address some other concerns on our empirical results such as confounding policy shock, timing of policy shock, or tax evasion measure. First, some may worry that our treatment effects are not caused by the tax policy change because the tax policy was bundled with a change in credit policy. In particular, the policy increased the down payment percentage if a buyer chooses to take a mortgage loan from the bank or HPF and the transacted unit is the second housing unit being purchased by her household in the sample city.[29] To address this concern, we divide our sample by the median age of the buyers (i.e., 32 years old) because younger buyers are less likely to purchase a second housing unit especially in our sample city with high housing prices. We replicate the specification of column (5) in Table 4 for both the young and old buyer groups. The results (Appendix Table A.3) show that the young buyer group is affected more by the policy change, which contradicts the argument that the policy effects are driven by the credit policy change.

Second, sellers and buyers may respond to the policy even before the actual policy implementation (i.e., the announcement period). To make sure that our estimates are robust to the specification of the policy change timing, we include the announcement period in the after period (i.e., take the announcement date as the implementation date) and rerun the regressions in Panel A of Table 4. The results are in Appendix Table A.4, with all five columns showing positively significant coefficients. The magnitudes of the estimated coefficients are smaller than the ones from Panel A of Table 4. Note that this should be expected since the treatment effect comes from the actual implementation of the policy instead of the announcement period, thus the effect is attenuated.

Third, some may worry that the timing of the policy implementation is endogenous, because the government wanted to curb speculation in the housing market with the policy tool. To mitigate possible concern about the policy enforcement's endogeneity, we make use of the announcement period to conduct a placebo test, as shown in Appendix Table A.5. We replace the policy implementation with the policy announcement (i.e., February 20, 2013 to March 30, 2013) and replicate the specifications in Table 4. With all specifications, the estimated coefficients are insignificant at all conventional levels. This further shows that the change in tax evasion is induced by the policy implementation.

Fourth, in some cases, the price finally registered with the local housing authority and thus adopted in the transaction tax calculation might be different from the price that the buyer and seller originally planned to report (i.e., the registered price recorded in our dataset). As a most likely case, the price originally submitted would have been rejected by the local housing authority if it were lower than the minimum required price; in this case, the buyer and seller would have had to report a higher price. Another possibility is that the buyer might have realized that she needed to apply for more mortgage loans and thus, had to raise the price reported to local housing and tax authorities.

To investigate the potential effect of such a reported price change, we merge the transactions in the before-announcement period, announcement period, and the short-run after implementation period with the official housing transaction registration data according to the addresses of the transacted units.[30] For all the 14,586 units transacted in these three sub-periods, we successfully merge the official registration data for 12,558 units (or 86.1%). As listed in Appendix Table C.4, for about 77% of the merged units, the price finally registered with the local housing authority is identical to the registered price recorded in our sample. The magnitude of deviation is generally small for the other 23% units. These patterns do not change between the cash buyers and loan buyers. In Appendix Table A.6, we replicate the main specifications in Tables 4, using only the observations whose registered prices kept unchanged, and the results remain robust.

Lastly, we use the relative price ratio (i.e., registered total price/actual total price) to replicate the main analysis in Table 4. The results are available in Appendix Table A.7. All the coefficients are significant at the 1% level. With the most comprehensive set of control variables, the relative gap between the registered price and actual price in the treatment group increases by 6.21% after the policy implementation. The results are consistent with the main results.

4.4. External validity

We investigate whether our findings can be generalized to other cities. For this purpose, we collect city-level monthly average unit prices of housing resales for 35 major cities in China from January 2012 to December 2013. The average registered unit price data are reported by local housing authorities. We also obtain average listing unit prices of housing resales from one major anonymous real estate data vendor in China, and adopt this listing price indicator as the proxy of average actual transaction prices. Therefore, we can construct a proxy for tax evasion at the city-month level using the gap between the average registered unit price and the average listing unit price.[31] Among these

[29] See http://www.gov.cn/zwgk/2013-04/01/content_2367217.htm for more information.

[30] We were only able to match the transactions that completed in the short run due to limitation of the government registration data.

[31] We use average unit price (i.e., per square meter price) because the total price information is not available for city-level data.

Table 6
Robustness check: Effects of the tax enforcement on buyer/seller characteristics and hedonic attributes (long-run).
This table reports the coefficient β_1 in Eq. (3), where the hedonic attributes are the outcome variables in Panel A, including unit size, floor level, number of bedrooms, built year of the unit, facing of the unit, the distance to the city center, and the distance to the nearest subway station; the buyer characteristics are the outcome variables in Panel B, including age, gender, local residents or not, working in the public sector or not, income, the time since buyer's registration with the brokerage firm, and whether the buyer is an HPF contributor; and the seller characteristics are the outcome variables in Panel C, including age, gender, local residents or not, working in the public sector or not, income, and whether the seller is an HPF contributor. The treatment group includes the resale transactions with holding periods less than 4 years; the resales with holding periods over 6 years serve as the control group. The analysis is based on the Tax Evasion Sample. The standard errors are two way clustered at the complex-day level in all the regressions. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: hedonic attributes	ln(Unitsize)	Floor	Bedroom	Builtyear	Facing	ln(Center)	ln(Subway)
Treat × after	0.0143	0.4399*	0.0073	2.2522***	−0.0353*	−0.0062	−0.0401
	(0.0183)	(0.2653)	(0.0325)	(0.3723)	(0.0191)	(0.0327)	(0.0480)
Observations	18,881	18,881	18,881	18,881	18,881	11,853	11,853
R-squared	0.0048	0.0177	0.0181	0.1206	0.0123	0.0178	0.0036
Panel B: buyer characteristics	B_age	B_gender	B_local	B_public	ln(B_income)	B_period	B_hpf
Treat × after	0.7792*	−0.0122	−0.0279	−0.0180	0.0508	1.1945	−0.0138
	(0.4140)	(0.0219)	(0.0235)	(0.0211)	(0.0404)	(1.1718)	(0.0225)
Observations	18,881	18,881	18,881	8600	8600	17,760	18,881
R-squared	0.0031	0.0025	0.0030	0.0019	0.0165	0.0077	0.0110
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dow FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel C: seller characteristics	(1)	(2)	(3)	(4)	(5)	(6)
	S_age	S_gender	S_local	S_public	ln(S_income)	S_hpf
Treat × after	−1.0256**	0.0282	−0.0176	0.0459*	0.0914	−0.0061
	(0.4277)	(0.0199)	(0.0157)	(0.0240)	(0.0595)	(0.0154)
Observations	18,840	18,881	18,881	4760	4761	18,881
R-squared	0.1112	0.0030	0.0593	0.0066	0.0134	0.0081
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Dow FE	Yes	Yes	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 7
Robustness check: Lee-bounds estimates (long-run).
This table reports the Lee-bounds estimates, where the logged actual-registered price gap is the outcome variable. In Panel A, columns (1) and (2) report the estimates by trimming the sample using the variable "floor level"; columns (3) and (4) report the estimates by trimming the sample using the variable "built year of the unit"; columns (5) and (6) report the estimates by trimming the sample using the variable "facing of the unit". Column (1) and (3) trim the 47.3% in the "control & after" group from below, and column (2) and (4) trim the 47.3% in the "control & after" group from above accordingly. Column (5) trims the units without the south aspect in the "control & after" group, and column (6) trims the units with the south aspect in the "control & after" group. In Panel B, columns (1) and (2) report the estimates by trimming the sample using the variable "buyer age"; columns (3) and (4) report the estimates by trimming the sample using the variable "seller age"; columns (5) and (6) report the estimates by trimming the sample using the variable "seller working in the public sector or not." Column (1) and (3) trim the 47.3% in the "control & after" group from below, and column (2) and (4) trim the 47.3% in the "control & after" group from above accordingly. Column (5) trims the units without seller working in the public sector in the "control & after" group, and column (6) trims the units with seller working in the public sector in the "control & after" group. The treatment group includes the resale transactions with holding periods less than 4 years; the resales with holding periods over 6 years serve as the control group. The analysis is based on the Tax Evasion Sample. The standard errors are two way clustered at the complex-day level in all the regressions. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Floor		Built year		Facing	
Trimming	Below	Above	Below	Above	Non-south	South
	ln(Agap)	ln(Agap)	ln(Agap)	ln(Agap)	ln(Agap)	ln(Agap)
Treat × after	0.2544***	0.1870***	0.1773***	0.2595***	0.2246***	0.1958***
	(0.0239)	(0.0231)	(0.0223)	(0.0249)	(0.0250)	(0.0250)
Observations	12,777	11,360	13,175	12,147	12,125	12,050
R-squared	0.6218	0.6195	0.6187	0.6059	0.6182	0.6181
Complex FE	Yes	Yes	Yes	Yes	Yes	Yes
Hedonic attributes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Dow FE	Yes	Yes	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes	Yes	Yes
Panel B	B_age		S_age		S_public	
Trimming	Below	Above	Below	Above	Non-public	Public
	ln(Agap)	ln(Agap)	ln(Agap)	ln(Agap)	ln(Agap)	ln(Agap)
Treat × after	0.1323***	0.3107***	0.2138***	0.2111***	0.2086***	0.2084***
	(0.0228)	(0.0227)	(0.0230)	(0.0219)	(0.0216)	(0.0213)
Observations	12,978	12,122	12,491	12,324	15,767	17,353
R-squared	0.6226	0.6099	0.6142	0.6205	0.5994	0.5938
Complex FE	Yes	Yes	Yes	Yes	Yes	Yes
Hedonic attributes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Dow FE	Yes	Yes	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes	Yes	Yes

35 cities, a number of cities also implemented the increase in capital gains taxes in March 2013, while others not, which allows us to conduct a DID analysis at the city level to examine the impact of the capital gains taxes on tax evasion.

We use the following two specifications to conduct this analysis:

$$Y_{i,t} = \beta_1 \times Treat_i \times After_t + \alpha_{it} + \delta_t + \epsilon_{i,t} \quad (5)$$

$$Y_{i,t} = \beta_1 \times Treat_i \times After_t + \beta_2 \times Treat_i{*}Before_t + \alpha_{it} + \delta_t + \epsilon_{i,t} \quad (6)$$

where $Y_{i,t}$ measures the listing-registered price gap in thousand *yuan* per square meter; $Treat_i$ is a dummy variable that takes the value 1 if city *i* follows the policy of capital gains tax increase in March 2013[32]; $After_t$ is a dummy variable that takes the value 1 after the policy change (i.e. the increase in capital gains tax); α_{it} is the city by month fixed effects to capture the city-specific seasonality in different months, and δ_t is the year by month fixed effects. In the second specification, we use January to August in 2012 as the baseline period. We define a before-period which is the six months before policy announcement (September 2012 to February 2013), and an after-period which is after March 2013. The standard errors are two way clustered at the city and month level.

Table 8 reports the regression results with columns (1) and (2) showing the results for Eq. (5) and columns (3) and (4) showing the results for Eq. (6). The treatment group includes the 7 cities in columns (1) and (3) and the 13 cities in columns (2) and (4). It is obvious from the regression results that the listing-registered price gap in the before-policy period is not significantly different from the baseline period (January 2012 to August 2012). However, the gap is significantly increased by 17.1%–20.3% (transforming the coefficients in columns (2) and (4) into growth rate) in the after-implementation period, which is consistent with the magnitude in the main result. The test of the parallel trend for the DID specification in Table 8 is available in Fig. A.5, which shows the validity of the DID research design.

Table 8
External validity check: 35 major cities.
This table reports the results in Eqs. (5) and (6), where the city-month level logged listing-registered unit price gap is the outcome variable. The treatment group includes the cities that implemented the capital gains tax enforcement in March 2013, while the control group includes the cities without any change on capital gains taxes. The first two columns report the results for Eq. (5) and the last two columns report the results for Eq. (6). The standard errors are two way clustered at the city-month level in all the regressions. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

	(1)	(2)	(3)	(4)
Variables	ln(Agap)	ln(Agap)	ln(Agap)	ln(Agap)
Treat × before			0.0871	0.0609
			(0.0720)	(0.0645)
Treat × after	0.2154**	0.1577**	0.2540**	0.1847**
	(0.0857)	(0.0687)	(0.1097)	(0.0874)
Observations	612	744	612	744
R-squared	0.9515	0.9465	0.9519	0.9467
City by month FE	Yes	Yes	Yes	Yes
Year by month FE	Yes	Yes	Yes	Yes

4.5. Heterogeneity on loan buyers and cash buyers

It is not surprising that market participants respond to the new policy enforcement by reducing their registered price to avoid paying part of the capital gains tax. However, it is interesting to ask why people do not report the lowest possible price (the minimum required price set by the tax authority) even before the policy change. We believe that the buyers' mortgage financing incentives play a dominant role here. As discussed in the policy background section, the buyer in a housing resale transaction has to balance the incentive of lowering transaction taxes and the incentive of increasing mortgage loans in setting the registered price. Because the loan amount is highly correlated with the registered price, the buyer needs to pay off any gap between the loan amount and the actual price upfront. In other words, the lower the registered price is, the more the buyer needs to pay in cash. After the capital gains tax increase, the marginal tax cost of a 1 *yuan* loan for a housing unit in the treatment group increases by 0.27 *yuan*,[33] assuming that the interest rate remains unchanged. Such a rapid increase in loan cost naturally encourages buyers in the treatment group to reduce their mortgage financing and lower registered prices.

Fig. 6 confirms this conjecture. We divide our tax evasion sample into cash transactions (i.e., buyer pays the full amount using cash) and mortgage loan transactions (i.e., buyer pays part of the amount using a loan from a commercial bank or the HPF) and plot the density distribution of the actual-registered price gap separately. It is evident from the graph that the distribution for the cash transactions is to the right of the distribution for the loan transactions. This confirms our conjecture that loan buyers evade less tax when compared with cash buyers because they need to balance the cost and benefit of tax evasion. In theory, the cash buyers should always report the lowest possible registered price or the minimum required price by the tax authority because they face no financing constraint.[34]

Given that the marginal cost of applying for mortgage loans increases, we first examine how the capital gains tax increase affects the financing choice of buyers in the housing market. In columns (1) and (2) of Table 9, the dependent variable is a dummy variable that takes the value 1 if the buyer of a transaction pays the full amount using cash and 0 if she uses a loan to cover at least part of the total amount. Column (1) uses the same specification as the most complete specification in Table 4 while column (2) additionally controls for buyer characteristics. For the most complete specification (column (2)), it suggests that the share of loan buyers in the treatment group decreases by about 8.39%. This is a remarkable decrease, given the baseline loan buyer ratio of 63% in the treatment group before the policy change. In addition to the change on the extensive margin in terms of financing choices, we also study the impact of the tax change on the intensive margin measured by the loan-to-value (LTV) ratio for each transaction, calculated as the principal of the mortgage loan divided by the actual total price. As presented in columns (3) and (4), we find that the LTV ratio is reduced by 8.08 percentage points with the full set of control variables. Given that the average size of the loan for mortgagors is 1,062,179 *yuan* for the treated unit before the policy change, on average, the size of the loan is reduced by about 85,824 (1,062,179 × 0.0808 = 85,824) *yuan* (about 13,775 USD) for the treated units.

[32] We construct two versions of the treatment variable in our regression. In columns (1) and (3), *Treat* takes the value 1 for the 7 cities that explicitly published the details on how to implement the policy. These 7 cities include Shanghai, Beijing, Nanning, Xiamen, Tianjin, Shenyang, and Chongqing. In columns (2) and (4), *Treat* takes the value 1 for the same 7 cities plus the following cities: Nanjing, Ningbo, Guangzhou, Hangzhou, Shenzhen, and Qingdao. The additional 6 cities announced to follow the capital gains tax increase but did not publish details on the procedure. Cities other than these 13 cities are taken as the control group (i.e., the cities that had no announcement related to the tax policy change) including Urumqi, Lanzhou, Nanchang, Hefei, Hohhot, Harbin, Dalian, Taiyuan, Chengdu, Kunming, Wuhan, Jinan, Haikou, Shijiazhuang, Fuzhou, Xining, Xi'an, Guiyang, Zhengzhou, Yinchuan, Changchun, and Changsha.

[33] This is calculated as $(0.056 + 0.01 + 0.2)/0.7 - (0.056 + 0.01 + 0.01)/0.7 = 0.2714$ with data from Table 1.

[34] In practice, a cash buyer should always report a registered price that is close enough to the minimum value with the broker's assistance. As discussed in Section 2.1, we cannot directly observe the minimum required registered price for each complex. However, we infer the minimum price of each housing complex by calculating the minimum reported price per square meter for units purchased by cash buyers before the policy change, and essentially test whether all cash buyers purchasing housing units from the same complex evade tax to a similar extent. In Appendix Fig. A.6 we plot the density of the registered/minimum price ratio for four different groups of the sample—cash & treat, cash & control, loan & treat, and loan & control. The graph shows that the cash & treat and cash & control groups have almost identical distributions around the ratio of 1, while the loan & treat and loan & control groups show very different distributions, with both of which substantially higher than 1.

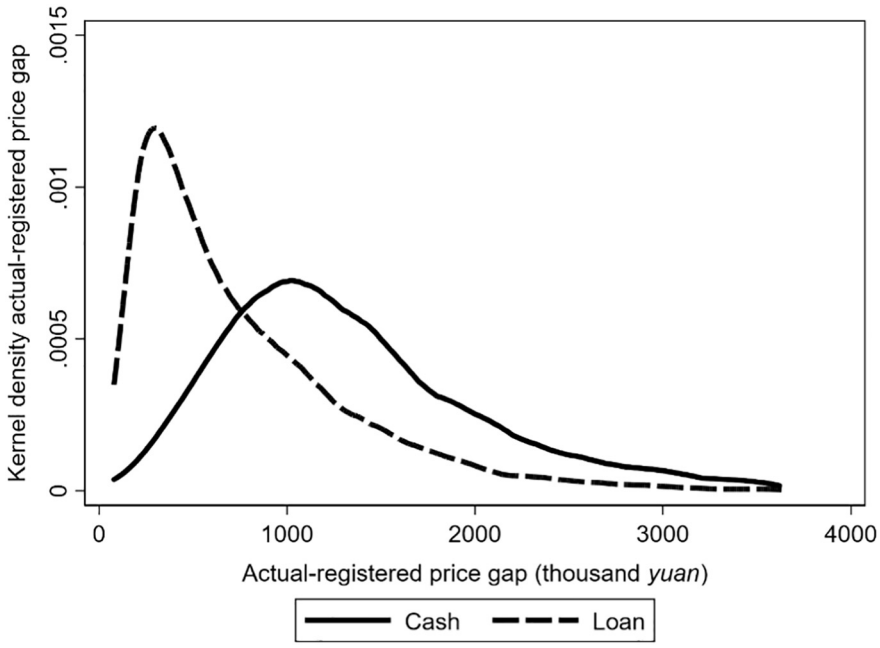


Fig. 6. Distribution of the actual-registered price gap: By payment methods. The cash buyer group includes transactions with buyers paying the full amount using cash; the loan buyer group includes transactions with buyers paying part of the amount using loans from commercial banks or the HPF. Authors' calculation using the Tax Evasion Sample. The Epanechnikov Kernel is applied with optimal bandwidth.

The above analysis suggests that the capital gains tax increase affects the financing choices of home buyers at both the extensive margin and the intensive margin. In addition to that, we also conduct heterogeneity analysis by cash buyers and mortgage buyers on their tax evasion behaviors to verify our conjecture that the policy strikes mortgage buyers and leads to more tax evasion among this group, while the policy does not change the tax evasion behavior of cash buyers. Panel A of Table 10 reports the regression results. The specification is again consistent with the most complete specification in Table 4, except that we divide the sample into different groups by payment method in columns (1) and (2). Column (3) conducts the same regression by interacting the DID estimator with the dummy variable that denotes the payment method. With the full set of control variables and the fixed effects, we find that the policy affects only the actual-registered price gap for loan buyers; by contrast, the buyers making full payment in cash are not affected by the policy, which is consistent with our conjecture that cash buyers would always report the minimum required price regardless of the policy change, and mortgage buyers would report lower registered price after the policy due to an increasing cost of the mortgage loan.

Panel B and C of Table 10 confirm this. We again adopt the DID design in Eq. (2) but use the registered price as the dependent variable in Panel B and the actual price as the dependent variable in Panel C. Relative to the control group, cash buyers do not change their registered price with the tax authority while loan buyers significantly lower the registered price. The results of the above heterogeneity analysis also suggest that, the effect of the capital gains tax increase exclusively concentrates on the loan buyers. The cash buyers always report the minimum required price, and thus are far less affected by the tax policy change. By contrast, with the capital gains tax increase, the loan buyers have to either pay more transaction taxes or reduce their mortgage borrowings, both of which negatively affect their housing demand. As the evidence, in Panel C of Table 10, the results show that the actual price paid by cash buyers is not affected by the policy, while loan buyers pay significantly less. According to the coefficient, the actual price paid by loan buyers in the treatment group decreases by about 2.17% after the policy implementation. This result indicates that loan buyers are at a disadvantage in the market as they can no longer afford the pretax price before the policy change.[35]

One last piece of evidence that the loan buyers are more affected by the policy change is presented in Fig. 7. The density plot shows little or moderate holding period change for cash buyers but substantial change for loan buyers. This suggests that loan buyers (or transactions involving loans) have a larger incentive to delay their transactions until the holding period exceeds 5 years. Such incentive becomes even larger after the policy change (tax rate increase). In addition, we also verify such bunching response by plotting the raw data (a replication of Panel B of Fig. 2) for cash buyers and loan buyers after the policy change. As shown in Appendix Fig. A.8, there is no evidence of bunching above 5 years for cash buyers (Panel A), while a stronger pattern of bunching above 5 years for loan buyers (Panel B), consistent with the message in Fig. 7.

5. Conclusion

Tax instruments are important policy tools in the real estate market. However, the existence of tax evasion may affect the outcome of a tax-

[35] In order to confirm that the parallel trend assumption stands for both the cash and loan buyers, we replicate the same event study for the two groups of buyers separately. The results are shown in Appendix Fig. A.7. Also, a threat to the causal inference of our heterogeneity analysis by cash and loan buyers is that some loan buyers who could afford to pay for the house in cash (but did not do so to amplify the investment return in a rising housing market) might switch to cash buyers after the policy. We assume that these switchers, if any, are likely to be in the upper part of the income distribution among loan buyers. Therefore, if such switching behavior largely explains the increase of cash buyers after the policy, we should observe that the income of loan buyers in the treatment group is significantly lower after the policy, relative to the control group. However, our additional test suggests that the income of loan buyers in the treatment group is not significantly different after the policy change. In addition, we also plot the income distribution for loan buyers in the control and treatment groups, before and after the policy change, and find that the income distribution in the four groups looks similar, in support of limited switching behavior from loan buyers to cash buyers. Having said that, we are fully aware of the potentially endogenous classification of heterogeneity groups and acknowledge that we cannot derive causal inference for the heterogeneity analysis on cash buyers and loan buyers. The additional tests mentioned above are available upon request.

Table 9
Effects of the tax enforcement on financing choice (long-run).
This table reports the coefficient β_1 in Eq. (2), where a dummy variable indicating whether the buyer is a cash buyer is the outcome variable in columns (1) and (2), and the loan-to-value ratio is the outcome variable in columns (3) and (4). The treatment group includes the resale transactions with holding periods less than 4 years; the resales with holding periods over 6 years serve as the control group. The analysis is based on the Tax Evasion Sample. The short-run results are shown in Table B.7. The standard errors are two way clustered at the complex-day level in all the regressions. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

	(1)	(2)	(3)	(4)
Variables	Cashbuyer	Cashbuyer	LTV	LTV
Treat × after	0.0997***	0.0839***	−0.0753***	−0.0808***
	(0.0191)	(0.0176)	(0.0279)	(0.0282)
Observations	17,939	17,939	5940	5940
R-squared	0.2481	0.3879	0.2582	0.2754
Complex FE	Yes	Yes	Yes	Yes
Hedonic attributes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Dow FE	Yes	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes	Yes
Buyer characteristics	No	Yes	No	Yes

related policy and even generate unintended consequences. Using unique data that precisely record actual and registered prices in the resale housing market in a major Chinese city, we document the pervasive tax evasion in the market. Using the increase in capital gains tax as a policy experiment, we find some suggestive evidence that market participants avoid taxes by postponing transactions until the holding period exceeds 5 years, although the responses are small and imprecisely estimated. The lack of more tax avoidance may be explained by the prevalent tax evasion in the market that could be less costly. Moreover, the capital gains tax increase widens the actual-registered price gap, a measure of tax evasion, by 23.3% in the affected units after the policy change.

Moreover, we observe 8.4% more cash buyers buying the treated units after the policy. The reason is that it becomes costlier for loan buyers to receive each extra *yuan* of loan after the policy because of the higher taxes associated with per *yuan* of registered price as well as loan amount. Therefore, the (wealthier) cash buyers crowd out (less wealthy) loan buyers due to the capital gains tax increase. Loan buyers of the affected units evade more taxes by reporting a lower registered price after the policy and consequently reduce their loan size from banks. For cash buyers, they do not have to make a tradeoff between evading taxes and getting bank loans, and thus, the policy has virtually zero effect on cash buyers' tax evasion.

Our findings can shed some light on the discussions about China's growing wealth inequality. Piketty et al. (2019) estimate that the Chinese top 10% wealth share (67% in 2015) is getting close to that of the United States (72%) and is much higher than in a country like France (50%). The increase in housing prices is an important factor contributing to the wealth growth of the Chinese. However, Piketty et al. (2019)'s estimation has no information regarding tax evasion which may underestimate the rise of inequality (see Section 4 of Piketty et al. (2019)). Our precise measurement of tax evasion in the housing market shows that the wealthier group (the cash buyers) evade more taxes, which suggests that incorporating tax evasion is likely to increase the current estimates on wealth inequality. To the best of our knowledge, there are three other contemporaneous papers that document tax evasion in China's housing market: Agarwal et al. (2019) investigate the role of intermediaries in aiding evasion; Fan et al. (2019) study the misreporting behavior under a housing transaction tax notch in Shanghai; and Dai and Xu (2018) discuss the association between a market intervention policy change and tax evasion in a descriptive approach in Beijing.

Natural solutions to tax evasion include facilitating better information reporting on taxes so that the policymakers have more information about tax evasion, or making penalties explicit. However, these measures may not be the solution to tax evasion in China's housing market because local governments may not want to strictly enforce housing policies which may lead to falling housing prices, given that real estate contributes to local economic growth. Overall, our research documents the behavioral responses of market participants on tax policy changes and provides references to policymakers that plan to apply tax instruments to the real estate market. Given that tax evasion cannot be fully eliminated, its impact should be considered when designing tax-related policies. In future research, it would be very helpful to understand the effectiveness of different types of interventions on tax compliance using field experiments such as De Neve et al. (2019), especially in developing countries where tax evasion is pervasive (Slemrod, 2019).

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Table 10
Effects of the tax enforcement on market outcomes: By payment methods (long-run).
This table reports the coefficient β_1 in Eq. (2) by different payment methods, where the logged actual-registered price gap, logged registered total price, and logged actual total price are the outcome variables in Panels A, B, and C, respectively. Column (1) uses the sample of cash buyers, column (2) uses the sample of loan buyers, while column (3) uses the full Tax Evasion Sample and creates an interaction of the DID term with the dummy of cash buyers. The treatment group includes the resale transactions with holding periods less than 4 years; the resales with holding periods over 6 years serve as the control group. The cash buyer group includes transactions with buyers paying the full amount using cash; the loan buyer group includes transactions with buyers paying part of the amount using loans from commercial banks or the HPF. The short-run results are shown in Table B.8. The standard errors are two way clustered at the complex-day level in all the regressions. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

	(1)	(2)	(3)
	Cash	Loan	Interaction
Panel A: actual-registered price gap	ln(Agap)	ln(Agap)	ln(Agap)
Treat × after	0.0215	0.2744***	0.2897***
	(0.0257)	(0.0314)	(0.0297)
Cashbuyer			0.4383***
			(0.0195)
Treat × after × cashbuyer			−0.2718***
			(0.0418)
Observations	5690	11,204	17,939
R-squared	0.7432	0.5579	0.6510
Panel B: registered total price	ln(RTP)	ln(RTP)	ln(RTP)
Treat × after	−0.0254	−0.1281***	−0.1370***
	(0.0162)	(0.0155)	(0.0145)
Cashbuyer			−0.1973***
			(0.0118)
Treat × after × cashbuyer			0.1194***
			(0.0214)
Observations	5690	11,204	17,939
R-squared	0.8499	0.7103	0.7549
Panel C: actual total price	ln(ATP)	ln(ATP)	ln(ATP)
Treat × after	−0.0013	−0.0219***	−0.0182***
	(0.0114)	(0.0055)	(0.0050)
Cashbuyer			0.0100*
			(0.0058)
Treat × after × cashbuyer			0.0134
			(0.0109)
Observations	5690	11,204	17,939
R-squared	0.9363	0.9495	0.9427
Complex FE	Yes	Yes	Yes
Hedonic attributes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes
Dow FE	Yes	Yes	Yes
Holiday FE	Yes	Yes	Yes

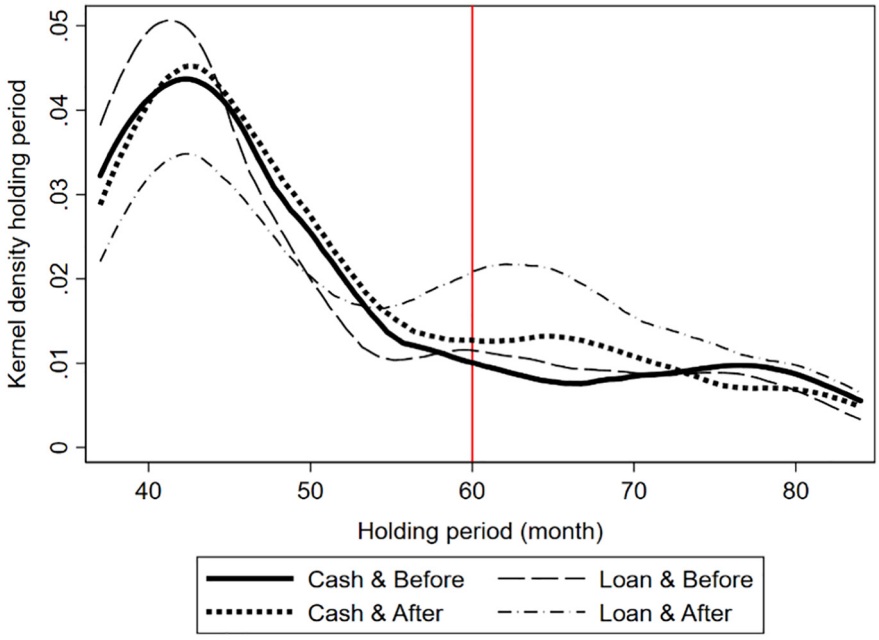

Fig. 7. Distribution of the holding period before and after the tax enforcement: By payment methods. We define the days between January 1 and February 19 as the before-announcement period, the days between February 20 and March 30 as the announcement period, and the days between March 31 and September 26 (180 days) as the after-implementation period (long-run). The cash buyer group includes transactions with buyers paying the full amount using cash; the loan buyer group includes transactions with buyers paying part of the amount using loans from commercial banks or the HPF. Authors' calculation using the Tax Avoidance Sample. The Epanechnikov Kernel is applied with optimal bandwidth. The vertical line denotes the 5-year holding period.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2020.104222.

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