Tax Evasion, Capital Gains Taxes, and the Housing Market

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November 2018

Abstract

In this paper, we exploit a policy shock that differentially increased capital gains taxes for some residential property sellers and document tax evasion in the residential resale market in China. Having *precise* information of the transaction price and the reported price to the tax authority, we show that after the capital gains tax increase, property seller's reported price at the tax authority is 15% lower. We also document that the policy has strong heterogeneous effects, wealthy cash buyers are 5.6% more likely to buy a house as opposed to buyers who need financing. This is mainly because financing buyers would like to have a higher reported price to secure higher bank financing but the seller wants to have a lower reported price to evade taxes. This exasperates wealth inequality in a rising housing market.

Keywords: tax evasion; capital gains tax; housing market; housing policy

JEL Codes: H26; R21; R31

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1. Introduction

Tax evasion is illegal, and it incurs large social cost. Tax evasion had distortionary consequences on the economy as it lowers tax revenues. Many government program and services cannot be supported and it has implications on redistributive policies of a country. According to IMF estimates, total tax losses due to tax heaven are over $400 billion for OECD member states and around $200 billion for lower-income countries[2]. Despite the growing literature documenting tax evasion through hidden wealth/income or under reporting of sales revenues, detecting tax evasion is very difficult and so these are conservative estimates[3]. Vast majority of tax evasion happens in plain sight but we don't have precise data to document it. In this paper, we will use a policy change on capital gains taxes for home sellers with less than five years of tenure and show using administrative data 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 in real estate market to cool down overheated market. The literature has focused on housing-related taxes, including both property tax and transfer tax, on transaction price and volume in the housing market[4]. However, real estate sector is also a market with prevalent tax evasion due to the high transaction value and the ensuing tax burden. As said by the 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[5]." According to a survey of 18 countries by OECD[6], 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

[2] The US Internal Revenue Service (IRS) reports tax gap for some tax years. The tax gap provides a rough gauge of the level of overall noncompliance and voluntary compliance. For year 2008 to 2010, the average annual tax liability is $2,496 billion with $2,038 billion paid voluntarily and on time. This results in a gross tax gap is of $458 billion. However, within the $458 billion tax gap, corporate income tax only takes up $44 billion which is less than 10% of the total tax gap. In contrast, individual income tax gap takes up $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 heaven (mainly applies to corporate income tax) is conservative in terms of the magnitude of tax loss.

[3] Alstadsæter, Johannesen, and Zucman (2017) and Zucman (2013) estimate that $5.6 Trillion is held in tax heavens 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 China and China's reported imports from Hong Kong. Artavanis, Morse, and Tsoutsoura (2016) estimate that 43-45% of self-employed income goes unreported and thus untaxed by inferring from bank's underwriting model. Kleven, Knudsen, Kreiner, Pedersen, and Saez (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 of diesel fuel, fell by a similar amount after the regulators used dyeing technology to identify untaxed fuel.

[4] See Dachis, Duranton, & Turner (2012), Fu, Qian, & Yeung (2016), Slemrod, Weber, & Shan (2017), Best & Kleven (2018) and Deng, Gyourko, & Li (2018).

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

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

can be easily under or over declared, which is documented in countries including but not limited to Australia, Canada, Germany, Ireland, Spain and Sweden. Therefore, it is an important topic to document tax evasion in the real estate market, and more importantly, to understand the consequences of tax evasion.

To investigate the role of tax changes on tax evasion, we take advantage of an administrative data 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[7], 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 a lower registered price to the local housing authority, who share the information to the local tax bureau. Both the registered price and the actual price are known to us in the dataset so 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 aimed to attack speculators in China. Specifically, in 2013 the government effectively increased the capital gains tax[8] of housing transactions with the transacted units' holding period less than five years, hoping that the potential sellers of those units might lower the price or postpone the sale until holding them for five years.

With such policy experiment and the existence of tax evasion in the housing market, we measure the extent of tax evasion in the real estate market and answer the following two questions. First, how does an increase in capital gains tax affect the tax evasion of market participants? Second, has the policy generated any unintended consequences due to the leeway on tax reporting? Specifically, how does the policy affect tax evasion of different types of buyers, such as mortgage buyers and cash buyers, and consequently affect the wealth distribution in an unintended way?

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; 2) we can clearly identify the affected units by the tax increase; 3) we have some information on the buyers, such as gender, age, hometown, income, occupation, as well as the financing of the house. In a difference-in-differences framework, we define the treated units as houses with holding period less than five years since the last transaction dates, which are subject to the 20% capital gains tax after the policy. We define the control group as houses being held for more than five 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 a grace period of slightly over a month, then implemented on March

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

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

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.

We find that the transaction volume declines by 33.3% for the treated group after the increase in capital gains tax. However, the actual prices paid by the buyers and received by the sellers are not affected. One possible reason for the insignificant effect on actual prices can be attributed to tax evasion. Buyers sign the contracts using actual prices with the sellers and report much lower prices to the tax bureau to avoid tax[9]. Therefore, at least a part of the treated resale transactions, are *de facto* not subject to the theoretical tax burden. We calculate the ratio of the registered price over the actual price -- a measure of tax evasion -- and find that the ratio is reduced by 5.4% after the increase in the capital gains tax for the treated units, suggesting that the buyers of the treated units report lower registered price after the policy, compared to similar units in the control group. If we take the tax evasion level before the policy as a benchmark, our estimate indicates that tax evasion is increased by 15% after the policy change. Our results survive from the test of parallel trend of the treatment and control group, different sets of fixed effects, and falsification test using the grace period as the treatment period. Our results also hold for other brokerage firms in the same city, as well as other major cities, and are likely to be a conservative estimate.

How does this leeway of tax reporting affect different types of buyers? 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. For mortgage buyers, a lower registered price (i.e., more tax evasion) would also restrict their 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). After the policy, the marginal cost of mortgage increases 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 (i.e. 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 mortgage buyers from formal financial institutions. In fact, we find that the share of mortgage buyers buying the treated units is reduced by about 5.6 percentage points after the policy, and the loan to actual price ratio (as a proxy for loan to value ratio) is reduced by 7.4 percentage points for the mortgage buyers buying the treated units after the policy. We also provide evidence that such effects are more likely to concentrate in young and relatively low-income households, which are typically less wealthy and less likely to involve in housing speculations. 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.

[9] It is a common practice for buyers to pay most taxes including the capital gains tax in China's residential resale market.

Our research makes three important contributions. First, we study an economically consequential market in China: the housing market, which accounts for 35.35% of the total national wealth in year 2015 (Piketty, Yang, & Zucman, 2017), and 9.5% of the total tax revenue in year 2017[10] (Ministry of Finance, 2018). Second, we have precise information regarding both the actual price and the reported price to the tax authorities to *directly* measure tax evasion instead of using imputed values that along with the policy shock that differentially increased capital gains taxes for some residential property sellers, allows us to have clean identification strategy to study the causal impact of capital gains tax increase on tax evasion behavior. Third and most importantly, our paper is the first to study the redistributive effect of tax evasion. Our results show that cash payers in the housing market, who are richer, evade more taxes than mortgage buyers, and are less impacted by anti-speculation tax policies. Our paper has direct implications to the discussion regarding the growing wealth inequality in China (Piketty et al., 2017). Incorporating the fact that the rich are more capable of evading more taxes due to less financial constraints may further exacerbate the estimated wealth inequality. Our estimates suggest that 694 million *yuan* wealth is transferred to the cash buyer group every month after five years of their home purchase due to the enforcement of the capital gains tax, and the booming housing market.

Our research contributes to three strands of literatures. First, our study can shed some light on the tax evasion literature. Many researchers have devoted themselves to the estimation of tax evasion amount that are mostly related to income tax. We focus on capital gains tax which is different. More importantly, with our dataset, we are able to recover the exact amounts that people evade and hence quantify the magnitude of tax evasion. In addition, we treat tax evasion as a behavioral response to the tax policy change and examine how it affects the market outcome. This is in line with the literature on behavioral responses, especially evasion, to tax policy changes (Balafoutas, Beck, Kerschbamer, & Sutter, 2015; Chetty, 2009; Feldman, Katuš čÁk, & Kawano, 2016; Hanlon, Maydew, & Thornock, 2015; Kleven et al., 2011; Merriman, 2010; Rees-Jones, 2018; Waseem, 2018).

Second, our paper is related to the growing literature studying the outcome of government interventions in the housing market (Agarwal, Badarinza, & Qian, 2018; Agarwal & Qian, 2017; Berger, Turner, & Zwick, 2016; Best & Kleven, 2018; Brogaard & Roshak, 2011; Chambers, Garriga, & Schlagenhauf, 2009; Cho & Francis, 2011; Deng et al., 2018; Dynan, Gayer, & Plotkin, 2013; Floetotto, Kirker, & Stroebel, 2016; Fu et al., 2016; Gervais, 2002; Hembre, 2016; Sommer & Sullivan, 2018; Zhou, 2018). Our paper adds to this strand of works 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.

[10] Please see full report available at http://gks.mof.gov.cn/zhengfuxinxi/tongjishuju/201801/t20180125_2800116.html.

Thirdly, our paper is also related to the discussions regarding tax evasion and corruption. Tax evasion is likely to be a consequence of corruption of tax collectors. For example, Khan, Khwaja, and Olken (2016) find that performance pay for tax collectors actually increase the bargaining power of the tax collectors over taxpayers, who either have to bribe the collectors or pay higher taxes. Sequeira (2016) finds that corruption can significantly reduce tariffs in southern Africa. Identifying the relationship between tax evasion and corruption of tax collectors is beyond the scope of this paper. However, this is a likely story given that corruption has been detected in many sectors in China (Agarwal, Qian, Seru, & Zhang, 2018; Cai, Henderson, & Zhang, 2013; Deng, Wei, & Wu, 2015; Fang, Gu, & Zhou, 2014).

The rest of the paper is structured as the following. Section 2 introduces the policy background; Section 3 discusses our unique dataset and the identification strategy in details; Section 4 shows 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, Gu, Xiong, & Zhou, 2016; Wu, Gyourko, & Deng, 2016). Taking a sample city as an example, Figure I adopts the constant-quality index by Wu, Deng, and Liu (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 Figure I. Such a dramatic housing price surge leads to concerns about the potential consequences, such as affordability problems and bubbles (Li, Qin, & Wu, 2018; Song & Xiong, 2018).

[Insert Figure I here]

Accordingly, the Chinese government has frequently implemented interventions in the housing market (Fang et al., 2016; Song & 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 I, four types of taxes are applied to housing resales in urban China, namely, stamp duty, deed tax, sales tax, and capital gains tax. Except stamp duty which is exempted for our sample period and accounts for only a very small portion of the total tax burden, 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. In order to curb housing speculations, the tax rate is substantially higher for frequent resales with short holding periods: if a unit is resold within five years, the resale transaction is subject to an

additional 5.6% of total price as the sales tax, and 20% of the realized capital gain as the capital gains tax. Meanwhile, larger units are subject to a higher deed tax rate and even a higher sales tax rate. However, unlike some other countries, such as Singapore[11], the tax rates for all these four transaction taxes do not vary with buyers' type (e.g., local households or migrants).

[Insert Table I here]

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 I (e.g., the capital gains tax and the sales tax), are in practice out-of-pocket expenses of the *buyer*[12]. 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 the housing unit in a specific resale, all these four types of transaction taxes are calculated based on the total price of the unit (or the realized capital gain; 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 evade these transaction taxes partially. 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 total actual 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 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

[11] In order to curb housing speculations from foreign buyers, the Singapore government started to implement Additional Buyer's Stamp Duty (ABSD) in 2011, whose tax rate depends on the nationality of the buyer. See (Deng et al., 2018) for more details of ABSD.

[12] For instance, supposing that the total transaction price for a resale unit is 5 million *yuan* RMB (100 square meters, with the holding period less than 5 years and realized capital gain of 2 million *yuan*), without any tax evasion, the total transaction tax would be 77,500 *yuan* for the buyer and 514,500 *yuan* for the seller, respectively. Besides transferring 5 million *yuan* of cash (or via mortgage loan) to the seller and submitting 77,500 *yuan* of buyer-side transaction tax to the local housing authority, the buyer also needs to submit 514,500 *yuan* of seller-side transaction tax to the local housing 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 gain, the capital gains tax is paid simultaneously with the completion of the transaction in China.

(specifically, the buyer) can evade the transaction taxes on the gap between the registered and actual total prices of this transaction[13].

The local housing authorities are aware of the "dual contract" phenomenon and have made some efforts to reduce such tax evasion. Typically, the local housing 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 dwelling unit, if the registered price for the registration is lower than the minimum price in the corresponding submarket, this registration application is rejected and the transaction parties need to submit a new registration application with a revised price. Three points are worth noting here. First, in order to avoid too many disputes and complaints, typically, the local housing authority chooses initially to set the minimum prices below the market level. In addition, these criteria are not frequently updated, even during boom periods. Thus, in most cases, the minimum prices are actually far below the market level. Second, the local housing authority does not publicly release the sub-market level minimum 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 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, Huang, & Chiang, 2015; Wang & 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 it is 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 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), 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[14]. Second, the monthly service,

[13] 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 to the registered price, and the transaction of the affiliated furniture, with the total price equaling to the gap between the actual and registered price. By this mean, the seller (or buyer) can still use the under-the-table contract to protect his or her rights even 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.

[14] More specifically, the HPF center or commercial bank would also 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

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 his/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 he/she can obtain would change proportionally with the total registered price. Therefore, the buyers need to make a tradeoff between lower transaction taxes and larger mortgage loans in determining the registered prices. However, cash payers, are not subject to such tradeoff and are likely to report the lowest possible registered price bound by the submarket-level minimum price.

2.2 Policy shocks in February 2013 and March 2015

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 well as one in March 2015.

As depicted in Figure I, after a stagnant period between early 2011 and mid-2012, housing prices in major cities started to increase rapidly again after 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 more than 10 policy measures, including the enforcement of the capital gains tax for housing resales[15]. In particular (see Table I), 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%

total price. With the existence of "dual contract", in most cases the registered total price is lower than the appraised total price of the unit, and thus is binding here.

[15] The other policy measures include tightened home purchase restrictions, more residential land supply, more investment in public housing, and stricter regulation of developers' land hoarding behavior. 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, as discussed later, none of the other measures affected the transaction taxes of housing resales, or imposed other different treatments on resale transactions with different holding periods.

of the realized capital gain, 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 realized capital gain[16]. 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.

In the context of our sample city, which experienced huge housing price appreciation in the years before, as shown in Figure I, such seemingly trivial stringent tax enforcement led to a striking increase in the effective 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* in March 2009 and resold it in March 2013 (i.e., with a holding period of 4 years). According to the constant-quality house price index in Figure I, 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* in March 2013. Since the holding period was less than five 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 value (i.e., about 53,000 *yuan*). However, following the new code effective since March 31, 2013, the capital gains tax can be calculated only as 20% of the realized capital gain, which would reach as high as 660,000 ((5.30-2)*20%*1000000=660,000) *yuan*. As a result, the total tax burden of this resale transaction would sharply increase from about 429,300 *yuan* (about 8.1% of total price) to about 1,036,300 *yuan* (about 19.6% of the total price). Obviously, such a dramatic increase in effective transaction tax rate provides strong tax evasion incentives for participants of resale transactions with holding periods less than five years. By contrast, housing resales with holding periods exceeding five years are exempted from the capital gains tax, and thus are not affected by this policy change.

In addition to this capital gains tax increase policy change, we explore another policy change—tax cut that occurred in March 2015. As depicted in Figure I, after mid-2014, most major Chinese cities, including our sample city, witnessed a mild decrease in housing prices. As a response, both the central government and the local governments relaxed the housing market intervention policies in late 2014 and early 2015. On March 30, 2015, the Ministry of Finance, the State Administration of Taxation, and the Ministry of Housing and Urban-Rural Development jointly announced that housing resale transactions with holding periods between two and five years would also be exempted from the sales tax[17]. Effective from March 31, 2015, only housing resales with holding

[16] 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 in the previous transaction recorded in the official registration system. Please see http://shiju.tax861.gov.cn/bjds/swcx/ssfgcx/display.asp?aa=1014&neir= for details.

[17] See http://szs.mof.gov.cn/zhengwuxinxi/zhengcefabu/201503/t20150330_1209184.html# for more details.

periods less than two years are subject to the sales tax, which equals 5.6% of the total transaction price (registered price). The tax codes for the other three transaction taxes remain unchanged.

3. Data and Empirical Strategy

3.1 Data

We introduce several proprietary datasets for the following empirical analysis. 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, covering all resale transactions within this brokerage company in 2013. 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. Figure II provides clear evidence of the pervasiveness of transaction tax evasion in the housing resale market. As for the extensive margin, panel A of Figure II depicts a remarkably high share of resale transactions with "dual contracts." Even if we adopt the criterion for the ratio between registered and actual prices ("registered-actual price ratio" hereafter)[18] as 99%, to allow for reasonable errors, the plot suggests that in 95–99% of the housing resales, the transaction parties underreported the transaction prices. As for the intensive margin, panel B depicts the monthly average registered-actual price ratios, which fluctuates between 63% and 66% in the year, with the annual average being 64%.

[Insert Figure II here]

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. We additionally have information on whether the holding period of a unit exceeded five years before the current transaction, which helps us to identify resale transactions subject to the capital gains taxes [19]. In addition, we merge the data with the local Housing Provident Fund contribution data, and acquire information on buyers' 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. Such official registration data also help identify whether the resale transaction

[18] For most of the discussions of the price gap between registered and actual prices in this study, we use the ratio between the registered and actual prices for the apparent reason that it is necessary to normalize the figure by the price level. When we use the absolute difference between them instead, we explicitly point it out.

[19] As listed in Table I, besides units with holding periods less than five 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 could not 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 difference-in-differences models.

was later cancelled by the seller and buyer. The definitions of the variables are listed in detail in Table A.1.

3.2 Empirical strategies

We empirically investigate the existence and consequence of transaction tax evasion in the housing resale market, using the change in tax policy enforcement in February 2013 as a quasi-natural experiment. We first focus on the impact of the capital gains tax enforcement on market outcomes, including transaction volume and prices. For this purpose, we adopt the standard difference-in-differences (DID) method. The treatment group includes all the resale transactions with holding periods less than five years, which were directly affected by the capital gains tax increase; the resales with holding periods over five years, which were exempted from the capital gains tax and thus, were not affected by the tax policy change, serve as the control group. 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 pre-announcement period, the days between February 26 and March 30 as the grace period, and the days after March 31 as the after-implementation (post) period. The days between February 20 and 25, when the information about the stricter housing market intervention had been released but the details of the tax policy change had not yet been announced, are complicated and difficult to classify, and thus, are not included in our analysis. Following this identification strategy, we have

$$Y_{i,j,t} = \beta_1 \times TREAT_{i,j,t} \times POST_{i,j,t} + \beta_2 \times TREAT_{i,j,t} + \beta_3 \times POST_{i,j,t} + X_i + \alpha_j + \delta_t + \varepsilon_{i,j,t} \quad (1)$$

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 unit price, registered unit price, and the ratio of the two as a measurement of tax evasion; $TREAT_{i,j,t}$ is the dummy variable for the treatment group; $POST_{i,j,t}$ is the dummy for the post 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; 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.

Similarly, from the transaction volume perspective, 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:

$$VOL_{k,t} = \gamma_1 \times TREAT_t \times POST_t + \gamma_2 \times TREAT_{i,j,t} + \gamma_3 \times POST_t + \delta_t + \varepsilon_t \quad (2)$$

where $VOL_{k,t}$ refers to the number of units sold (i.e., signing the actual contacts) on day t in the treatment (k=1) or control (k=0) group; $TREAT_t$ refers to the transaction volume in the treatment group; and the other variables are the same as for Equation (1).

For the policy change in March 2015, we employ the same empirical strategy but focus only on change in tax evasion after the policy change.

4. Results

In this section, we first describe the stylized facts around the policy change and next report the impact of the policy on transaction volume. We then focus on the impact of the tax increase on the actual and reported prices of the transacted units. In the next part, we provide heterogeneous analyses by documenting the different tax evasion behaviors of cash buyers and mortgage buyers, and discuss the implications of such heterogeneity for market outcomes. We further provide more discussions regarding the financing cost of mortgage buyers, and the asymmetric effect of the tax increase and tax cut on tax evasion. We then discuss the implication of the findings for wealth redistribution and present the robustness checks. Lastly, we discuss the external validity of our main finding by generalizing to other brokerage firms in the same city and other cities in China.

4.1 Description and stylized facts

We first describe the transaction data in Table II. To understand the dynamics of the policy announcement and implementation better, we divide our sample period into four sub-periods in Table II. The first sub-period (pre-announcement period) has 50 days between January 1, 2013, the date that our sample starts, and February 20, when the preliminary information of the policy change was released to the public. The second sub-period (grace period) has 33 days between February 26, when the tax enforcement was formally announced, and March 31, when it was implemented in the sample city. The third sub-period is another 50 days right after the policy implementation which we consider the short run; the last sub-period is 180 days after the policy implementation which we consider the long run. Note that the 50 days in the third sub-period overlaps with the fourth sub-period.

[Insert Table II here]

Because we examine the residential resale market, whether a transaction occurs (volume) and the transaction price (price) are determined by the bargaining between the buyer and seller. The average number of daily transactions is 122.96 units before the policy announcement and increases to 245 units (99.3% increment) during the grace period. This number then largely decreases to 81.88 units (33.4% decrement from the pre-announcement period) right after the policy implementation. In the long run, the average number of daily transactions climbs back to 110.86 units, which is still a 9.8% decrement from the pre-announcement period. In terms of the (actual) transaction price, the unit price (i.e., *yuan* per square meter) rises from 29,608 *yuan* before the

policy to 32,215 *yuan* right after the policy implementation (8.8% increase) and further rises to 34,522 *yuan* in the long run (16.6% increase).

In Table III, we present the statistics of the four periods in our analysis by treatment status. For the treatment group (i.e., sellers' holding period less than five years), the volume drops hugely by 49/2% from a daily mean of 60.92 units to 30.96 units. The average actual total price of the treated transactions, on the other hand, increases from 2.35 million *yuan* (unit price of 27,740 *yuan*) in the pre-announcement period to 2.58 million *yuan* (unit price of 29,959 *yuan*) in the short run, a 9.6% (8.0%) change. For the control group, the volume decreases from 62.04 units to 50.88 units in the short run after the policy implementation but return to 72.75 units in the long run, which is even higher than the pre-announcement period. The transacted units experience a similar increase in terms of the per square meter price although the total price increase is more moderate. It becomes clear that the major effect of the capital gains tax increase is discouraging transactions in the treatment group. However, the capital gains tax increase does not affect the housing price surge in the treatment group.

[Insert Table III here]

As previously illustrated in Section 2, the buyer and seller need to determine two different prices when they bargain with each other. One price is the actual total price (i.e., *yin* price), the actual amount paid to the seller by the buyer. The other is the registered total price (i.e., *yang* price) that is reported to the tax authority for calculating the tax responsibility. The average ratio between the registered and actual prices, is 66%, 64%, 64%, and 64% for the pre-announcement period, grace period, short run, and long run in Table II. Table III allows us to examine the ratios for the treatment and control groups separately. The ratio for the treatment group decreases from 64 % (pre-announcement) to 59% (shortly after the implementation), while the ratio for the control group decreases only slightly from 67% to 66%. Such a difference suggests that the policy change may induce more tax evasion in the treatment group.

Table II also reports other variables about the resale transactions, including the housing units' hedonic attributes, some basic demographic variables of the buyers, and the payment method. Most of these variables are similar before the announcement, during the grace period, and after the policy implementation, with only two exceptions. First, the composition of the treatment and control groups are very different before and after the policy. While 50% of the transacted units belong to the control group before the policy announcement, this proportion increases to 56% over the grace period and 62% (66% in the long run) after the policy implementation. Second, we observe a substantial change in buyers' payment method. Only 35% of the buyers choose to make the full payment via cash before the policy announcement, increasing to 40% right after the policy implementation and 39% in the long run. Table III further shows that, such an increase mainly comes from the treatment group, whose cash payer ratio increases from 41% to 50% after the

policy change, while the corresponding number increases only from 30% to 34% for the control group.

4.2 Policy's impact on transaction volume and price

The most direct outcome of an increase in capital gains tax should be on the likelihood of deals and hence transaction volume. As discussed in Shan (2011), a capital gains tax increase may generate a "lock-in" effect, which depresses transaction volume in the market because sellers would like to postpone realizing capital gains to avoid the tax.

Figure III shows the effect of a capital gains tax increase on transaction volume without controlling for other factors. Each data point represents the total transaction volume at the daily (panel A) or weekly (panel B) level. It is easy to observe the spike during the grace period, because buyers and sellers rush to complete the transactions to avoid paying the additional tax. However, the transaction volume then sharply decreases after the policy implementation, especially in the treatment group.

[Insert Figure III here]

In columns (1) and (4) of Table IV, we regress the daily number of transactions on the treatment variables following Equation (2). The results show that the tax policy change decreases the transaction volumes of the treatment group significantly relative to the control group. It is not surprising that the enforcement reduces the number of transactions in the treatment group, as buyers would seek either to substitute their targeted houses with similar ones in the control group, because they can 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 after the policy implementation to the grace period to avoid paying higher taxes. Overall, the policy enforcement reduces the number of transactions in the treatment group by 33.3%[20] in the short run and 41.6%[21] in the long run.

[Insert Table IV here]

We then analyze the impact of the capital gains tax increase on the actual transaction price. In our context, buyers pay the capital gains taxes if there is any. Thus, in theory, the actual transaction price (pretax price) of the treated units should decline if the buyers need to pay higher taxes for the units (instead of evading all the increased taxes), and the buyers are not perfectly inelastic regarding housing demand. The major effect of the capital gains tax increase on price is presented in Figure IV. Panel A shows the short-run effect while Panel B shows the long-run effect. In both panels, the horizontal axis denotes time and the vertical axis denotes the average actual unit price.

[20] 1-exp(-0.405)=0.333
[21] 1-exp(-0.538)=0.416

The average prices in both the treatment and control groups increase after the policy implementation.

[Insert Figure IV here]

To provide a more conclusive pattern, we report the regression results on unit price for both the short and long run in Table IV. Following Equation (1), we control for different sets of variables including complex fixed effects, hedonic attributes of the transacted units, month fixed effects, day of week fixed effects, and holiday fixed effects. With the DID study design, the regression shows that there is no significant change in unit price after the tax policy change in the treatment group compared to the control group, at least in the short run. The coefficient of β_1 is negative in the long run, but it is at most marginally significant from both the statistical and economic perspectives. Thus, the more stringent capital gains tax enforcement leads only to little or no price margin[22]. Even though we find little impact of the capital gains tax increase on actual transaction price, we cannot conclude that the policy is ineffective in cooling down housing prices, which needs to be evaluated in a market without tax evasion. In the next subsection, we show that the change in transaction volume and price may be related to buyers' tax evasion induced by the policy change.

4.3 Policy's impact on registered price and tax evasion

In this subsection, we study the impact of the capital gains tax increase on tax evasion. The effect of the policy change on tax evasion can be visualized in Figure V, with panel A for the short run and panel B for the long run. It is evident from both panels that the capital gains tax increase enlarges the gap between the registered and actual prices (i.e., reduces the registered-actual price ratio) of the treatment group, leaving the control group mostly unchanged. In addition, the ratio does not change in the grace period when the tax enforcement is announced but not yet implemented. This pattern is also presented in Figure VI, which plots the density distribution of the registered-actual price ratio.

[Insert Figure V here]

[Insert Figure VI here]

In Table V, we report the regression results on tax evasion with the DID study design in Equation (1). In panel A, the dependent variable is the registered price of the transacted unit. The first two columns report the results for the short run while the last two report the results for the long run. With the most complete set of control variables, the regression analysis shows that the tax policy

[22] One may worry that the prices of the control units are also affected by the policy due to the substitution between treated and control units. We test this hypothesis by comparing the residuals of the actual prices in the control group before and after the policy shock, after partial out time fixed effect, complex fixed effect and hedonic attributes of the houses. We do not find significant differences for the residuals in the pre-period and post-period.

change leads to a drop in registered unit price by about 7.9 percentage points in the treatment group right after the implementation, compared with the control group. This result is very similar in the long run.

[Insert Table V here]

Panel B of Table V reports the results in which the dependent variable is the ratio of the registered over actual price. In all the specifications and for both the short and long runs, the reported coefficients of the DID term are negative and statistically significant at the 1% level. With the most complete set of control variables, the regression shows that the tax policy change leads to a drop in the registered-actual price ratio by about 5.4 percentage points in the treatment group right after the implementation, compared with the control group. This suggests that the buyers of the treated units report lower registered price after the policy change, compared to similar units in the control group. Given the baseline underreporting level of 36% (i.e., the baseline registered-actual price ratio of 64%) in the treatment group before announcement, this is a 15% increase (5.4%/36%=15%) in the intensive margin of underreporting in the short run. On average, the underreporting of price increases by 133,696 *yuan* in the short run. This result and magnitude is very similar in the long run. This result also confirms the observations in Figures V and VI, suggesting that, as a response to the tax increase, the transaction parties of the treatment group further lower the registered price they report to the tax authority relative to the actual price.

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. To make sure that the comparison between the control and treatment group is valid, we compare the housing transaction characteristics in the control group before and after the policy in two ways: first, we conduct t-tests using the means of the variables before and after the policy; second, in order to partial out other seasonality factors that may confound the before and after comparison, we control for the seasonality dummies and compare the residual terms before and after[23]. Table A.2 shows the t-test results for the observables in our dataset including unit price, transaction volume, ratio between registered and actual prices. Other than the three variables, we also show the t-test results for buyers' characteristics and hedonic attributes of the transacted housing units. As shown in the table that the unit price of the control group is significantly higher after the policy shock as well as the percentage of cash buyers. In terms of the buyers' characteristics and housing hedonics, we find that the percentage of female buyers has gone up and people are buying smaller units with smaller number of bedrooms in the control group. These patterns imply that change in the control group by comparing the raw means. However, due to possible seasonality and trending, we also show the results of the residuals after detrending the time series data. The right panel of Table A.2 shows the results and none of the variables is significant at any conventional level. This further

[23] For housing characteristics and buyer characteristics, we partial out day-of-the-week fixed effect, holiday dummy, and a linear trend for month; For actual price and the ratio of registered to actual price, we also partial out complex fixed effect and hedonic housing attributes.

suggests that the change in the control group is largely due to trending in the data. Given the extensive use of time fixed effects in our analyses, the concern should have been mitigated.

4.4 Heterogeneity on mortgage 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 by the tax authority and government housing bureau) 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*[24], 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.

To test the above mentioned pattern, we conduct a heterogeneous analysis to examine the policy's effect on buyers' tax evasion with different payment methods. Panel A of Table VI reports the regression results. The specification is consistent with columns (2) (short run) and (4) (long run) in Table V, but we divide the sample into different groups by payment method. With the full set of control variables and the fixed effects, we find that the policy, in the short run, affects only the registered-actual price ratio for mortgage loan buyers; by contrast, the buyers making full payment in cash are not affected by the policy. In the long run, the same result stands, which is consistent with our conjecture that buyers do not simply take the lower bound when they report the registered price, as that would largely reduce their loan amount. However, after the policy implementation, borrowing money from formal financial institutions incurs a higher cost owing to the increased tax rate. Buyers would then further reduce the registered price and try to self-finance. Those who are already paying the full amount do not face this constraint, because they are already reporting the lower bound before the policy implementation. In panel B of Table VI, we again adopt the DID design in Equation (1) but use the registered price as the dependent variable. It is easy to observe that the cash payers do not change their registered price reported to the authority while mortgage loan buyers significantly lower the registered price, which further confirms our conjecture.

[Insert Table VI here]

[24] 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 I.

The results of the above heterogeneity analysis also suggest that, the effect of the capital gains tax increase disproportionally concentrates on the mortgage 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 mortgage loan buyers have to either pay more transaction taxes, or reduce their mortgage borrowings, both negatively affect their housing demand. As the evidence, in panel C of Table VI, we divide our sample by payment method and rerun the regression analysis on unit price in Table IV with the most complete set of control variables. The results show that the actual unit price paid by cash payers is not affected by the policy, while mortgage loan buyers pay significantly less. According to the coefficient, the actual unit price paid by mortgage loan buyers in the treatment group decreases by about 2% right after the policy implementation, which remains in the long run. This result indicates that mortgage loan buyers are at a disadvantage in the market as they can no longer afford the pretax price before the policy change.

For the same reason, the capital gains tax also has the unintended consequence of reducing the leverage of mortgage buyers from formal institutions, or even crowds out mortgage buyers from the market. We investigate the effect of the policy on mortgage financing in Table VII. For the extensive margin, column (2) suggests that the share of mortgage loan buyers in the treatment group decreases by about 5.6%. This is a remarkable decrease, given the baseline mortgage loan buyer ratio of 40.7% in the treatment group before the policy change. For the intensive margin, we calculate LTV ratio for each transaction by dividing the principal of the mortgage loan by the actual total price as a proxy. We find that the LTV ratio is reduced by 7.4 percentage points with the full set of control variables. To calculate the magnitude, for mortgagors, the average size of the loan is 1,109,410 *yuan* for the treated unit before the policy change. Therefore, the size of the loan is reduced by about 82,207 *yuan* for the treated units. On average, the underreporting increases by 133,696 *yuan*, leading to tax savings of 35,563 *yuan* per transaction. The calculation suggests that the reduction in loan amount can mostly be attributed to the lowered registered price[25]. These results stand in the long run, too.

[Insert Table VII here]

In Table VIII, we provide the last piece of evidence on the heterogeneous effect on cash payers and mortgage loan buyers, which further highlights the disadvantage of the mortgage loan for buyers facing the tax policy change. In both the probit and linear probability models, we explain the likelihood of a contract being cancelled after the resale contract is signed and registered with the local housing authority. It is evident that the mortgage loan buyers in the treatment group are more likely to cancel their contracts after the policy than the cash payers are. The results in column (6) suggest that, after the policy change, the likelihood of a contract being cancelled for mortgage buyers in the treatment group is about 4% higher than that of the cash buyers. It is economically

[25] Under reporting of the registered price would decrease the mortgage loan size of a typical unit in our sample by 0.0974*17,336.14*85*0.7=100,468 *yuan*.

significant, considering that the ratio of a contract being cancelled is 2% for mortgage loan buyers in the treatment group before the policy. Although we cannot figure out the exact reason for contract cancellations, it is highly possible that they are caused by buyers' inability to fulfill the payment requirement with less mortgage loans from formal institutions after the tax policy change.

[Insert Table VIII here]

The above discussions in this subsection suggest that the increase in capital gains tax significantly affects the tax evasion and borrowing capability of mortgage buyers. There are two potential types of mortgage loan buyers, leading to distinct policy implications. On the one hand, investors or even speculators in the housing market might use leverage to enhance their investment profitability. On the other hand, young or relatively low-income households might not have accumulated enough equity and thus, have to rely on mortgage loans. Our further evidence listed in Table IX suggests that the crowding-out effect revealed above in this subsection is more likely to concentrate on the latter type. The first five columns of the table focus on the potential change in buyers' characteristics in the treatment group. The results suggest that buyers in the treatment group are more likely than those in the control group to be older and have higher income after the policy implementation. Meanwhile, the last column of the table also points to a larger unit size in the treatment group right after the policy change. All these results suggest that less wealthy households, which have to rely more on mortgage financing, are more vulnerable to the tax policy change.

[Insert Table IX here]

4.5 Discussion on mortgagors' incremental borrowing cost

Mortgage buyers face a tradeoff between borrowing more money from the bank and paying more taxes. Before the policy, for each additional *yuan* of reported registered price, the buyer needs to pay roughly 0.076 *yuan* in taxes and has access to an additional 0.7 *yuan* mortgage from the bank (assuming a 30% down payment). Therefore, the net additional down payment prepared by the buyer is reduced by 0.624 (0.7-0.076=0.624) *yuan* at the cost of 0.076 *yuan* taxes. If we normalize the unit, for each additional *yuan* reduction in down payment, the financing cost is 0.076/0.624=0.12 *yuan*. Therefore, if the cost of borrowing one additional *yuan* from informal financing (e.g. relatives and friends) is more than 0.12 *yuan*, the buyer keeps reporting higher registered prices and borrowing from the bank[26], otherwise borrowing from informal financing or self-financing sources for the down payment.

[26] Here, we assume that the marginal cost of financing an additional *yuan* of mortgage is zero for two reasons: first, in China, the mortgage rate (4.9% in our sample period) is very close to the risk-free opportunity cost (4-5% risk free wealth management products in commercial banks); second, the mortgage origination fee is only around 1,000 *yuan,* which is negligible.

After the tax enforcement policy, for each additional *yuan* of reported registered price, the buyer of the treated units needs to pay roughly 0.266 *yuan* taxes and has access to an additional 0.7 *yuan* mortgage from the bank. Therefore, the net additional down payment prepared by the buyer is reduced by 0.434 (0.7-0.266=0.434) *yuan* at the cost of 0.266 *yuan* in taxes. In other words, for each additional 1-*yuan* reduction in down payment, the cost is now 0.266/0.434=0.61 *yuan*. Therefore, for the treated transactions, the marginal cost of an additional *yuan* in down payment financing increases from 0.12 *yuan* to 0.61 *yuan* after the policy change.

Given the change in the marginal cost of down payment financing, we are interested in the borrowers' cost function. However, it is impossible to recover such a cost function, because we have only two marginal costs at two different equilibria (before and after the policy change). Therefore, using the regression analysis, we draw only the two points on the cost function without assuming any functional form, where the x-axis is the down payment of the borrowers, and the y-axis is the marginal cost of borrowing an additional *yuan* of down payment. Specifically, we perform a DID analysis whereby the outcome variable is the down payment of each transaction (regression results are available in Table A.3 in the Appendix).

Figure VII plots the points on the cost function for all the mortgagors and for heterogeneous groups of mortgagors. The upper left figure shows the results for all mortgagors: the x-axis of the "before" point is the average size of down payment for the buyers of treated units before the policy, and the y-axis is the corresponding marginal cost of an additional unit (in thousand *yuan*) of down payment; the x-axis of the "after" point is the x value of the "before" point plus the causal effect of the policy on the down payment, which is shown from the regression results (column 1 of Table A.3). The stars next to the "after" point represent the significance level of the regression coefficients. Thus, the figure suggests that the mortgagors move from the "before" point to the "after" point after the policy change.

[Insert Figure VII here]

The upper right panel of Figure VII shows the "before" and "after" points by the median age (32 years old) of mortgage buyers. Apparently, older buyers have less liquidity constraint, as suggested by the position of the "before" points. Moreover, the slope of the two points is steeper for the younger group, which indicates a possibly steeper marginal cost function of young buyers, because they face more challenges in financing additional dollars from informal channels. The lower left panel of Figure VII shows the two points by local and non-local buyers. Non-local buyers seem to be more liquidity constrained in terms of the average down payment before the policy, as well as a steeper slope of the cost function. This is consistent with the intuition that non-local buyers are more likely to have less equity, while local buyers are more likely to own a unit in the city already or to have inherited a unit from their parents, which they can then use as equity. Lastly, the lower right panel of Figure VII shows the two points by the median of annual income (47,450 *yuan*).

This result is consistent with our intuition that high wage earners are less liquidity constrained and are likely to have a flatter marginal cost curve.

4.6 Implications for wealth distribution

In Table VII, we show that the share of cash buyers increases by approximately 5.6% after the policy shock, because some mortgage buyers are crowded out by cash buyers owing to increasing financing difficulty after the enforcement of the capital gains tax. Such a shift of buyer composition may have implications for overall wealth distribution, because cash buyers are generally richer than mortgage buyers are.

In order to understand the wealth distribution implication, we compute the average growth rate of each complex (unadjusted for hedonic characteristics due to small transaction volumes at the complex level) and impute the one-year to five-year rate of return and the absolute wealth change for each transaction after the policy (short run)[27]. As presented in Table X, overall, cash buyers earn approximately 2–2.8% higher rate of return in the first 1 and 2 years of the holding period than mortgage buyers can, possibly owing to the ability to finance houses in better locations and the flexibility in buying houses at the desired timing. Such difference becomes more significant and larger in magnitude if we compare the net wealth gain of cash buyers and mortgage buyers. On average, cash buyers harvest approximately 424,000 *yuan* more than mortgage buyers do in a five-year time horizon, considering that cash buyers earn higher rates of returns and buy larger units.

[Insert Table X here]

The estimate in Table VII that the share of cash buyers increases by approximately 5.6% after the policy shock can be translated into a net increase of 3.44 cash buyers every day[28]. The market share of the brokerage firm in our dataset is 37.2% at the beginning of 2013. Therefore, the net increase of the number of cash buyers in the whole resale market in this city is around 9.25 (3.44/37.2%) per day, equivalent to an increase of 277.5 (9.25*30) cash buyers every month. The average net increase of approximately 2.5 million *yuan* in net wealth of this group of buyers in 5 years (row 10, column 4 of Table X), which suggests that 694 million *yuan* (277.5*2.5 million) in wealth is transferred to the cash buyer group every month five years after their home purchase. Given that the cash buyers are already in the top percentiles of wealth distribution, the enforcement of the capital gains tax ultimately exacerbates wealth inequality by transferring wealth gains from relatively poor buyers to richer buyers.

[27] The data source for computing the complex level growth rate is from: 1) the brokerage firm that we use in this paper (2013-2015); and 2) 13 major brokerage firms in this anonymous city (2016-Sep 2018).
[28] The number of cash buyers in the "before&treat" group is 3,046 in 50 days, which translates into 60.92 cash buyers every day. 60.92*5.6%=3.44.

4.7 Asymmetric effect of policy on tax evasion

Up to now, we have shown that an increase in the capital gains tax increases homebuyers' tax evasion. Is this effect symmetric? In other words, would a cut in transaction tax reduce homebuyers' tax evasion? To answer this question, in this subsection, we explore another policy shock that occurred in March 2015 in the same city.

Making use of the policy shock in March 2015, we define the treated units as those with holding periods between 2 and 5 years[29]. These units were subject to the 5.6% sales tax before March 2015, but were exempted from the tax after the policy change. The units with a holding period more than five years or less than two years were not affected and thus are in the control group. Therefore, the marginal tax cost of mortgage loans decreases, and we are interested in whether mortgage buyers respond to the tax cut by reporting higher registered prices relative to actual prices. Table XI shows the estimation results. The tax cut in March 2015 does not affect tax evasion in the treatment group as shown in columns (1) and (2). This result indicates that tax evasion in the housing market is asymmetric. In other words, mortgage buyers respond to the tax increase by evading more but do not respond to tax cut by evading less. Such an asymmetric result is consistent with the loss aversion explanation, because people perceive extra tax expenses as losses. This documented behavior is consistent with the explanation that is provided by Rees-Jones (2018). In the loss domain, the per-dollar marginal utility is higher than in the gain domain. Hence, people are more likely to evade tax to avoid the change when increased tax amount is perceived as a loss but are less likely to do so when the decreased tax amount is perceived as a gain.

[Insert Table XI here]

4.8 Robustness checks

In this subsection, we conduct robustness checks to ensure the validity of our research design. There may be concern 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 grace period to conduct a placebo test, as shown in Table XII. We replace the policy implementation with the policy announcement and replicate the specifications in Table V. With all specifications, the

[29] To construct the identifier for the treatment and control groups, we need to figure out whether the holding periods of the transacted units are less than two years, between two years and five years, or longer than five years. Our dataset has a variable indicating whether a unit has been held for at least five years but does not have a similar variable for the two-year threshold. Hence, we back out this information by examining the actual paid 5.6% sales tax after the policy change (i.e. the tax cut). If a transaction happened after the policy change and paid the 5.6% sales tax, it indicates the holding period was less than two years when transacted. For the transactions that happened before the policy change, we have to match them to the most recent previous transaction using the characteristics of the transacted units. We are able to match 16.5% of the sample and those non-matched transactions are a random subset of all the transactions. In our regression analysis, we only include the transactions with which we have the accurate information on the holding period.

estimated coefficients are insignificant at all conventional levels. This further shows that the change in tax evasion is induced by the policy implementation.

[Insert Table XII here]

Third, we verify the parallel trend assumption of the DID specification by conducting two additional tests. On the one hand, we further divide the pre-policy period and take the first three weeks as the benchmark period. $PRE_{i,j,t}$ is the period (four weeks) after the benchmark period but before the policy announcement. The definitions of other variable are the same as in Equation (1). The model specification is

$$\begin{aligned} RATIO_{i,j,t} = {} & \beta_1 \times TREAT_{i,j,t} \times PRE_{i,j,t} + \beta_2 \times TREAT_{i,j,t} \times POST_{i,j,t} \\ & + \beta_3 \times TREAT_{i,j,t} + \beta_4 \times PRE_{i,j,t} + \beta_5 \times POST_{i,j,t} + X_i \\ & + \alpha_j + \delta_t + \varepsilon_{i,j,t} \end{aligned} \tag{3}$$

The regression results in Table XIII show that we indeed have a parallel trend between the control and treatment groups. Relative to the control group, the treatment experienced no significant change from the benchmark period to the pre-policy period.

[Insert Table XIII here]

On the other hand, we conduct a dynamic event study with the following specification as shown in Equation (4), where the first three weeks are used as the benchmark period.

$$\begin{aligned} RATIO_{i,j,t} = {} & \sum_{k=4}^{20} \beta_k \times TREAT_{i,j,t} \times 1\{Week_{i,j,t} = \mathrm{k}\} + \theta \times TREAT_{i,j,t} \\ & + \pi_k \times 1\{Week_{i,j,t} = \mathrm{k}\} + X_i + \alpha_j + \delta_t + \varepsilon_{i,j,t} \end{aligned} \tag{4}$$

Figure VIII plots the coefficients as well as the 95% confidence intervals of the coefficients β_k. Relative to the control group, the price ratio for the treatment group significantly decreases after the policy implementation, with no significant change before the policy implementation.

[Insert Figure VIII here]

Fourth, we use the absolute amount of price gap (i.e. actual total price – registered total price) to replicate the main analysis in Table V. The results are available in Appendix Table A.4. All coefficients are significant at the 1% level. With the most comprehensive set of control variables, the absolute gap between the registered price and actual price in the control group increases after the policy implementation. This result stands in both the short run (column (2)) and long run (column (4)).

Lastly, in some cases, the price finally registered with the local housing authority and thus, adopted in the transaction tax calculation might be different to the price that the buyer and seller originally

planned to report (i.e., 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 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 he/she needed to apply for more mortgage loans and thus, had to raise the price reported to local housing and tax authorities.

In order to investigate the potential effect of such a reported price change, we merge the transactions in both the pre-announcement period and the short-run after implementation period with the official housing transaction registration data according to the addresses of the transacted units. For all the 18,327 units transacted in these two sub-periods, we successfully merge the official registration data for 15,897 units (or 86.7%). As listed in Table A.5, for about 75% 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 25% units. These patterns do not change between the cash buyers and the mortgage buyers. In Appendix Table A.6, we replicate the main specifications in Tables V and VI, using only the observations whose registered prices kept unchanged, and the results remain robust.

4.9 External validity

Some may concern the external validity of our research because our main analysis is from one major brokerage firms in one major city, in particular, whether our results hold for other brokerage firms in this city too. We argue this is *not* a concern in our study since the brokerage firm we in our dataset accounts for the largest market share in this city and is more regulated, customers may actually engage in less evasion. As we also have the whole universe of the registered prices in the city for the same sampling period, we can test this hypothesis by comparing the registered price from our collaborated firm and other brokerage firms. Consistent with our conjecture, Table XIV Panel A shows that after the policy change in March 2013, taking other brokerage firms as the control group and our collaborating firm as the treatment group, the declines in registered prices reported by our collaborating firm are significantly lower than the declines in registered prices from other firms. Under the assumption that the transacted housing units are not selected among different firms, this result suggests that home buyers in other brokerage firms may engage in even more tax evasion than the buyers in our sample after the policy change. Therefore, our estimates in the current study are conservative regarding the average response in tax evasion to the tax policy change.

In addition, we collect city-level monthly average prices of housing resales for 35 major cities in China from Jan 2012 to Dec 2013. The average registered price data are reported by local housing authorities. We also obtain average listing 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. We use the following two specifications to conduct external validity check using the monthly level data:

$$RATIO_{i,t} = \beta_1 TREAT_i \times POST_{i,t} + \alpha_i + \delta_t + \epsilon_{i,t} \quad (5)$$

$$RATIO_{i,t} = \beta_1 TREAT_i \times POST_{i,t} + \beta_2 TREAT_i * PRE_{i,t} + \alpha_i + \delta_t + \epsilon_{i,t} \quad (6)$$

where $TREAT_i$ is a dummy variable that takes the value 1 if city i follows the policy of capital gains tax increase in March 2013[30]. $POST_{i,t}$ is a dummy variable that takes the value 1 after city i implements the policy change (i.e. the increase in capital gains tax). α_i is city fixed effects and δ_t is month fixed effects. In the second specification, we use January to August in 2012 as the baseline period. We further define a pre-period which is the six months before policy announcement (September 2012 to February 2013). Table XIV Panel B reports the regression results with columns 1 and 2 showing results from the first model and columns 3 and 4 showing results from the second model. The $TREAT$ variable is for the 7 cities in columns 1 and 3 and for the 13 cities in columns 2 and 4. It is obvious from the regression results that the ratio in the pre-policy period is not significantly different from the before period but the post period is 3.5-4.1% significantly lower ($p<0.1$) than the before period. Given the average registered to listing price ratio in the treated cities before the policy shock (0.59 in the first treatment definition and 0.63 in the second treatment definition), the estimates suggest a 9.5-10% growth in tax evasion. The above result suggests that our estimate for the one major city could be a lower bound: the average registered to actual price ratio in that major city dropped from 0.66 to 0.64 after the policy, which is a weighted effect of the reduction (5.6%) in the treatment group (approximately one third of the transactions) and a zero effect in the control group (approximately two thirds of the transactions). This is a smaller magnitude compared to the estimate in the 35-city analysis.

[Insert Table XIV here]

5. Conclusion

Tax instruments are important policy tools in the real estate market. However, the existence of tax evasion may affect the outcome of tax-related policy and even generate unintended consequences. Using a 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 in March 2013 as a policy experiment, we find that the increase in capital gains

[30] We construct two versions of the treatment variable in our regression. Treat1 takes the value 1 for the 7 cities that explicitly published the details on how to implement the policy. These seven cities include Shanghai, Beijing, Nanning, Xiamen, Tianjin, Shenyang, and Chongqing. Treat2 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 gain 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, Huhehaote, Harbin, Dalian, Taiyuan, Chengdu, Kunming, Wuhan, Jinan, Haikou, Shijiazhuang, Fuzhou, Xining, Xi'an, Guizhou, Zhengzhou, Yinchuan, Changchun, and Changsha.

tax has very marginal effect on transaction prices of the affected housing units subject to capital gains tax increase, if not zero. A very likely reason for this little policy effect is due to tax evasion: buyers of the treated units underreport their actual transaction price more by approximately 5.4 percentage points after the policy.

More importantly, the impact of the capital gains tax increase on tax evasion is very heterogeneous among cash buyers and mortgage buyers. After the policy, it becomes more costly for mortgage buyers to receive each extra *yuan* of loan because of the higher taxes associated with per *yuan* registered price. Therefore, mortgage 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 thus the policy has virtually zero effect on cash buyers' tax evasion. Moreover, we observe 5.6% more cash buyers buying the treated units after the policy, indicating that the wealthier cash buyers crowd out less wealthy mortgage buyers due to the capital gains tax increase. In a booming housing market, our results suggest that the wealthier group enjoys more wealth increase from the housing market, which may further exacerbate wealth inequality in China.

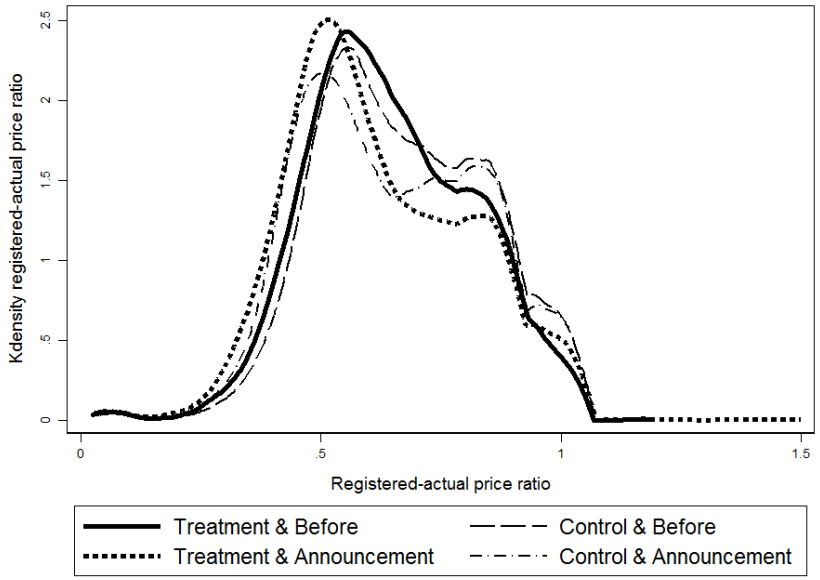
Our findings can shed some light on the discussions about China's growing wealth inequality. Piketty et al. (2017) estimate that the Chinese top 10% wealth share (67% in 2015) is getting close to that of the United States (72%) and is much higher than in a country like France (50%). The increase in housing prices is an important factor contributing to the wealth growth of the Chinese. However, Piketty et al. (2017)'s estimation has no information regarding tax evasion which may underestimate the rise of inequality (see Section 4 of Piketty et al. (2017)). Our results provide two pieces of new evidence along this line: first, our precise measurement of tax evasion in the housing market shows that the wealthier group (the cash buyers) evade more taxes; second, the wealthier group may benefit more from booming market if government tightens the tax enforcement. Both pieces of evidence suggest that incorporating tax evasion is likely to increase the current estimates on wealth inequality.

Tax instruments may not work as the policy maker would intend. In this paper, we show the redistributive consequence of a capital gains tax increase, which may deviate from the policy goal, (i.e., curbing speculation in the housing market), and may amplify wealth inequality due to the crowding out of the less wealthy buyers. Our research provides references to policy makers 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.

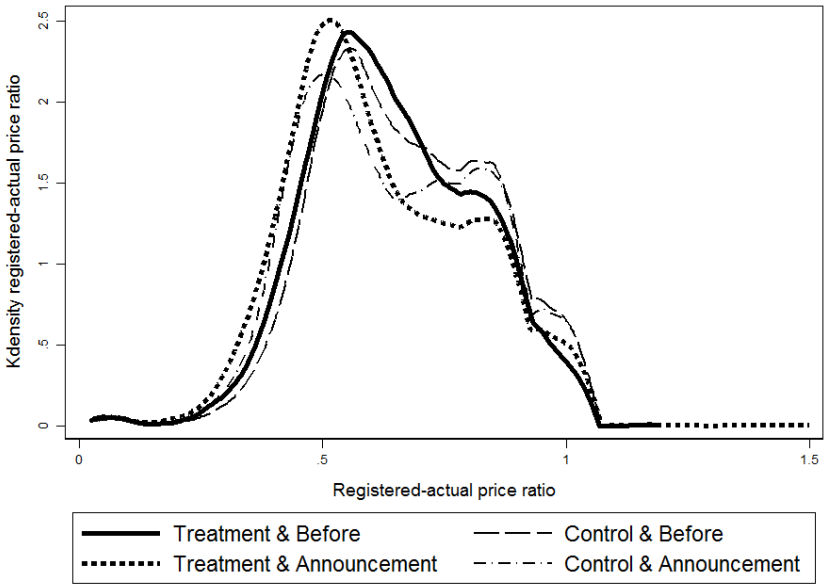
Appendix

Figure A.1

Distribution of Registered-actual Price Ratio (Placebo Test)



A. Short Run



B. Long Run

Note. Epanechnikov Kernel is applied with optimal bandwidth.

Table A.1

The Definitions of the Variables

Variable	Explanation
A. Transaction Information	
a_totalprice	Actual total price (thousand *yuan*)
r_totalprice	Registered total price (thousand *yuan*)
a_unitprice	Actual unit price (*yuan*)
r_unitprice	Registered unit price (*yuan*)
volume	Transaction volume (unit)
ratio	Registered-actual price ratio
agap	Absolute gap between registered price and actual price (thousand *yuan*)
cancel	Contract cancellation
dp	Down payment (thousand *yuan*)
B. Housing Attribute	
if_full5	Resold within 5years
unitsize	Housing area (sq.m.)
floor	Floor level of the unit
bedroom	Number of bedrooms
build_year	Complete year of the building
C. Individual Characteristic	
cashpayer	Whether the buyer was a cash payer
b_gender	Gender of the buyer; 1 = male, 0 = female
b_local	Whether the buyer was born in the sample city
b_age	Age of the buyer
b_income	Yearly income of the buyer (*yuan*)
b_public	Whether the buyer worked in the public sector

Data source: Micro-level housing resale transaction data are from one of the largest housing brokerage firms in the sample city, covering all resale transactions in this brokerage company in 2013.

Table A.2

Effect of the Tax Enforcement on Down Payment: By Buyer Characteristics (Long Run)

	Raw					Detrend (residual)				
	Before		After		Diff	Before		After		Diff
Variables	N	Mean	N	Mean	MeanDiff	N	Mean	N	Mean	MeanDiff
a_unitprice	3,102	31000	2,544	34000	-2.1e+03***	3,058	-0.0001	2,521	0.0001	-0.0002
r_unitprice	3,102	20000	2,544	21000	-1.1e+03***	3,058	0.0003	2,521	-0.0004	0.0007
ratio	3,102	0.670	2,544	0.663	0.007	3,058	0.0004	2,521	-0.0005	0.0009
volume	50	62.040	50	50.880	11.160	\	\	\	\	\
cashpayer	3,102	0.302	2,544	0.339	-0.036***	3,102	-0.004	2,544	0.00500	-0.010
b_age	3,091	35.010	2,527	34.630	0.388	3,091	-0.039	2,527	0.0470	-0.086
b_gender	3,094	0.553	2,535	0.577	-0.024*	3,094	-0.001	2,535	0.00100	-0.002
b_local	3,094	0.362	2,535	0.346	0.016	3,094	0.001	2,535	-0.00100	0.002
b_public	1,472	0.214	1,214	0.220	-0.006	1,472	0.001	1,214	-0.00100	0.002
b_income	1,472	52000	1,214	51000	1008	1,472	40.550	1,214	-49.17	89.730
unitsize	3,102	82.080	2,544	78.090	3.989***	3,102	0.058	2,544	-0.0710	0.129
floor	3,079	6.716	2,535	6.594	0.122	3,079	-0.019	2,535	0.0240	-0.043
bedroom	3,102	2.055	2,544	1.994	0.061***	3,102	-0.001	2,544	0.00100	-0.002

Note. This table reports the before and after difference in terms of actual unit price, registered unit price, actual to registered price ratio, transaction volume, buyers' characteristics and housing characteristics for the housing units in the control group. The left panel reports the raw difference, while the right panel reports the detrended difference. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

Table A.3

Effect of the Tax Enforcement on Down Payment: By Buyer Characteristics (Long Run)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Loan	Young	Old	Non-local	Local	Low income	High income
VARIABLES	ln(*DP*)	ln(*DP*)	ln(*DP*)	ln(*DP*)	ln(*DP*)	ln(*DP*)	ln(*DP*)
TREAT x *POST*	0.0577***	0.0330	0.0983*	0.0604**	0.0522	0.0611	0.131*
	(0.0204)	(0.0276)	(0.0561)	(0.0267)	(0.0653)	(0.0696)	(0.0761)
Observations	9,413	6,387	2,995	7,060	2,325	2,096	2,091
R-squared	0.841	0.848	0.900	0.848	0.915	0.904	0.916
Complex FE	YES	YES	YES	YES	YES	YES	YES
Hedonic Attributes	YES	YES	YES	YES	YES	YES	YES
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
Baseline Average	1594.12	1377.57	1956.04	1549.45	1696.35	1391.64	1711.79

Note. This table reports the coefficient β_1 in Equation (1) by buyer characteristic, where down payment is the outcome variable. Robust standard errors clustered at the complex level are used 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.

Table A.4

Robustness Check: Magnitude of Price Gap (in thousand *yuan*)

	(1)	(2)	(3)	(4)
	Short Run		Long Run	
VARIABLES	Agap	Agap	Agap	Agap
TREAT × *POST*	109.4***	125.8***	82.2***	94.3***
	(28.07)	(26.10)	(18.37)	(16.69)
Observations	10,111	10,111	25,769	25,769
R-squared	0.750	0.784	0.675	0.723
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
Actual Total Price	NO	YES	NO	YES

Note. This table reports the coefficient β_1 in Equation (1). The first two columns report the estimate on the short-run (50 days) effect; while the last two columns report the estimate on the long-run (180 days) effect. Robust standard errors clustered at the complex level are used 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.

Table A.5

Comparison of Two Data Sources

		Full sample		Cash		Loan	
		Freq.	Share	Freq.	Share	Freq.	Share
Registered price from local housing authority / Registered price in our data	~ 0.90	1,153	7.25%	459	7.59%	694	7.05%
	0.90 ~ 0.97	758	4.77%	336	5.56%	422	4.28%
	0.97 ~ 1.00	458	2.88%	144	2.38%	314	3.19%
	1.00	11,920	74.98%	4,518	74.71%	7,402	75.15%
	1.00 ~ 1.03	403	2.54%	118	1.95%	285	2.89%
	1.03 ~ 1.10	414	2.60%	176	2.91%	238	2.42%
	1.10 ~	791	4.98%	296	4.89%	495	5.03%
Matched sample		15,897	100.00%	6,047	100.00%	9,850	100.00%
Full sample		18,327		7,142		11,185	

Note. This table compares the registered price in our data (provided by the brokerage firm) and the data from the registration database of the local housing authority.

Table A.6

Robustness Check: Refined Sample (Short Run)

VARIABLES	(1) Full sample *RATIO*	(2) Cash *RATIO*	(3) Loan *RATIO*
TREAT x *POST*	-0.0551***	-0.00692	-0.0617***
	(0.0111)	(0.0174)	(0.0153)
Observations	6,487	2,316	4,171
R-squared	0.553	0.809	0.565
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
Actual Total Price	YES	YES	YES

Note. This table reports the coefficient β_1 in Equation (1) by different payment methods. The sample is limited to the observations with the same registered price from the resale transaction dataset and the official housing transaction registration dataset. The table reports the estimate on the short-run (50 days) effect. Robust standard errors clustered at the complex level are used 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.

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Tables

Table I

Summary of Transaction Taxes for Housing Resales in the Sample City

	Before Mar 30, 2013		Since Mar 31, 2013	
	Buyer	Seller	Buyer	Seller
Stamp Duty (exempted)	0.05% of total price	0.05% of total price	0.05% of total price	0.05% of total price
Deed Tax	● 1% of total price for first home under 90 sq.m. ● 1.5% of total price for first home between 90 and 140 sq.m. ● 3% of total price for all other cases	N.A.	● 1% of total price for first home under 90 sq.m. ● 1.5% of total price for first home between 90 and 140 sq.m. ● 3% of total price for all other cases	N.A.
Sales Tax	N.A.	● 5.6% of total price if the holding period since previous transaction was less than 5 years. ● 5.6% of realized capital gain since previous transaction if: 1) the holding period since previous transaction exceeded 5 years; and 2) the floor area of the unit was larger than 140 sq.m. ● 0% for all other cases.	N.A.	● 5.6% of total price if the holding period since previous transaction was less than 5 years. ● 5.6% of realized capital gain since previous transaction if: 1) the holding period since previous transaction exceeded 5 years; and 2) the floor area of the unit was larger than 140 sq.m. ● 0% for all other cases.
Capital Gains Tax	N.A.	● 20% of realized capital gain since previous transaction, or 1% of total price, if the holding period since 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 realized capital gain since previous transaction if the holding period since previous transaction was less than 5 years, or the seller owned more than one unit in the city. ● 0% for all other cases.

Note: Information collected by authors from official sources.

Table II

Summary Statistics on the Key Variables around the Tax Enforcement

	Pre-Announcement			Grace Period			After-Implementation (Short run)			After-Implementation (Long run)		
Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
A. Transaction Information												
a_totalprice	6,148	2389.57	1268.09	8,085	2631.19	1526.13	4,094	2526.08	1350.87	19,955	2699.46	1449.27
r_totalprice	6,148	1519.36	827.79	8,085	1598.99	960.00	4,094	1545.53	821.19	19,955	1679.96	936.76
a_unitprice	6,148	29608.42	13274.77	8,085	32585.39	14278.18	4,094	32215.38	13705.03	19,955	34521.86	13646.52
r_unitprice	6,148	18851.97	9115.26	8,085	19862.35	9062.39	4,094	19765.41	9191.44	19,955	21448.84	9211.64
volume	50	122.96	83.12	33	245.00	119.01	50	81.88	41.46	180	110.86	53.65
ratio	6,148	0.66	0.17	8,085	0.64	0.19	4,094	0.64	0.18	19,955	0.64	0.18
agap	6,148	870.21	730.23	8,085	1032.20	928.06	4,094	980.55	821.88	19,955	1019.50	828.26
cancel	5,343	0.03	0.16	6,930	0.01	0.11	3,624	0.03	0.18	\	\	\
dp	1,737	1477.18	1051.21	2,710	1576.03	1092.71	1,504	1600.99	1189.59	7,775	1634.43	1105.88
B. Housing Attribute												
if_full5	6,148	0.50	0.50	8,083	0.56	0.50	4,092	0.62	0.49	19,912	0.66	0.47
unitsize	6,148	84.85	35.93	8,085	84.92	47.75	4,094	82.14	35.55	19,955	81.77	44.64
floor	6,110	7.40	6.09	8,034	7.41	6.10	4,074	7.06	5.90	19,855	7.27	5.92
bedroom	6,148	2.00	0.79	8,085	2.00	0.80	4,094	2.00	0.76	19,955	1.97	0.76
build_year	6,140	2000.48	7.45	8,073	2000.20	7.48	4,092	1999.95	8.01	19,947	1999.89	7.84
C. Individual Characteristic												
cashpayer	6,148	0.35	0.48	8,085	0.41	0.49	4,094	0.40	0.49	19,955	0.39	0.49
b_gender	6,127	0.55	0.50	8,060	0.56	0.50	4,080	0.56	0.50	19,903	0.55	0.50
b_local	6,127	0.39	0.49	8,060	0.39	0.49	4,080	0.37	0.48	19,903	0.36	0.48
b_age	6,117	35.39	10.12	8,036	34.86	10.37	4,070	35.05	10.59	19,860	34.93	10.49
b_income	2,787	50153.38	28885.25	3,809	50945.77	30637.73	1,897	49275.38	31811.69	8,740	53347.23	32375.84
b_public	2,787	0.21	0.41	3,809	0.23	0.42	1,897	0.21	0.41	8,740	0.20	0.40

Note. We define the days on and before February 19, 2013 as the pre-announcement period, the days between February 26 and March 30 as the grace period, and the days since March 31 as the post 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 post period. The summary statistics are calculated by the authors from the data.

Table III

Summary Statistics around the Tax Enforcement: By Treatment and Control Groups

		Treatment				Control			
		Pre-Announcement	Grace Period	After (Short run)	After (Long run)	Pre-Announcement	Grace Period	After (Short run)	After (Long run)
a_totalprice	Obs	3,046	3,575	1,548	6,817	3,102	4,508	2,544	13,095
	Mean	2353.38	2679.82	2579.99	2645.50	2425.11	2591.23	2494.24	2728.32
	Std. Dev.	1355.61	1741.56	1526.32	1579.02	1174.98	1326.92	1231.21	1375.86
r_totalprice	Obs	3,046	3,575	1,548	6,817	3,102	4,508	2,544	13,095
	Mean	1473.79	1602.03	1472.40	1542.67	1564.11	1596.31	1590.77	1752.24
	Std. Dev.	911.32	1129.71	895.37	967.30	734.00	799.88	769.20	912.24
a_unitprice	Obs	3,046	3,575	1,548	6,817	3,102	4,508	2,544	13,095
	Mean	27739.59	30628.34	29958.53	32069.77	31443.51	34127.35	33590.6	35790.33
	Std. Dev.	10711.62	13155.22	12013.1	12456.62	15160.73	14922.93	14474.76	14023.96
r_unitprice	Obs	3,046	3,575	1,548	6,817	3,102	4,508	2,544	13,095
	Mean	17336.14	18242.60	17083.17	18566.89	20340.44	21146.79	21402.16	22950.82
	Std. Dev.	7243.88	8349.71	7378.37	7678.09	10426.06	9394.12	9786.26	9567.32
vol	Obs	50	33	50	180	50	33	50	180
	Mean	60.92	108.33	30.96	37.87	62.04	136.61	50.88	72.75
	Std. Dev.	41.73	53.57	15.91	18.10	42.01	66.17	26.70	37.27
ratio	Obs	3,046	3,575	1,548	6,817	3,102	4,508	2,544	13,095
	Mean	0.64	0.62	0.59	0.60	0.67	0.65	0.66	0.66
	Std. Dev.	0.17	0.18	0.17	0.16	0.17	0.19	0.19	0.18
agap	Obs	3,046	3,575	1,548	6,817	3,102	4,508	2,544	13,095
	Mean	879.58	1077.80	1107.58	1102.83	861.00	994.92	903.47	976.08
	Std. Dev.	732.20	999.13	863.58	840.79	728.28	863.99	785.92	818.31
cashpayer	Obs	3,046	3,575	1,548	6,817	3,102	4,508	2,544	13,095
	Mean	0.41	0.47	0.50	0.49	0.30	0.37	0.34	0.35
	Std. Dev.	0.49	0.50	0.50	0.50	0.46	0.48	0.47	0.48

Note. We define the days on and before February 19, 2013 as the pre-announcement period, the days between February 26 and March 30 as the grace period, and the days since March 31 as the post 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 post period. The treatment group includes the resale transactions with holding periods of less than five years, which were directly affected by the capital gains tax enforcement; the resales with holding periods of over 5 years, which were exempted from the capital gains tax and thus, not affected by the enforcement, serve as the control group. The summary statistics are calculated by the authors from the data.

Table IV

Effect of the Tax Enforcement on Housing Market Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Short Run			Long Run		
VARIABLES	ln(*VOL*)	ln(*PRICE*)	ln(*PRICE*)	ln(*VOL*)	ln(*PRICE*)	ln(*PRICE*)
*TREAT*x*POST*	-0.4050**	-0.0048	-0.0144	-0.538***	0.0004	-0.0083*
	(0.1850)	(0.0169)	(0.0099)	(0.1740)	(0.0129)	(0.0050)
Observations	191	10,240	10,111	451	26,060	25,769
R-squared	0.567	0.033	0.905	0.633	0.063	0.901
Complex FE	NO	NO	YES	NO	NO	YES
Hedonic Attributes	NO	NO	YES	NO	NO	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

Note. This table reports the coefficient β_1 in Equation (2) in columns 1 and 4, where transaction volume is the outcome variable. This table reports the coefficient β_1 in Equation (1) in columns 2, 3, 5, and 6, where actual unit price is the outcome variable. The first three columns report the estimate on the short-run (50 days) effect, while the last three columns report the estimate on the long-run (180 days) effect. Robust standard errors clustered at the complex level are used in columns 2, 3, 5, and 6, while robust standard errors are used in columns 1 and 4. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

Table V

Effect of the Tax Enforcement on Tax Evasion

	A. Registered Price			
	(1)	(2)	(3)	(4)
	Short Run		Long Run	
VARIABLES	ln(*PRICE*)	ln(*PRICE*)	ln(*PRICE*)	ln(*PRICE*)
*TREAT*x*POST*	-0.0887***	-0.0786***	-0.0675***	-0.0620***
	(0.0178)	(0.0162)	(0.0104)	(0.00993)
Observations	10,111	10,111	25,769	25,769
R-squared	0.676	0.717	0.631	0.666
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
Actual Total Price	NO	YES	NO	YES

	B. Registered/Actual Ratio			
	(1)	(2)	(3)	(4)
	Short Run		Long Run	
VARIABLES	*RATIO*	*RATIO*	*RATIO*	*RATIO*
*TREAT*x*POST*	-0.0513***	-0.0541***	-0.0395***	-0.0412***
	(0.00889)	(0.00869)	(0.00548)	(0.00538)
Observations	10,111	10,111	25,769	25,769
R-squared	0.466	0.484	0.392	0.414
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
Actual Total Price	NO	YES	NO	YES

Note. This table reports the coefficient β_1 in Equation (1), where registered unit price and registered-actual price ratio are the outcome variables in panels A and B, respectively. The first two columns report the estimate on the short-run (50 days) effect, while the last two columns report the estimate on the long-run (180 days) effect. Robust standard errors clustered at the complex level are used 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.

Table VI

Effect of the Tax Enforcement on Tax Evasion: By Payment Method

A. Registered/Actual Ratio

	(1)	(2)	(3)	(4)	(5)	(6)
		Short Run			Long Run	
VARIABLES	Cash *RATIO*	Loan *RATIO*	Interaction *RATIO*	Cash *RATIO*	Loan *RATIO*	Interaction *RATIO*
*TREAT*x*POST*	-0.0041	-0.0570***	-0.0627***	-0.0054	-0.0535***	-0.0546***
	(0.0152)	(0.0118)	(0.0106)	(0.0091)	(0.0069)	(0.0064)
*TREAT*x*POST* xCashpayer			0.0548***			0.0458***
			(0.0155)			(0.0099)
Observations	3,761	6,350	10,111	9,873	15,896	25,769
R-squared	0.681	0.477	0.551	0.560	0.368	0.488

B. Registered Price

	(1)	(2)	(3)	(4)	(5)	(6)
		Short Run			Long Run	
VARIABLES	Cash ln(*PRICE*)	Loan ln(*PRICE*)	Interaction ln(*PRICE*)	Cash ln(*PRICE*)	Loan ln(*PRICE*)	Interaction ln(*PRICE*)
*TREAT*x*POST*	-0.0051	-0.0974***	-0.105***	-0.0028	-0.0940***	-0.0971***
	(0.0339)	(0.0220)	(0.0194)	(0.0195)	(0.0123)	(0.0114)
*TREAT*x*POST* x Cashpayer			0.0973***			0.0948***
			(0.0306)			(0.0195)
Observations	3,761	6,350	10,111	9,873	15,896	25,769
R-squared	0.792	0.753	0.718	0.733	0.693	0.678

C. Actual Price

	(1)	(2)	(3)	(4)	(5)	(6)
		Short Run			Long Run	
VARIABLES	Cash ln(*PRICE*)	Loan ln(*PRICE*)	Interaction ln(*PRICE*)	Cash ln(*PRICE*)	Loan ln(*PRICE*)	Interaction ln(*PRICE*)
*TREAT*x*POST*	-0.0001	-0.0202**	-0.0191**	0.0090	-0.0146***	-0.0157***
	(0.0287)	(0.00920)	(0.00839)	(0.0121)	(0.00530)	(0.00501)
*TREAT*x*POST* xCashpayer			0.0216			0.0244**
			(0.0200)			(0.0106)
Observations	3,761	6,350	10,111	9,873	15,896	25,769
R-squared	0.875	0.954	0.905	0.878	0.939	0.902
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

Note. This table reports the coefficient β_1 in Equation (1) by different payment methods, where registered-actual price ratio, registered unit price, and actual unit price are the outcome variables in panels A, B, and

C, respectively. Columns 1 and 4 use the sample of cash buyers, columns 2 and 5 use the sample of mortgage buyers, while columns 3 and 6 use the full sample and create an interaction of the DID term with the dummy of cash buyers. The first three columns report the estimate on the short-run (50 days) effect, while the last three columns report the estimate on the long-run (180 days) effect. Robust standard errors clustered at the complex level are used 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.

Table VII

Effect of the Tax Enforcement on Loan-to-Value Ratio

	A. Short Run			
	(1)	(2)	(3)	(4)
VARIABLES	*Cash payer*	*Cash payer*	*LTV*	*LTV*
TREAT x *POST*	0.0609**	0.0564**	-0.0765**	-0.0741*
	(0.0264)	(0.0248)	(0.0377)	(0.0385)
Observations	10,111	10,057	3,172	3,158
R-squared	0.396	0.491	0.438	0.446
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
Individual Characteristic	NO	YES	NO	YES
	B. Long Run			
	(1)	(2)	(3)	(4)
VARIABLES	*Cash payer*	*Cash payer*	*LTV*	*LTV*
TREAT x *POST*	0.0346**	0.0345**	-0.0419**	-0.0410**
	(0.0162)	(0.0150)	(0.0174)	(0.0176)
Observations	25,769	25,646	9,148	9,118
R-squared	0.274	0.395	0.312	0.326
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
Individual Characteristic	NO	YES	NO	YES

Note. This table reports the coefficient β_1 in Equation (1), where whether the buyer was a cash payer is the outcome variable in columns 1–2, and loan-to-value is the outcome variable in columns 3–4. Panel A reports the estimate on the short-run (50 days) effect, while panel B reports the estimate on the long-run (180 days) effect. Robust standard errors clustered at the complex level are used 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.

Table VIII

Effect of the Tax Enforcement on Contract Cancellation: By Payment Method (Short Run)

	(1)	(2)	(3)	(4)	(5)	(6)
	Probit Model			Linear Probability Model		
	Cash	Loan	Interaction	Cash	Loan	Interaction
VARIABLES	*Cancel*	*Cancel*	*Cancel*	*Cancel*	*Cancel*	*Cancel*
*TREAT*x*POST*	-0.544	0.554*	0.513**	-0.0051	0.0257	0.0263*
	(0.641)	(0.326)	(0.258)	(0.0259)	(0.0169)	(0.0150)
TREAT x *POST*			-0.890*			-0.0398*
x Cashpayer			(0.474)			(0.0237)
Observations	3,205	5,662	8,867	3,205	5,662	8,867
R-squared	0.715	0.643	0.537	0.501	0.409	0.308
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
Actual Total Price	YES	YES	YES	YES	YES	YES

Note. This table reports the coefficient β_1 in Equation (1) by different payment methods. Columns 1 and 4 use the sample of cash buyers, columns 2 and 5 use the sample of mortgage buyers, while columns 3 and 6 use the full sample and create an interaction of the DID term with the dummy of cash buyers. The first three columns report the estimate based on the probit model, while the last three columns report the estimate based on the linear probability model. Robust standard errors are used 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.

Table IX

Effect of the Tax Enforcement on Buyer Characteristics and Unit Size

A. Short Run

VARIABLES	(1) B_old	(2) B_gender	(3) B_local	(4) B_public	(5) B_highincome	(6) ln(size)
TREAT x *POST*	0.0399*	-0.0220	0.0128	-0.0011	-0.0098	0.0532***
	(0.0204)	(0.0210)	(0.0208)	(0.0254)	(0.0309)	(0.0181)
Observations	10,185	10,205	10,205	4,683	4,683	10,240
R-squared	0.006	0.002	0.006	0.002	0.003	0.011
Month FE	YES	YES	YES	YES	YES	YES
Dow FE	YES	YES	YES	YES	YES	YES
Holiday FE	YES	YES	YES	YES	YES	YES

B. Long Run

VARIABLES	(1) B_old	(2) B_gender	(3) B_local	(4) B_public	(5) B_highincome	(6) ln(size)
*TREAT*x*POST*	0.00672	-0.00937	-0.0175	-0.0016	0.0388*	-0.0090
	(0.0148)	(0.0153)	(0.0146)	(0.0180)	(0.0226)	(0.0135)
Observations	25,935	25,988	25,988	11,507	11,507	26,060
R-squared	0.004	0.002	0.004	0.002	0.011	0.005
Month FE	YES	YES	YES	YES	YES	YES
Dow FE	YES	YES	YES	YES	YES	YES
Holiday FE	YES	YES	YES	YES	YES	YES

Note. This table reports the coefficient β_1 in Equation (1), where the buyer characteristics are the outcome variables in columns 1–5. The size of the unit in log form is the outcome variable in column 6. Panel A reports the estimate on the short-run (50 days) effect, while panel B reports the estimate on the long-run (180 days) effect. Robust standard errors clustered at the complex level are used 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.

Table X

Housing Returns by Mortgage Buyers and Cash Buyers

	Loan		Cash		
Variables	Obs	Mean	Obs	Mean	MeanDiff
1-year return	1,704	0.0840	1,120	0.104	-0.020*
2-year return	2,222	0.0460	1,450	0.0730	-0.028***
3-year return	2,211	0.390	1,428	0.404	-0.014
4-year return	1,952	1.166	1,170	1.166	0.001
5-year return	2,222	0.912	1,329	0.928	-0.017
1-year wealth change	1,704	178.2	1,120	222.8	-44.629*
2-year wealth change	2,222	114.4	1,450	177.7	-63.266***
3-year wealth change	2,211	895.9	1,428	1065	-168.939***
4-year wealth change	1,952	2597	1,170	3091	-494.606***
5-year wealth change	2,222	2077	1,329	2501	-424.276***

Note. This table reports the 1-year to 5-year return rate of the houses purchased by the mortgage buyers and cash buyers after the policy. For each transaction, we compute the average growth rate of each complex (unadjusted for hedonic characteristics owing to small transaction volumes at the complex level) and impute the return and absolute wealth change for each transaction. Transactions after the policy (short run) are used for this analysis. Columns 1 and 3 report the total number of observations. Columns 2 and 4 report the average return (wealth change in absolute terms, in thousand RMB, of the two groups. Column 5 reports the net difference of column 2 and column 4, as well as the t statistics of the mean test. * indicates significance at the 0.1 level; ** indicates significance at the 0.05 level; *** indicates significance at the 0.01 level.

Table XI

Effect of the Tax Cut in 2015: Mortgage Buyer

VARIABLES	(1) *RATIO*	(2) *RATIO*
*TREAT*x*POST*	0.0218	0.00311
	(0.0147)	(0.0175)
Observations	26,427	24,731
R-squared	0.124	0.310
Complex FE	NO	YES
Hedonic Attributes	NO	YES
Month FE	YES	YES
Dow FE	YES	YES
Holiday FE	YES	YES
Actual Total Price	YES	YES

Note. This table reports the coefficient β_1 in Equation (1), where registered-actual price ratio is the outcome variable. Both the columns use the sample of mortgage buyers. Robust standard errors clustered at the complex level are used in all the regressions.

Table XII

Robustness Check: Placebo Test Based on the Grace Period

VARIABLES	(1) *RATIO*	(2) *RATIO*	(3) *RATIO*
*TREAT*x*GRACE*	0.0004	-0.0022	-0.0051
	(0.0062)	(0.0072)	(0.0070)
Observations	14,231	14,034	14,034
R-squared	0.010	0.411	0.454
Complex FE	NO	YES	YES
Hedonic Attributes	NO	YES	YES
Month FE	YES	YES	YES
Dow FE	YES	YES	YES
Holiday FE	YES	YES	YES
Actual Total Price	NO	NO	YES

Note. This table reports the coefficient β_1 in Equation (1), where registered-actual price ratio is the outcome variable. Robust standard errors clustered at the complex level are used in all the regressions.

Table XIII

Robustness Check: Parallel Trend

	(1)	(2)	(3)	(4)
	Short Run		Long Run	
VARIABLES	*RATIO*	*RATIO*	*RATIO*	*RATIO*
*TREAT*x*PRE*	-0.0037	-0.0071	-0.0009	-0.0046
	(0.0112)	(0.0111)	(0.0096)	(0.0095)
*TREAT*x*POST*	-0.0529***	-0.0570***	-0.0399***	-0.0432***
	(0.0099)	(0.0097)	(0.0065)	(0.0064)
Observations	10,111	10,111	25,769	25,769
R-squared	0.466	0.484	0.393	0.414
Complex FE	YES	YES	YES	YES
Month FE	YES	YES	YES	YES
Dow FE	YES	YES	YES	YES
Holiday FE	YES	YES	YES	YES
Hedonic Attributes	YES	YES	YES	YES
Actual Total Price	NO	YES	NO	YES

Note. This table reports the coefficient β_1 and β_2 in Equation (3), where registered-actual price ratio is the outcome variable. The first two columns report the estimate on the short-run (50 days) effect, while the last two columns report the estimate on the long-run (180 days) effect. Robust standard errors clustered at the complex level are used 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.

Table XIV

External Validity Check

A. Registered Price of Different Brokerage Firms

	(1)	(2)	(3)	(4)
	Short Run		Long Run	
VARIABLES	ln(*PRICE*)	ln(*PRICE*)	ln(*PRICE*)	ln(*PRICE*)
TREAT x *POST*	0.170***	0.092***	-0.006	0.103***
	(0.046)	(0.023)	(0.018)	(0.031)
Observations	22,466	19,467	51,085	46,233
R-squared	0.039	0.592	0.048	0.428
Complex FE	NO	YES	NO	YES
Hedonic Attributes	NO	YES	NO	YES
Month FE	YES	YES	YES	YES
Dow FE	YES	YES	YES	YES
Holiday FE	YES	YES	YES	YES

B. Registered/Actual Price in 35 Cities

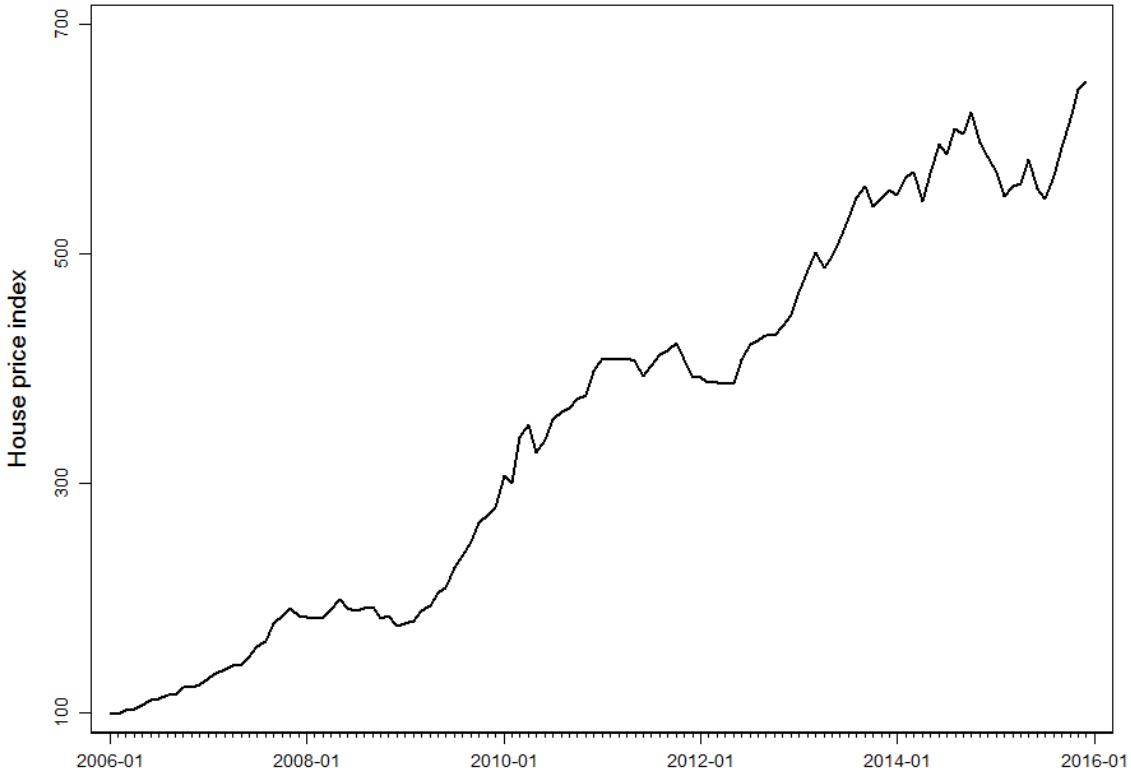
	(1)	(2)	(3)	(4)
VARIABLES	*RATIO*	*RATIO*	*RATIO*	*RATIO*
TREAT x *PRE*			-0.018	-0.017
			(0.016)	(0.014)
TREAT x *POST*	-0.042*	-0.037**	-0.054*	-0.048**
	(0.023)	(0.018)	(0.029)	(0.023)
Observations	695	839	666	804
R-squared	0.860	0.855	0.859	0.855
City FE	YES	YES	YES	YES
Year-month FE	YES	YES	YES	YES

Note. Panel A reports the coefficient β_1 in Equation (1), where registered unit price in log form is the outcome variable. The treatment group are the housing transactions in the collaborating brokerage company of main results, while the control group are the housing transactions in the other brokerage firms. The first two columns report the estimate on the short-run (50 days) effect; while the last two columns report the estimate on the long-run (180 days) effect. Robust standard errors clustered at the complex level are used in all the regressions. Panel B reports the coefficient β_1 in Equations (5) and (6), where city-month level registered price to listing price ratio is the outcome variable. The treatment group are the cities that implemented the capital gains tax increase in March 2013, while the control group are the cities without any change on capital gains taxes. The first two columns report the results in Equation (5) and the last two columns report the results in Equation (6). Robust standard errors clustered at the city level are used 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.

Figures

Figure I

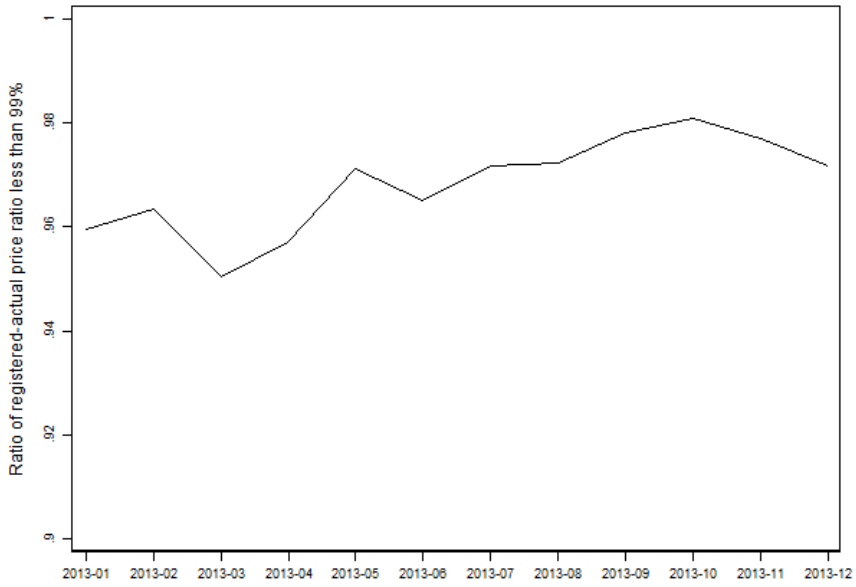
Monthly Constant-quality Housing Price Index in the Sample City, 2006–2015



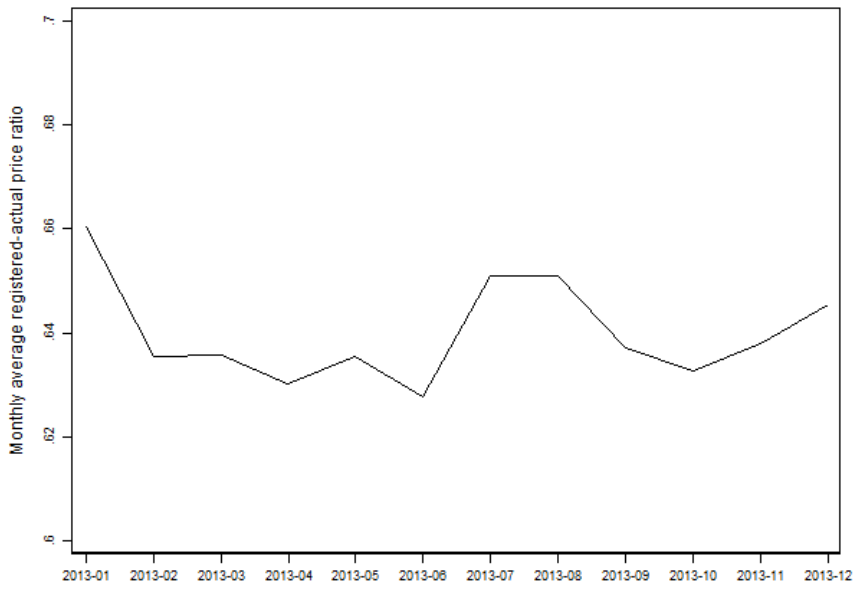
Note. The methodology on constructing the constant-quality housing price index is available in Wu, Deng, and Liu (2014).

Figure II

Monthly Statistics on Registered-actual Price Ratios in 2013



A. Percentage of Resale Transactions with Registered-actual Price Ratios Less Than 99%

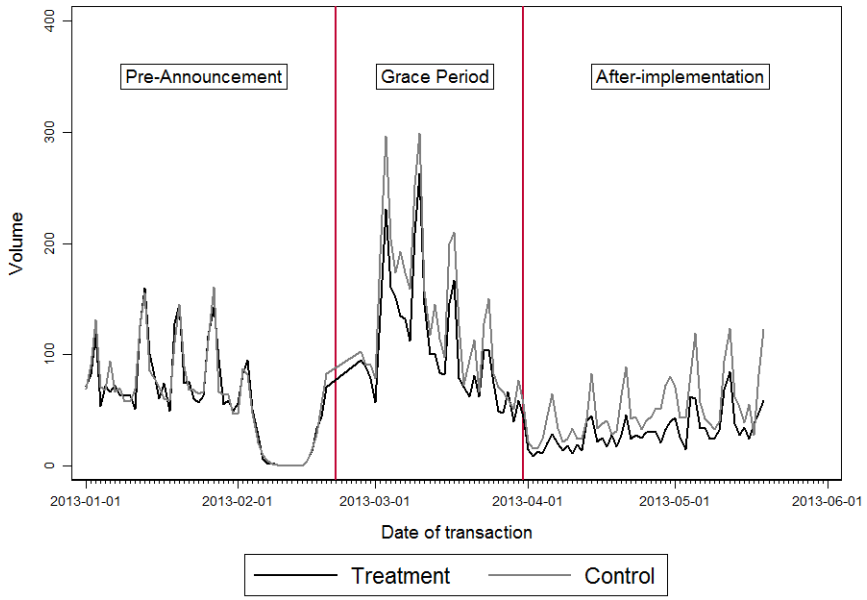


B. Average of Registered-actual Price Ratios

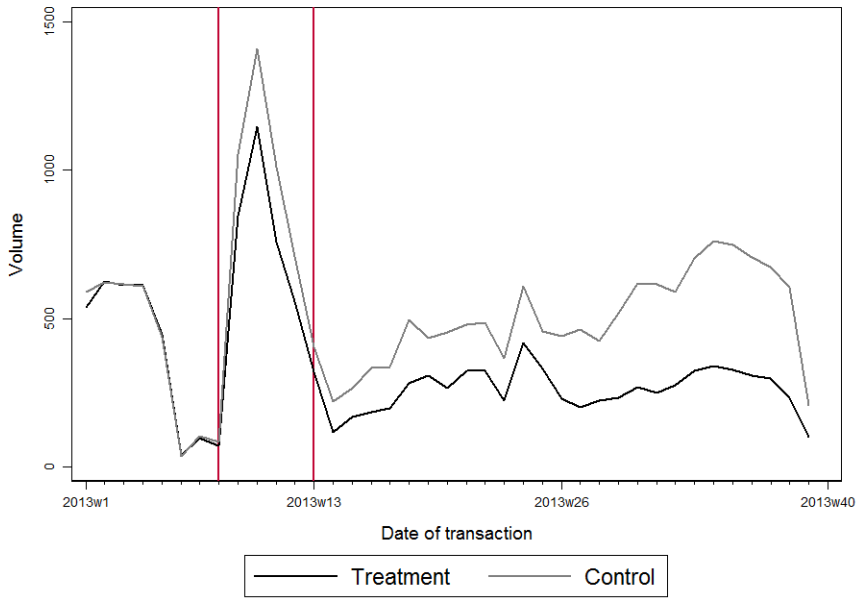
Note. Authors' calculation from the transaction data.

Figure III

The Transaction Volume around the Tax Enforcement



A. Short Run



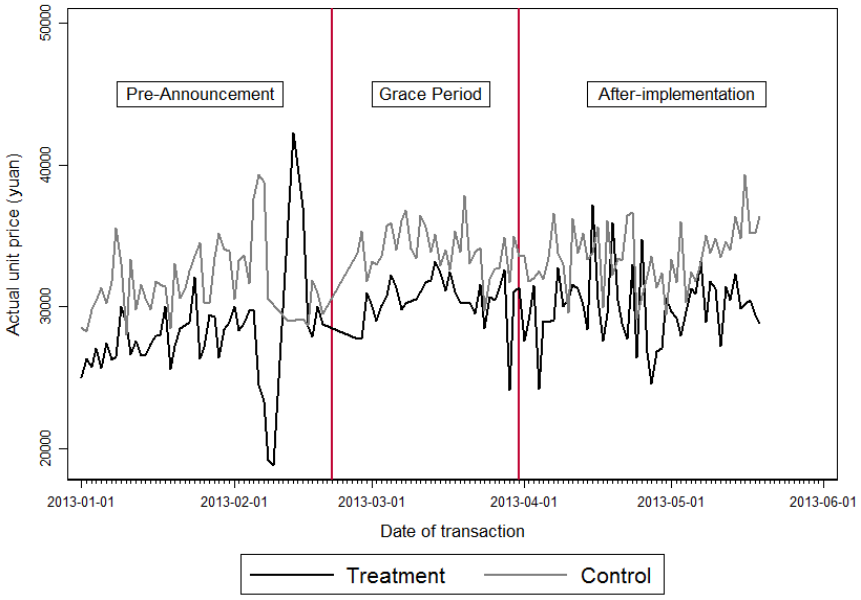
B. Long Run

Note. We define the days on and before February 19, 2013 as the control period, the days between February 26 and March 30 as the grace period, and the days since March 31 as the post period. For the short-run

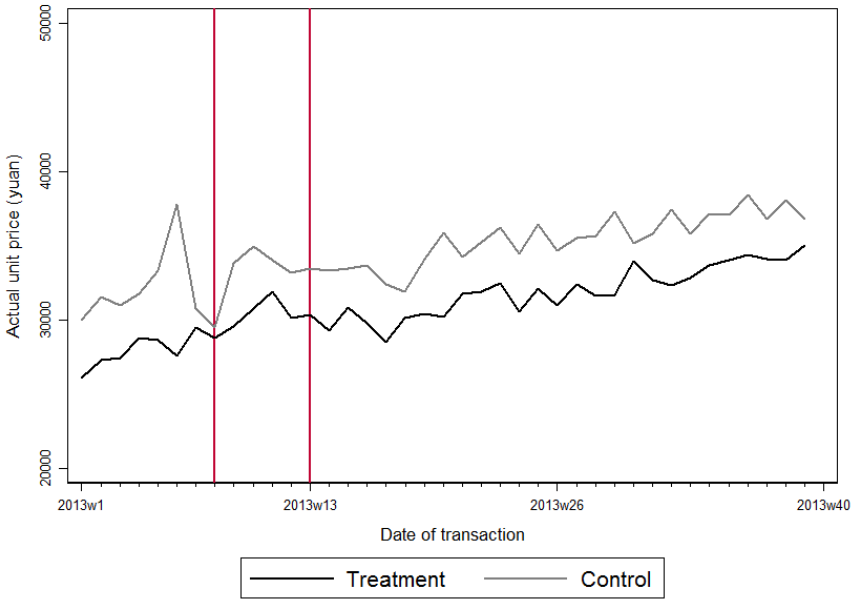
analysis, we include 50 days after March 31 as the post period; for the long-run analysis, we include 180 days after March 31 as the post period.

Figure IV

The Average Actual Unit Price around the Tax Enforcement



A. Short Run

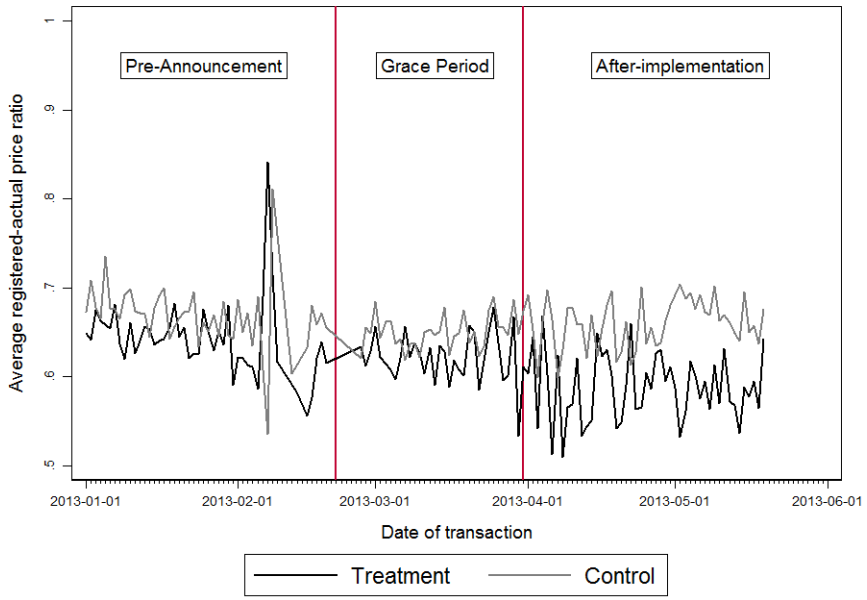


B. Long Run

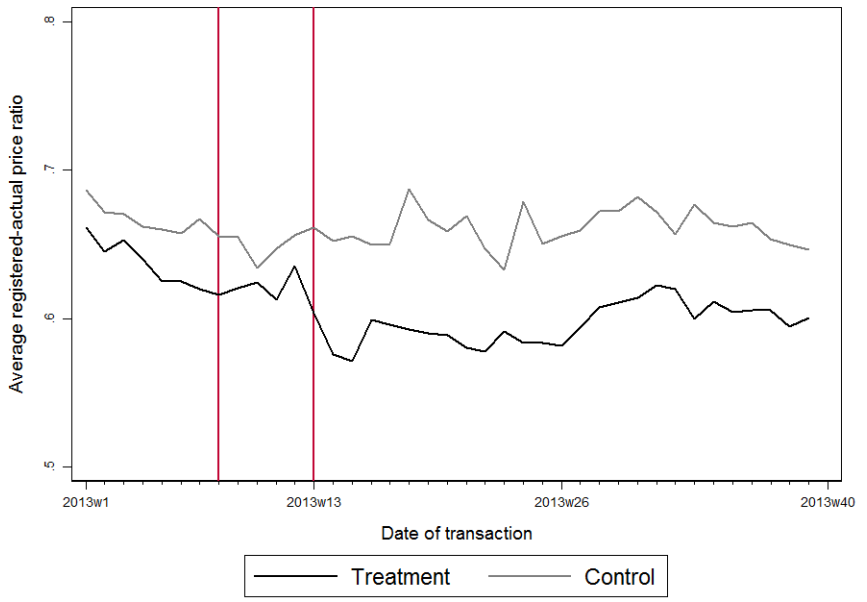
Note. We define the days on and before February 19, 2013 as the control period, the days between February 26 and March 30 as the grace period, and the days since March 31 as the post period. For the short-run analysis, we include 50 days after March 31 as the post period; for the long-run analysis, we include 180 days after March 31 as the post period.

Figure V

The Average Registered-actual Price Ratio around the Tax Enforcement



A. Short Run



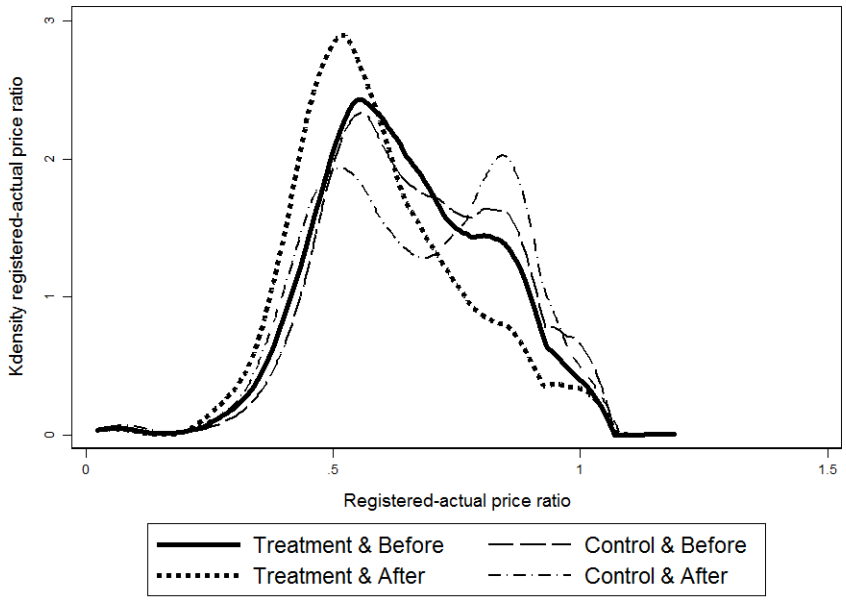
B. Long Run

Note. We define the days on and before February 19, 2013 as the control period, the days between February 26 and March 30 as the grace period, and the days since March 31 as the post period. For the short-run

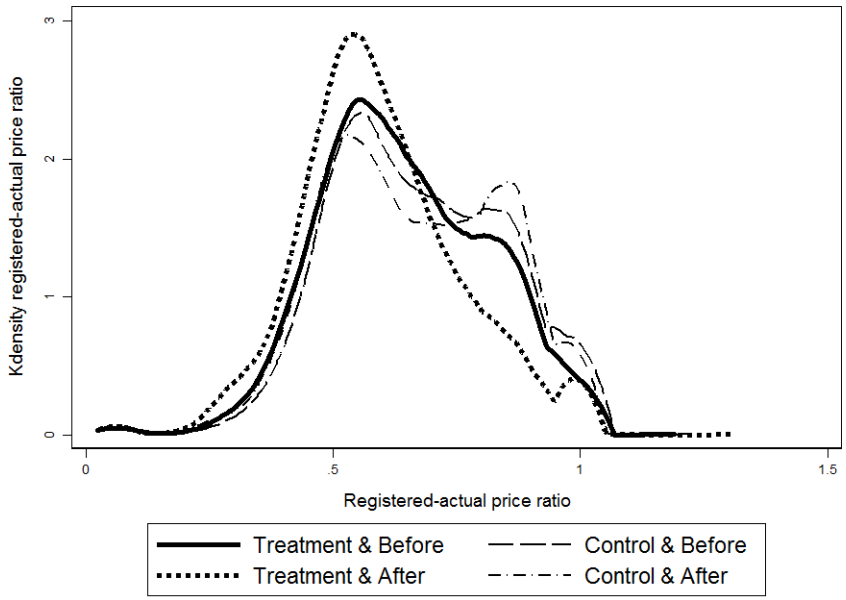
analysis, we include 50 days after March 31 as the post period; for the long-run analysis, we include 180 days after March 31 as the post period.

Figure VI

Distribution of the Registered-actual Price Ratio before and after the Tax Enforcement



A. Short Run

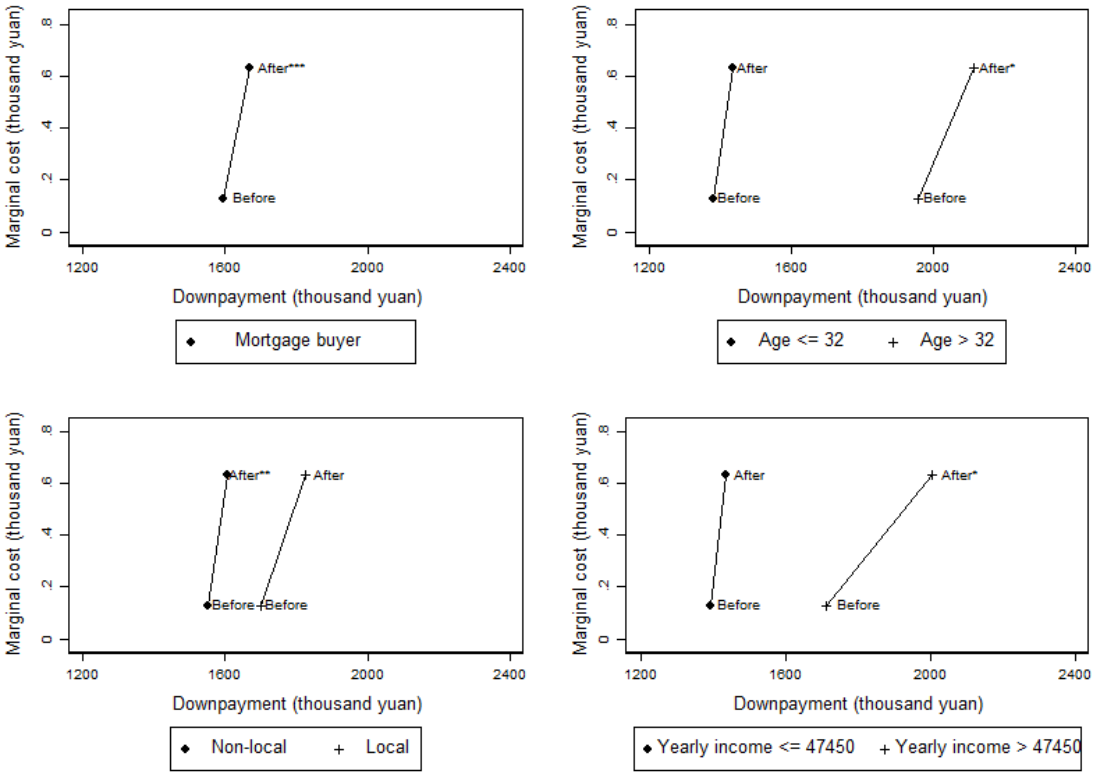


B. Long Run

Note. Epanechnikov Kernel is applied with optimal bandwidth.

Figure VII

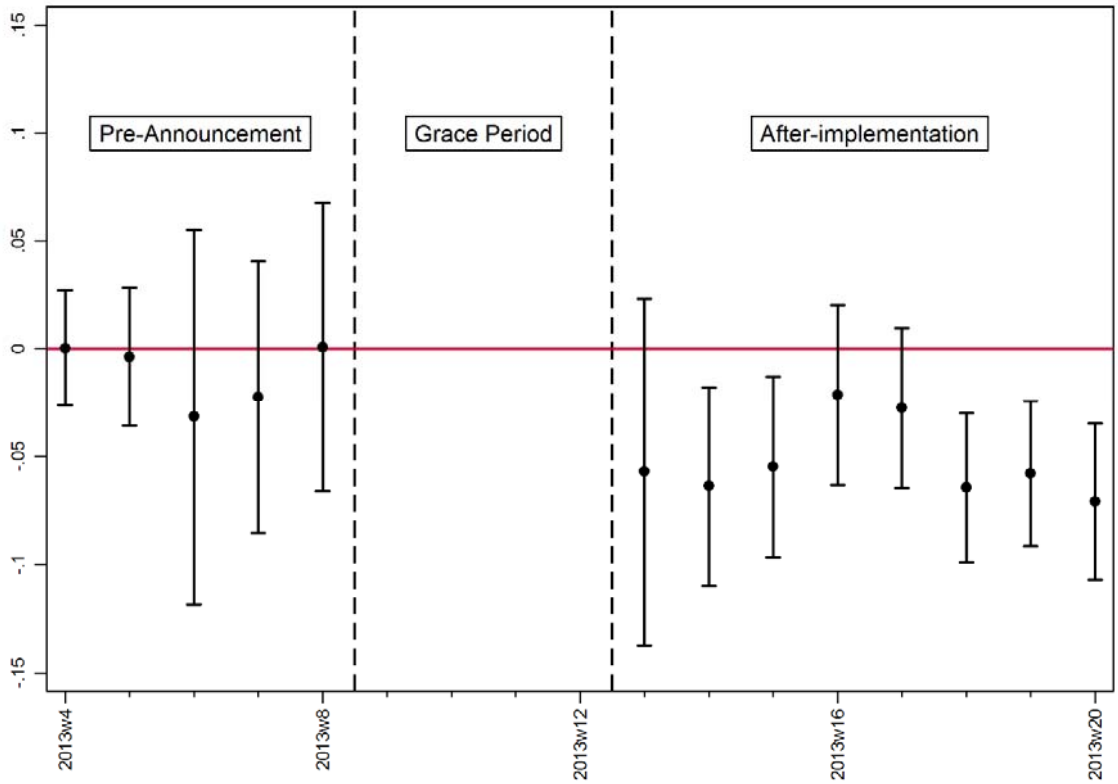
Effect of the Tax Enforcement on Mortgagors' Incremental Borrowing Cost



Note. The x-axis of the "before" point is the average size of the down payment for the buyers of treated units before the policy, and the y-axis is the corresponding marginal cost of an additional unit (in thousands) of down payment; the x-axis of the "after" point is the x value of the "before" point plus the causal effect of the policy on down payment, which are shown from the regression results (Table A.3). The stars next to the "after" point represent the significance level of the regression coefficients.

Figure VIII

Effect of the Tax Enforcement on Registered-actual Price Ratio: Event Study



Note. The figure visualizes the coefficients estimated in Equation (4), with both the coefficients and 95 percent confidence intervals reported. Weeks 1–3 are the benchmark weeks. The observations in the grace period are not included in the regression.