Understanding Misreporting: New Evidence from Responses to a Housing Transaction Tax*

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Abstract

Using proprietary data from a large real estate brokerage company that records both true and reported prices of its housing transactions, this paper provides new evidence of misreporting. Cross-sectionally, we document a series of stylized facts of misreporting featured by a novel three-segment reporting pattern. Dynamically, we show that underreporting could negatively respond to the tax rate, which implies that the tax erosion effect emphasized by the bulk of the literature could be lessened due to less underreporting among those remaining transactions. All these facts are reconcilable by a simple model that depicts the tradeoff between tax-saving and misreporting costs.

Keywords: misreporting, tax evasion, housing transaction tax, tax notch.

JEL Codes: H2, H26, R3.

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1 Introduction

An important yet challenging topic and a setting to study it. Misreporting (often driven by tax evasion) is potentially prevalent and practically important because it affects tax revenues for the government, incurs resource costs of hiding (for misreporters) and enforcement (for the government), and creates additional efficiency costs due to a higher tax rate necessary to reach a given revenue target. However, it is also highly challenging to study empirically. As pointed out by Slemrod (2019), due to tax evaders' incentive to conceal their behavior, most empirical analyses of tax evasion don't even have a reliable measure of tax evasion. Because of this major challenge, while many researchers have tried various methods to study it, empirical evidence of misreporting has still been too scarce compared to other topics studied in economics. As a result, some important questions related to misreporting are insufficiently studied. For example, while the relationship between the tax rate and misreporting is crucial for tax policy making, there has been little compelling evidence of it. Due to insufficient empirical evidence, the understanding of the misreporting behavior is far from adequate. In this paper, we exploit a unique data set that allows us to measure misreporting directly under the setting of a housing transaction tax, which enables us to provide new evidence and enrich the understanding of misreporting.

Institutional background and unique data. A business income tax with a notch applies to second-hand housing transactions in Shanghai.[1] Under certain qualifications, if the housing sales price reported (P_R) to the Housing Management Bureau (HMB) is below a certain threshold $\overline{P}$, no business income tax is due; if instead, $P_R > \overline{P}$, then a business income tax with a rate $t = 5.6\%$ (a 5% tax plus a 0.6% surtax) needs to be paid. This exemption of tax for cheaper houses is designed to reduce the tax burden and incentivize housing ownership for less wealthy families. Such a policy thus creates a jump of tax liability at the notch point $\overline{P}$.

Under a setting with a self-reported tax base, the reported sales price P_R could deviate from the true sales price P_T. This setting, combined with the tax notch policy, creates misreporting. The HMB and the tax bureau only observe P_R. But real estate brokerage companies that help deal with second-hand housing transactions maintain records of both P_R and P_T. It is important to note that since the true sales prices P_T are the basis for the commissions paid to the brokerage company, the company has an incentive to record P_T accurately.

Our major data source comes from a large real estate brokerage company, which handles about 15% (104,787 in 681,824) of all second-hand housing transactions in Shanghai during April 2015-June 2017. By including both P_T and P_R for all houses transacted via the brokerage company,

[1] The form of the tax notch differs across different cities in China. For example, while the business income tax has a notch for the total reported sales price of a house in Shanghai, it has a notch for the reported sales price per area (in square meters) rather than for the total sales price in Beijing.

our unique data allow us to directly observe the extent of misreporting for *each* transaction. As a result, we can study the misreporting phenomenon in the most straightforward way and avoid making potentially strong assumptions that are often used to measure misreporting.

Novel cross-sectional reporting patterns. We start with documenting a set of cross-sectional stylized facts on misreporting. First, the bunching of reported prices P_R at the tax notch provides preliminary evidence of misreporting in response to the notch. By contrast, the distribution of true prices P_T is entirely smooth around the notch. In addition, our placebo tests confirm that bunching does reflect responses to the notch policy and that it does not exist when such policy does not exist. Second and more importantly, we document a novel three-segment reporting pattern by examining the median P_R for each P_T bin. The first segment features honest reporting: for houses with P_T below the notch, P_R equals P_T. The second segment corresponds to the bunching evidence: for P_T in a range above the notch, P_R equal the notch value. The third segment exhibits (largely) constant underreporting regardless of P_T for houses with P_T well above the notch, which is very surprising and counterintuitive. In addition, we observe a jump between the second and the third segments. By exploring the heterogeneity of misreporting, we show that the three-segment reporting pattern not only exists for the median P_R for each bin of P_T but also exists for different percentiles of P_R.

A simple model that reconciles the cross-sectional evidence. To reconcile all these cross-sectional empirical patterns (bunching of P_R at the notch, a smooth distribution of P_T, the three-segment misreporting pattern, heterogeneity of misreporting), we introduce a simple model with parsimonious elements to depict the main tradeoff that drives the misreporting behavior under a tax notch. When deciding whether and how much to underreport, individuals trade off saved taxes with perceived misreporting costs. Following the seminal work of Allingham and Sandmo (1972) and Yitzhaki (1974), the true price P_T is taken as given when individuals choose the reported price P_R under a notched tax schedule.[2] The misreporting costs are assumed to increase convexly with the amount of tax evasion. This assumption simply specifies that the punishment for tax evasion is progressively more severe if a larger amount of tax is evaded. Our model neatly generates the observed three-segment reporting pattern and the bunching of P_R at the notch. The counterintuitive third segment is explained by the lack of an independent role of P_T in either the benefit or the cost side of misreporting. By introducing idiosyncratic misreporting costs, our model can also explain much of the observed heterogeneity of misreporting. Finally, an extended version of our model generates two key findings. First, if the true tax base is determined by costly efforts, then we should not only observe bunching of the reported tax base but also bunching of the true tax base at

[2]Note that P_T may be affected by the notch compared to the case where the notch does not exist (Kopczuk and Munroe, 2015; Slemrod et al., 2017; Best and Kleven, 2018). But as discussed in section 5.2.3, our analysis of misreporting holds even if P_T is endogenously determined.

the notch even though the true tax base is unobservable to the tax bureau, which does not receive support from our evidence. Second and more importantly, the analysis of misreporting (either theoretically or empirically) does not rely on whether the true tax base is exogenous. Even if the true tax base is affected by the tax rate, the misreporting decision is made in the same way as if the true tax base is taken as given.

Counterintuitive relationship between the tax rate and misreporting. While our model is used to reconcile the cross-sectional patterns, it generates a surprising prediction on the dynamic relationship between the tax rate and misreporting. In particular, our model predicts that in response to a decrease in the tax rate, the entire three-segment reporting pattern would shift rightwards, implying that a lower tax rate could generate more evasion. By exploiting a tax reform that reduced the tax rate above the tax notch, we show that the shifting of the whole reporting pattern is consistent with the prediction of our model. Quantitatively, using a difference-in-differences (DD) approach, we obtain elasticities of underreporting with respect to the tax rate ranging from -0.9 to -0.2, depending on different samples and regression specifications. Our findings imply that while a marginal increase in the tax rate would create efficiency losses by reducing transactions of second-hand houses, it could also reduce underreporting among those who still sell their houses, which stands in contrast with the bulk of the literature that emphasizes how an increase in the tax rate would erode the tax base.

Contributions to the literature. Our paper mainly contributes to the literature on misreporting and tax evasion.[3] First, we document new cross-sectional stylized facts of misreporting featured by a notable three-segment reporting pattern, which arguably provides a textbook example to illustrate the misreporting response to a notch policy. To our best knowledge, this paper is the first to document the empirical relationship between the reported tax base and the true tax base under a tax notch, despite the prevalence of policy notches in practice (Blinder and Rosen, 1985; Slemrod, 2013). Many papers have focused on the bunching evidence when there is a policy notch (e.g., Kleven and Waseem, 2013; Best and Kleven, 2018; Chen et al., 2021). While the second segment of our reporting pattern corresponds to the well-expected bunching evidence, the pattern depicted in the third segment is entirely out of expectation and counterintuitive. Analyzing the whole pattern of misreporting (i.e. studying the reporting pattern at all levels of the true tax base rather than merely estimating responses to a tax policy at the average level or with limited heterogeneity) distinguishes our work from the other papers that study misreporting and tax evasion.

Second, we provide new evidence of the relationship between the tax rate and misreporting, on

[3] See Andreoni et al. (1998), Alm (1999), Slemrod and Yitzhaki (2002), Sandmo (2005), Slemrod (2007), and Slemrod (2019) for a comprehensive review of the literature.

which there has been little compelling empirical evidence in the literature. While the bulk of the tax literature estimates how an increase in the tax rate would erode the tax base, our finding implies that the tax base erosion could be lessened due to less underreporting among those remaining transactions. The literature studies how an increase in the tax rate could erode the tax base from two aspects. First, most tax papers focus on the distortionary effects of taxes on various "real" margins (i.e. not due to misreporting, such as employment, number of transactions, true transaction prices). As long as it is not a Pigouvian tax imposed on a case where there is a negative externality, these distortionary effects would result in erosions of the tax base and cause efficiency losses. Second, a much smaller literature that studies evasion responses to taxes mostly finds that an increase in the tax rate could further erode the tax base by either increasing underreporting (e.g., Fisman and Wei (2004), import tariffs in China) or increasing tax evasion (e.g., Clotfelter (1983), personal income tax in the U.S.; Marion and Muehlegger (2008), diesel fuel tax in the U.S.; Kleven et al. (2011), individual income tax in Denmark). An exception is Feinstein (1991), who finds some (yet mixed) evidence that an increase in the marginal tax rate might reduce tax evasion for personal income tax in the U.S., which is in line with our evidence, though he has not proposed a model to reconcile this counterintuitive result.

While these new facts are obtained from the specific setting under consideration in this paper, our model proposed to reconcile these facts helps provide external validity to these results. Note that our model is nothing special but a cost minimization model with convex misreporting costs. And the key elements of the model are consistent with the standard tax evasion model (e.g., Allingham and Sandmo, 1972; Yitzhaki, 1974) and other relevant literature (e.g., Scotchmer, 1987; Dharmapala et al., 2011) except that our model employs these elements in a different way from the standard tax evasion model.[4] Thus, as the natural predictions of our simple model, the facts documented in this paper are possible to be observed in other similar settings when appropriate data are obtained. The reasons for these facts being undocumented before are mostly due to the difficulty in measuring misreporting and the unavailability of appropriate data.

Furthermore, we use ideal data to study tax evasion, which makes our empirical findings more compelling than studies that resort to indirect measures of tax evasion. Most papers indirectly study evasion by proposing innovative and diverse approaches to infer the extent of evasion.[5] Papers providing direct evidence of evasion mostly use stratified random audits data (e.g., Clotfelter, 1983;

[4]We show that while the three-segment reporting pattern documented in this paper is not a natural result of the standard Allingham-Sandmo (AS) tax evasion model (Online Appendix E), it can be generated under a specific version of the AS model with risk-neutral taxpayers and a discontinuous audit probability or penalty with respect to misreporting if some parameter condition is satisfied (Online Appendix F). The key difference between our model and the AS model lies in the way how misreporting costs are introduced in the model and how they interact with the tax liability. In addition, the empirical reporting pattern supports our assumptions of the misreporting cost function. Our model is advantageous in both its simplicity and its explanatory power.

[5]For example, see Pissarides and Weber (1989), Fisman and Wei (2004), Feldman and Slemrod (2007), Marion and Muehlegger (2008), Gorodnichenko et al. (2009), Artavanis et al. (2016), Cullen et al. (2021).

Kleven et al., 2011), while others measure evasion by comparing self-reported value and third-party reports (e.g., Carrillo et al., 2017) or using leaked data from offshore financial institutions (e.g., Alstadsæter et al., 2019). Different from them, we obtain proprietary data from a private real estate brokerage company, which maintains records of true and reported prices of its housing transactions. This helps us overcome the major challenge in studying misreporting (i.e. the measurement of misreporting) and avoid making potentially strong assumptions that are often used to measure misreporting.

We also contribute to the literature on behavioral responses to housing-related taxes. Previous papers have mainly emphasized real responses rather than misreporting, which is appropriate given their institutional backgrounds.[6] In the U.S., brokerage companies are required to report transaction prices to the government, and to make this information publicly available.[7] Since people can easily search for their neighbors' housing prices online, it is much easier to ascertain whether people are misreporting. In addition, because a precisely estimated housing value is required to calculate the property tax due, there is a larger incentive for the local government to ensure a correctly reported housing price. In China, however, brokerage companies are not required to publicize housing transaction prices online and property taxes have not been implemented broadly. Because of these differences, it is reasonable to expect little misreporting in the U.S. second-hand housing transactions and non-trivial misreporting in China. Our paper contributes to this literature by providing compelling evidence of misreporting for housing-related taxes.[8]

Several concurrent papers (Agarwal et al., 2020; Agarwal et al., 2019; Montalvo et al., 2020) also obtain data from private real estate brokerage companies that record both true prices and reported prices of housing transactions and provide evidence of misreporting for housing-related taxes. However, the focus of our paper is different from theirs, due to the different institutional backgrounds and specific policies under consideration.[9] Importantly, the tax notch setting enables us to document sharp cross-sectional evidence of misreporting, which combined with our other evidence, presents a series of new patterns of misreporting that are not documented in any of these papers. The negative relationship between the tax rate and underreporting also has not been documented in these papers. In addition, none of them has obtained an elasticity of underreporting

[6] For example, see Kopczuk and Munroe (2015), Best and Kleven (2018). Relatedly, Besley et al. (2014) study the incidence of a housing transaction tax in the U.K.

[7] We are indebted to Ingrid Ellen for pointing this out.

[8] Related to our work, Tam (2016) studies tax avoidance responses to time notches in housing transaction taxes in Hong Kong and Singapore. Slemrod et al. (2017) study behavioral responses to a price and time notch of residential real estate transfer taxes in Washington D.C. and provide empirical evidence that there is a manipulation of the sales price to evade taxes. Del Carpio (2014), Castro and Scartascini (2015), Brockmeyer et al. (2021) study tax compliance of property tax under different settings.

[9] In particular, Agarwal et al. (2020) study how a capital gains tax affects tax evasion in the real estate market in China. Agarwal et al. (2019) study the role of agents in tax fraud. Montalvo et al. (2020) study the relation between over-appraisal and tax evasion among housing transactions in Spain.

with respect to the tax rate, while we have. Thus, we provide complementary new evidence of misreporting compared to these papers.

Finally, this paper joins a fast-growing literature that studies misreporting and tax evasion in developing countries (e.g., Best et al., 2015; Pomeranz, 2015; Carrillo et al., 2017; Naritomi, 2019; Almunia et al., 2021) by providing evidence from China, the largest developing country in the world.[10]

The remaining sections of this paper are organized as follows. Section 2 introduces the institutional background. Section 3 introduces the data. Section 4 shows cross-sectional evidence of misreporting. Section 5 proposes a simple model to reconcile the cross-sectional reporting patterns and generates a prediction on the relationship between the tax rate and misreporting. Section 6 studies the empirical relationship between the tax rate and misreporting. Section 7 concludes.

2 Institutional Background

2.1 Housing Transactions in China

For most families in China, houses are their major asset. House prices increased 12.7% on average annually from 2000 to 2017. Houses are normally expensive, especially in major cities. For example, it takes about 30 years' annual income for a family to buy a 90 m^2 flat in Shanghai, which is the most populous city in China.[11]

There are two kinds of housing sales: new houses and second-hand houses. In China, new houses are directly sold to customers by real estate development companies, while second-hand houses are traded between individuals.[12] However, second-hand housing transactions rarely happen directly between individuals for two reasons:[13] First, information barriers and search costs prevent potential transactions from happening.[14] This calls for a platform to gather and publicize information and facilitate transactions. Second, the procedures related to second-hand housing transactions are complicated; individuals may find it too costly to understand and resolve all issues on housing evaluation, taxes, loans from banks, and housing management. Therefore, most

[10]Relatedly, Chen et al. (2021) find evidence that Chinese firms relabeled expenses as R&D in response to a corporate tax notch.

[11]See Fang et al. (2016), Glaeser et al. (2017), and Chen and Wen (2017) for more details about China's housing market.

[12]In major Chinese cities, housing purchases are mostly second-hand houses. The supply of new houses is much smaller than second-hand houses (see Figure 3). This is especially true for housing purchases in the downtown area, where new houses are rare.

[13]In Shanghai, during our study period (April 2015-June 2017), only 14.76% of all second-hand housing transactions happened between individuals without using a realtor.

[14]See Wheaton (1990), Genesove and Han (2012), and Diaz and Jerez (2013) for a literature on search and matching in the housing market.

individuals rely on real estate brokerage companies to handle second-hand housing transactions.[15]

One special feature of second-hand housing transactions in China is that the listing agent (working for the seller) and the selling agent (working for the buyer) involved in a transaction typically belong to the same brokerage company. This makes the cost of hiding misreporting information in China much lower than in a market where the listing agent and the selling agent for a transaction often belong to different brokerage companies, as in the U.S.

2.2 Major Housing Transaction Tax

Business income tax (BIT) is the major tax paid in second-hand housing transactions. In China, once a buyer and a seller sign a contract on a housing sale with a true sales price P_T (i.e. the amount effectively received by the sellers), they need to report a sales price P_R to the Housing Management Bureau (HMB). Often, these two prices differ. Because only P_R is observable to the HMB, it is used as the base for taxes. A business income tax is levied on second-hand housing transactions if the reported price P_R is above a certain threshold $\overline{P}$.[16] The exemption of transaction tax for cheaper houses is designed to reduce tax burdens and incentivize housing ownership for less wealthy families, which is also adopted by many other countries. If $P_R \leq \overline{P}$, no business income tax is due; if instead, $P_R > \overline{P}$, then a business income tax with a rate $t = 5.6\%$ (a 5% tax plus a 0.6% surtax) needs to be paid. The policy, therefore, creates a jump of tax liability at the notch point. The jump of tax liability equals $t(\overline{P} - P_0)$ for most houses, where P_0 denotes the original purchase price by the house's seller.[17] By contrast, new houses do not face such a tax notch.[18] By law, the business income tax should be remitted by sellers and thus it is the sellers who would bear potential punishment for evasion.[19]

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[15] This is also true in the U.S. (see Barwick et al., 2017).

[16] In addition to this constraint, the house area should not exceed 140 square meters to be exempted from the business income tax. The fraction of houses exceeding 140 square meters is quite small (only 8% in our real estate brokerage company data). Thus, to focus on the incentive created by the price notch, we restrict our analysis to houses below 140 square meters.

[17] For houses held for over 2 years, the jump of tax liability equals $t(\overline{P} - P_0)$. For houses held for less than 2 years, the jump of tax liability equals $t\overline{P}$. Because of this large tax difference, most people would not sell their houses before holding at least two years. While our data do not have the holding period information, staff of the tax bureau informed us that most houses are sold after being held for at least 2 years. In particular, we are informed that among all second-hand housing transactions in the Baoshan district (this is where we can get relevant information from tax officials) of Shanghai, the ratio of transacted houses held for less than 2 years is 4.94% in 2017, 3.02% in 2018, 2.14% in 2019, and 1.57% in 2020.

[18] It is not that new housing transactions do not need to pay any business income taxes. They do. But the business income taxes are levied as a fraction of the aggregate revenue for all new housing sales of a real estate development company. Therefore, there is no tax notch for individual new housing sales.

[19] But in practice, all taxes and fees in the process of housing sales are paid by buyers to the sellers and then remitted by sellers to the tax authority. It is the buyers who paid all transaction taxes and fees probably because the second-hand housing market in Shanghai is mainly a seller's market, given the limited housing supply and high demand. Studying the tax incidence needs to know how P_T has been affected by the tax, which is difficult to answer under our setting.

Table 1 shows tax notches in Shanghai from 2008 to now. The notch level is determined as 1.44 times the average housing price in a broad region, varies across regions, and is subject to change occasionally over time. Since houses are more expensive in the inner city than in the outer region, the notch thresholds are also higher in the inner city. In particular, there are three locations for notch purposes: within the inner ring, between outer and inner rings, and outside the outer ring.[20] Figure 1 shows the heatmap of second-hand transactions in Shanghai. For the period after November 20, 2014, the notch for housing transactions within the inner ring, between the outer and inner ring, and outside the outer ring is 450w RMB, 310w RMB, and 230w RMB, respectively.[21] There are also several changes in the notches over time. Two changes occurred on March 1, 2012 and November 20, 2014, when the notches for all locations increased discontinuously. These policy changes provide us opportunities to examine the behavioral responses to tax notch changes.

VAT-for-BIT reform. Since May 1, 2016, the business income tax (BIT) has been replaced by the value-added tax (VAT). This is a major tax reform in China, designed to improve the taxation system. Prior to the reform, economic activities were subject to either BIT or VAT depending on the industry. But the BIT is imposed on the value of sales, which may result in a double taxation problem (i.e. cascade tax). The VAT avoids this problem, as it is applied to the added value at each link of the production chain. Thus, the tax reform was designed to abolish the BIT entirely and have all industries subject to the VAT. Since the tax reform, industries like construction, real estate, financial, and consumer services sectors have switched from the BIT to the VAT regime.

But for second-hand housing transactions, this reform only changed the tax rate slightly, without changing the tax base or any other aspect. Both before and after the reform, the tax base is the same.[22] The tax notch did not change; the standard for houses to enjoy the lower rate also did not change. The enforcement of the tax also did not experience an explicit change: while the replacement of the BIT by the VAT may result in an enforcement change in most provinces because the BIT is managed by the local tax bureau and the VAT is managed by the national tax bureau (which arguably imposes stricter enforcement), Shanghai is unique in that it never has separate local tax bureau and national tax bureau.[23] The only aspect which has changed is the effective tax

Thus, we only make a tentative discussion of the incidence issue in section K.

[20] Shanghai is geographically divided into several regions by overpasses, which are described as rings because they are like circles. Starting from the city center, two rings (inner ring and outer ring) divide Shanghai into three broad regions.

[21] Here w denotes *wan* in Chinese, which is a very commonly used unit. 1w=10,000. The exchange rate is 1 USD=6.14 RMB (2014), 6.23 RMB (2015), 6.64 RMB (2016), 6.75 RMB (2017).

[22] Both before and after the reform, for houses held for less than 2 years, the tax base is the sales revenue, like the typical BIT. For houses held for at least 2 years, the tax base is the value increase, like the typical VAT. Since most houses are transacted after being held for at least 2 years, the BIT for most second-hand housing transactions had the VAT property even before the VAT-for-BIT tax reform.

[23] The 1994 tax sharing system reform resulted in the separation of the local tax bureau and the national tax bureau in each province, city, and county. The national tax bureau managed taxes that the central government has the right to

rate: it decreases from 5.6% to $\frac{5.6\%}{1.05} = 5.33\%$. We utilize this reform to study the impact of the tax rate on misreporting in section 6.

2.3 Other Taxes and Fees

There are several other taxes and fees involved in second-hand housing transactions, which are discussed in detail in Online Appendix A. Importantly, they have nothing to do with the BIT notch. For example, the deed tax is proportional to P_R and the rate depends on the number of houses a buyer has and the area of the house; the personal income tax is 1% of P_R and can be exempted if the seller has owned the house for at least 5 years and it is the only house of the seller; the commission fee paid to the brokerage company is 1-3% of P_T. Thus, they do not play a major role in our analysis of the impact of the BIT notch on misreporting, though they may account for some side facts that cannot be solely explained by the BIT notch, which we will discuss later.

3 Data

Data from a real estate brokerage company. Our major data come from a large private real estate brokerage company in Shanghai. We obtain data on all second-hand residential housing transactions dealt with by this company from April 2015 to June 2017. The data include 104,787 cases, covering about 15% of all second-hand housing transactions in Shanghai during this period. The unique advantage of this dataset is that it includes both true sales prices (i.e. the amount effectively paid by the buyers) and the prices reported to the Housing Management Bureau (HMB) and tax authority. Since the true sales prices (unknown to the HMB or tax authority) are the basis for the commissions paid to the brokerage company, the company has an incentive to record them accurately. The reported price, on the other hand, is used as the basis for housing transaction taxes and could differ from the true sales prices. This dataset, therefore, provides us with a unique opportunity to study potential misreporting behavior in housing transactions. Other information provided by this dataset includes the transaction date, house area, and location (within the inner ring, between the outer and inner rings, or outside the outer ring). As shown in Table 2, reported prices are consistently lower than true prices in the median, the 90th, and the 10th percentiles. The mean true price and mean reported price are 365.93w RMB and 313.70w RMB, respectively,

share (e.g., consumption tax, VAT) and the local tax bureau managed taxes that are solely shared by itself (e.g., BIT, personal income tax). While the national tax bureau of a lower administrative level (e.g., city) is mainly under the leadership of the national tax bureau of a higher administrative level (e.g., province), the local tax bureau is mainly under the leadership of the local government at the same administrative level. Thus, in most provinces, the replacement of the BIT by the VAT resulted in a change in the tax bureau that is in charge of the tax, which would result in a change of enforcement. Shanghai and Tibet are the only two regions that have not experienced such a change because there has been only one tax bureau that managed all kinds of taxes in these two regions.

suggesting an average evasion ratio of 14.3% in this dataset.

Housing Management Bureau data. We also obtain data from the HMB on all housing transactions in Shanghai. The data include not only second-hand housing transactions (February 2011-June 2017) but also new housing transactions (January 2012-August 2015). Houses included in this dataset are also for residential use. Since the HMB only has information on reported prices but not true prices, the HMB data are only used for placebo tests. Other information provided by this dataset includes the transaction date, house area, and location. Figure A2 shows the number of housing transactions and prices in the data.

Representativeness of the brokerage company data. Since the private real estate brokerage company only covers about 15% of all second-hand housing transactions in Shanghai during the sample period, it is not necessarily representative of the whole housing market in Shanghai. To see the difference, we examine summary statistics of key variables for second-hand housing transactions handled by this company versus all transactions recorded by the HMB during April 2015-June 2017. Table 2 shows that while the distributions of housing areas are comparable in the two datasets, reported prices are generally larger for houses handled by the brokerage company. Houses handled by the brokerage company are also more likely to be located within the inner ring or between the outer and inner ring, where houses are more expensive. Within each region, the housing prices are higher for houses handled by the brokerage company. These are not surprising as transactions of more expensive houses are more likely to be dealt with by a brokerage company.

4 Cross-Sectional Evidence of Misreporting

4.1 Distributions of True Prices and Reported Prices

We begin by first examining the distributions of true prices P_T and reported prices P_R, respectively. Since the business income tax is based only on P_R rather than P_T, we expect a smooth distribution of P_T and bunching of P_R at the notch. Since the notch point varies by region, we examine different regions separately. We cluster observations in bins with a width=10w RMB.

Using the brokerage company data, Figure 2 shows the distributions of P_T and P_R for the same transactions in each region separately.[24] We see a smooth distribution of P_T and clear bunching of P_R at the notch for all three regions. The bunching of P_R at the notch provides preliminary evidence of misreporting in response to the tax notch.

[24]Figure A3 shows the distribution of P_T and the distribution of P_R for the same region in separate figures to make the distribution of each figure clearer.

In addition, we perform placebo tests using the HMB data (the brokerage company data cannot be used to do these tests due to a much shorter data period) to confirm that bunching does reflect responses to the notch policy and that it does not exist when such policy does not exist (see Online Appendix B for more details). First, we show that, consistent with the policy that only second-hand housing transactions face tax notches while new housing sales do not, the bunching of reported housing sales prices at the notch only exists for second-hand housing transactions but not for new housing sales (Figure 3). Second, by exploiting the abolition of old notches and the introduction of new notches, we show that bunching only exists when a notch is implemented and disappears after an old notch is abolished (Figure 4).

4.2 Novel Stylized Fact: The Three-Segment Reporting Pattern

Figure 5 shows the empirical pattern of reported housing transaction prices against true transaction prices. To examine the representative behavioral responses, we focus on the median reported price for each true price bin.[25] The relationship between the reported price P_R and the true price P_T exhibits a novel three-segment reporting pattern. In particular, in the first segment, for $P_T \leq \overline{P}$, we observe $P_R = P_T$, i.e. honest reporting. In the second segment, for P_T in a certain range above $\overline{P}$, we observe $P_R = \overline{P}$, which corresponds to the bunching of P_R at the notch. In the third segment, we observe a largely constant underreporting for P_T well above $\overline{P}$, which seems quite surprising because it is puzzling to see houses with huge price gaps having approximately the same extent of underreporting and tax evasion. Furthermore, we observe a small jump between the second and the third segments (less salient in panel a and more salient in panels b and c).

4.3 Heterogeneity of Misreporting

While Figure 5 shows the reporting pattern of the representative (i.e. median) individuals, heterogeneous reporting behaviors could well exist. In Online Appendix G, we examine the heterogeneity of misreporting on two margins. The main empirical findings are summarized below.

On the "intensive margin", we examine heterogeneous misreporting patterns within each bin of P_T. It is worth noting that we do not restrict our sample only to observations that misreport sales prices, thus different from the usual understanding of the intensive margin. Instead, we include all observations, regardless of whether the sales prices are misreported or not. The aim is to examine the rich heterogeneity of misreporting at different percentiles and examine whether the predicted three-segment reporting pattern holds even at finer-divided percentiles. Figure A6 and Figure A7

[25]Using the mean reported price (Figure A4) shows a smoother pattern because of the heterogeneity of misreporting, which we show below.

show that the three-segment reporting pattern not only exists for the median P_R for each bin of P_T but also exists for different percentiles of P_R.

On the extensive margin, we explore the heterogeneity of whether people underreport or not. Figure A8 shows that for $P_T \leq \overline{P}$, about 20-40% of transactions have underreported sales prices. For $P_T > \overline{P}$, the fraction of transactions that underreport sales prices is close to 1 for P_T close enough to $\overline{P}$, then decreases as P_T rises, and finally becomes largely constant as $P_T > P^*$.

5 A Simple Model of Misreporting

In this section, we introduce a simple model with parsimonious elements to depict the main tradeoff (rather than using a structural model to precisely match empirical moments) that drives the misreporting behavior under the tax notch. While the model is used to reconcile the cross-sectional patterns (bunching of P_R at the notch, a smooth distribution of P_T, the three-segment misreporting pattern, heterogeneity of misreporting) documented in section 4, it generates a surprising prediction on the dynamic relationship between the tax rate and misreporting, which guides our further empirical investigation in section 6.

5.1 Model Setting

Like the seminal work of Allingham and Sandmo (1972) and Yitzhaki (1974), in our model, the true price P_T is taken as given when individuals choose the reported price P_R. But as discussed later in section 5.2.3, our analysis of misreporting holds even if P_T is endogenously determined. There is a tax notch at $\overline{P}$ and a tax is due only if the reported price P_R is above the tax notch. The tax liability equals $t(P_R - P_0)$, where t denotes the tax rate and P_0 denotes the original purchase price by the house's seller.

We make two simplifying assumptions. First, misreporting $P_U \equiv P_T - P_R$ is associated with a cost $C(.)$, which is a function of the amount of tax evaded.[26] Assume $C > 0$, $C' > 0$, $C'' > 0$, and C is continuously differentiable, i.e. misreporting costs increase convexly with the scale of tax evasion. This assumption is broadly consistent with China's criminal law, which specifies that punishment for tax evasion is progressively more severe if a larger amount of tax is evaded.[27]

[26] In Online Appendix C, we have shown a detailed analysis of the perceived misreporting cost based on our empirical findings. Note that while Allingham and Sandmo (1972) assume the cost function to be $C(P_U)$, the famous response by Yitzhaki (1974) assumes the function to be $C(tP_U)$. Yitzhaki (1974) shows that the latter is more in line with tax laws in many countries. We adopt the Yitzhaki (1974) assumption and explicitly test it against the Allingham and Sandmo (1972) assumption in Online Appendix C.1.

[27] *The Criminal Law of the People's Republic of China* (as amended on November 4, 2017) clause 201 specifies the punishment for tax evasion. In particular, if the tax evasion amount is between 10,000 RMB and 100,000 RMB or is 10-30% of the tax due, the evader can be imprisoned for less than 3 years and fined 1-5 times the evaded tax amount. If the tax evasion amount is over 100,000 RMB or over 30% of the tax due, the evader can be imprisoned for 3-7 years

In addition, this assumption also nests a more specific misreporting cost function $C(.) = p\theta$, where p denotes the probability of auditing and $\theta > 0$ denotes fines on tax evasion. Both p and θ are functions of tax evasion tP_U with $p' > 0$ and $\theta' > 0$. This presumption of a higher auditing rate when more revenue is at stake echoes the past literature that emphasizes the role of enforcement costs of tax audits. For example, Scotchmer (1987), Dharmapala et al. (2011) argue that due to the limited enforcement resources owned by the government, it is optimal for the government to limit audits to those with presumed tax evasion above a certain threshold. Detection could occur with delay (i.e. even if the audit rarely happens now, it may well happen in the future, as has happened to the famous actress Fan Bingbing in 2018 and China's "live-streaming queen" Viya in 2022 who were fined hundreds of millions RMB for tax evasion after being audited), and could occur in multiple ways, perhaps from bank records or information from the buyer of the property. While audits in practice seem to be rare, it is the threat of detection that really mattered. Although our analysis does not rely on this specific case, it would help provide economic intuitions that are difficult to be conveyed using the general form of the misreporting cost function $C(.)$ in section 5.3.

Second, all other taxes and fees are ignored. Although other taxes and fees help explain some side facts (e.g., misreporting below the tax notch), they do not drive the major misreporting pattern.

A person chooses P_R to minimize the total cost (tax liability plus misreporting cost):[28]

$$min_{P_R}\, t\cdot(P_R - P_0)\cdot\mathbf{1}[P_R > \overline{P}] + C(t(P_T - P_0)\cdot\mathbf{1}[P_T > \overline{P}] - t(P_R - P_0)\cdot\mathbf{1}[P_R > \overline{P}]), \tag{1}$$

where $\mathbf{1}[P_R > \overline{P}]$ is a dummy indicating whether $P_R > \overline{P}$ holds and $\mathbf{1}[P_T > \overline{P}]$ is a dummy indicating whether $P_T > \overline{P}$ holds.[29]

and fined 1-5 times the evaded tax amount. If tax evasion is extremely severe, life imprisonment or even the death penalty is possible. The misreporting costs may not only include fines and imprisonment, but also moral costs and resource costs of misreporting.

[28] Allingham and Sandmo (1972) framed the problem as a utility maximization problem. To avoid making assumptions about the utility function, we focus on a cost minimization problem, which is a necessary condition for any form of utility maximization. Thus, it may be the weakest condition that should be satisfied under this setting.

[29] If we consider the possibility that the house would be resold in the future, then future taxes and future misreporting costs need to be added to the total cost. The present value of tax liability when the house is resold next time is $\delta \cdot t_{future} \cdot (P_{R,future} - P_R) \cdot \mathbf{1}[P_{R,future} > \overline{P}_{future}]$, where δ denotes the discount factor and variables with a subscript $future$ denote corresponding parameters in a future time when the house is resold. Note that the potential tax increase due to current underreporting not just depends on the current reported price P_R, but critically depends on whether the future reported price $P_{R,future}$ would exceed a future unknown notch $\overline{P}_{future}$. Since the latter condition is difficult to consider in current decision making and δ may be quite small given that most houses would not be traded in the near future, the consideration of future resales is unlikely to drive the choice of currently reported price P_R.

5.2 Reconciling Cross-Sectional Reporting Patterns

5.2.1 Three-segment reporting pattern

If $P_T \leq \overline{P}$, given our model's simplifying assumptions, the model forecasts that $P_R = P_T$. Since misreporting is assumed to be associated with a positive cost, there is no gain to misreporting for prices with $P_T \leq \overline{P}$, where the tax due is already zero.

If $P_T > \overline{P}$, due to the jump of tax liability, there would be a strong incentive to underreport. Since $C' > 0$, the solution P_R can only be either $\overline{P}$ or some value larger than $\overline{P}$. If $P_R = \overline{P}$, then the total cost is $C(tP_T - tP_0)$. If $P_R > \overline{P}$, then total cost is $t \cdot (P_R - P_0) + C(t(P_T - P_R))$; in this case, the first order condition $t - tC'(t(P_T - P_R)) = 0$ yields a unique solution given that $C'' > 0$:

$$P_R = P_T - \frac{C'^{-1}(1)}{t}.$$

This solution implies the same optimal choice for P_U regardless of P_T, i.e. $P_U \equiv P_T - P_R = \frac{C'^{-1}(1)}{t}$. Note that we need $P_T - \frac{C'^{-1}(1)}{t} > \overline{P}$ to ensure an interior solution (here we mean $P_R > \overline{P}$). This requires the true price P_T to be above the notch $\overline{P}$ by at least $\frac{C'^{-1}(1)}{t}$.

With this solution, the total cost when $P_R > \overline{P}$ would be

$$C_{Interior} = t \cdot P_T - C'^{-1}(1) + C(C'^{-1}(1)) - tP_0,$$

where $P_T > \frac{C'^{-1}(1)}{t} + \overline{P}$.

Comparing it with

$$C_{Notch} = C(tP_T - tP_0),$$

which is the total cost when P_R equals the notch point $\overline{P}$, we can predict how people would choose.

Figure 6 illustrates the choice based on the total cost of reporting housing prices. Since $C' > 0$ and $C'' > 0$, C_{Notch} convexly increases starting from $\overline{P}$. By contrast, $C_{Interior}$ is a linear cost line with a slope of t. A linear line can at most cross once with a convex and increasing curve. If C_{Notch} always stays above $C_{Interior}$, we should not see any bunching at the notch, which contradicts our empirical evidence. Thus, C_{Notch} must cross once with $C_{Interior}$ and we have a unique solution to $C_{Interior} = C_{Notch}$. Denote the solution as P^*. Since $C_{Interior}$ starts from $P_T > \frac{C'^{-1}(1)}{t} + \overline{P}$, we know $P^* > \frac{C'^{-1}(1)}{t} + \overline{P}$. For houses with true prices close enough to the notch, i.e. $P_T \in (\overline{P}, P^*)$, the optimal choice is $P_R = \overline{P}$. This would create bunching at the notch point $\overline{P}$. For houses with true prices well above the notch, i.e. $P_T > P^*$, the optimal choice is $P_R = P_T - \frac{C'^{-1}(1)}{t}$. Thus, if people share a similar misreporting function, or, under a much weaker condition, if the curvature of the misreporting cost function $C(.)$ is largely the same for all individuals at $t = 100\%$, then the underreported amount $\frac{C'^{-1}(1)}{t}$ would be largely the same for all $P_T > P^*$.

The simple tax evasion model predicts a three-segment reporting pattern

$$P_R = \begin{cases} P_T & if\, P_T \leq \overline{P}, \\ \overline{P} & if\, P_T \in (\overline{P}, P^*)\,, \\ P_T - \frac{C'^{-1}(1)}{t} & if\, P_T > P^*, \end{cases} \tag{2}$$

where P^* is the solution to $C_{Interior} = C_{Notch}$.

Figure 7 illustrates the pattern graphically. In the first segment ($P_T \leq \overline{P}$), there is no misreporting; the relation between the reported price against the true price is a 45° line. In the second segment ($P_T \in (\overline{P}, P^*)$), we have $P_R = \overline{P}$, which predicts bunching for the distribution of P_R at $\overline{P}$. In the third segment ($P_T > P^*$), we expect to see a largely constant underreporting amount. In addition, our model also predicts a jump between the second and the third segments. For $P_T > P^*$, we have $P_R = P_T - \frac{C'^{-1}(1)}{t}$; when $P_T \to P^{*+}$, $P_R \to P^* - \frac{C'^{-1}(1)}{t}$. Since we already know $P^* > \frac{C'^{-1}(1)}{t} + \overline{P}$, we have $P_R \to P^* - \frac{C'^{-1}(1)}{t} > \overline{P}$ when $P_T \to P^{*+}$. This shows a clear jump of P_R as a function of P_T at P^*. Although without assuming a functional form of $C(.)$, we cannot describe the extent of this jump, it suffices to explain the observed small jump between the second and the third segments in Figure 5. Overall, our model built on parsimonious and reasonable assumptions neatly predicts the three-segment reporting pattern and bunching of P_R at the notch.

Intuitions for constant underreporting in the third segment. At a first glance, one might find the observed misreporting pattern to be surprising and implausible. After all, why should the multi-million RMB apartments underreport the same amount as the apartments with P_T just above P^*? This occurs because the marginal benefit from underreporting (i.e. tax saving) simply depends on the tax rate, while the marginal penalty depends on tax rate times an increasing function of the total tax evaded. In particular, the penalty does not depend on the percent of taxes evaded. Since P_T does not play any independent role in either benefit or cost side in our simple model, it is not surprising that the underreporting amount is independent of P_T in the interior solution segment (i.e. the third segment).[30]

5.2.2 Heterogeneity of misreporting

The heterogeneity of misreporting described in section 4.3 suggests that there are some omitted factors impeding people from behaving exactly as our simple model predicts.

[30] Note that by assuming an interior solution, equation 1 can be rewritten as $min_{P_U}\, tP_T - tP_U + C(tP_U)$. Then it is clear that P_T should have no impact on P_U.

Idiosyncratic misreporting costs. In Online Appendix G, we examine various factors that could help explain the observed heterogeneity. It turns out that while it may be too ambitious to use one factor to parsimoniously reconcile all the intensive margin heterogeneity, idiosyncratic misreporting costs λ alone (by making the misreporting costs as $\lambda \cdot C$ in the model) could explain much of the heterogeneous reporting patterns. In particular, the model predicts that people with a higher λ, e.g., those who have a higher moral cost of misreporting or those for whom the possibility of saving taxes through underreporting the sales price is less salient, tend to underreport less. Figure A10 illustrates how a change in λ would cause the shifting of the whole misreporting pattern, which is largely consistent with the observed "intensive margin" heterogeneity of misreporting. The idiosyncratic misreporting costs λ not only help explain the "intensive margin" heterogeneity of misreporting, but also help explain the lack of misreporting for some observations with $P_T > \overline{P}$. If only λ is large enough, people would find that the optimal reporting is just honest reporting. This is consistent with findings in the literature that people tend to comply more than what is suggested by a model with rational agents under reasonable audit probability and fines. The high misreporting costs imposed on people due to psychological or cultural aspects of tax compliance such as social norms, tax morale, patriotism, guilt, and shame may underlie the excessively high compliance rate widely documented in the literature (e.g., Andreoni et al., 1998).

Other taxes. Another omitted factor is the existence of other taxes, which could help explain the misreporting for $P_T \leq \overline{P}$. For houses with $P_T \leq \overline{P}$, even if they are not subject to the business income tax, they are still subject to a personal income tax, which is 1% of P_R (it can be exempted only if the seller has owned the house for at least 5 years and it is the only house of the seller), and a deed tax, which is proportional to P_R and the rate depends on the number of houses a buyer has (1-1.5% if it is the first house owned by a family, 3% if otherwise). These taxes are sufficient to create an incentive for those with $P_T \leq \overline{P}$ to underreport prices, while the idiosyncratic misreporting costs and diverse house ownerships (i.e. owning years and whether it is the first house owned by a family, the information which we unfortunately do not have) could help explain why misreporting does not exist for all houses with $P_T \leq \overline{P}$.

5.2.3 Important implications of an extended model

We have thus far modeled misreporting by taking the true tax base (e.g., true housing sales price) as given. In other settings, the true tax base (e.g., true earnings) may be endogenously determined by the efforts of decision-makers. In Online Appendix D, we extend our model to a case where the true tax base, e.g., earnings y_T, is determined by costly efforts. Suppose that the tax bureau can only observe the reported earnings y_R and levy an income tax on y_R at a rate t only if $y_R > \bar{y}$.

When the true tax base is determined by costly efforts, we obtain two key results:

First, the tax notch would additionally cause bunching of the true tax base y_T at the notch $\bar{y}$ and a hole for a range above $\bar{y}$ in the distribution of y_T. This would occur despite the true tax base being unobservable to the tax bureau. By contrast, in our model, as P_T is taken as given, we expect to see a smooth distribution of P_T. This difference provides additional support for our model because we do observe a smooth distribution of P_T in our data.

Second and more importantly, the relationship between the true tax base and the reported tax base holds exactly as in our benchmark model where the true tax base is taken as given. That is, the decision on the discrepancy between the true tax base and the reported tax base does not depend on how the true tax base is determined. Thus, the analysis of misreporting (either theoretically or empirically) does not rely on whether the true tax base is exogenous.[31] Even if the true tax base is affected by the tax rate, the misreporting decision is made in the same way as if the true tax base is taken as given.

5.3 Predicted Relationship Between Tax Rate and Misreporting

Main prediction. How would the three-segment misreporting pattern be affected by a tax rate change? There should be no change for the first segment: we still expect $P_R = P_T$ for $P_T \leq \overline{P}$. The general patterns for the second and third segments also do not change: the second segment is still a flat line starting from $\overline{P}$ to P^*; the third segment is still a parallel downward shifting of the 45° line that crosses the origin point. The only changes are the place of P^* and the extent of the downward shifting in the third segment.

First, consider how P^* would be affected theoretically. Note that P^* is the solution to $C_{Interior} = C_{Notch}$, where $C_{Interior} = t \cdot P_T - C'^{-1}(1) + C(C'^{-1}(1)) - tP_0$ and $C_{Notch} = C(tP_T - tP_0)$. Taking derivative of the equation with respect to t and simplifying it, we have

$$\frac{dP^*}{dt} = -\frac{P^*}{t} < 0,$$

which implies that as the tax rate decreases, P^* would increase.

Second, we examine how the extent of the downward shifting would change. For the third segment, $P_R = P_T - \frac{C'^{-1}(1)}{t}$. It is clear that $\frac{C'^{-1}(1)}{t}$ would increase when t decreases. Thus, we predict that the downward shifting would be greater after the tax rate decreases.

These responses are summarized graphically in Figure 8. Overall, our model predicts that in response to a decrease in the tax rate, the entire three-segment reporting pattern would shift rightwards, which implies a negative relationship between the tax rate and underreporting.

[31] But if one wants to study how the reported tax base is affected by the tax rate compared to the counterfactual, then whether the true tax base is exogenous matters. The focus on misreporting enables us to avoid this empirical difficulty.

Economic intuition. This prediction seems controversial and counterintuitive as the literature mostly focuses on how an increase in the tax rate would erode the tax base. To provide an intuition for this prediction, note that our model, by assuming a convex misreporting cost, echoes the previous literature (e.g., Scotchmer, 1987; Dharmapala et al., 2011) that emphasizes that due to the limited enforcement resources owned by the government, it may be optimal to focus auditing on firms or individuals with potential tax evasion above a certain threshold. The higher (perceived) auditing rate for a higher revenue at stake (brought about by a higher tax rate for a given P_U) could lead to a negative relationship between the tax rate and underreporting.

Theoretical ambiguity. Finally, it is worth noting that if the misreporting cost is a function of underreporting P_U (as initially assumed in Allingham and Sandmo (1972)) rather than tax evasion tP_U, then the model would predict a positive relationship between the tax rate and underreporting (see Online Appendix C.1 for a detailed discussion). Given this potential theoretical ambiguity and the counterintuitive forecast generated by our model, we proceed next to empirically examine the impact of a change in the tax rate on misreporting.

6 Empirical Relationship Between Tax Rate and Misreporting

In this section, we first test our model's prediction by qualitatively examining the responses of the whole misreporting pattern to a tax rate change brought about by the 2016 tax reform. Then we apply a difference-in-differences approach to quantitatively estimate the impact of the tax rate on misreporting.[32]

6.1 Responses of the Whole Misreporting Pattern to a Tax Rate Change

Since May 1, 2016, the business income tax has been replaced by the value-added tax. The notch point for second-hand housing transactions did not change; the standard for houses to enjoy the lower rate did not change; the enforcement also did not experience an explicit change. The only thing that has changed is the effective tax rate, which decreased from 5.6% to 5.33%.

In Figure 9, we show the empirical misreporting pattern for pre-reform months (April 2015-April 2016) and that for post-reform months (May 2016-June 2017). Although the change in re-

[32]When a tax notch or a kink exists, the bunching approach is often used to estimate parameters of interest (Saez, 2010; Chetty et al., 2011; Kleven, 2016). In Online Appendix H, we provide a detailed explanation of why we do not use the bunching approach to estimate the response of underreporting to tax in this setting. In brief, we maintain that the bunching pattern is only a symptom of the three-segment reporting pattern in our setting. Therefore, simply using the bunching pattern to estimate the elasticity of evasion with respect to the tax rate does not fully utilize the information available to us. Our estimates are directly based on the three-segment reporting pattern and thus better use the information provided in this setting.

porting patterns for houses within the inner ring is less salient, changes in reporting patterns of houses between the outer and inner rings and houses outside the outer ring are in line with our model's prediction. In particular, we do observe that P^* shifts to the right and that the third segment shifts downwards by a larger extent after the tax rate decrease, though the extent is small as the tax rate cut is small.

6.2 The Impact of the Tax Rate on Misreporting

6.2.1 The DD approach

The tax reform and the three-segment reporting pattern motivate us to use a difference-in-differences (DD) approach to estimate the impact of the tax rate on misreporting. In particular, the first segment is used as the control group because the tax rate did not change for houses with $P_T \leq \overline{P}$; the third segment is used as the treatment group because the tax rate decreased slightly for houses with $P_T > P^*$. We exclude the second segment because misreporting there is constrained by the notch. Since P^* could also be affected by a change in t, as a robustness check, we restrict our sample to a subset of the third segment to avoid potential overlap of the second and third segments under different tax rates.

The identifying assumption of the DD analysis is that the treatment and control groups share parallel counterfactual trends of P_U without other policy changes. While there is no other policy change that could drive the trends of P_U in the two groups to diverge during our sample period, two potential issues exist. First, the time-varying distribution of P_T may cause divergence of P_U between the two groups. We address this issue by using a constant weight in our regression analysis. In particular, we use the frequency of P_T in each bin (width=10w RMB) during the pre-reform period as the constant weight. We check the sensitivity of our estimates to the choice of weights by also using the weight based on the post-reform period. Second, because the control group consists of houses with low P_T and the treatment group consists of houses with high P_T, the trends of P_U in the two groups may evolve differently.[33] We address this issue by interacting P_T with month fixed effects η_m, which controls for differential time paths for houses with different P_T.[34]

The DD specification is

$$P_{U,irm} = \alpha \cdot Treat_{irm} + \beta_{ITT} \cdot Treat_{irm} \cdot Post + \eta_{rm} + P_{T,irm} \cdot \eta_m + \varepsilon_{irm}, \qquad (3)$$

[33] While our simple model predicts zero P_U in the first segment and a constant P_U in the third segment, as we discussed earlier in this paper, factors omitted in our simple model could lead to deviations of empirical misreporting from this sharp prediction.

[34] Relatedly, Freyaldenhoven et al. (2019) propose a 2SLS approach to use covariates that are not affected by the focal policy but by confounders to condition out the pre-trends caused by confounders between the treatment and control groups. However, in our case, we do not find appropriate covariates to implement this approach.

where $P_{U,irm}$ denotes price underreporting for transaction i in region r in month m. $Treat_{irm}$ is an indicator for transactions with $P_{T,irm}$ above the notch in region r (the third segment), which experienced a decrease in the tax rate after the reform. *Post* is a time indicator for observations after the reform. α measures the difference between the two groups before the reform. β_{ITT} measures the intent-to-treat (ITT) effect. Region-month fixed effects η_{rm} control for factors varying with both region and time (e.g., differential development paths of the housing market across regions over time, where regions are as divided in Table 1). Note that by including η_{rm}, we have also controlled for region-invariant time-specific factors (e.g., inflation or any time-specific policy change that affects the housing market in all regions simultaneously) and time-invariant region-specific factors (e.g., differential economic levels and population density across regions). $P_{T,irm} \cdot \eta_m$ controls for the differential time paths for houses with different P_T. ε_{irm} is the error term. We do not control for house fixed effects η_i because very few houses are transacted more than once in our sample.

There are several important points worth noting here. First, it is important to note that the analysis of misreporting does not depend on whether P_T is endogenously determined, as shown in section 5.2.3. The decision of P_U is the same for a given P_T, regardless of whether P_T is affected by the tax rate. Thus, whether the treatment and control groups are "endogeneously determined" in this setting does not affect our analysis of misreporting. This is a novel feature generated by our unique research focus on misreporting. Second, it is worth noting that while the existence of other taxes like the PIT may affect the levels of evasion in the cross-sectional distribution of misreporting (this is also why we do not use the cross-sectional misreporting pattern to study the quantitative relation between the tax rate and evasion), since other taxes did not experience changes during our study period and worked independently to the BIT, our DD estimates are unlikely to be contaminated by the existence of other taxes. Third, as introduced in the institutional background, the enforcement of the tax did not experience an explicit change after the tax reform.[35] Thus, the DD estimates would reflect the response of misreporting to the tax rate.

Although the identifying assumption of the DD specification cannot be directly tested, we use an event study to examine whether the treatment and control groups share parallel pre-trends after addressing issues discussed above. The regression specification is

$$P_{U,irm} = \alpha \cdot Treat_{irm} + \sum_{s \neq 0} \beta_s \cdot 1[m = m_0 + s] \cdot Treat_{irm} + \eta_{rm} + P_{T,irm} \cdot \eta_m + \varepsilon_{irm}, \tag{4}$$

where m_0 denotes the month just before the reform and β_s denotes the coefficient of the event study.

[35] Whether there is an induced change in the enforcement due to the tax rate change (this intuition is provided in section 5.3) is unknown and untestable using our data. But if there is such evidence, then our DD estimates still reflect the response of misreporting to the tax rate, with the induced change in the enforcement being the mechanism via which the tax rate affects misreporting.

Figure 10 shows the event study results. Panel (a) shows that using weights based on the pre-reform distribution of P_T, the relative pre-trends between the treatment and control groups evolve around zero, suggesting largely parallel pre-trends. The treatment group experiences a gradual increase of P_U relative to the control group after the reform (except for the 11th month after the reform, though we confirm that there is no relevant policy change at that time point). The extent of the post-reform divergence between the two groups is small, which is consistent with Figure 9, given that the decrease in the tax rate is quite small (from 5.6 pp. to 5.33 pp.). Panel (b) shows similar evidence using weights based on the post-reform distribution of P_T.

Based on the above evidence, we estimate the impact of the tax rate on misreporting, using the following specification

$$P_{U,irm} = \alpha \cdot Treat_{irm} + \beta_{2SLS} \cdot TaxRate_{irm} + \eta_{rm} + P_{T,irm} \cdot \eta_m + \varepsilon_{irm}, \tag{5}$$

where $TaxRate_{irm}$ denotes the tax rate (in percentage point) based on prices reported to the tax bureau P_R. The regression is estimated using a 2SLS approach, with $TaxRate_{irm}$ instrumented by $Treat_{irm} \cdot Post$.

6.2.2 Estimation results

Impacts of the tax rate on misreporting. Table 3 Panel (A) reports the reduced-form results estimated using specification 3 and Panel (B) reports the 2SLS estimates. In columns 1 and 2, the treatment group consists of observations in the third segment based on Figure 5. Using weights based on the pre-reform distribution of P_T (column 1), the reduced-form result shows that underreporting on average increases by 4.91w RMB due to the reform. The 2SLS result suggests that a one percentage-point (pp.) increase in the tax rate on average is associated with 4.03w RMB less underreporting.[36] To better interpret this result, using the weighted mean of P_U for the third segment of 97.97w RMB (misreporting is close to zero for the first segment), we convert this number to a semi-elasticity of underreporting with respect to the tax rate: $\frac{dlnP_U}{dt} = \frac{dP_U}{dt} \cdot \frac{1}{P_U} = -\frac{4.03}{97.97} = -0.04$. This means that a one pp. increase in the tax rate is associated with about 4% less underreporting in our sample. Column 2 also shows an estimate of -0.04 using weights based on the pre-reform distribution of P_T. The first-stage F-statistics are large in all regressions, suggesting that a weak IV is unlikely to be a concern.

Sensitivity. While our estimates are robust to the choice of weights, they could nonetheless still be sensitive to the choice of treatment groups. Because the 2SLS estimate measures the local

[36] In columns 1-4, the coefficients of the first-stage estimate range from -0.69 to -1.34, which are larger in magnitude than the nominal tax rate decrease above the notch (about -0.27). This is due to the existence of non-compliers in the treatment group and defiers in the control group, as discussed below.

average treatment effect (LATE) on the population of compliers (Angrist and Pischke, 2008), these estimates may depend on the relative distribution of compliers in the regression sample. In the third segment based on Figure 5 (i.e. $P_T > 600$w RMB within the inner ring, $P_T > 450$w RMB between the outer ring and the inner ring, $P_T > 330$w RMB outside the outer ring), among the 18,372 observations in the third segment ($P_T > P^*$), there are 2,981 observations (non-compliers) reporting $P_R \leq \overline{P}$.[37] We examine the sensitivity of the 2SLS estimates by restricting our sample to a subset of the third segment (i.e. $P_T > 700$w RMB within the inner ring, $P_T > 600$w RMB between the outer ring and the inner ring, $P_T > 400$w RMB outside the outer ring), which plausibly have a higher fraction of compliers according to our model. Indeed, among the 8,947 observations in this subset of the third segment, there are only 422 observations (non-compliers) reporting $P_R \leq \overline{P}$. Using this treatment group, columns 3 and 4 generate semi-elasticities of P_U with respect to the tax rate of -0.16 or -0.14, depending on the weights used.

Semi-elasticities. Overall, we obtain semi-elasticities of P_U with respect to the tax rate ranging from -0.16 to -0.04 depending on the sample and weights used in the regression. These estimates imply that a one pp. increase in the tax rate is associated with 4% to 16% less misreporting in our sample. By contrast, in the seminal empirical paper on tax evasion, Fisman and Wei (2004) estimate that a one pp. increase in the tariff rate is associated with a 3 percent increase in underreporting (defined as the difference between Hong Kong's reported exports to mainland China and China's reported imports from Hong Kong). These estimates obtained under different institutional backgrounds are helpful for us to form a more comprehensive sense of misreporting behavior.

Elasticities. Our estimates imply elasticities of P_U with respect to the tax rate (i.e. $\frac{dlnP_U}{dlnt}$) ranging from -0.9 to -0.2. Note that when the misreporting cost only depends on tax evasion tP_U, we have $\frac{dlnP_U}{dlnt} = -1$. This implies that the misreporting cost may not only depend on tax evasion but possibly also depend on P_U. For the ease of analysis, assume that the misreporting cost is an additively separable function of tP_U and P_U. Then the misreporting cost can be written as $C_1(tP_U) + C_2(P_U)$.[38] Under the assumptions that $C_1'' > 0$, $C_2' > 0$, $C_2'' > 0$, then $\frac{dlnP_U}{dlnt}$ is bounded below by -1. When $C_2(P_U)$ dominates, we have $\frac{dlnP_U}{dlnt} = \frac{t}{P_U C_2''} > 0$. Therefore, the deviation of our estimates of $\frac{dlnP_U}{dlnt}$ from -1 suggests the extent of the role that C_2 plays in the misreporting cost. The elasticities implied by our estimates in columns 3 and 4 of Table 3 (ranging from -0.9 to

[37] Among the 54,652 observations in the first segment ($P_T \leq \overline{P}$), only 14 observations (defiers) report P_R above the notch.

[38] With this assumption, the total cost under an interior solution (i.e. $P_R > \overline{P}$) becomes $t \cdot (P_R - P_0) + C_1(t(P_T - P_R)) + C_2(P_T - P_R)$,which yields a first order condition $t - tC_1'(t(P_T - P_R)) - C_2'(P_T - P_R) = 0$ or $t = tC_1'(tP_U) + C_2'(P_U)$. Taking derivatives with respect to t in both sides of the last equation above and simplifying it, we obtain that $\frac{dP_U}{dt} = \frac{1 - C_1' - tP_U C_1''}{t^2 C_1'' + C_2''}$, which yields an elasticity of underreporting with respect to the tax rate as $\frac{dlnP_U}{dlnt} = \frac{t - tC_1' - t^2 P_U C_1''}{t^2 P_U C_1'' + P_U C_2''} = -1 + \frac{C_2' + P_U C_2''}{t^2 P_U C_1'' + P_U C_2''}$.

-0.8), which are our preferred estimates due to plausibly capturing responses of a higher fraction of compliers as argued above, suggest that C_2 play a relatively small role in our setting. In particular, the standard errors for implied elasticities in columns 3 and 4 are 0.19 and 0.17, respectively. Thus, the null hypothesis that the implied elasticity equals -1 cannot be rejected even at the 10% level of significance, which justifies our focus on C_1 in our benchmark model.

Caveats. It is worth emphasizing that our estimates are driven by the one-shot small decrease of the tax rate above the notch from 5.6 pp to 5.33 pp. This imposes some caveats on the generalization of our estimates. First, as well recognized, misreporting responses depend on the specific institutional environment. The estimate obtained from one type of tax in a country may not apply to another tax or in a different country. For this reason, estimates under various settings are helpful to enrich the understanding of misreporting responses to the tax rate. Second, the impact of the tax rate on misreporting could be nonlinear. Thus, even for the same tax, our estimate may not necessarily apply to a larger extent of the tax rate change. In fact, as discussed in detail in Online Appendix J, the *marginal* response of misreporting to tax (reflected in our DD estimates) and the *aggregate* response of misreporting to tax (reflected in the cross-sectional misreporting pattern) together imply a nonlinear relationship between the tax rate and misreporting. While we are unaware of literature documenting the nonlinear impact of the tax rate on misreporting, Benzarti et al. (2017) find that prices respond more to increases than to decreases in value-added tax rates, which provides evidence for nonlinear responses under the VAT setting.

7 Conclusion

Using a unique data set, we provide the first evidence of the empirical relationship between the reported tax base and the true tax base at all levels of the true tax base under a tax notch. This relationship includes not only the expected bunching for those whose true sales price is modestly above the notch, but also a largely constant amount of price underreporting for those with a true sales price further above the notch. To reconcile this and other cross-sectional patterns of misreporting documented in this paper, we propose a simple model building on assumptions consistent with the institutional background and relevant literature. This model generates a surprising forecast that underreporting should fall when the tax rate goes up, which we test and confirm using our data. Our finding implies that while a marginal increase in the tax rate would create efficiency losses by reducing transactions of second-hand houses, it could reduce underreporting among those who still sell their houses (this may bring about efficiency and equity gains), which stands in contrast with the bulk of the literature that emphasizes how an increase in the tax rate would erode the tax base.[39]

We conclude with some important caveats. First, this paper focuses exclusively on misreporting behavior and its responses to the tax rate. We admit that the impacts of the tax rate on other aspects (e.g., the true transaction price, number of transactions, incidence of tax) are important for evaluating the comprehensive impact of the tax. But they are less relevant for our research focus and thus are not examined in this paper. Accordingly, a welfare analysis of the housing transaction tax falls beyond the scope of this paper.

Second, while there is a tax notch in our setting, we do not follow much of the literature to focus on the bunching behavior or use the bunching method. This is because the bunching behavior is just a small (and unsurprising) part of a series of stylized facts documented in this paper. The unique data and our focus on misreporting enable us to present new evidence of misreporting that has never been documented in the previous literature.

Third, while we document a series of new facts of misreporting, we leave some further questions unanswered. For example, partly due to data limitations, we do not consider the role of agents (as discussed in Agarwal et al., 2019) in making the misreporting decision. While collusion is possible among agents, buyers, and sellers, we are unable to study it. In addition, we are also unable to empirically reveal the mechanism via which the tax rate affects misreporting. While in section 5.3 we provide an intuition that an induced change in the enforcement could be a channel by which the tax rate affects misreporting, we are unable to test it using our data. Subsequent research that addresses these issues would provide further insights to help us understand misreporting and guide us towards making better policy decisions.

[39] In Online Appendix I, we show preliminary evidence that a drop in the tax rate may increase transactions of second-hand houses. In Online Appendix K, we discuss the implication of the negative impact of the tax rate on underreporting in more detail. The finding that a marginal increase in the tax rate could reduce underreporting may imply both equity gains and efficiency gains. From the equity perspective, since underreporting in our setting is made by those trading expensive houses, reducing underreporting would bring about equity gains. From the efficiency perspective, since underreporting involves resource costs of income hiding and enforcement, less underreporting implies efficiency gains. But whether these gains would be overwhelmed by the efficiency losses brought about by reduced transactions is beyond the scope of this paper.

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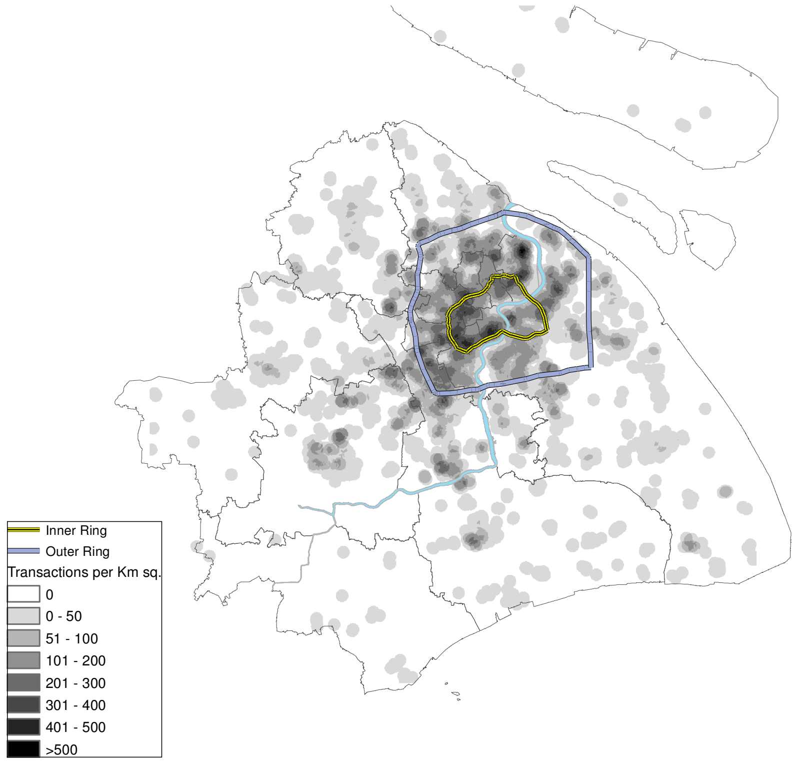
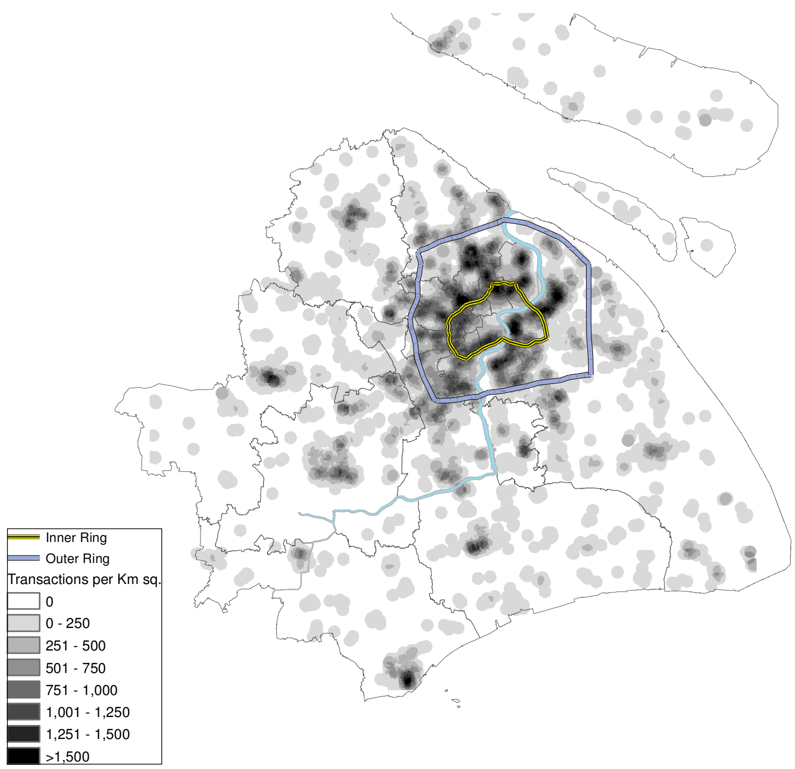
Figure 1: Heatmap of Second-Hand Housing Transactions in Shanghai

(a) Transactions Handled by the Sample Brokerage Company



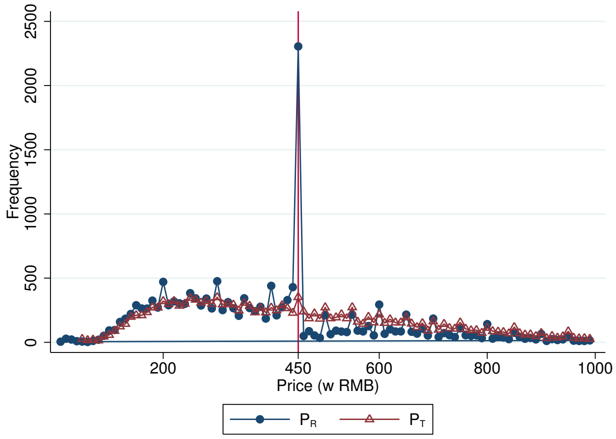
(b) All Transactions Recorded by the HMB



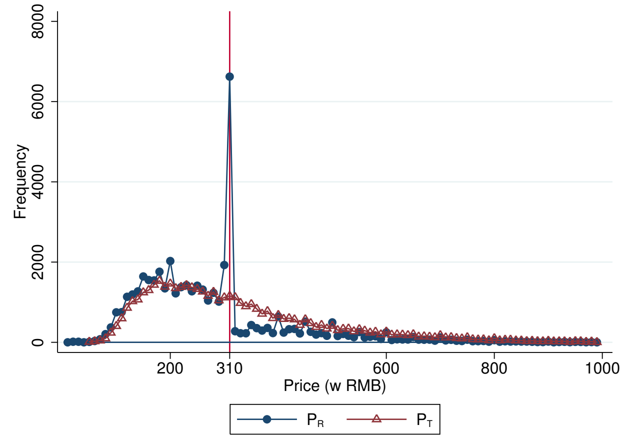
Notes: Data period is April 2015-June 2017.

Figure 2: Distribution of True Prices and Reported Prices

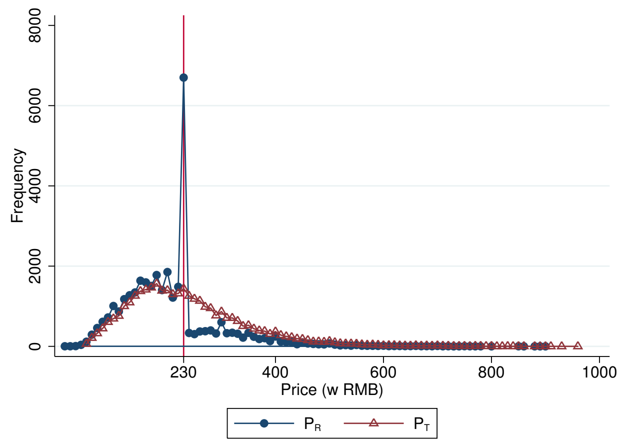
(a) Within the Inner Ring



(b) Between the Outer and Inner Rings



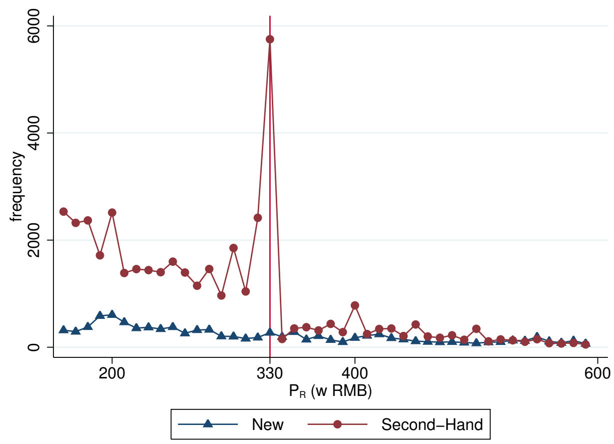
(c) Outside the Outer Ring



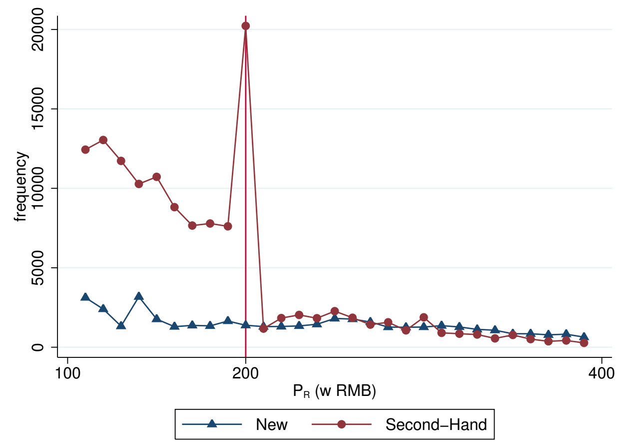
Notes: bin width=10w RMB. The vertical line indicates the tax notch. This figure shows that the distribution of true prices is smooth, while the distribution of reported price has a clear bunching at the tax notch for each region.

Figure 3: Reported Prices of New vs. Second-Hand Housing Transactions

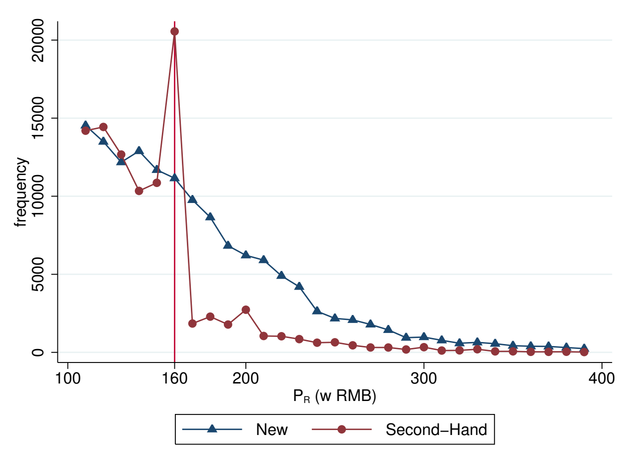
(a) Within the Inner Ring



(b) Between the Outer and Inner Rings

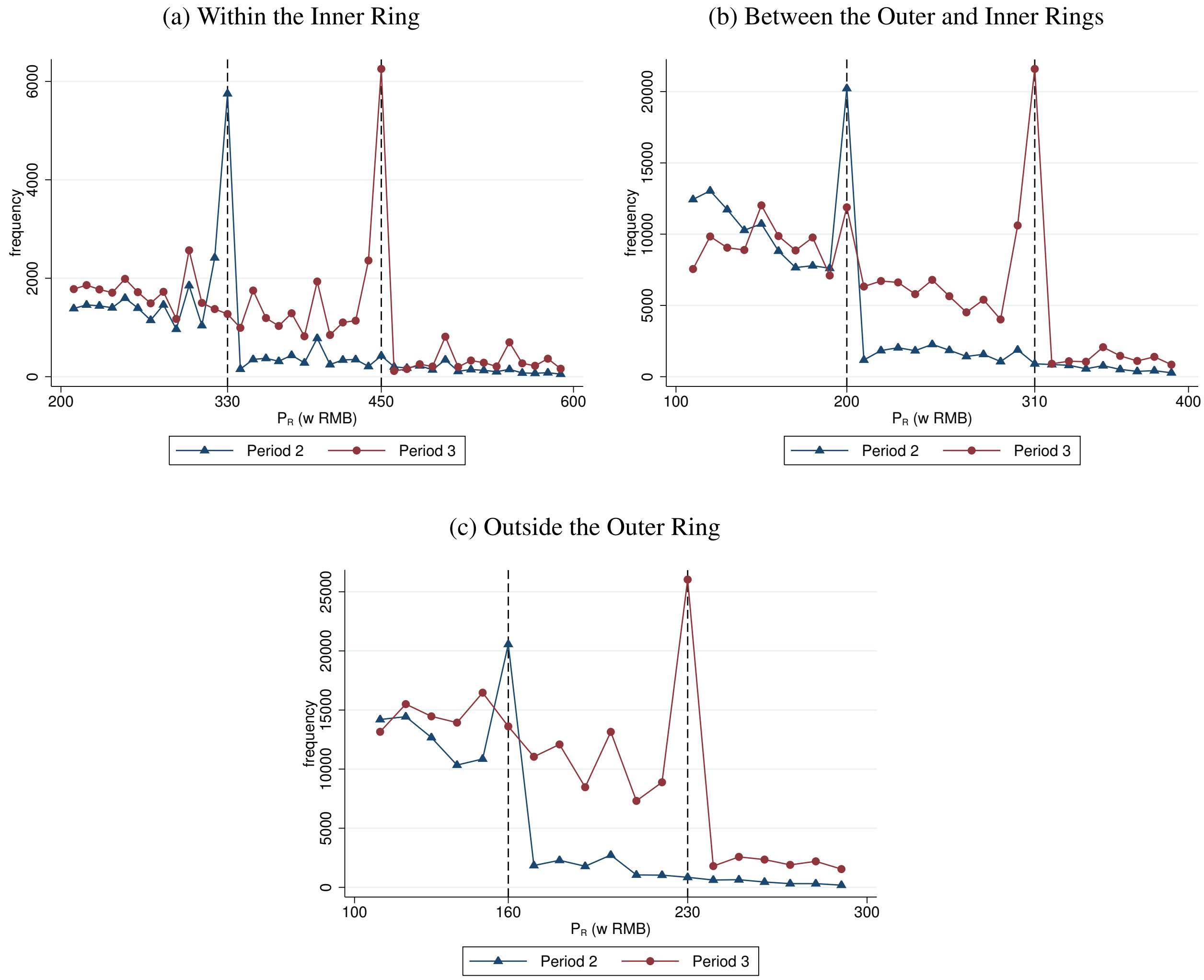


(c) Outside the Outer Ring



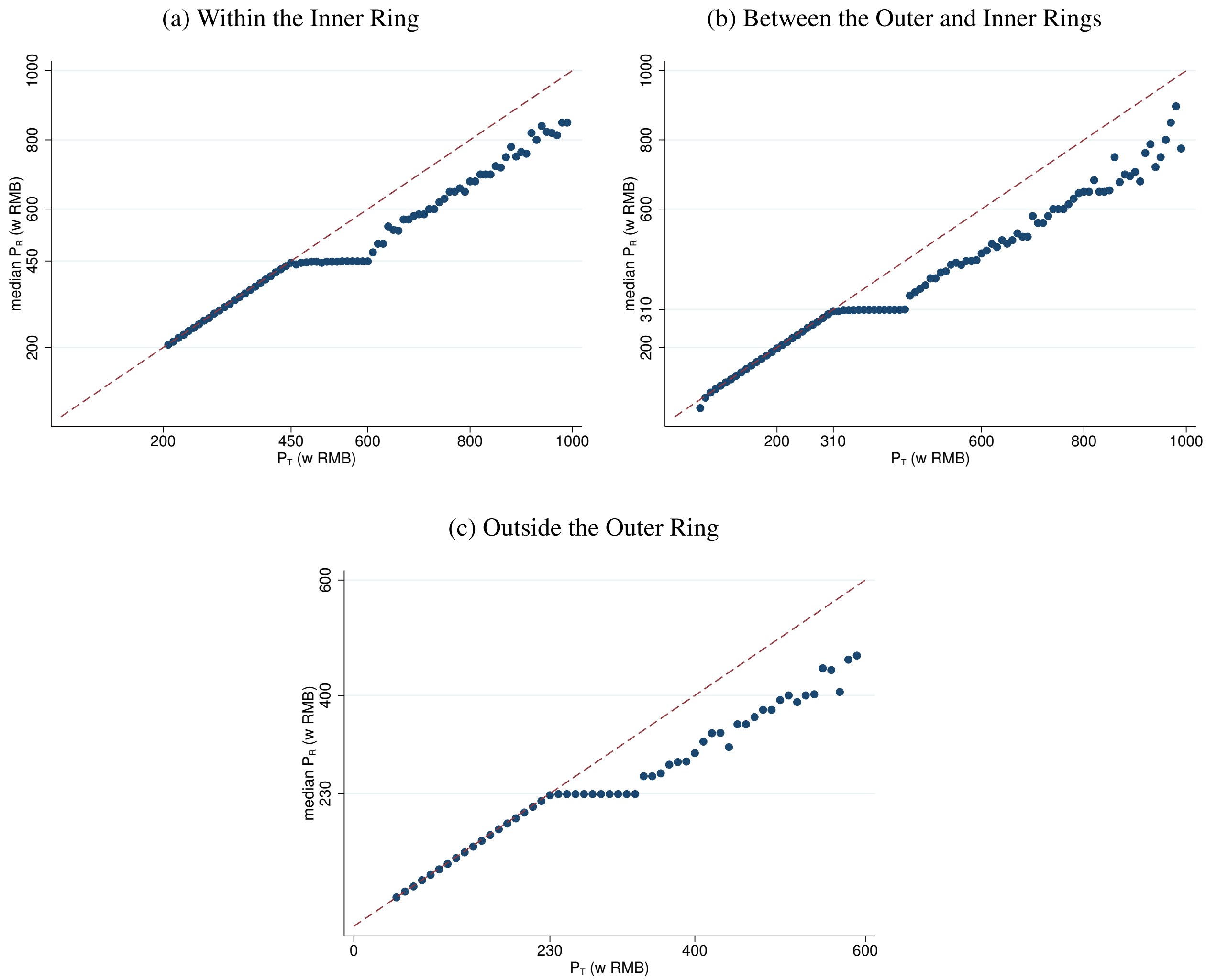
Notes: bin width=10w RMB. Sample period: March 1, 2012 – Novermber 19, 2014. This figure shows that while there is a clear bunching at the business income tax notches for the reported prices of second-hand housing transactions, the distribution of reported prices for new housing sales is smooth around the notch. This is consistent with the policy that only second-hand transactions face tax notches while new housing sales do not.
Data source: Shanghai Housing Management Bureau.

Figure 4: Dynamic Responses of Bunching to Notch Changes



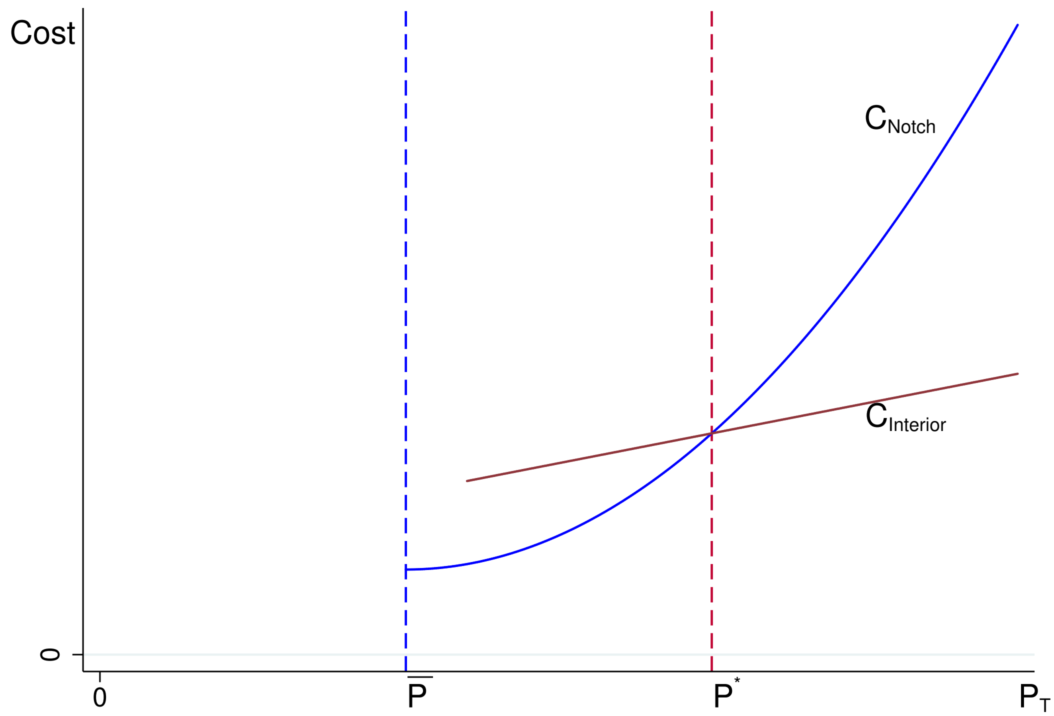
Notes: bin width=10w RMB. Period 2: March 1, 2012-November 19, 2014. Period 3: November 20, 2014-June 30, 2017. This figures shows that in all locations, bunching exists only when a notch is implemented, i.e. bunching disappears in old notches and forms in new notches. See Table 1 for tax notches in different periods in each location.
Data source: Shanghai Housing Management Bureau.

Figure 5: Empirical Pattern of Reported Housing Prices Against True Prices



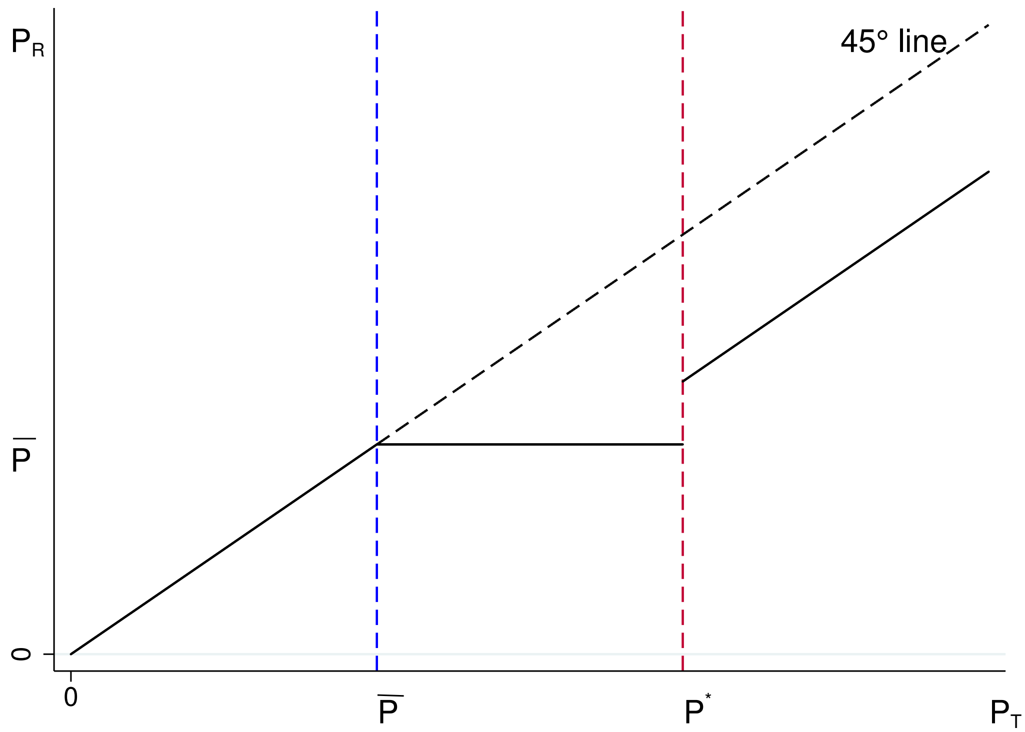
Notes: bin width=10w RMB. The dashed line is a 45-degree line. This figure shows the median reported price for each bin of true prices. It shows that for each notch, the empirical pattern of reported prices against true prices is consistent with the predicted pattern suggested in Figure 7.

Figure 6: Total Cost of Reporting Housing Prices



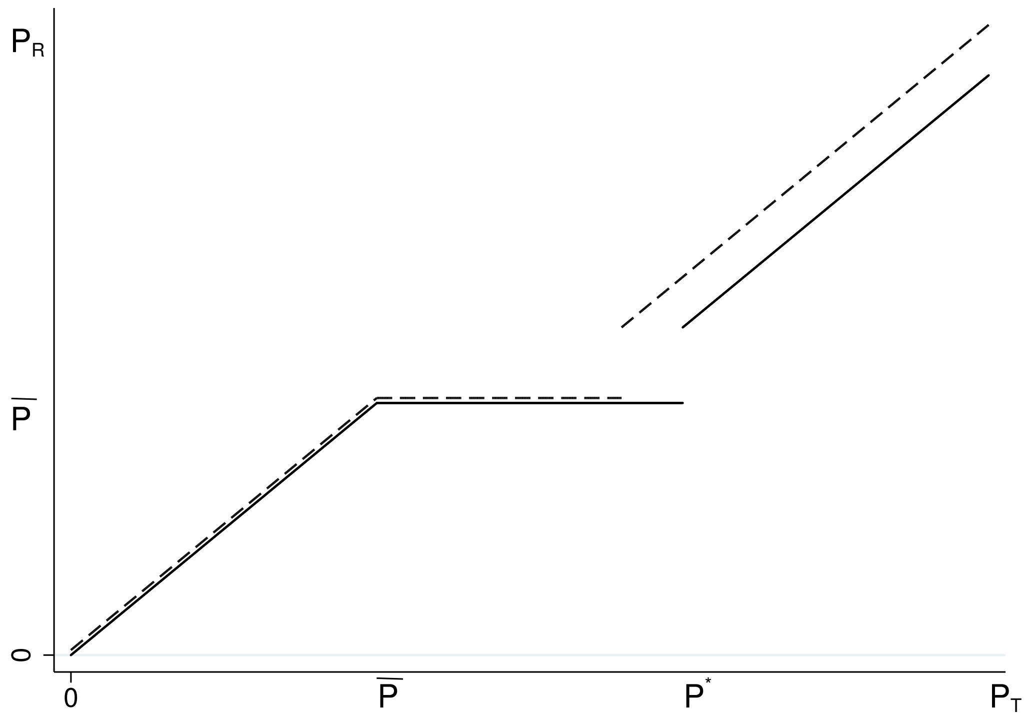
Notes: Total cost equals tax liability plus misreporting cost when a reported price P_R is chosen for a given true price P_T. $C_{Interior}$ is the optimal total cost when $P_R > \overline{P}$, and C_{Notch} is the total cost when $P_R = \overline{P}$, where $\overline{P}$ denotes the notch point.

Figure 7: Predicted Price Reporting Pattern for Housing Sales



Notes: P_R denotes the reported price. P_T denotes the true price. $\overline{P}$ denotes the notch point. P^* is the cross point of $C_{Interior}$ and C_{Notch}. $C_{Interior}$ (C_{Notch}) denotes the cost when the optimal choice of P_R for a given P_T is an interior (notch) solution.

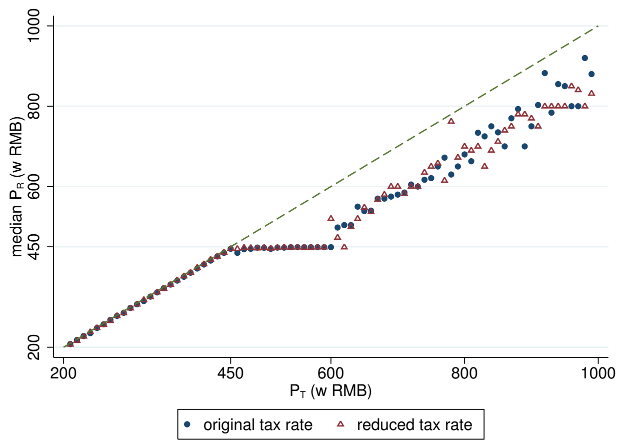
Figure 8: Predicted Shifting of the Whole Reporting Pattern to a Decrease in the Tax Rate



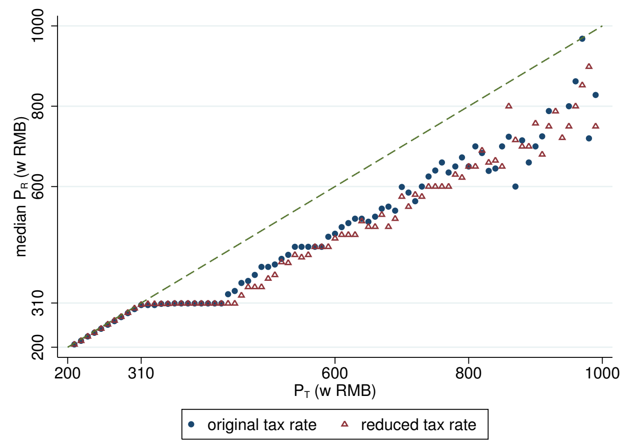
Notes: The dashed lines show the original reporting pattern, while the solid lines show the reporting pattern after a decrease in the tax rate above the notch $\overline{P}$.

Figure 9: Responses of the Whole Misreporting Pattern to a Reduced Tax Rate

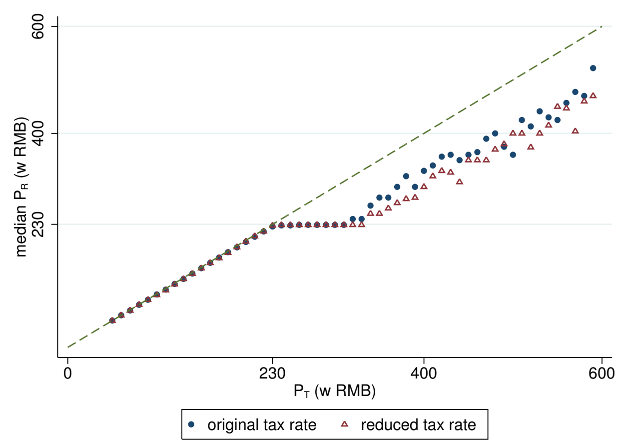
(a) Within the Inner Ring



(b) Between the Outer and Inner Rings



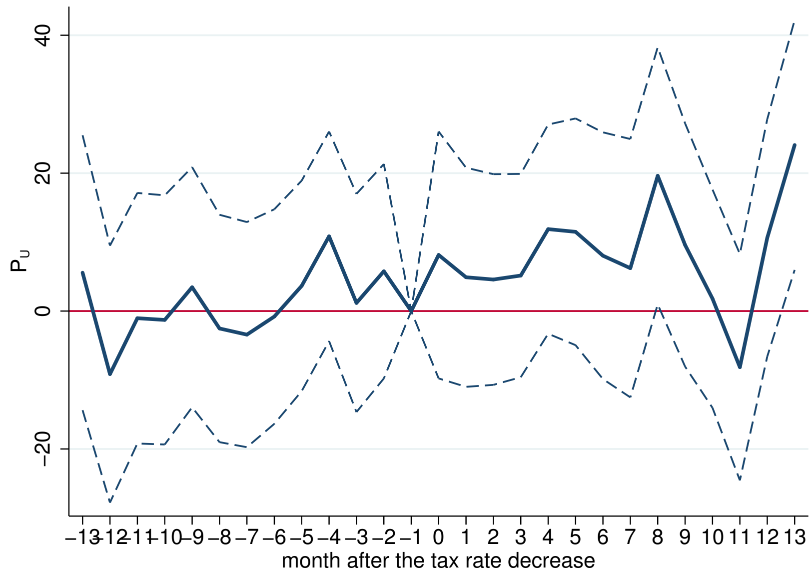
(c) Outside the Outer Ring



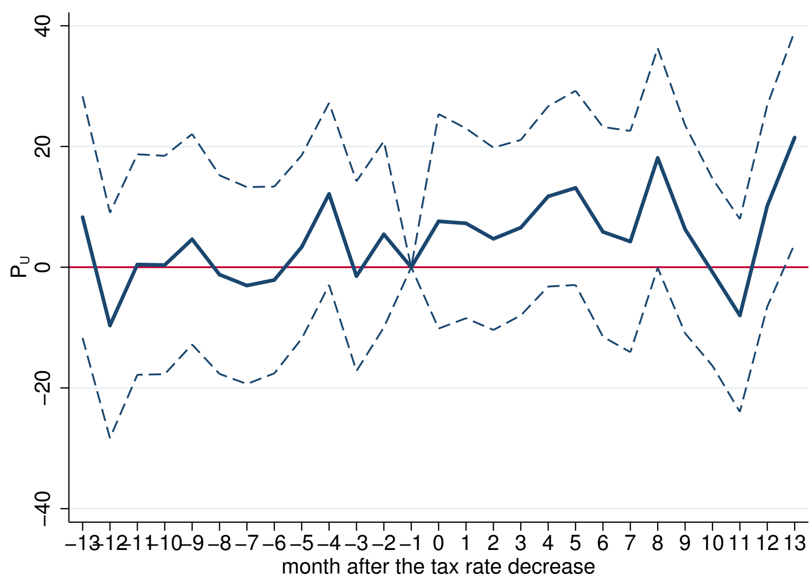
Notes: bin size=10w RMB. The dashed line is a 45-degree line. This figure shows the shift of the whole misreporting pattern after the VAT-for-BIT reform reduces the tax rate for transactions with P_R above the tax notch from 5.6% to 5.33% after May 1, 2016.

Figure 10: The Impact of the Tax Reform on Misreporting: Event Study Evidence

(a) Weights based on the pre-reform distribution of P_T



(b) Weights based on the post-reform distribution of P_T



Notes: The event study specification is shown as equation 4. Dashed lines show the 95% confidence interval of the point estimate. The first segment is used as the control group as the tax rate does not change for houses with $P_T < \overline{P}$; the third segment is used as the treatment group as the tax rate decreases slightly for houses with $P_T > P^*$. We exclude the second segment because misreporting there is constrained by the notch. The reference period is set to be the month prior to the reform. The figure shows that using either weight, the treatment and control groups share parallel pre-trends; the treatment group experiences a gradual increase of P_U relative to the control group after the reform.

Table 1: Shanghai Housing Tax Notches

Location	Period 1 (2008-Feb 29, 2012)	Period 2 (Mar 1, 2012-Nov 19, 2014)	Period 3 （Nov 20, 2014-Now）
Within the Inner Ring	245	330	450
Between Outer and Inner Ring	140	200	310
Outside the Outer Ring	98	160	230

Notes: Unit is w RMB. Here w denotes *wan* in Chinese, which is a very commonly used unit. 1w=10,000. For example, in 2015, the exchange rate is 1 USD=6.23 RMB, a house worth 450w RMB is about 0.72 million USD.

Table 2: Representativeness of the Brokerage Company Data

	Brokerage Company Data		HMB Data
Price (w RMB)	True	Reported	Reported
Mean	365.93	313.70	243.07
S.D.	286.96	255.26	281.62
Median	280	235	186
90th Percentile	695	582	449
10th Percentile	140	124	75
Area (square meter)			
Mean	83.06		83.65
S.D.	40.56		71.88
Median	75.30		75.19
90th Percentile	135.25		131.85
10th Percentile	41.25		41.59
Location (fraction)			
Within the Inner Ring	0.20		0.14
Between Outer and Inner Ring	0.46		0.37
Outside the Outer Ring	0.34		0.49
Observations	104787		681824

Notes: 1w=10,000. Data period is April 2015-June 2017. This table shows that while the distributions of areas of second-hand houses are comparable in two data sets, reported prices are generally larger for houses handled by the brokerage company. Houses handled by the brokerage company are more likely to be located within the inner ring or between the outer and inner ring, where houses are more expensive. Even within each region, the housing prices are higher for houses handled by the brokerage company (not shown in this table to save space). This is not surprising as transactions of more expensive houses are more likely to be dealt with by a brokerage company.

Table 3: The Impact of the Tax Rate on Misreporting

Dependent variable: P_U	[1]	[2]	[3]	[4]
Panel (A): Reduced-form results				
Treat	49.97***	46.99***	53.71***	51.84***
	(1.26)	(1.28)	(2.07)	(2.04)
Treat x Post	4.91***	4.64***	12.24***	11.87***
	(1.72)	(1.71)	(2.65)	(2.63)
Panel (B): 2SLS results				
Treat	68.01***	62.16***	149.97***	138.41***
	(5.35)	(4.61)	(18.88)	(17.20)
Tax Rate (percentage point)	-4.03***	-3.47***	-17.87***	-16.09***
	(1.38)	(1.25)	(3.79)	(3.48)
First-stage coefficent for Tax Rate	-1.22***	-1.34***	-0.69***	-0.74***
	(0.03)	(0.03)	(0.02)	(0.02)
First-stage F-stat	1696	1741	1245	1301
Other Controls	√	√	√	√
Weight: pre-reform distribution of P_T	√		√	
Weight: post-reform distribution of P_T		√		√
Treatment group: the 3rd segment in Figure 5	√	√		
Treatment group: a subset of the 3rd segment			√	√
Observations	72,952	73,023	63,527	63,598
Weighted mean of DV for the treatment group	97.97	96.7	110.86	112.78
Implied semi-elasticity	-0.04***	-0.04***	-0.16***	-0.14***
	(0.01)	(0.01)	(0.03)	(0.03)
Implied elasticity	-0.23***	-0.20***	-0.90***	-0.80***
	(0.08)	(0.07)	(0.19)	(0.17)

Notes: Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Misreporting P_U is defined as true price P_T minus reported price P_R. Other controls include region-month fixed effects and the differential time paths for houses with different P_T. The third segment consists of houses with $P_T > 600$w RMB within the inner ring, $P_T > 450$w RMB between the outer ring and the inner ring, $P_T > 330$w RMB outside the outer ring. A subset of the third segment consists of houses with $P_T > 700$w RMB within the inner ring, $P_T > 600$w RMB between the outer ring and the inner ring, $P_T > 400$w RMB outside the outer ring. Implied semi-elasticities and elasticities are self-calculated following the procedure described in the text.

Online Appendix

(Not For Publication)

A Other Relevant Taxes and Fees

Deed tax. The deed tax is proportional to P_R and the rate depends on the number of houses a buyer has and the area of the house. For the first house owned by a family, the tax rate is 1% if the house is below 90 m^2, 1.5% if above 90 m^2. If it is not the first house owned by a family, the tax rate is 3%, regardless of the area.

Personal income tax. By law, the personal income tax (PIT) involved in a second-hand housing transaction is levied on the seller. But in reality, it is mostly the buyer who pays it. The PIT is exempt if the seller has owned the house for at least 5 years and it is the only house of the seller. If either condition is not satisfied, the PIT needs to be paid. There are two ways to calculate the PIT amount. First, it can be calculated as 20% times (sales price - original purchase price - taxes and fees paid in the transaction). To use this method, the seller must provide a complete and accurate certificate for the purchase price of the house. Second, it can be 1% times the reported sales price. In practice, individuals will choose whichever method that generates a lower tax. In Shanghai, most cases adopt the latter method. As house prices have been growing fast in China, the second method is likely to yield a lower tax liability for most cases.

Commission fee. For houses transacted with the help of real estate brokers, a commission fee (mostly paid by buyers) varying from 1-3% of the true price applies. There are some other fees, though they are minor and negligible.

B Placebo Tests

B.1 New Housing Sales vs. Second-Hand Housing Transactions

Since only second-hand transactions face tax notches while new housing sales do not, the reported prices for new housing sales are expected not to respond to the business income tax notches. We examine this using data from the Shanghai Housing Management Bureau. Figure 3 shows the distributions of reported prices in period 2 (March 1, 2012-Nov 19, 2014).[40] As expected, while there is clear bunching at the business income tax notch for second-hand housing transaction prices, the distribution of new housing sales prices is entirely smooth around the notch. The number of new housing sales within the outer ring is much less than that of second-hand housing transactions, due to a restricted supply of new houses there. By contrast, there is little restriction on new housing sales outside the outer ring. Instead, the distribution of new housing sales in that region is similar to that of second-hand housing transactions in places away from the notch, while the sharp contrast is clear around the notch.

B.2 Dynamic Responses of Bunching to Notch Changes

As another placebo test, we use the HMB data to explore dynamic bunching responses of P_R to notch changes. We test if bunching exists only when a notch is implemented. We report evidence in period 2 (March 1, 2012-Nov 19, 2014) versus period 3 (Nov 20, 2014-June 30, 2017) because observations in period 1 are relatively few (7.94%), while those in period 2 (40.16%) and period 3 (51.9%) are more comparable. Figure 4 shows that in all locations, there is no bunching at the notch point when the tax notch is not implemented,[41] and that bunching disappears in old notches and forms in new notches clearly.

[40] Using data in period 3 (Nov 20, 2014-June 30, 2017) shows a similar pattern.

[41] One exception is that there is some bunching at 200w RMB even when there is no notch there. This presumably is the well-known bunching at rounder numbers due to behavioral reasons (Kleven and Waseem, 2013; Kleven, 2016).

C Understanding the Perceived Misreporting Cost

In this section, we first discuss potential alternative forms of the misreporting cost function and their implications. Although we cannot exhaust all alternatives, the empirical findings support our main model against major alternative assumptions of misreporting cost. In the end, we discuss the perceived misreporting cost in detail.

C.1 Yitzhaki (1974) vs Allingham and Sandmo (1972)

In our model, the misreporting cost is assumed to be a function of tax evasion, as did in Yitzhaki (1974). If we instead assume that the misreporting cost is a function of underreporting, as did in Allingham and Sandmo (1972), then the model would also generate a three-segment reporting pattern. However, the prediction about the response to a tax rate change would be just the opposite.

To see this, note that with a cost function $C(P_T - P_R)$, the first-order condition would be $t - C'(P_T - P_R) = 0$, which renders $P_U \equiv P_T - P_R = C'^{-1}(t)$. Since $C' > 0$, this implies that underreporting increases with t in the third segment. This is in sharp contrast with the implication obtained by assuming a cost function $C(t(P_T - P_0) \cdot \mathbf{1}[P_T > \overline{P}] - t(P_R - P_0) \cdot \mathbf{1}[P_R > \overline{P}])$. The reason for the contrast lies in how the marginal cost (MC) of underreporting depends on the tax rate t: When the cost function is $C(P_T - P_R)$, the MC of underreporting is independent of t; when the cost function is $C(t(P_T - P_0) \cdot \mathbf{1}[P_T > \overline{P}] - t(P_R - P_0) \cdot \mathbf{1}[P_R > \overline{P}])$, the MC of underreporting is $tC'(t(P_T - P_R))$ when $P_R, P_T > \overline{P}$, clearly a function of t. The way t enters the marginal cost-benefit tradeoff determines the contrast between the two models. Previous literature largely remains silent about a serious test between these two assumptions. Fortunately, our data allow us to conduct a direct test between the two contrasting predictions.

Section 6.1 shows how a small tax rate decrease affects the misreporting pattern. When the cost function is $C(t(P_T - P_0) \cdot \mathbf{1}[P_T > \overline{P}] - t(P_R - P_0) \cdot \mathbf{1}[P_R > \overline{P}])$, the third segment of the reporting pattern is $P_R = P_T - \frac{C'^{-1}(1)}{t}$, which will shift downwards as t decreases. By contrast, when the cost function is $C(P_T - P_R)$, the third segment is $P_R = P_T - C'^{-1}(t)$, which will shift upwards as t decreases. The empirical evidence in section 6.1 supports the Yitzhaki (1974) assumption against the Allingham and Sandmo (1972) assumption, at least under our policy environment.

C.2 Refuting Many Other Alternative Assumptions

The empirical reporting pattern documented in Figure 5 rules out many alternative assumptions of misreporting cost. First of all, it suggests there exists a positive misreporting cost. If there is no misreporting cost, P_R can be any value below $\overline{P}$ for $P_T \leq \overline{P}$, and we should also observe $P_R \leq P_T$ for all $P_T > \overline{P}$. Since we do not see these, some positive misreporting costs must exist. Second, it suggests the misreporting cost is not fixed, is not linear, and is not concave. If the misreporting cost is ϕ, i.e. fixed and common to all individuals, people will simply choose P_R to be either P_T or zero, whichever giving the smaller total cost. Then we would expect to see people with $P_T > \frac{\phi}{t}$ choose $P_R = 0$. Since we do not see this, this cannot be true. If the misreporting cost is linear, i.e. $C(tP_U) = \alpha + \beta(tP_U)$, with $\alpha > 0$, $\beta > 0$, we should expect to observe all people choose $P_R = \overline{P}$ (if $\beta < 1$) or $P_R = \infty$ (if $\beta > 1$). Since we do not see this, this also cannot be true. Furthermore, if the cost function is concave, a graph similar to Figure 6 implies that we should see $P_R = \overline{P}$ for all transactions with P_T larger than a certain value. But we do not see this in Figure 5. Thus, the misreporting cost cannot be concave.[42]

C.3 No Misreporting Cost, Only Government-Guided Price?

There is a concern that factors other than misreporting costs might generate the reporting pattern we observe. A potential factor that may directly affect the reporting pattern in our setting is the government-guided price. We do not introduce this in the main text because (1) we are unable to empirically study its impact due to its property-specific and time-varying feature, and (2) it is unlikely to drive the empirical misreporting pattern. These points are discussed in detail below.

The government-guided price can be regarded as an imperfect way to curb misreporting by the government. Since the government cannot observe the true housing transaction prices, to combat tax evasion, it sets a so-called government-guided price P_G to prevent unreasonably low reported price. P_G works as a lower bound for reported price per area rather than the reported price itself.[43] Let A denote the area of a house, then it requires that $\frac{P_R}{A} \geq P_G$. An important feature of P_G is that it is property-specific and time-varying, which is determined based on the recent record of reported prices of second-hand housing transactions. This implies that the impact of P_G on misreporting is idiosyncratic and may not yield general implications on misreporting. Besides, while the real estate brokerage companies could have a reasonable estimate of it based on previous transactions, P_G is not publicly available (also unknown to us). Therefore, while P_G will affect misreporting mechanically (by requiring $P_R > AP_G$), we are unable to empirically account for the extent to which P_G affects misreporting.

Can P_G alone drive the major empirical reporting patterns documented in this paper? Without

[42] Another potential form of the misreporting cost function is $P_T \cdot f(\frac{P_T - P_R}{P_T})$, i.e. the cost depends on the true price times a function f of the evasion ratio. In this case, the interior solution is $P_R = (1 - C'^{-1}(t))P_T$, i.e. reported price is a proportional fall from the true price. But this is inconsistent with the third segment (the interior solution part), which suggests this possibility does not hold. In addition, in practice, since P_T is unknown to the government, this cost function may be inconsistent with a plausible government auditing and enforcement regime.

[43] For new housing sales, the government normally sets an upper bound for housing sales prices, in order to curb the fast growth of new housing sales prices. For second-hand housing transactions, in rare cases, the government may also set an upper bound for transaction prices.

misreporting costs, given the tax-saving benefit from underreporting and assuming that P_G is publicly known, people will simply choose $P_R = A \cdot P_G$. For a specific house with a given area A, it is unlikely for the government to choose a P_G for the property to generate the striking reporting pattern documented in Figure 5. Therefore, P_G alone is unlikely to drive the empirical patterns we see and there must exist some misreporting costs.

C.4 How to Understand the Misreporting Cost?

It is well recognized that it is the perceived or expected costs rather than the objective or actual costs that matter for decision making. Thus, in our case, the misreporting cost should also be interpreted as the perceived cost. But what is in the perceived cost? The law clearly states the punishment for tax evasion. Individuals may have a vague impression of the potential punishment but may not know the details of the law clauses. Yet the real estate brokerage company, with the help of professional lawyers in their team, must know it well. Given the law clauses, even if currently there were no fines or punishment for misreporting in housing transactions, the taxpayers see the possibility that the tax authority may trace back their underreporting in the future and punish them, as has been recently experienced by top earners and enterprises that have evaded or avoided taxes in China. Thus, the perceived cost of misreporting reasonably exists.

Given the lack of evidence of auditing and punishment on misreporting in second-hand housing transactions (though auditing and punishment are prevalent for many other taxes), the objective costs seem to be much lower than the perceived costs. Yet this is just what has been documented in the literature. In a comprehensive review of the literature on tax compliance, Andreoni et al. (1998) find that people comply much more than what is predicted by theory and the actual auditing rate and fines. The discrepancy between a standard model and actual tax compliance reflects the difference between the actual noncompliance cost and the perceived non-compliance cost. While the fundamental reason for this discrepancy still waits for further research, an immediate implication is that policymakers can achieve a high compliance target by paying much lower actual costs.

D A Model of Misreporting where the True Tax Base is Determined by Costly Efforts

In this section, we consider a setting where the true tax base is determined by costly efforts. A person of ability a pays effort e to get true earnings $y_T = af(e)$, with $f' > 0$ and $f'' < 0$. Under an environment with weak enforcement, the person may report y_R, which could be different from y_T, to the tax bureau. A tax notch exists so that if $y_R < \bar{y}$, no tax is due; otherwise if $y_R > \bar{y}$, the person needs to pay income tax with a rate t on all reported earnings. Same as the spirit of our model, suppose that a convex misreporting cost $C(.)$ arises if the person underreports and it is a function of the tax evasion. Assume effort e incurs a money metric cost $g(e)$, with $g' > 0$ and $g'' > 0$. Then the person chooses e and y_R to solve the problem

$$max_{e,y_R}\, y_T - g(e) - t \cdot y_R \cdot \mathbf{1}[y_R > \bar{y}] - C(ty_T \cdot \mathbf{1}[y_T > \bar{y}] - ty_R \cdot \mathbf{1}[y_R > \bar{y}]). \tag{6}$$

The FOC w.r.t. y_R would yield a contingent solution of y_R for each possible y_T, which is entirely the same as the three-segment pattern in our model

$$y_R = \begin{cases} y_T & if\, y_T \leq \bar{y}, \\ \bar{y} & if\, y_T \in (\bar{y}, y^*)\ , \\ y_T - \frac{C'^{-1}(1)}{t} & if\, y_T > y^*. \end{cases}$$

This shows that even in a setting where the true tax base is chosen by the decision-maker and may be affected by the tax, the relation between the reported tax base and the true tax base can be studied independently. Then plugging the optimal choice of y_R as a function of y_T into 6, the utility of each case would be

$$U = \begin{cases} y_T - g(e) & if\, y_T \leq \bar{y}, \\ y_T - g(e) - C(ty_T) & if\, y_T \in (\bar{y}, y^*)\ , \\ y_T - g(e) - ty_T + C'^{-1}(1) - C(C'^{-1}(1)) & if\, y_T > y^*. \end{cases}$$

A person with endowed ability a then chooses the optimal e to maximize utility.

The solution implies bunching of y_T at $\bar{y}$. To see this, suppose a person with ability a chooses an effort level e so that $y_T \to \bar{y}^+$, i.e. approaching $\bar{y}$ from above. This yields a utility level $y_T - g(e) - C(ty_T)$. But the person can slightly lower e to ensure $y_T \leq \bar{y}$, which would yield a utility level

$y_T - g(e)$, strictly larger than $y_T - g(e) - C(ty_T)$. As a result, the tax notch would cause bunching of y_T at $\bar{y}$ and a hole for a range above $\bar{y}$ in the distribution of y_T, same as predicted in Kleven and Waseem (2013).

E Contrast with the Allingham-Sandmo Model

The standard tax evasion model, the AS model, named after Allingham and Sandmo (1972), is built on the maximization of expected utility with two possible states: being audited and not being audited. Subsequent papers that study tax evasion mostly follow the AS model to build the conceptual framework. In the following, we first show that the AS model does not naturally predict the empirical misreporting pattern documented in this paper. Then we discuss the key difference between our model and the AS model.

AS model does not naturally predict the three-segment reporting pattern. Under the standard tax evasion model, a person facing a tax notch solves the following utility maximization problem

$$max_{P_R}\,(1-p)U(P_T - tP_R \cdot \mathbf{1}[P_R > \overline{P}]) + pU(P_T - tP_R \cdot \mathbf{1}[P_R > \overline{P}] - F(tP_T \cdot \mathbf{1}[P_T > \overline{P}] - tP_R \cdot \mathbf{1}[P_R > \overline{P}])),$$

where p is the probability of being audited.[44] $F \equiv 1+\theta$, where $\theta > 0$ denotes fines on tax evasion. Here P_T and P_R denote the true and reported tax base (e.g., income or transaction price). There is a tax notch at $\overline{P}$, above which an average tax rate t applies and below which no tax is due.[45]

For $P_T < \overline{P}$, since no tax is due, there is no need to misreport. We have $P_R = P_T$.

For $P_T > \overline{P}$, the solution is either $P_R = \overline{P}$ or $P_R > \overline{P}$. When $P_R = \overline{P}$, the total utility is $U_{Notch} = (1-p)U(P_T) + pU(P_T - FtP_T)$.

Note that when $P_R > \overline{P}$, the FOC is $(1-p)U'(P_T - tP_R) = p(F-1)U'(P_T - tP_R - F(tP_T - tP_R))$. Without an explicit utility function, there is no closed-form interior solution, without which we cannot compare U_{Notch} and $U_{Interior}$ and thus cannot predict the three-segment reporting pattern.

Therefore, we adopt a simple functional form $U(x) = lnx$ to see if it yields a three-segment reporting pattern. This functional form has reasonable property $U' > 0$ and $U'' < 0$ and would yield a closed-form interior solution

$$P_R = \frac{F(1-p)}{F-1}P_T - \frac{1-pF}{t(F-1)}P_T.$$

Plugging it back to the expected utility, we have $U_{Interior} = ln(P_T) + ln(1-t) + (1-p)ln\frac{F(1-p)}{F-1} + pln(pF)$.

Comparing it with $U_{Notch} = ln(P_T) + pln(1-Ft)$, obviously, only corner solutions exist. If

[44]Here we assume a constant probability of auditing p, as adopted in Allingham and Sandmo (1972). If p is not constant but depends on the amount of tax evasion (Slemrod (2019)), the solution would become more complicated. But again, the three-segment reporting pattern, in general, cannot be predicted.

[45]Here we ignore the initial purchasing price P_0 in the optimization problem to simplify the analysis. Considering it only complicates the algebra but does not affect the result of the analysis.

$ln(1-t)+(1-p)ln\frac{F(1-p)}{F-1}+pln(pF)>pln(1-Ft)$, we have $P_R=\overline{P}$ for all $P_T>\overline{P}$. If $ln(1-t)+(1-p)ln\frac{F(1-p)}{F-1}+pln(pF)<pln(1-Ft)$, then $P_R=\frac{F(1-p)}{F-1}P_T-\frac{1-pF}{t(F-1)}P_T$, i.e. P_R and P_T have a proportional relation for all $P_T>\overline{P}$. This is in sharp contrast with the second and third segments in the empirical pattern of misreporting.

Is it possible that by introducing idiosyncratic misreporting costs the AS model can also predict the second and third segments of the misreporting pattern? No. In that case, the AS model only predicts that for houses with $P_T>\overline{P}$, a portion will choose $P_R=\overline{P}$ and the remaining portion will underreport a fraction of P_T. But the AS model does not predict that some threshold P^* will separate the two portions like in our model, and thus is inconsistent with the empirical pattern.

Key difference between the AS model and our model. The key difference between our model and the AS model is not that our model is based on cost minimization while the AS model is based on utility maximization.[46] In fact, the three-segment reporting pattern can be derived from the following utility maximization

$$max_{P_R} U\{P_T-t\cdot P_R\cdot\mathbf{1}[P_R>\overline{P}]-C(tP_T\cdot\mathbf{1}[P_T>\overline{P}]-tP_R\cdot\mathbf{1}[P_R>\overline{P}])\},$$

where $U'>0$.

Then what is the key difference? It is the way the misreporting cost is introduced in the model and how it interacts with the tax liability. In our model, misreporting cost enters the model as an additively separable term like tax liability. This is reasonable because both the misreporting cost and tax liability affect utility by affecting people's disposable income. By contrast, in the AS model, since tax liability enters into both scenarios (being audited or unaudited), under usual concave utility function, it will affect the total expected utility very differently from the misreporting cost, which is implicitly embedded in the probability of auditing and the fines.

[46] The difference between the two modeling approaches is also briefly mentioned in footnote 5 of Slemrod (2019).

F Contrast with the Kleven et al. (2011) Model

Kleven et al. (2011) considered a version of the AS model with risk-neutral taxpayers and an endogenous audit probability that depends on reported tax base. Using our notations above, a person in the Kleven et al. (2011) model facing a tax notch solves the following problem

$$max_{P_R}(1-p)(P_T - tP_R \cdot \mathbf{1}[P_R > \overline{P}]) + p(P_T - tP_R \cdot \mathbf{1}[P_R > \overline{P}] - F(tP_T \cdot \mathbf{1}[P_T > \overline{P}] - tP_R \cdot \mathbf{1}[P_R > \overline{P}])),$$

where $p = p(P_U)$ with $p'(P_U) > 0$. $F \equiv 1+\theta$, where $\theta > 0$ denotes fines on tax evasion.[47]

For $P_T < \overline{P}$, since no tax is due, there is no need to misreport. We have $P_R = P_T$.

For $P_T > \overline{P}$, the solution is either $P_R = \overline{P}$ or $P_R > \overline{P}$.

(1) When $P_R = \overline{P}$, the total utility is $U_{Notch} = (1-p)P_T + p(P_T - FtP_T) = (1-pFt)P_T$, where $p = p(P_T - \overline{P})$.

(2) When $P_R > \overline{P}$, the individual's problem becomes

$$max_{P_R}(1-p)(P_T - tP_R) + p(P_T - tP_R - F(tP_T - tP_R)),$$

which can also be written as

$$max_{P_R} P_T - tP_R - pFtP_U,$$

or

$$max_{P_U} tP_U + (1-t)P_T - pFtP_U.$$

The FOC yields $FP_U p'(P_U) = 1 - p(P_U)F$. Like the third segment of the predicted reporting pattern derived from our model, this also predicts a constant misreporting P_U (assuming the existence of an interior solution of the FOC equation). However, as pointed out in Kleven et al. (2011), due to the assumptions of risk neutrality and a linear penalty in evaded tax (note that the housing transaction tax in Shanghai is linear above the notch and thus this is not an assumption in our paper), the marginal tax rate t has no impact on misreporting P_U in this model. This is in contrast with our empirical evidence and our model prediction, which shows a negative relation between t and P_U. But if we assume instead that p is a function of tP_U, then it will also generate a negative relation between t and P_U like our simple model.

Note that the FOC yields a constant interior solution P_U^* independent of P_T. Then the utility

[47] Here we ignore the initial purchasing price P_0 in the optimization problem to simplify the analysis. Considering it only complicates the algebra but does not affect the result of the analysis.

corresponding to the interior solution is $U_{Interior} = tP_U^* + (1-t)P_T - p(P_U^*)FtP_U^*$, which increases linearly with P_T.

By contrast, $U_{Notch} = (1-pFt)P_T$, where $p = p(P_T - \overline{P})$, is a nonlinear function of P_T. Since $\frac{dU_{Notch}}{dP_T} = 1 - pFt - \frac{dp}{dP_T}FtP_T = 1 - Ft(p + \frac{dp}{dP_T}P_T)$ and $\frac{d^2U_{Notch}}{dP_T^2} = -Ft(2\frac{dp}{dP_T} + \frac{d^2p}{dP_T^2}P_T)$, it is clear that without further assumptions we cannot conclude on what occasions the interior solution or the notch solution would be chosen.

Discontinuous jump of audit probability. Now consider the case where the audit probability is a discontinuous function of misreporting, as considered in Kleven et al. (2011). We follow Kleven et al. (2011) to assume that p is continuously differentiable with respect to P_U. To simplify the analysis, assume that $p = 1$ if $P_U > \overline{P_U}$ and $p = 0$ if $P_U < \overline{P_U}$, where $\overline{P_U}$ is some threshold for evasion.[48] Then the interior solution is the same as above and we still have $U_{Interior} = tP_U^* + (1-t)P_T - p(P_U^*)FtP_U^*$, which increases linearly with P_T. Since P_U^* is independent of P_T, we denote $a \equiv tP_U^* - p(P_U^*)FtP_U^*$ to simplify notation. Then $U_{Interior} = a + (1-t)P_T$.

Now $U_{Notch} = (1-pFt)P_T$. Given the assumed discontinuous jump of p and note that $P_U = P_T - \overline{P}$ for the notch solution, we have $U_{Notch} = P_T$ if $P_T < \overline{P} + \overline{P_U}$ and $U_{Notch} = (1-Ft)P_T$ if $P_T > \overline{P} + \overline{P_U}$. To obtain the three-segment reporting pattern documented in our paper, we need $U_{Notch} > U_{Interior}$ for P_T below a certain threshold and $U_{Notch} < U_{Interior}$ for P_T above that threshold. Given that $1 > 1-t > 1-Ft$ (note that $F > 1$), the necessary condition for the above condition to hold is $t(1-F)(\overline{P} + \overline{P_U}) < a < t\overline{P}$. Thus, when the audit probability is a discontinuous function of misreporting, the Kleven et al. (2011) model could also generate the three-segment reporting pattern when some parameter condition is satisfied. Finally, from the setting of the maximization problem $max_{P_U} \, tP_U + (1-t)P_T - pFtP_U$, it is clear that if we consider instead a case where the penalty F is a discontinuous function of misreporting while the audit probability is constant, the conclusion is the same.

[48] Technically, we need to have p change gradually but radically from 0 to 1 around $P_U = \overline{P_U}$.

G Heterogeneity of Misreporting

G.1 Intensive Margin

Figure A8 and Figure A9 show that there is non-trivial heterogeneity among individuals choosing whether to misreport or not. It is possible that, even for houses with the same P_T, there exists some heterogeneity in their misreporting patterns. To examine the extent of misreporting for different people with houses of the same P_T, and to examine whether the predicted three-segment reporting pattern holds not only for the representative (i.e. median) ones, but also holds for other groups, we examine the reporting pattern of different percentiles of P_R for the same bin of P_T. It is worth noting that we do not restrict our sample only to observations that misreport sales prices. Instead, we include all observations, regardless whether the sales prices are misreported or not. This is different from the "intensive margin" we normally think of, where only the evading observations would be included. Overall, we observe that the three-segment reporting pattern not only exists for the median P_R for each bin of P_T but also for different percentiles of reporting.

Figure A6 overlaps the 10th percentile, median, and 90th percentile of P_R against each 10w RMB bin of P_T. First of all, it is clear that the 10th and 90th percentiles P_R differ from the median. In fact, the 90th percentile of P_R is largely the same as P_T; this simply says that for almost all levels of P_T, there are some people honestly reporting, which has been suggested in Figure A9. The 10th percentile pattern is much more interesting, which exhibits a proportional downward shifting (rather than a parallel downward shifting) of the median pattern.

When we further look at finer-divided percentiles, more heterogeneity emerges. Figure A7 uses housing transactions between the outer and inner ring as an illustration. Using the other two regions yields the same results. Panel (a) shows the distribution of P_R that is above the median for each bin of P_T and panel (b) shows the distribution of P_R that is below the median for each bin of P_T. In both panels, there is clear heterogeneity. One notable distinction is that, above the median, houses with $P_T < \overline{P}$ do not misreport, while below the median, even houses with $P_T < \overline{P}$ would underreport.

G.2 Extensive Margin

The benchmark model predicts that no one with $P_T \leq \overline{P}$ will misreport and everyone with $P_T > \overline{P}$ will underreport. Is this true in the data? Figure A8 shows that even for $P_T \leq \overline{P}$, about 20-40% transactions underreport the transaction prices. For $P_T > \overline{P}$, the underreporting fraction is close to 1 for P_T above but close enough to $\overline{P}$, then decreases as P_T rises, and finally becomes largely constant as $P_T > P^*$. This pattern implies that there are other factors impeding people from making the response as our simple model predicts. One omitted factor is the existence of other taxes. For houses with $P_T \leq \overline{P}$, even if they are not subject to the business income tax, they are still subject to a personal income tax, which is 1% of P_R, and a deed tax, which is proportional to P_R and the rate depends on the number of houses a buyer has (1-1.5% if it is the first house owned by a family, 3% if otherwise, though we do not have such information to calculate the deed tax due for each buyer). These taxes are sufficient to create an incentive for those with $P_T \leq \overline{P}$ to underreport prices. For houses with $P_T > \overline{P}$, there will always be some people not responding to the tax incentive, due to large perceived costs or simple ignorance. This is consistent with the finding that people tend to comply more than is predicted by a model with rational agents under reasonable audit probablity and fines. It is generally believed that psychological or cultural aspects of tax compliance such as social norms, tax morale, patriotism, guilt, and shame could help explain the empirical high compliance rate (e.g., Andreoni et al., 1998).

Figure A9 shows the fraction of honest reporting. It shows a complementary picture of Figure A8 but is worth showing explicitly.[49] It is especially worth noting that honest reporting is least when P_T is just above $\overline{P}$, and gradually increases and becomes stable beyond a certain point.

G.3 Possible Explanations for the Heterogeneity

In the following, we explore whether the heterogeneity of misreporting could be explained by some possible factors, including other taxes and fees, housing loans, and idiosyncratic misreporting costs. We find that while they (even taking together) cannot explain all the heterogeneous misreporting pattern, idiosyncratic misreporting costs alone can explain much of the heterogeneity.

Other taxes. While other taxes help explain the underreporting for some $P_T < \overline{P}$, they cannot explain the major heterogeneous misreporting pattern we observe. Other taxes are proportional to P_R even if P_R is less than $\overline{P}$. Denote the average tax rate for all other taxes as τ, then for houses with $P_T < \overline{P}$,the problem to solve becomes

$$min_{P_R}\, \tau \cdot P_R + C(\tau(P_T - P_R)).$$

Then for houses with $P_T < \overline{P}$, the solution is $P_R = P_T - \frac{C'^{-1}(1)}{\tau}$, which implies a constant underreporting even for $P_T < \overline{P}$. This is inconsistent with Figure A6. The empirical pattern we observe also implies that potential impacts of other taxes are dwarfed by the business income tax when analyzing the misreporting responses to the tax notch. This is reasonable since the amount of other taxes is small relative to the business income tax involved in housing transactions.

Housing loans. If people tend to buy a house using loans from a bank, the bank will decide the loan amount based on the reported price P_R. The bank sets L to be equal to a loan multiplier ρ_L

[49] Overreporting is very rare and negligible in our data.

times P_R, and ρ_L in general should not exceed 70% as required by the People's Bank of China.[50] Then the down payment paid by the buyer equals $P_T - L$. For buyers who face large down payment pressure, they may have an incentive to report a higher P_R. For loans to make a difference in accounting for the heterogeneous misreporting patterns, there must be $P_R < P_E$. Then we focus on the binding cases, i.e. $L = \rho_L \cdot P_R$. In China, given that people generally expect a much higher growth rate (above 10% per year in most of the past 20 years) for housing price than interest rate (annual nominal rate around 2%), people would prefer loans to be as high as possible. For a given wealth, a larger loan for a house implies a smaller down payment, which leaves a larger remaining wealth for buying another house or investing in other profitable projects with a return higher than the interest rate. Suppose the benefit of using loans can be represented by $g(L)$, with $g' > 0$. Then the individual's problem becomes

$$min_{P_R}\, t \cdot (P_R - P_0) \cdot \mathbf{1}[P_R > \overline{P}] + C(t(P_T - P_0) \cdot \mathbf{1}[P_T > \overline{P}] - t(P_R - P_0) \cdot \mathbf{1}[P_R > \overline{P}]) - g(L).$$

Let's focus on the houses with P_T well below $\overline{P}$ so that they are unlikely to choose P_R above $\overline{P}$. Without considering loans, there will be no misreporting. Considering loans will give them an incentive to overreport. However, this is not observed in the data. Thus, housing loans are unlikely to be the driving factor for the observed misreporting pattern.

Idiosyncratic misreporting costs. Different people may face different misreporting costs. This is particularly true as the relevant cost is the perceived cost. Even facing the same objective cost, different people might perceive it differently, let alone the potential diverse objective misreporting costs, due to the diverse knowledge and available help from lawyers and brokers. Suppose the cost function now takes the form of $\lambda \cdot C(t(P_T - P_0) \cdot \mathbf{1}[P_T > \overline{P}] - t(P_R - P_0) \cdot \mathbf{1}[P_R > \overline{P}])$, where $\lambda > 0$ and may differ across people.[51] λ could represent various factors that affect misreporting costs, such as the perceived probability to be caught for evasion, or the moral cost of underreporting. Then we would obtain the following reporting pattern:

$$P_R = \begin{cases} P_T & if\, P_T \leq \overline{P}; \\ \overline{P} & if\, P_T \in (\overline{P}, P^*)\,; \\ P_T - \frac{C'^{-1}(\frac{1}{\lambda})}{t} & if\, P_T > P^*. \end{cases}$$

[50] ρ_L may depend on estimated wealth, income, and previous credit history of buyers by banks. But we do not have such information.

[51] If we consider an alternative form of misreporting cost $C(\lambda \cdot t(P_T - P_R))$ rather than $\lambda \cdot C(t(P_T - P_R))$, it is easy to show that the same results hold.

P^* is the solution to $C_{Interior} = C_{Notch}$, where $C_{Notch} = C(tP_T - tP_0)$ and $C_{Interior} = tP_T - C'^{-1}(\frac{1}{\lambda}) + \lambda \cdot C(C'^{-1}(\frac{1}{\lambda})) - tP_0$. How would λ affect the above reporting pattern? Since $C'' > 0$, $\frac{C'^{-1}(\frac{1}{\lambda})}{t}$ would decrease with λ. This is intuitive, as a higher cost would make people underreport less. How would P^* depend on λ? Obviously, C_{Notch} does not depend on λ. Using envelop theorem, we know $\frac{dC_{Interior}}{d\lambda} = C(C'^{-1}(\frac{1}{\lambda})) > 0$. Thus, from Figure 6, we know that P^* decreases with λ.

Figure A10 illustrates the impact of an increasing λ on the misreporting pattern. This can explain much of the heterogeneous reporting patterns as shown in Figure A6. In particular, it predicts that for people with a higher λ, e.g., those for whom the possibility of saving taxes through underreporting the sales price is less salient at the time of sale or those who have a higher moral cost of misreporting, they would underreport less than the median. It also predicts that, for people with $P_T < \overline{P}$, none of them would misreport. In addition, it predicts that if only λ is large enough, people would find that the optimal reporting is just honest reporting. Thus, the idiosyncractic misreporting cost λ not only help explain the "intensive margin" heterogeneity of misreporting as discussed above, but also help explain the lack of misreporting for some observations with $P_T > \overline{P}$. These predictions are all consistent with the misreporting pattern in Figure A7 panel (a), though a heterogeneous λ per se still cannot explain the misreporting pattern in Figure A7 panel (b). It cannot explain why people with a P_T below certain inflation of $\overline{P}$ would underreport a fraction of their true income.

G.4 Misperception as a Possible Explanation

All these heterogeneous misreporting patterns can be reconciled by simply introducing misperception. Suppose people could perceive true price with some deviation. Denote the perceived price as $P_T^{Perceived} = \delta P_T$ with $\delta > 0$. The heterogeneity of δ would then reconcile all these heterogeneous misreporting patterns. For the representative people who do not misperceive the true price, we have $\delta = 1$. Their misreporting is described by the three-segment pattern as shown in Figure 7. For people who underperceive the true price (e.g., they misperceive the down payment as the true price), i.e. $\delta \in (0,1)$, they choose P_R as if their true price is $\delta P_T < P_T$; thus, their reporting pattern is a proportional downward shifting of the median people, as shown in panel (b) of Figure A7. Likewise, for people who overperceive the true price (e.g., they misperceive the after-tax price as the true price), i.e. $\delta > 1$, they choose P_R as if their true price is $\delta P_T > P_T$; thus, their reporting pattern is a proportional upward shifting of the median people, as shown in panel (a) of Figure A7.

In reality, factors considered above could explain variation of the misreporting pattern. Yet since the misperception of true price alone is sufficient to reconcile the most important heterogeneity observed here, it is worth special attention. Instead of regarding it as a final explanation, we maintain that it should be more appropriately regarded as a descriptive explanation or an intermediate step to a more fundamental explanation. Any successful explanation should at least be able to reconcile it. But how to interpret the misperception? It is certainly not because people do not know the exact value of P_T. Instead, it is possibly because choosing the optimal P_R based on a given P_T requires weighing the tax saving against the misreporting cost in a precise way. Since the optimal solution is not simple and obvious, it is not surprising that people would make a decision that deviates from the optimal choice, which makes them behave as if they misperceived the true price, like making optimization errors. After all, it is well known that people may make mistakes even in some simplest decision making situations. Since people are not thinking machines, it is quite reasonable that some people overperceive while some underperceive the true price when facing complicated decision making.[52]

[52] A growing literature examines various misperceptions under tax and related settings. A widely documented fact is the misperception between marginal tax (price) rate and average tax (price) rate (e.g., de Bartolome (1995), Liebman and Zeckhauser (2004), Ito (2014), Rees-Jones and Taubinsky (2020)). Feldman et al. (2016) find evidence that people misperceive the tax liability change due to a lump-sum Child Tax Credit change as an increase in their marginal tax rate. Therefore, it is not surprising that some people behave as if they misperceive the true price. Importantly, the misperception is centered around a correct perception of the true price, which makes the misperceptions like standard errors around the mean value (correct perception).

H Some Discussion about the Bunching Approach

When a tax notch or a kink exists, the bunching approach is often used to utilize the distribution of the tax base distorted by the tax change, then compared to the counterfactual undistorted distribution of the tax base, to estimate parameters of interest (Saez, 2010; Chetty et al., 2011; Kleven, 2016). But in this paper, we do not use the bunching approach to estimate the response of underreporting to tax. As pointed out in Einav et al. (2017), the observed bunching pattern of P_R can be consistent with different models. Under the setting of a housing transaction tax notch studied in this paper, either our model or any other utility maximization model (e.g., the traditional Allingham-Sandmo model) can generate the observed bunching pattern. However, as we have discussed earlier, the three-segment pattern is consistent with our model but in general cannot be generated by the AS model. Since the bunching pattern is only a symptom of the three-segment reporting pattern in our setting, our analysis should focus on the three-segment pattern rather than the bunching pattern.

Some readers suggest comparing the "standard" bunching estimates only using information of P_R with the "true" estimates using the information of both P_R and P_T, which might assess and help relax assumptions behind the bunching estimates. However, obtaining a bunching estimate using the information of P_R starts from choosing a model that illustrates how P_R would deviate from P_T due to the tax notch; yet there does not exist a unique model for us to choose. Thus, any difference between the "standard" bunching estimate and the "true" estimate only reflects the deviation of the chosen model from the true mechanism underlying people's misreporting response. Therefore, such a comparison is unlikely to generate general guidance on how the assumptions of the bunching approach can be relaxed to obtain a reliable distribution of the true tax base P_T when only the information of the reported tax base P_R is available.

To sum up, simply using the bunching pattern to estimate the elasticity of evasion with respect to the tax rate does not fully utilize the information we have, and the estimate would critically depend on assumptions of the model. By contrast, our regression analysis has accounted for the nonlinear relation between P_T and P_U as implied by the three-segment pattern. Thus, while the bunching approach might be the best available approach in a setting when only the information of P_R is available, it is not an ideal choice when we have information of both P_R and P_T for each transaction.

I Impact of the Tax Rate on Trade Volumes

Here we exploit the small decrease in the tax rate caused by the tax reform in May 2016 to examine the impact of the tax rate on trade volumes. Like in section 6.2, we use those with P_T in the first segment as the control group and those with P_T in the third segment as the treatment group.

The reduced-form specification is

$$Q_{gm} = \alpha \cdot Treat_g + \beta_{ITT} \cdot Treat_g \cdot Post + \gamma \cdot Post + \varepsilon_{gm},$$

where Q_{gm} denotes number of transactions (unit is thousand) of group g in month m. $Treat_g$ equals 1 if group g is the treatment group. *Post* is a time indicator for observations after the reform. β_{ITT} measures the intent-to-treat (ITT) effect. Column 1 of Table A1 shows that the tax reform caused trade volumes to increase by 1.31K.

To examine whether the treatment and control groups share parallel pre-trends, we conduct an event-study analysis using the specification below

$$Q_{gm} = \alpha \cdot Treat_g + \sum_{s \neq 0} \beta_s \cdot 1[m = m_0 + s] \cdot Treat_g + \gamma \cdot Post + \varepsilon_{gm}, \tag{7}$$

where m_0 denotes the month just before the reform and β_s denotes the coefficient of the event study. Figure A11 shows that the treatment and control groups share parallel pre-trends. The treatment group experiences a gradual increase of trade volumes relative to the control group after the reform.

Based on the above evidence, we estimate the impact of the tax rate on trade volumes, using the following specification

$$Q_{gm} = \alpha \cdot Treat_g + \beta_{2SLS} \cdot TaxRate_{gm} + \gamma \cdot Post + \varepsilon_{gm},$$

where $TaxRate_{gm}$ denotes the average tax rate (in percentage point) of group g in month m. The regression is estimated using a 2SLS approach, with $TaxRate_{gm}$ instrumented by $Treat_g \cdot Post$. Columns 2 and 3 of Table A1 show the first-stage and 2SLS estimates. Our estimates suggest that a one percentage-point (pp.) increase in the tax rate on average is associated with a drop of 2.56K transactions.

J Marginal vs Aggregate Responses of Misreporting to Tax

The *marginal* response of misreporting to tax and the *aggregate* response of misreporting to tax together imply a nonlinear relation between the tax rate and misreporting. On the one hand, the cross-sectional evidence in Figure 2 suggests a positive relationship between the tax rate and *aggregate* underreporting. This is not surprising, since if there is no tax (i.e. tax rate equals zero), there would be no underreporting at all. So aggregately, a positive relationship between the tax rate and underreporting naturally holds. On the other hand, by exploiting a *marginal* change in the tax rate, our DD estimates imply a negative relationship between the tax rate and underreporting when the tax rate experiences a small change around 5.6 pp.[53] While our DD estimate captures the impact on underreporting of a one-shot small change (from 5.6 pp to 5.33 pp) in the tax rate, the cross-sectional bunching evidence in Figure 2 captures the aggregate response of underreporting to a much bigger change in the tax rate (from 0 to 5.6 pp). The negative elasticity implied by the DD estimates and the positive elasticity implied by the bunching figure implies an even larger positive elasticity when the tax rate is closer to zero. Correspondingly, our model weakly forecasts no misreporting when the tax rate $t = 0$ (when the tax rate equals zero, there is no gains from underreporting, but underreporting may still involve some costs), a large jump in misreporting when $t = \varepsilon$ where $\varepsilon \to 0^+$, and then a negative impact on misreporting for all further increases in t. Thus, this nonlinear relationship between the tax rate and misreporting is broadly consistent with our model.

[53] While the cross-sectional bunching evidence in Figure 2 cannot provide a comparable estimate with our DD estimates, it is still possible to use the dynamic change of bunching around the tax reform to obtain a comparable estimate. As our model shows, bunching would capture both constrained response (the second segment) and unconstrained response (the third segment) to the tax notch. Since it is desirable to focus on the unconstrained response, we focus on the change of the third segment after the tax reform. Figure A5 shows the empirical pattern of tax evasion against each P_T bin around the tax reform. Overall, as the tax rate decreases from 5.6 pp to 5.33 pp, the turning point P^* shifts rightwards by around 20 wRMB and the unconstrained tax evasion E in the third segment (while we use the median figure here, using the mean figure shows similar changes) increases by about 1 wRMB (as before, the change for houses within the inner ring is less salient). Note that $dE = d(tP_U) = dt \cdot P_U + t \cdot dP_U$, we have $dP_U = \frac{dE - dt \cdot P_U}{t} \simeq \frac{1-(5.33-5.6)\times 97.97}{(5.33+5.6)/2} = 5.02$. This rough calculation implies a semi-elasticity of $\frac{dlnP_U}{dt} = \frac{dP_U}{dt} \cdot \frac{1}{P_U} = \frac{5.02}{5.33-5.6} \times \frac{1}{97.97} = -0.19$, which is broadly in line with our DD estimate.

K Incidence Issue and Policy Implications

Incidence issue. Since the incidence issue is beyond the focus of our study of misreporting, we only make a brief and tentative discussion here. Empirically, we do not find any evidence that P_T is affected by tax in our setting. In particular, there is no bunching evidence of P_T at the notch, which contrasts with the prediction of a model where the true tax base is determined by costly efforts rather than taken as given.[54] This complete passthrough result (tax completely passes onto buyers), while quite extreme, has been documented in the recent literature that studies the US tariff incidence during the 2018 trade war.[55] It implies either completely inelastic demand or completely elastic supply. But if it is due to completely inelastic demand, then the tax should have no impact on trade volumes, which contradicts our finding in Online Appendix I that a drop in the tax rate increases trade volumes. Thus, a completely elastic supply is more likely in our case, which reflects that the bargaining power heavily lies on the seller side in Shanghai's second-hand housing market during our study period. The excessive bargaining power of sellers may be driven by the regulated supply of new houses plus a limited supply of second-hand houses combined with excessive demand for houses in Shanghai, a city with top education resources in China and where housing prices have experienced very high growth rates during the past decades.

Policy implications. Here we discuss the policy implications on a potential increase in the tax rate based on our findings. First, the mechanical impact of a tax rate increase would create equity gains because the tax applies only to more expensive houses which are traded by wealthier families.

Second, our empirical findings imply that a marginal tax rate increase could reduce underreporting and tax evasion. This finding implies both equity gains and efficiency gains. From the equity perspective, since underreporting in our setting is made by those trading expensive houses, reducing underreporting would bring about further equity gains. From the efficiency perspective, since underreporting involves resource costs of income hiding and enforcement, less underreporting implies efficiency gains.

Third, if a tax rate change does not affect P_T (based on the complete passthrough result discussed above) but affects the number of sales of the second-hand houses Q (our estimates in Online Appendix I imply that suggest that a one-percentage-point increase in the tax rate on average is as-

[54] Besides, we do not have an alternative exogenous benchmark to help judge whether P_T is indeed affected by the tax. Besley et al. (2014) use the independent surveyor's valuation of houses as an exogenous benchmark to assign treatment and control groups. They claim that by using the surveyor's valuation, they can avoid selecting on their endogenous variable, the transaction price. However, Figure 5 of Besley et al. (2014) shows that the distribution of surveyor's valuation highly coincides with that of transaction prices and that there is clear bunching of surveyor's valuation at the tax notch, which implies that the surveyor's valuation is probably not exogenous. Overall, we feel that a convincing exogenous benchmark is very hard to find.

[55] Papers that study the 2018 trade war (Amiti et al., 2019; Fajgelbaum et al., 2020; Cavallo et al., 2021; Jiao et al., 2020) unanimously find that the increased US tariff entirely passed onto the demand side.

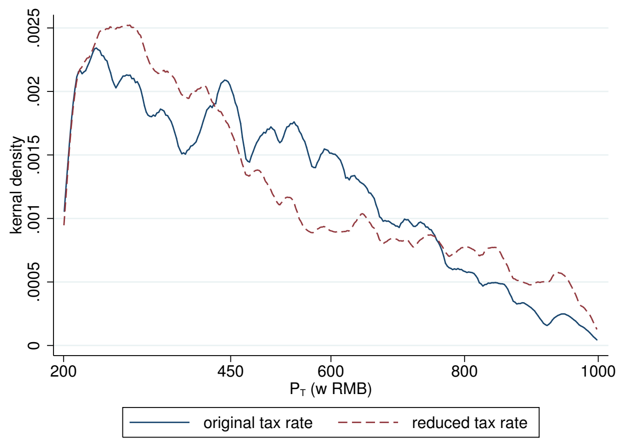
sociated with a drop of 2.56K transactions), then a tax rate increase would create efficiency losses, which approximate $\frac{1}{2}tP_T\triangle Q$ if we assume that the demand curve has a constant slope and ignore responses in other margins. But note that renting would plausibly increase as a response to decreased sales of houses, then the efficiency losses caused by a tax rate increase would be overestimated if we only focus on the responses in the second-hand housing sales.

Therefore, our findings imply that while a marginal increase in the tax rate could create efficiency losses by reducing transactions of second-hand houses, it would also reduce underreporting and bring about efficiency gains and additional equity gains to that caused by its mechanical impact. Compared to the case where a tax rate has a positive relationship with underreporting, our finding points to additional equity and efficiency gains (as opposed to additional equity and efficiency losses) created by a tax rate increase. A complete welfare analysis of a tax change depends not only on its impact on tax evasion and its real impacts (e.g., on true prices and trading volumes), but also depends on the resource costs of tax evasion and social weights put on families with different housing values. With relevant parameters estimated or assumed, a welfare analysis could be done (e.g., Gorodnichenko et al., 2009). Yet this is well beyond the scope of this paper, which focuses exclusively on misreporting.

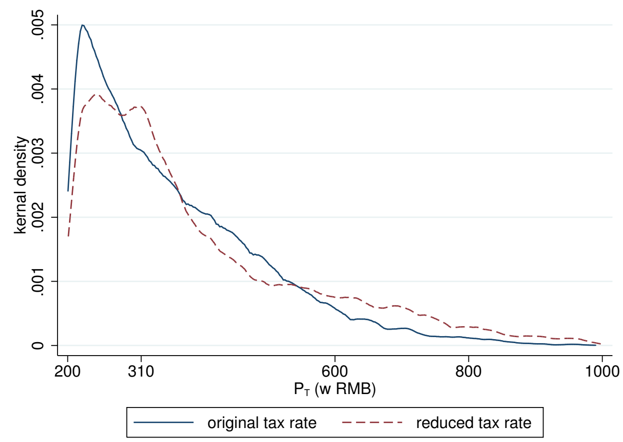
L Appendix Tables and Figures

Figure A1: Distributions of True Prices Around a Tax Rate Change

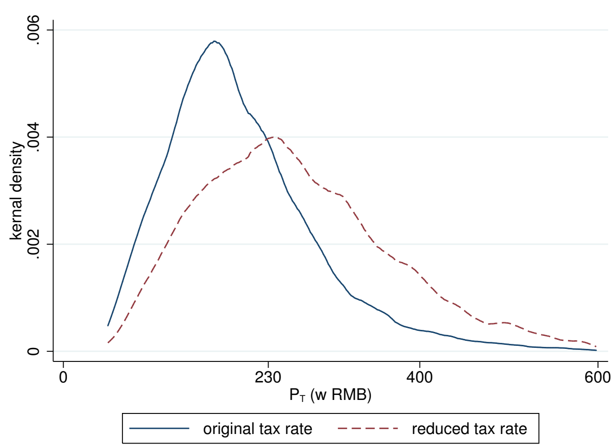
(a) Within the Inner Ring



(b) Between Outer and Inner Ring



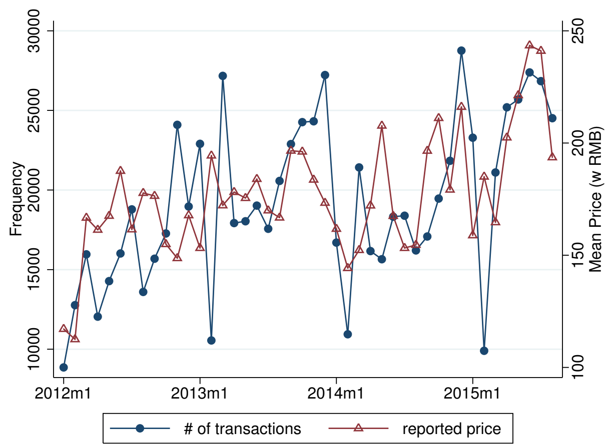
(c) Outside the Outer Ring



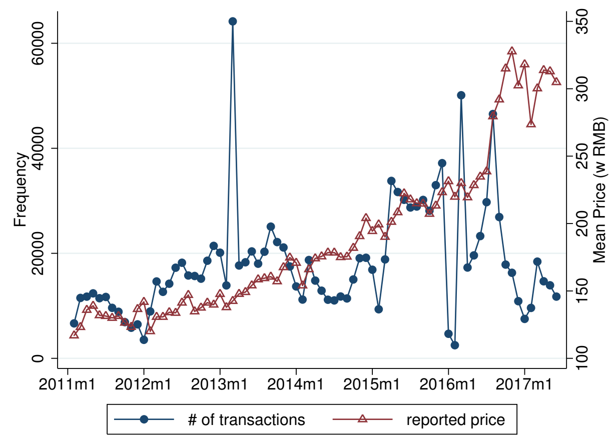
Notes: The figures show the Epanechnikov kernel density curves fitted with a bin size equal to 10w RMB. To make the distributions more comparable, we focus on same months for the pre-reform period (May 2015-April 2016) and the post-reform period (May 2016-April 2017).

Figure A2: Number of Housing Transactions and Prices in Shanghai

(a) New Houses (HMB): Jan 2012-Aug 2015



(b) Second-Hand Houses (HMB): Feb 2011-June 2017



(c) Second-Hand Houses from a Real Estate Brokerage Company: April 2015-June 2017

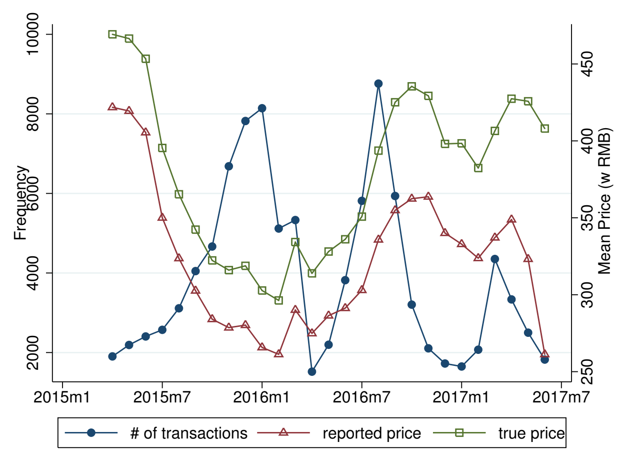
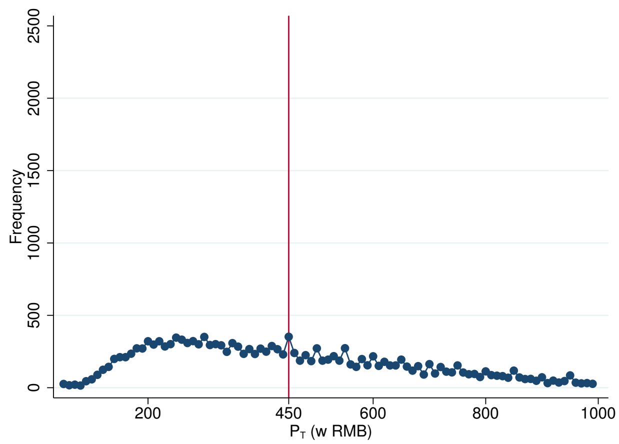
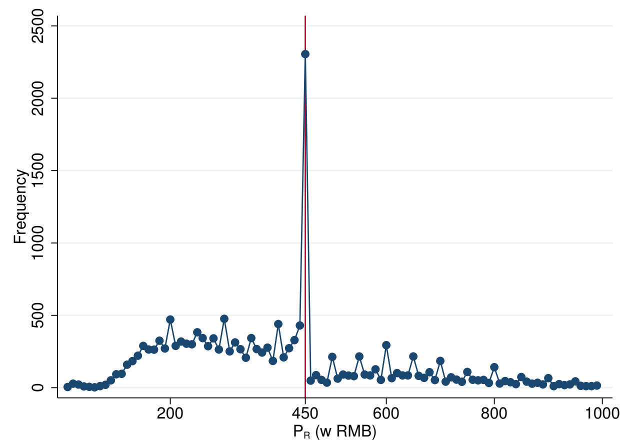


Figure A3: Distribution of True Prices and Distribution of Reported Prices

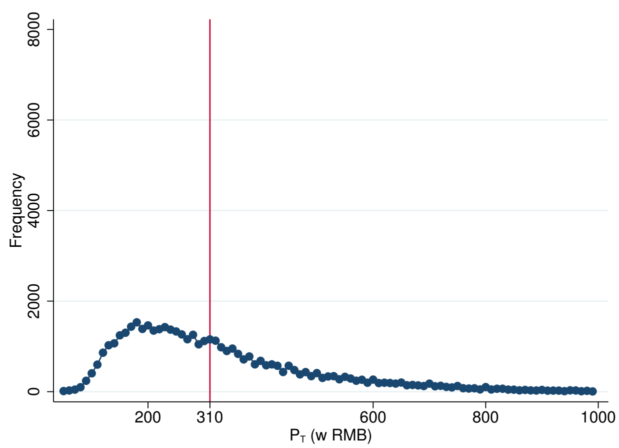
(a) True Prices: Within the Inner Ring



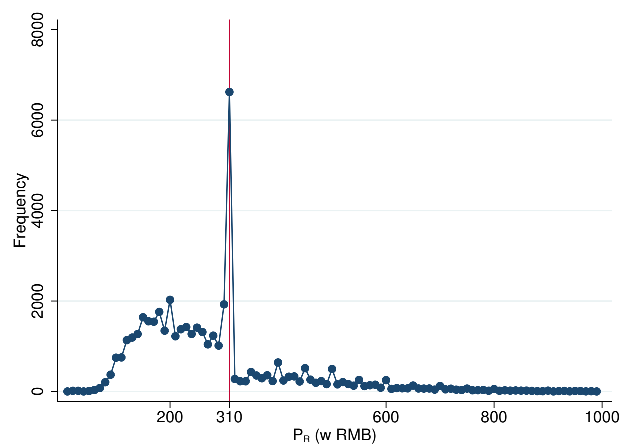
(b) Reported Prices: Within the Inner Ring



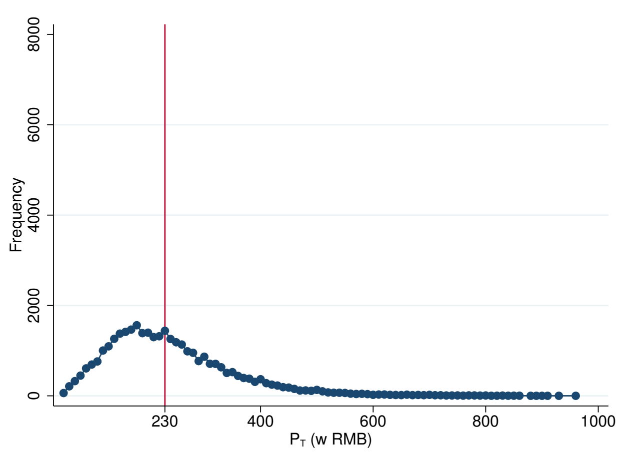
(c) True Prices: Between Outer and Inner Ring



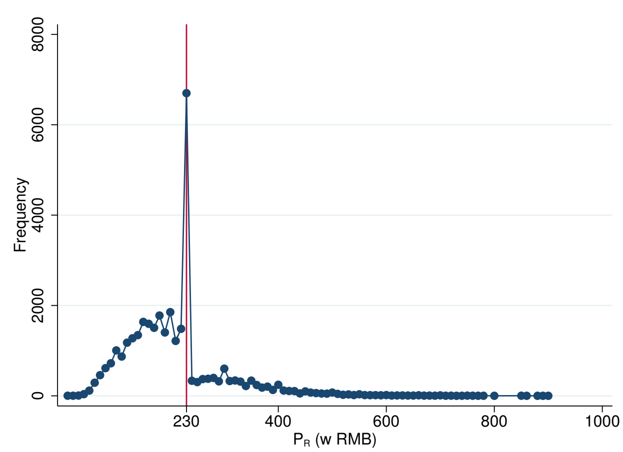
(d) Reported Prices: Between Outer and Inner Ring



(e) True Prices: Outside the Outer Ring



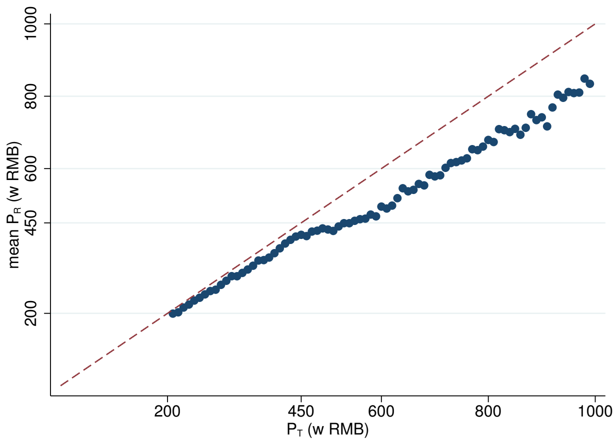
(f) Reported Prices: Outside the Outer Ring



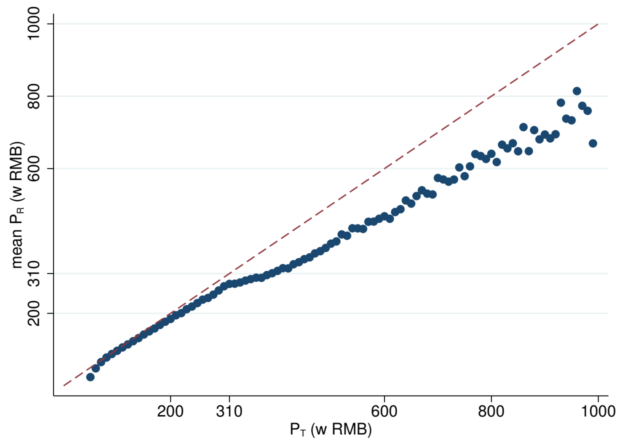
Notes: bin width=10w RMB. The vertical line indicates the tax notch. This figure shows that the distribution of true prices is smooth, while the distribution of reported price has a clear bunching at the tax notch for each region. To facilitate comparison between the distributions of reported prices and true prices, we use the same y-axis for the same region.

Figure A4: Empirical Pattern of Mean Reported Housing Prices Against True Prices

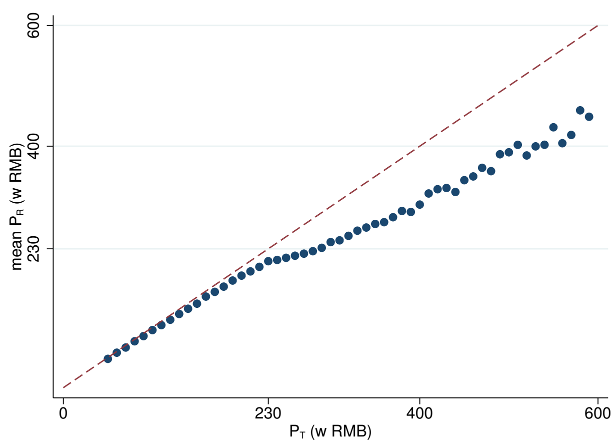
(a) Within the Inner Ring



(b) Between Outer and Inner Ring



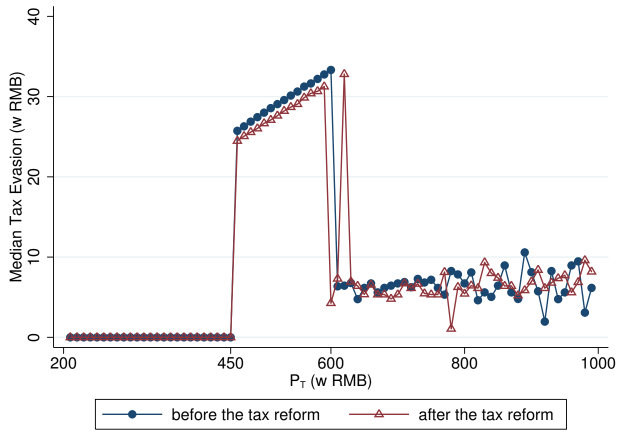
(c) Outside the Outer Ring



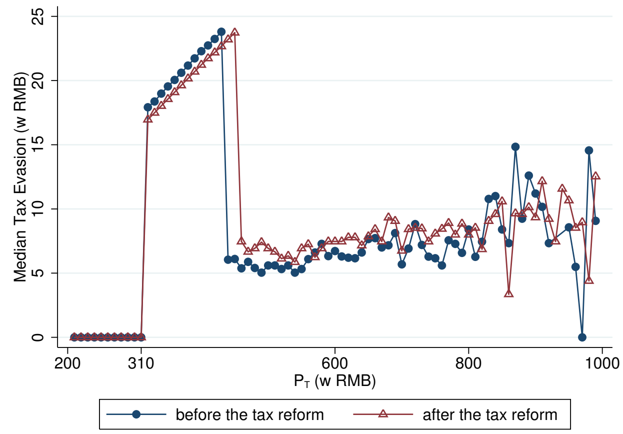
Notes: bin width=10w RMB. The dashed line is a 45-degree line. This figure shows the mean reported price for each bin of true prices. Because of the heterogeneity of misreporting, it shows a smoother reporting pattern than using the median reported price, as in Figure 5.

Figure A5: Empirical Pattern of Tax Evasion Against True Prices Around the Tax Reform

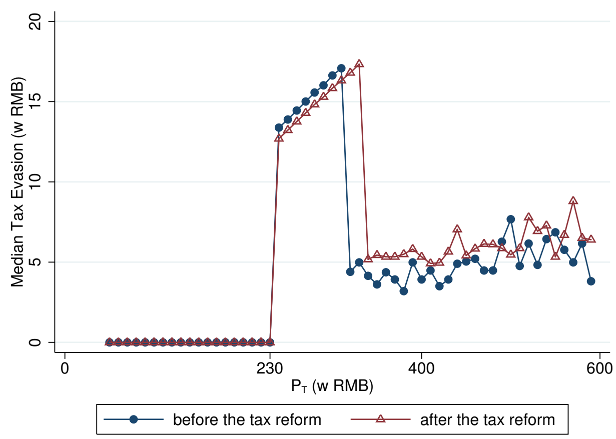
(a) Within the Inner Ring



(b) Between Outer and Inner Ring



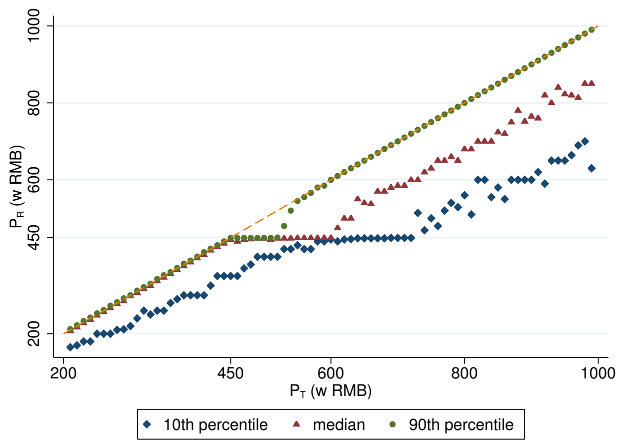
(c) Outside the Outer Ring



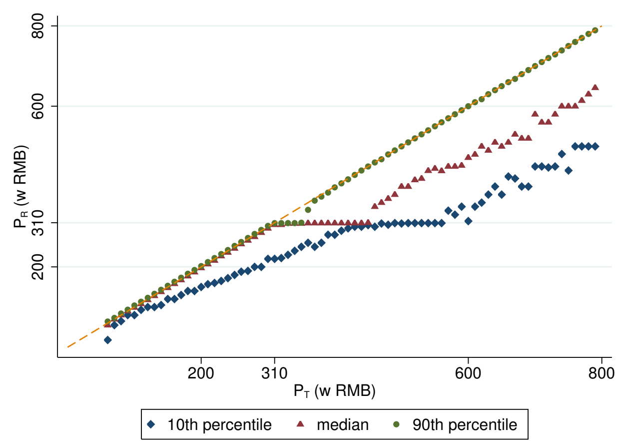
Notes: bin width=10w RMB. For each transaction, tax evasion is calculated as $E \equiv tP_T \cdot \mathbf{1}[P_T > \overline{P}] - tP_R \cdot \mathbf{1}[P_R > \overline{P}]$. This figure shows the shift of the empirical pattern of tax evasion against true prices after the VAT-for-BIT reform reduces the tax rate for transactions with P_R above the tax notch from 5.6% to 5.33% after May 1, 2016.

Figure A6: Heterogeneity of Misreporting Patterns

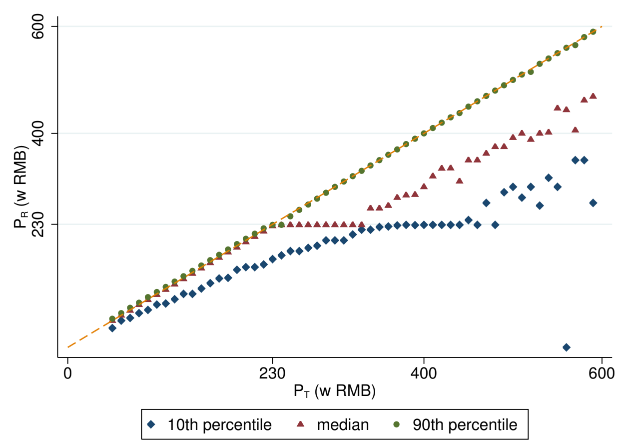
(a) Within the Inner Ring



(b) Between Outer and Inner Ring



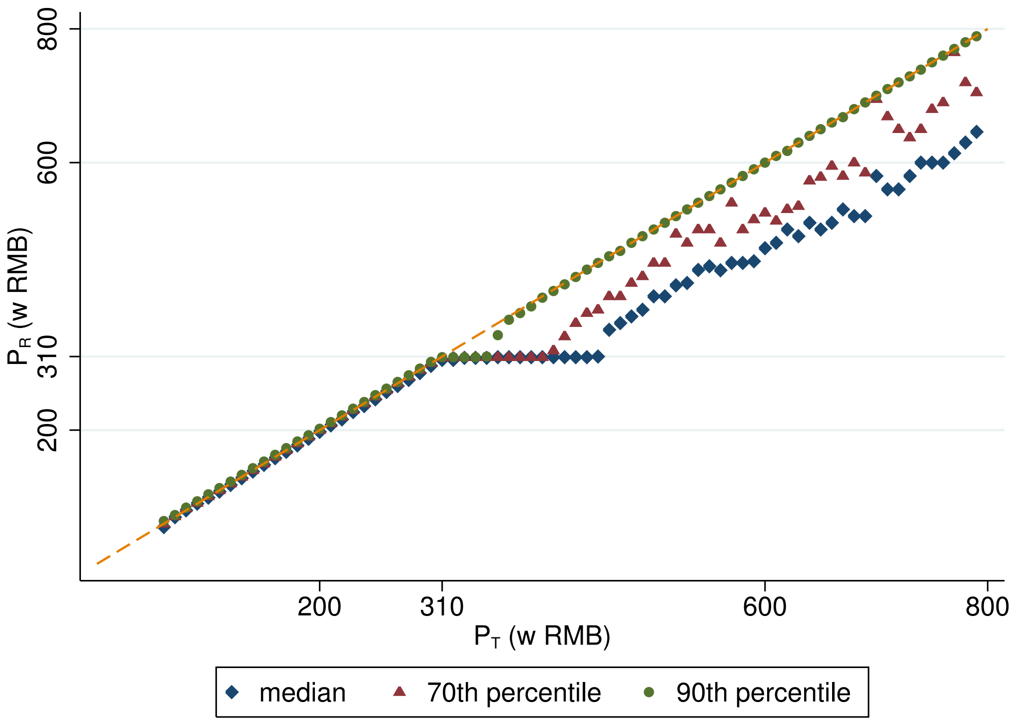
(c) Outside the Outer Ring



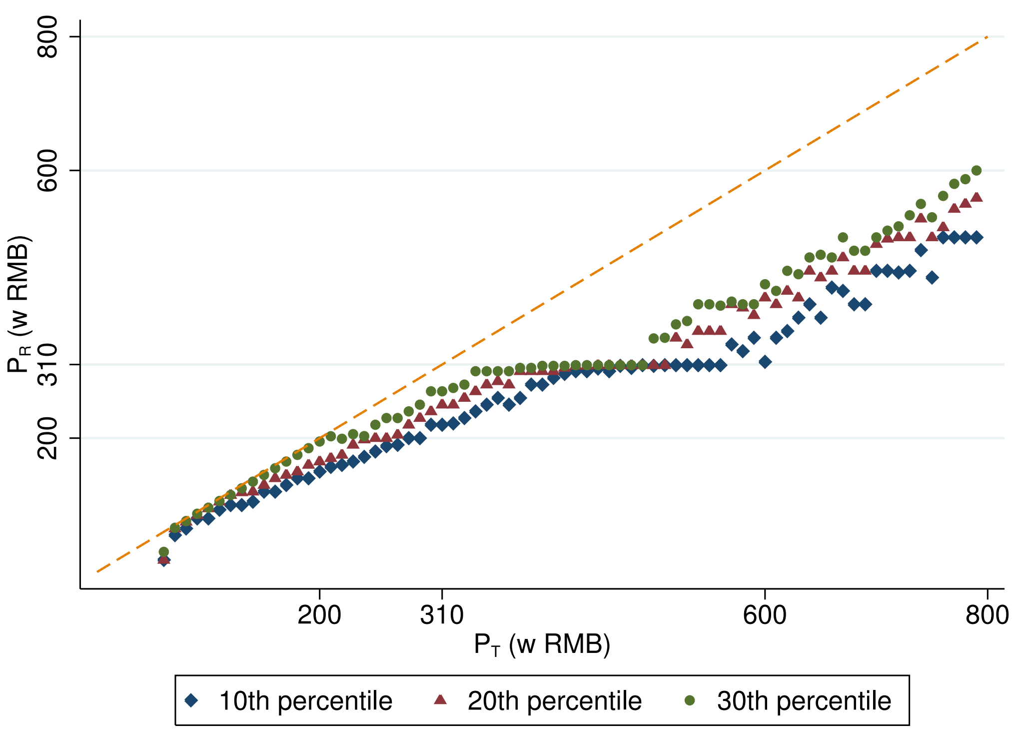
Notes: bin width=10w RMB. The dashed line is a 45-degree line.

Figure A7: Heterogeneity of Misreporting Patterns: Between Outer and Inner Ring

(a) Above Median



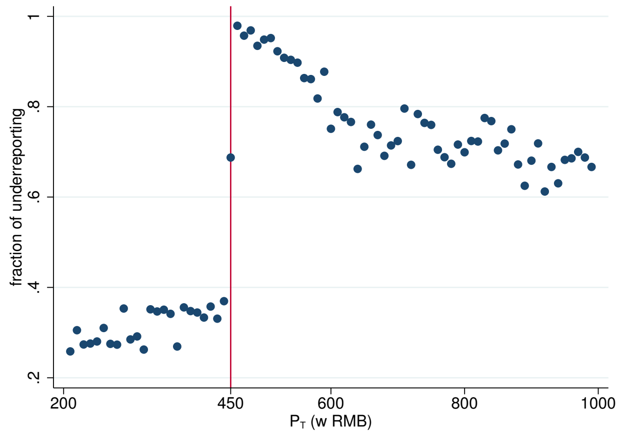
(b) Below Median



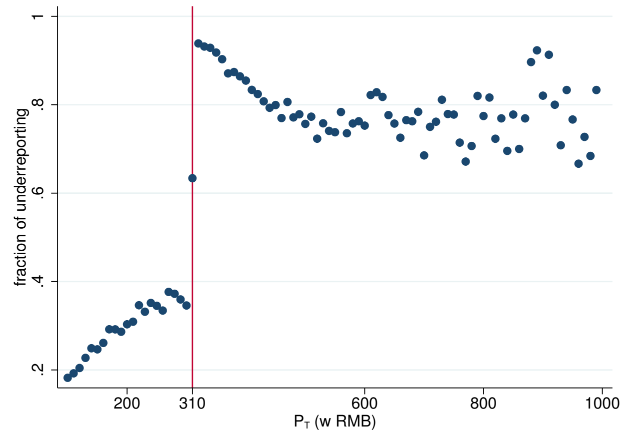
Notes: bin width=10w RMB. The dashed line is a 45-degree line.

Figure A8: Fraction of Price Underreporting

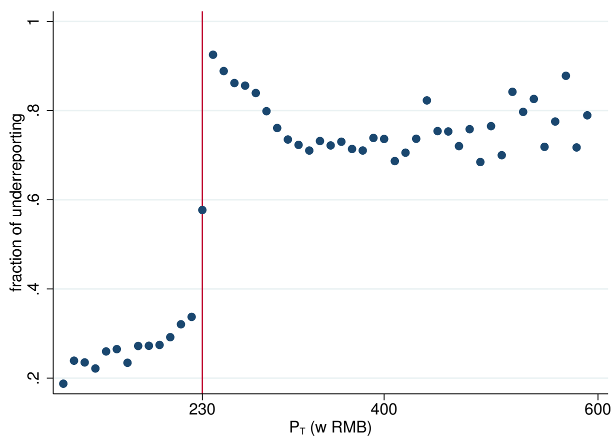
(a) Within the Inner Ring



(b) Between Outer and Inner Ring



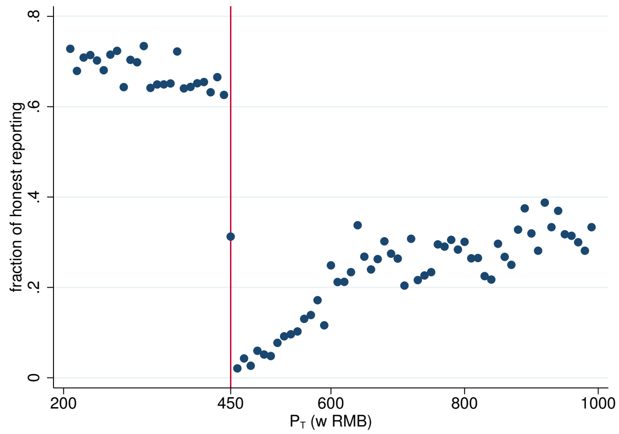
(c) Outside the Outer Ring



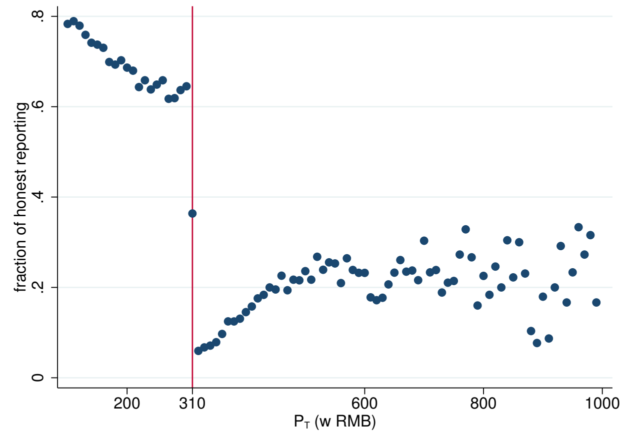
Notes: bin width=10w RMB. The solid line denotes the notch. Each dot describes the fraction of houses whose prices are underreported among all transactions in a bin.

Figure A9: Fraction of Honest Reporting

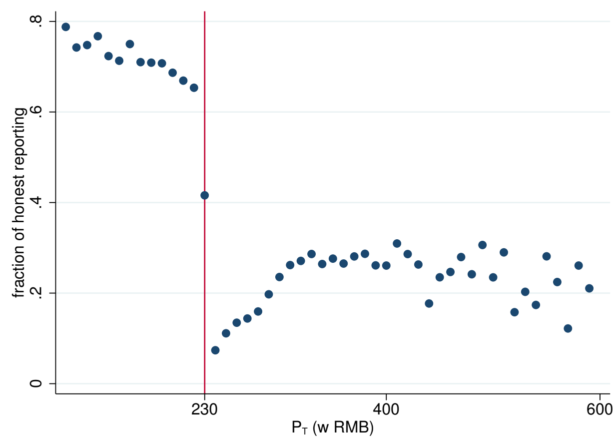
(a) Within the Inner Ring



(b) Between Outer and Inner Ring

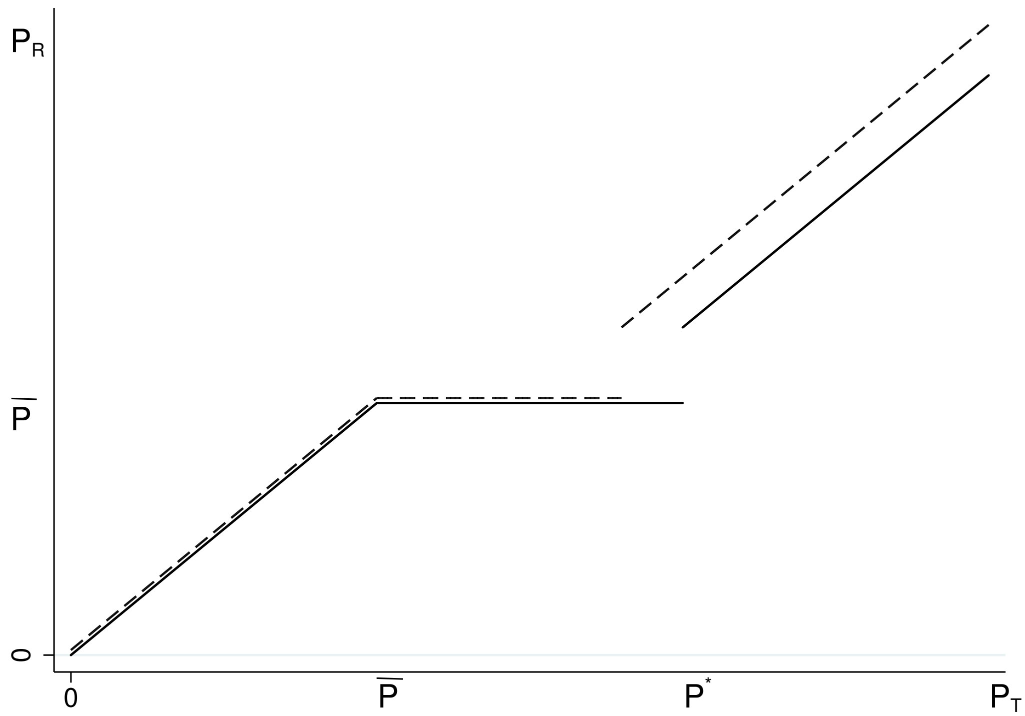


(c) Outside the Outer Ring



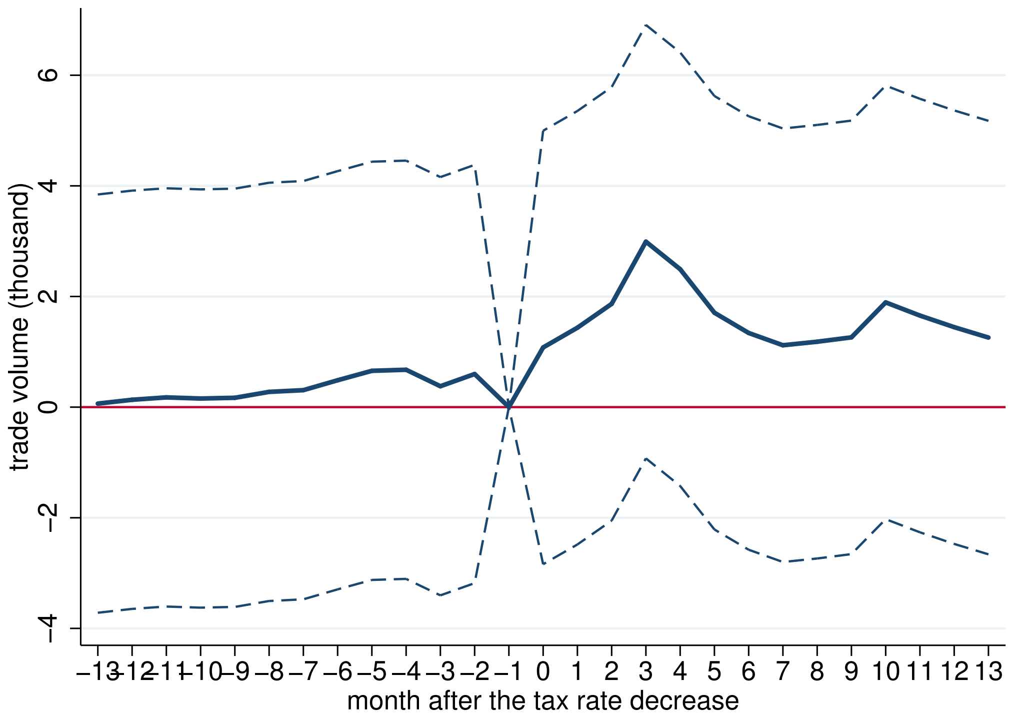
Notes: bin width=10w RMB. The solid line denotes the notch.

Figure A10: Misreporting Patterns under Idiosyncratic Misreporting Costs



Notes: The solid lines show the original reporting pattern, while the dashed lines show the reporting pattern for people with a higher misreporting cost λ.

Figure A11: The Impact of the Tax Reform on Trade Volumes: Event Study Evidence



Notes: Transactions with P_T in the first segment are assigned as the control group and those with P_T in the third segment are assigned as the treatment group. The event study specification is shown as equation 7. Dashed lines show the 95% confidence interval of the point estimate. The reference period is set to be the month prior to the reform. The figure shows that the treatment and control groups share parallel pre-trends; the treatment group experiences a gradual increase of trade volumes relative to the control group after the reform.

Table A1: Impact of the Tax Rate on Trade Volumes

	[1]	[2]	[3]
Dependent variable:	volume (K)	tax rate	volume (K)
Treat	-2.02***	4.906***	10.55**
	(0.38)	(0.06)	(4.78)
Post	-0.97**	-0.002	-0.98***
	(0.37)	(0.05)	(0.37)
Treat x Post	1.31**	-0.511***	
	(0.53)	(0.08)	
Tax Rate (percentage point)			-2.56**
			(1.03)
Observations	54	54	54
Regression	Reduced form	First stage	2SLS
Adjusted R-squared	0.367	0.996	
First-stage F-stat			44.54

Notes: Transactions with P_T in the first segment are assigned as the control group and those with P_T in the third segment are assigned as the treatment group. In the 2SLS estimation, we use $Treat \times Post$ as the instrument for the tax rate. Each observation measures the number of transactions (thousand) of a group in a month.