Inside Job: Evidence from the Chinese Housing Market

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Abstract

Using a comprehensive data set of housing transactions in Beijing, China, we find that government officials are more likely than non-officials to buy housing units before government-imposed housing purchase restriction policy. We also find that officials use their power to speed up the loan application process and pay higher prices to complete the transactions before the policy. Overall, the results suggest that officials trade on private information gained during the policy-making process.

Keywords: Government Officials, Insider Trading, Housing Market, Housing Purchase Restriction Policy, China

JEL Code: G14, O21, R31, R38

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1   Introduction

Government officials often have access to private information gained from their involvement in policy-making processes, making government self-capture and rent-seeking by government insiders possible and often inevitable (Zupan 2017). Such behavior not only causes concerns of equity but also may undermine the efficiency and effectiveness of government policy. Despite its importance, however, it is empirically challenging to identify government self-capture because detailed data on financial activities of government officials are often unavailable due to the illicit and secretive nature of government self-capture. The empirical evidence to date, mostly on stock trading by government officials using data from the United States (Ziobrowski et al. 2004, Rajgopal and White 2017, Ziobrowski et al. 2011, Huang and Xuan 2018), is, at best, ambiguous. Furthermore, these findings may only represent a tip of the iceberg because government self-capture may extend well beyond stock trading and may be much more severe in developing countries (Khwaja and Mian 2011). In this paper, we attempt to fill this gap by examining housing purchase activities of Chinese government officials.

Specifically, we examine whether Chinese government officials use policy-related private information to their own advantage when buying housing units. The rapid price increases in the Chinese housing market make housing investment one of the most profitable, if not the most profitable, investment choices in China. The profitability provides great incentives for officials to take advantage of policy-related information when investing in housing.

The Chinese housing market experienced a remarkable boom during the first two decades of the 21st century (Fang et al. 2016, Wu, Gyourko, and Deng 2016). According to Fang et al. (2016), real house prices in major Chinese cities grew at a rate of 13.1% per year between 2003 and 2013. Similarly, Wu, Gyourko, and Deng (2016) document a 10% annual real house

price growth rate since 2006. Such rapid increases in house prices raised widespread concerns about housing bubbles and housing affordability, triggering policy responses by the central and local governments. However, the initial conventional macroprudential and fiscal policies, such as increasing down payment requirements, higher mortgage interest rates, and capital gains taxes, were proved to be not very effective. Starting from April of 2010, in a further effort to cool down the housing markets, 46 major cities in China introduced various forms of housing purchase restrictions.

On April 30, 2010, the Beijing municipal government announced the first such housing purchase restriction policy, allowing each household to buy only one more housing unit after April 30, 2010, regardless of how many units the household already owns. Unfortunately, however, the first policy did very little in curbing house price growth, and the house price kept rising at a fast pace (Fang et al. 2016). In China, land is owned and monopolistically supplied by local governments. As such, the local governments can directly affect house prices through land supply. On the other hand, land transfer fees are one of (if not the) most important sources of revenue for local governments (Pan, Huang, and Chiang 2015), a phenomenon widely known as land finance in China. These local governments, often facing budget deficits, have strong incentives to maximize their revenue from land transfer fees by managing land supply (Wang and Hui 2017), and hence making the house purchase restriction policy ineffective. In this paper, we examine whether government officials profit from the policy by using policy-related private information.

In particular, we examine whether government officials become more or less likely to buy housing units before the public announcement of the policy. If the officials believe that house prices will decline due to the policy, they will become less likely to buy. On the contrary, they will become more likely to buy if they believe that house prices will increase regardless of the policy.

We use housing transaction data from the Beijing Housing Provident Fund (HPF) for empirical analysis. The Beijing HPF data report, among other things, whether a buyer is a government official. We find that government officials, relative to non-officials, are more likely to buy housing units before the announcement of the housing purchase restriction policy. Combined with the fact that house prices kept rising after the policy, this result suggests that government officials are able to trade on policy-related private information in the housing market.

We also examine whether the results are simply driven by seasonality, that is, officials are likely to buy housing units during a particular period of time during the year. To this end, we find that officials are only more likely to buy houses before the policy announcement but not during the same time period of other years when there is no policy announcement, suggesting that the baseline results are not driven by seasonality.

To ensure that the results are not driven by contemporaneous confounding events that affect officials' and non-officials' housing purchase decisions differently, we examine whether officials are also more likely to buy new housing units before the policy announcement. During our sample period, most new housing units are sold by developers in pre-sales. As such, the purchase decisions of new housing units are determined long before the actual transaction dates, and therefore are unlikely to be affected by the policy announcement. In this regard, we indeed find that the policy announcement has no effect on officials' purchase of new housing units.

Government official status may be correlated with general ability and competency, which drive better investment decisions in the housing market. Unfortunately, we can never fully control buyers' ability. Instead, we examine whether non-officials holding high positions in non-government institutions are also more likely to buy housing units before the policy announcement. Buyers holding high positions in non-government institutions are also likely

to have higher ability, but do not have access to policy-related private information. We find that these buyers are not more likely to buy before the policy announcement, suggesting that buyers ability correlated with official status is unlikely to drive our baseline results.

Due to the likely very short window between knowing the information and the actual implementation of the policy, it is crucial that the buyers move quickly to close the transactions before the policy. In this regard, we indeed find that the processing time, that is, the time between HPF loan application and transaction closing, becomes shorter before the announcement of the first policy for government officials relative to non-official buyers. The result suggests that government officials were able to speed up their loan application process in order to complete the transaction before the policy. We also find that officials are more likely to initiate loan application about 60 to 90 days before the policy announcement. Furthermore, we find some evidence that officials are willing to pay higher prices before the policy announcement, suggesting that they use high prices to speed up the negotiation and bargaining process.

Due to the ineffectiveness of the first policy, the Beijing municipal government issued a second housing purchase restriction policy on February 16, 2011. Different from the first policy, house prices started to decline shortly after the implementation of the second policy. In sharp contrast, we find that government officials are not more likely to buy housing units before the announcement of the second policy.

Our paper contributes to the literature on political connections. The early literature on this topic focuses almost entirely on the benefits accured to politically connected firms or investors, that is, government capture by private special-interest groups from the demand-side of the policy-making process (Zupan 2017). For example, Fisman (2001), Khwaja and Mian (2005), Faccio (2006), and Faccio, Masulis, and McConnell (2006) document that politically connected firms benefit from higher stock valuation, higher probabilities of the

bailout, or better loan terms.[1] Gao and Huang (2016) find that politically connected hedge funds earn higher returns.

On the other hand, little attention has been devoted to political capture by government insiders on the supply-side of the policy-making process, that is, government officials. Yet, government insiders have the motive, means, and opportunities to extract rents at the expense of the general public, the so-called government self-capture. Identifying and understanding government self-capture is of critical importance as it is even blamed for the decline of nations (Zupan 2015). However, the evidence of direct incentives of and direct benefits accrued to politicians is limited because such benefits are often difficult to observe or to quantify (Khwaja and Mian 2005). Several recent papers use financial information disclosed by US politicians to examine whether these politicians earn abnormal returns when trading stocks. Ziobrowski et al. (2004) find that US senators earn abnormal returns when trading stocks. In contrast, C. Eggers and Hainmueller (2013) find no abnormal returns of stock trading by US lawmakers. In a more recent paper, Huang and Xuan (2018) again confirm the findings in Ziobrowski et al. (2004). We add to this literature by extending the investigation beyond stock trading and beyond the US. More importantly, we are the first to identify government officials' specific actions related to specific policies.

Our paper is also related to the literature on insider trading. Most existing studies focus on corporate insiders and examine whether insider trades contain information (Lakonishok and Lee 2001, Fernandes and Ferreira 2008, Cohen, Malloy, and Pomorski 2012, Akbas, Jiang, and Koch 2018). The most closely related to our research is the literature on insider trading patterns around specific events (e.g., John and Mishra 1990, John and Lang 1991,

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[1]More recent papers on this topic include Claessens, Feijen, and Laeven (2008), Faccio and Parsley (2009), Goldman, Rocholl, and So (2013), Cooper, Gulen, and Ovtchinnikov (2010), Duchin and Sosyura (2012),La Porta, Lopez-de Silanes, and Zamarripa (2003), Tahoun (2014), Akey (2015), and Acemoglu et al. (2016)

Lee, Mikkelson, and Partch 1992, and Ke, Huddart, and Petroni 2003). We add to this literature by examining government insiders housing transactions.

Finally, the paper is related to the literature documenting rent-seeking behavior by Chinese government officials in the Chinese real estate market. Chen and Kung (2018) find that companies linked to high government officials receive large discounts in Chinas primary land market. Fang, Gu, and Zhou (2019) find that officials pay lower prices for housing units than non-officials.

The rest of the paper is organized as follows. Section 2 describes the Chinese housing market and the housing purchase restriction policies; section 3 discusses the data and the sample construction method; section 4 presents the main results; and section 5 concludes.

2   Chinese Housing Market and the Housing Purchase Restriction Policy in Beijing

Before 1994, urban housing units were allocated either by the government or state-owned enterprises, and the housing market simply did not exist in China. Housing markets gradually emerged following the housing reform initiated in 1994. The housing markets grew rapidly after 1998 when the traditional housing allocation model was completely abolished and housing properties were privatized (Fang et al. 2016, and Fang, Gu, and Zhou 2019). The low mortgage interest rates set by the Chinese central bank in the aftermath of the 1997 Asian Financial Crisis spurred the first wave of rapid house price growth.

During the 2008-2009 global financial crisis, the Chinese government adopted a series of policies to stimulate the residential real estate market, including the four trillion RMB (about $600 billion) stimulus package, the reduction of the mortgage interest rates, and the relaxation of down-payment requirements. All these policies triggered the second wave of

dramatic house price appreciation. As a result, housing prices rose to a record high level, triggering big concerns about housing bubbles and housing affordability. To curb the soaring house price and cool down the housing market, the State Council of China issued the memo on resolutely curbing the rapid increase of housing prices in some cities on April 17th, 2010, which was called the "New Ten Articles" by the industry. The memo stipulated that "local governments may take temporary measures to limit the purchase of housing units in a certain period..."

Following the guidelines of the State Council, the municipal government of Beijing took the lead in announcing and implementing the first housing purchase restriction policy on April 30, 2010, which says that each household can only buy one additional housing unit. As a result, households in Beijing can purchase only one additional housing unit no matter how many units they already own before April 30, 2010. As such, buying a housing unit before April 30 does not affect their ability to buy after the policy announcement. The policy, while effective in slightly decreasing the growth rate of house prices (Du and Zhang 2015), did not stop the house prices from rising higher, as shown in Figure 1.

[Insert Figure 1 About Here]

The ineffectiveness of the policy is in fact no surprising. While most consider China's house price problem is demand-side problem, fueled by housing speculation

Due to the ineffectiveness of the first policy, the Beijing municipal government announced the second housing purchase restriction on February 16, 2011. Under the second policy, only households who are Beijing residents (with Hukou), or those who have paid social security or personal income taxes for five consecutive years or more in Beijing, were eligible for purchasing one housing unit. Households with Hukou in Beijing are allowed to own at most two housing units. The second policy is considered more restrictive than the first one due to the exclusion of more than 35% of the population who live in Beijing without Beijing Hukou.

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In fact, house prices started to level off and to decline shortly after the second policy, as shown in Figure 1. In this paper, we focus mostly on the first policy because the second policy is well expected by the public.

3   Data and Sample Construction

Our data is from the application records of the Beijing Housing Provident Fund (HPF) guarantee center from 2007 to 2012. The Chinese HPF, modeled after the Central Provident Fund of Singapore, was founded in 1994 as a nationwide mandatory savings program for housing. One objective of the HPF was to facilitate the privatization of the housing markets. Both employees and employers are required to contribute a certain percentage of the employees' salary to the fund. The contribution requirements are controlled by local municipal governments. In Beijing, the minimum is 5% of employees' salary. The interest rates paid on the fund are also set by the government. Members of the fund can then borrow from the fund at a subsidized rate for the only purpose of buying housing units. As such, the HPF is often the first choice of financing when members of the HPF buy housing units.

When the members of HPF purchase housing units in Beijing, they submit applications for subsidized mortgages from HPF,[2] which are guaranteed by the HPF guarantee center. The HPF guarantee center collects background information about the applicants, and also information about the housing units. Most importantly, applicants are required to report their government official status in the applications. Because the HPF is linked to employers, the information on official status is very accurate.

Using the HPF transaction data has several advantages. First, the identification of

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[2]The underwriting and servicing of the HPF mortgages are delegated to commercial banks. In addition to the regular underwriting process, the loan application also has to be approved by the HPF guarantee center.

government officials is accurate. Second, compared with transactions financed by mortgages issued by a particular commercial bank (e.g. Fang, Gu, and Zhou 2019), there are fewer selection issues involved. However, the HPF data also has its limitations. First, we are only able to capture a small fraction of all the transactions. Second, due to the extra regulatory burden, the processing time of HPF mortgages is usually longer than commercial bank mortgages. As such, they are less likely to be used to time the policy announcement, which will then bias against any findings, that is, our results may underestimate the true effect.

The data record detailed information on mortgage applications and housing transactions including both the application date and the mortgage issuance date, which is also the transaction closing date. It is important to identify the exact closing dates to categorize whether the transaction occurred before or after the policy. In addition to the key variables mentioned above, the data also provide information on age, income, education level, characteristics and the location of the housing units.

For our purpose, we only include transactions occurred no earlier than 182 days before and no later than 182 days after the first policy, that is, from October 30th, 2009 (182 days before the first policy announcement) to October 30th, 2010 (182 Days after the first policy announcement). Furthermore, in our baseline analysis, we only include existing housing unit sales. In China, the timing of new housing sales is determined by real estate developers. During our sample period when the real estate market is very hot, most new housing units are sold via pre-sales. As such, it is almost impossible to time the market with new housing sales. In fact, in a placebo test, we indeed find that officials are not more likely to buy new housing units right before the policy announcement.

We first identify government officials as those with the official rank at the Ke (equivalent to section chief) or above levels. We expect that these officials to be more likely to have policy-related private information than those lower-ranked officials (Fang, Gu, and Zhou

2019). We then present the summary statistics for officials and non-officials separately in Table 1. All variables are measured at the time of the transactions. There are 13,029 existing housing unit sales in our sample, among which 658 housing units (5.05%) were bought by government officials and 12,371 units (94.95%) were bought by non-officials. Compared with non-officials, government official buyers are older and more likely to be male. They have similar income as non-officials. However, they buy larger housing units and at the same time pay significantly lower per unit prices. Officials have a 19% probability of buying a housing unit in the 60-day window before the policy announcement, while non-officials have a 16% (very close to the unconditional probability of 60/365) probability of buying in the same window.

[Insert Table 1 about Here]

4   Empirical Results

4.1   Baseline Results

We begin by examining whether officials know about the upcoming housing purchase restriction policy and buy housing units before the implementation of the policy. Before we present the regression results, we first present some visual evidence. Figure 2 plots the total number of housing units bought by officials and non-officials semi-monthly from November of 2009 to November of 2012. The solid vertical line indicates the announcement date of the first housing restriction policy. The number of housing units bought by officials has an upward trend before the announcement day of the first policy. Compared with the non-officials, the trend of housing transactions by officials are higher starting from about two months before the first housing policy, and the difference disappears shortly after the policy

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announcement. These patterns are consistent with the idea that government officials become more likely to buy housing units before the policy announcement.

[Insert Figure 2 about Here]

We then proceed to present the regression results. Our focus is the timing of home purchases by officials. We first define *Window 60* as a dummy variable that equals one if the transaction occurred during the 60-day window before the policy announcement. We then examine whether officials are more likely to buy housing units during that window using the following specification,

$$Window\ 60_{i,j,t} = \alpha + \beta Official_i + \gamma_1 X_{i,t} + \gamma_2 Z_{j,t} + \varepsilon_{i,j,t} \tag{1}$$

where i indexes buyer, j indexes transaction, and t indexes transaction time, $Official_i$ is a dummy variable that equals one if buyer i is a government official ranked at the Ke or above levels; $X_{i,t}$ is a set of buyer characteristics, including age, age squared, income, income squared, gender, education level, and years working; $Z_{j,t}$ is a set of transaction characteristics, including the size of the housing unit, the price of the housing unit, the loan-to-value ratio, and the term on the loan.

This specification is equivalent to the following difference-in-differences specification. First, we expand all housing unit buyers in our sample into daily observations. Second, we define a dummy variable B, which equals one if the buyer bought a housing unit on that day, and zero otherwise. Third, we run the following difference-in-differences specification, $B_{i,j,t} = \alpha_t + \beta Official_i \times Window60_{i,j,t} + \gamma_1 X_{i,t} + \gamma_2 Z_{j,t} + \varepsilon_{i,j,t}$. In fact, we obtain exactly the same results using this specification. However, the specification as in Equation (1) is computationally more efficient.

The results are presented in Table 2. We do not include any control variables in Column

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(1), include buyer characteristics in Column (2), and then further include transaction characteristics in Column (3). We add control variables gradually because some of the control variables, especially transaction characteristics, may be simultaneously determined as the timing of the transaction. In all columns, the coefficients on *Official* are positive and statistically significant, suggesting that government officials are more likely to buy housing units during the 60-day window before the policy announcement. The effect is also economically significant. Officials have about a 3.5 percentage points higher probability than non-officials to buy housing units during the 60-day window. The unconditional probability that a buyer buys a housing unit during the 60-day window is 16.44 percent (60/365). The 3.5 percent probability represents a 20% increase relative to the unconditional probability.

[Insert Table 2 about Here]

Most control variables are not statistically significant, except for age and homeownership status. Age and homeownership may represent the buyers' experience with the housing markets, and are therefore related to their ability to time the market. We also want to point out that the ability to time the market is not correlated with income, suggesting that the socioeconomic factors may not be important in affecting their ability to time and predict government policy. The R-squares are very small, also suggesting that timing the policy announcement is very difficult.

4.2 Identification Tests

4.2.1 Seasonality

The results above show that officials are more likely to buy housing units during the 60-day window before April 30, 2010. One concern is that the results may be driven by seasonal factors affecting officials and non-officials differently, that is, officials may be simply more

likely to buy housing units in March and April due to some unknown factors. To mitigate this concern, we examine whether officials are also more likely to buy in March and April of other years. In particular, we focus on the 365-day windows surrounding April 30 of 2008, 2009, 2011, and 2012, and re-estimate Equation (1) with transactions occurred during those windows.

The results are presented in Table 3. In contrast to the results in Table 2, the coefficient estimates on *Official* are either insignificant or negative, suggesting that the results in Table 2 are unlikely to be driven by seasonality.

[Insert Table 3 about Here]

4.2.2 New Housing Units Sale

While the results in Table 3 mitigate the concern that the results may be driven by seasonality, the results may still be driven by other confounding events occurred around the same time as the housing purchase restriction policy announcement. To mitigate this concern, we examine whether government officials are also more likely to buy new housing units before the announcement of housing purchase restriction policy.

In China, the timing of new housing sales is determined by real estate developers. Most of the new housing units in China are sold by the developers through pre-sales, and the exact delivering time is determined by the developers. As such, it is almost impossible to time the market with new housing sales. As such, if the baseline results are indeed driven by officials' preemption of the policy announcement, the effect is unlikely to show up in new housing unit sales. On the contrary, if the results are driven by other factors affecting officials and non-officials differently, we may also observe the results on new housing unit sales.

[Insert Table 4 about Here]

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We therefore re-estimate Equation (1) on new housing unit sales. In contrast to the results in Table 2, the coefficient estimates are much smaller and statistically insignificant. These results suggest that the baseline results are unlikely to be driven by other confounding factors that may have affected officials and non-officials differently.

4.2.3 Unobservable Ability

Another concern is that government officials ability to time the market may not come from policy-related information, but instead from their general competency or ability. Unfortunately, it is very difficult, if not impossible, to control general competency or ability of a purchaser. To mitigate this concern, we instead examine whether non-official purchasers holding high positions in non-government institutions exhibit similar behavior. To the extent that the high positions also proxy for the purchasers' unobservable ability, we should find a similar effect if the results above are driven by unobservable purchaser ability. The HPF data report the generic positions the purchasers hold. We define a high position as those positions as "General Managers", "Department Managers", or "Directors". In contrast, non-official purchasers not holding high positions report themselves as "Workers" or "Employees". We then estimate the following specification to examine whether purchasers with high positions are also more likely to buy housing units before the announcement of the policy,

$$Window\ 60_{i,j,t} = \alpha + \beta High\ Position_i + \gamma_1 X_{i,t} + \gamma_2 Z_{j,t} + \varepsilon_{i,j,t} \tag{2}$$

In this exercise, we exclude government officials. The results are presented in Table 5. In contrast to the results presented in Table 2, the coefficient estimates on High Position are all close to zero and statistically insignificant. To the extent that High Position also proxies for purchasers unobservable ability, the results in Table 5 suggest that the baseline results

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are unlikely to be driven by unobservable purchaser ability.

[Insert Table 5 about Here]

4.3 Event Dynamics

Next, we examine the dynamics of the official housing buy behavior to ensure that the baseline effect only shows up in the short time window before the policy announcement. To this end, we examine whether officials are more likely to buy housing units during the fourteen 15-day windows surrounding the policy announcement, from 120 days before to 90 days after the policy announcement. In particular, we replace the dependent variable in Equation (1) with the dummy variables for these 15-day windows. We then plot the coefficient estimates on *Official* in Figure 3.

Before the policy announcement, the coefficient estimates are all positive. However, the coefficients are only statistically significant for the 60-45, 45-30, and 30-15 day windows before the policy announcement. Furthermore, the coefficient estimates almost increase monotonically as the time gets closer to the policy announcement, suggesting that officials learn more and more information about the policy. In sharp contrast, the coefficient estimates turn negative immediately after the policy announcement, suggesting that officials who would have bought the housing units after bought before the policy announcement. The results reinforce that the baseline results are truly driven by the policy.

[Insert Figure 3 about Here]

4.4 Application Time Windows

We have so far focused on the time window surrounding the closing time. However, it takes significant time to close a housing transaction deal, and the closing time is not com-

pletely under buyers' control. In this subsection, we therefore focus on the loan application time to examine whether government officials are more likely to apply for the HPF loans before the announcement of the policy. Studying the application time also allows us to pin down the exact time when government officials are informed about the policy. In particular, we examine whether officials are more likely to initiate the loan application process during the four thirty-day windows before the policy announcement.

The results are presented in Table 6. The dependent variables are *Apply 30*, *Apply 30-60*, *Apply 60-90*, and *Apply 90-120*, which equal one if the application time is within 30-days before, between 30 days before and 60 days before, between 60 days before and 90 days before, and between 90 days before and 120 days before the policy announcement. The coefficient estimate is positive and statistically significant only in Column (3), suggesting that most officials are informed about 60 to 90 days before the policy announcement.

[Insert Table 6 about Here]

4.5 Loan Processing Time

The officials who get access to the information need to complete the transactions before the announcement of the policy. The amount of time needed to complete a transaction has two components, the time for housing search and the time for loan processing and approval. While we do not have data on housing search time, we have data on loan processing time. Government officials, rushing to complete the transaction before the policy implementation, may then use their official power to press the HPF guarantee center to speed up the loan approval process.

The processing time is defined as the number of days between the loan application date and the closing date. We examine whether the processing time is shorter for government

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officials before the announcement of the policy using the following specification,

$$Process\ Time = \alpha_t + \beta_1 Official_i \times Apply\ 60-90 + \beta_2 Official_i + \gamma_1 X_{i,t} + \gamma_2 Z_{j,t} + \varepsilon_{i,j,t} \quad (3)$$

where the dependent variable is the natural logarithm of the processing time; α_t is the application time fixed effects; $Apply\ 60-90$ equals one if the loan application date is between 60 days before to 90 days before the policy announcement. We choose the 60 to 90 day window because the results above suggest that officials are likely to be informed between 60 days before to 90 days before the policy announcement.

The results are presented in Table 7. The coefficients on the interaction term between *Official* and $Apply\ 60-90$ are all negative and statistically significant, suggesting that the loan processing time for officials is shorter than non-officials during the 60 to 90 day window before the policy announcement. The results are therefore consistent with the idea that officials who get access to the policy-related information pressed the HPF center to speed up the process to complete the transaction before the policy announcement. The economic magnitude is also large, the loan processing time for official buyers is about six days shorter than non-officials, which is about 18% of the median loan processing time in the sample.

[Insert Table 7 about Here]

4.6 Transaction Prices

In addition to shortening the loan processing time, the official buyers can also reduce the negotiation/bargaining time to ensure that the transaction closes before the policy announcement. One way to reduce the negotiation/bargaining time is to increase the prices they offer. We therefore expect that the officials will pay relatively higher prices than non-officials before

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the policy announcement. To this end, we estimate the following,

$$Log\ Unit\ Price = \alpha_t + \beta_1 Official_i \times Window\ 60_{i,j,t} + \beta_2 Official_i + \gamma_1 X_{i,t} + \gamma_2 Z_{j,t} + \varepsilon_{i,j,t}, \quad (4)$$

where $Log\ Unit\ Price$ is the natural logarithm of the price per square meter; α_t is the transaction time fixed effects. If officials pay higher prices before the policy announcement, we should expect β_1 to be positive.

The results are presented in Table 8. The coefficient estimates of β_1 are all positive and are statistically significant if we control all buyer and transaction characteristics. The result is therefore consistent with the argument that officials pay a relatively higher price to speed up the transaction process.

[Insert Table 8 about Here]

4.7   Robustness Checks

In the analysis above, we only considered housing buyers with official ranks at the Ke or above levels as officials. There are, however, other officials at the below Ke levels. In this subsection, we include these lower-ranked officials, and define the variable, *All Official*, which equals to one if the buyer is a government official (including ranking below the Ke level). The number (and the percentage) of all official buyers in our sample is 2,785 (21.4%), much larger than the number (and the percentage) in our baseline analysis. We then replace the independent variable *Official* in Equation (1) with *All Official* and re-estimate Equation (1). The results are presented in Columns (1)-(3) of Panel A of Table 9. Consistent with the baseline results, the coefficient estimates on *All Official* are all positive and statistically significant. However, the magnitudes of the estimates are smaller than those in Table 2, suggesting that lower-ranked officials are less likely to take advantage of the policy-related

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private information.

To provide further evidence on the different effects on higher- versus lower-ranked officials, we define a variable, *Low Official*, which equals one if the buyer is an official with below-Ke ranks. We then include both *Official* and *Low Official* in the regressions. The results are present in Columns (4)-(6) of Panel A of Table 9. While the coefficient estimates on *Official* and *Low Official* are all positive and statistically significant, the coefficients on *Low Official* are much smaller and statistically less significant. These results are consistent with the fact that lower-ranked officials have less access to policy-related private information and therefore are less able to take advantage of the policy.

[Insert Table 9 about Here]

The dependent variable in the baseline analysis is a dummy variable, however, we use the OLS for estimation in all the results reported above. In this section, we show that the results are robust with nonlinear probability models. In particular, we present the marginal effects of the Probit model estimation in Panel A of Table 9. Similar to the results presented in Table 2, the marginal effects are all positive and statistically significant. Furthermore, the magnitudes of the marginal effects are also similar to the magnitudes of the OLS coefficient estimates presented in Table 2.

We focused on the transactions occurred between 182 days before and 182 days after the policy announcement in our baseline analysis. In this section, we instead focus on a shorter window, between 91 days before and 91 days after the policy announcement. The OLS estimation results over this shorter window are presented in Panel B of Table 9. The coefficients on *Official* are again all positive and statistically significant. In fact, the magnitudes of the coefficients are even larger than those in Table 2.

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4.8   The Second Housing Purchase Restriction Policy

As described above, the first housing policy, the focus of our analysis so far, was not really effective in curbing house price growth, and in February 2011, the Beijing city government announced the second policy. We do not focus on the second policy in the above analysis because the second policy was well anticipated by the public. However, it might still be interesting to examine whether officials also take advantage of private information related to the second policy. As such, we focus on transactions occurred between 182 days before and 182 days after the second policy announcement (February 16, 2011). We then re-define *Window 60* as the 60-day window before the second policy announcement, and re-estimate Equation (1).

The results are presented in Table 10. In sharp contrast to the results on the first policy, presented in Table 2, the coefficients on Official are all negative and only one coefficient is statistically significant when no controls are included. Different from the ineffectiveness of the first policy, the second policy is more effective, which explains why the officials are not more likely to buy housing units before the second policy announcement.

[Insert Table 10 about Here]

5   Conclusion

We use the housing transactions by government officials and non-officials to examine whether government officials use information gained during the policy-making process to trade. In particular, we find that government officials are more likely to buy housing units before the announcement of the housing purchase restriction policy in Beijing, China. Furthermore, we also find that government officials are able to speed up the loan application process and pay higher prices to complete the transactions before the policy. Overall, we

provide evidence that government officials trade on private information in the housing market.

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Figure 1: Housing Price Index and Housing Purchase Restriction Policy in Beijing
This figure illustrates the monthly resale housing price index from 2009Q1 to 2012Q1. The data source is from Fang, Gu, Xiong and Zhou (2016). The solid vertical line indicates the implementation of first housing purchase restriction policy on April 30th, 2010. The dash vertical line indicates the implementation of second housing purchase restriction policy on February 16th,2011.

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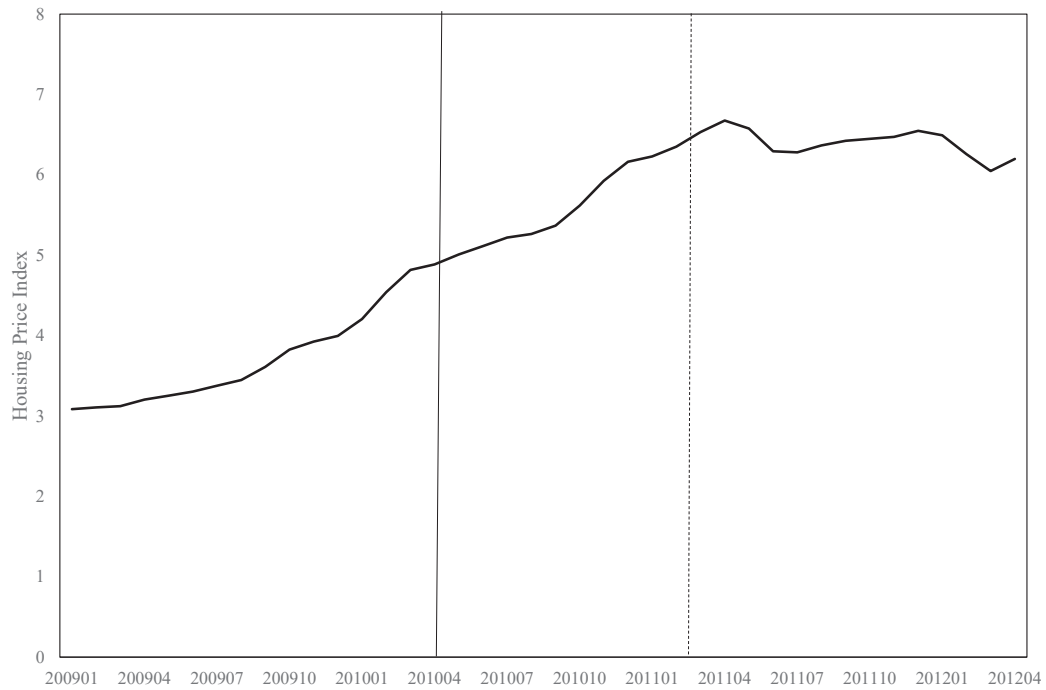
Figure 2: Housing Transactions by Officials and Non-officials
This figure illustrates the number of transactions by officials and non-officials in Beijing from November 2009 to November 2012. Data is constructed based on our sample. The solid vertical line indicates the implementation of the first housing purchase restriction policy on April 30th, 2010.

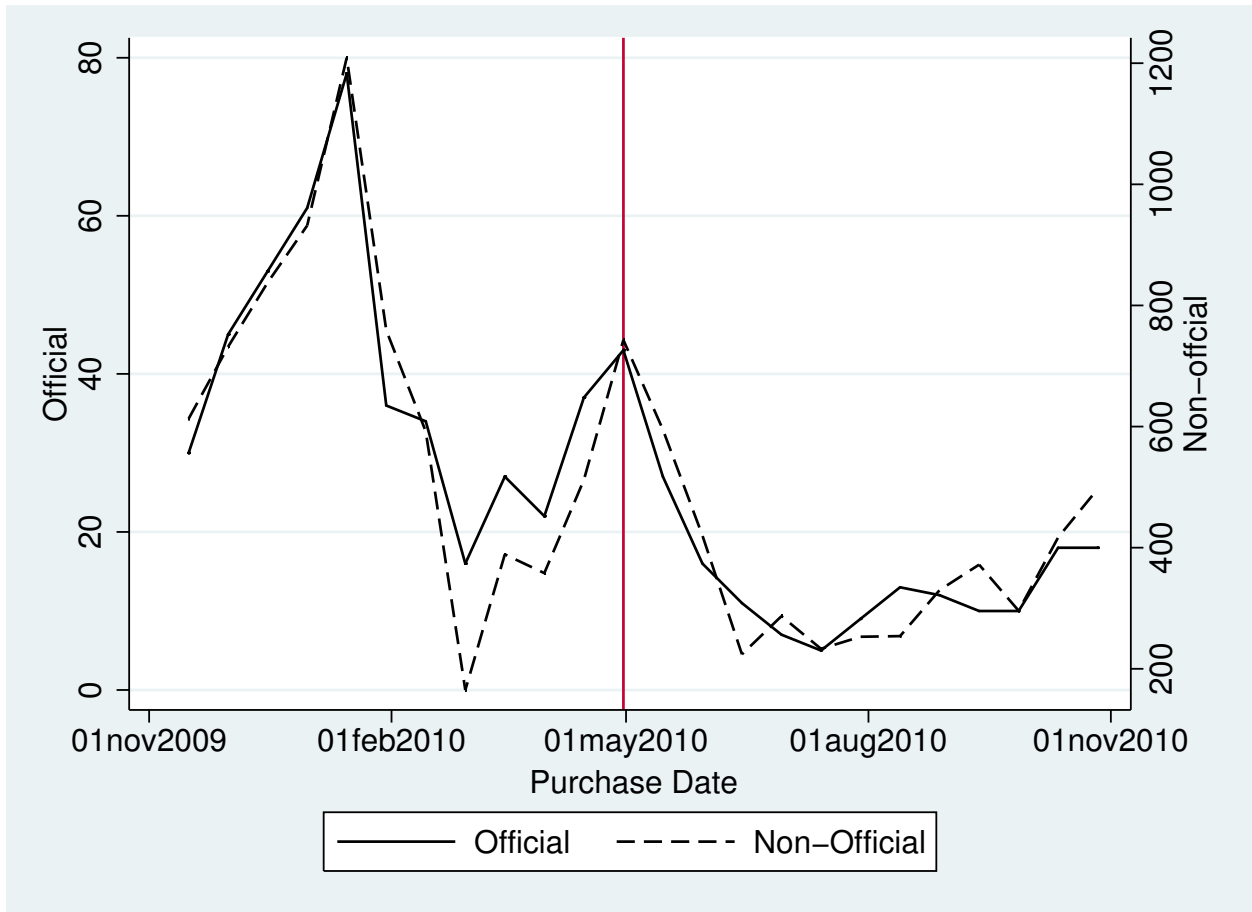Figure 3: Event Dynamics Plot
This figure present the coefficient estimates and their confidence intervals for the 15-day windows surrounding the policy announcement.
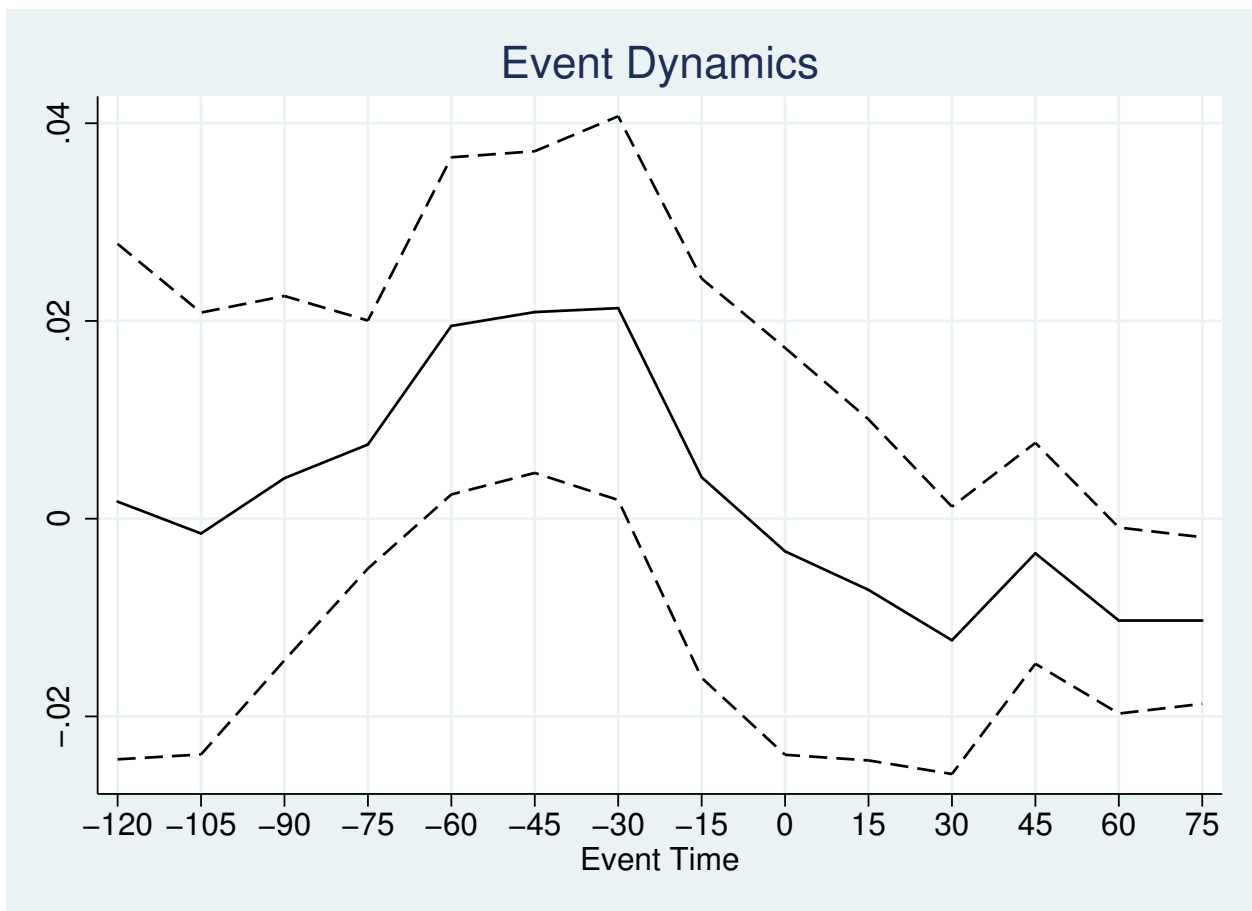
Table 1: Summary Statistics

The table presents the summary statistics of the transactions between October 30th, 2009 and October 30th, 2010, with Panel A for transactions by government officials and Panel B by non-officials. *Window* 60 is a dummy variable that equals one if the transaction occurred during the 60-day window before the policy announcement, and zero otherwise. *Age* is the purchasers age. *Income* is the purchasers monthly income measured in thousand RMB. *Gender* is a dummy variable that equals one if the purchaser is male, and zero otherwise. *Year Working* is the total number of years the purchaser has worked. *Year Current Working* is the number of years the purchaser has worked in the current institution. *Homeowner* is a dummy variable if the purchaser own a house already. *College* is a dummy variable if the purchaser has a college degree or above and zero otherwise.

	Obs.	Mean	Std. Dev.	p25	p50	p75
Panel A. Officials						
Window_60	658	0.19	0.39	0.00	0.00	0.00
High Official	658	0.23	0.42	0.00	0.00	0.00
Low Official	658	0.77	0.42	1.00	1.00	1.00
Age	658	3.70	0.22	3.53	3.71	3.87
Income	657	11.56	0.47	11.25	11.50	11.82
Female	658	0.36	0.48	0.00	0.00	1.00
College	658	0.71	0.46	0.00	1.00	1.00
Year Working	658	20.22	10.36	12.00	20.00	29.00
Year Current Working	658	14.30	10.32	5.00	13.00	22.00
Size	658	91.20	36.34	63.00	86.00	110.00
Unit Price	658	9,778	5,561	5,605	8,145	12,774
Total Price	658	787,953	363,932	520,000	740,000	1,000,000
Loan-to-Value	658	0.69	0.13	0.60	0.76	0.80
Loan Term	658	18.89	6.66	15.00	20.00	21.00
Process Time	658	37.65	17.42	26.00	34.00	45.00

	Obs.	Mean	Std. Dev.	p25	p50	p75
Panel B. Non-Officials						
Window_60	12,371	0.16	0.37	0.00	0.00	0.00
High Position	12,371	0.04	0.20	0.00	0.00	0.00
Age	12,371	3.45	0.19	3.33	3.40	3.56
Income	12,300	11.44	0.61	11.07	11.43	11.88
Female	12,371	0.39	0.49	0.00	0.00	1.00
College	12,371	0.71	0.46	0.00	1.00	1.00
Year Working	12,371	9.52	7.78	4.00	7.00	13.00
Year Current Working	12,371	6.45	6.76	2.00	4.00	8.00
Size	12,371	78.28	29.07	58.00	71.00	93.00
Unit Price	12,371	10,691	5,637	6,423	9,207	14,008
Total Price	12,371	759,998	335,660	500,000	730,000	1,000,000
Loan-to-Value	12,371	0.71	0.12	0.62	0.78	0.80
Loan Term	12,371	22.36	6.54	20.00	20.00	30.00
Process Time	12,371	37.89	17.41	27.00	34.00	44.00

Table 2: Baseline Results

The table shows the OLS estimation results of our baseline model, $Window\ 60_{i,j,t} = \alpha + \beta Official_i + \gamma_1 X_{i,t} + \gamma_2 Z_{j,t} + \varepsilon_{i,j,t}$. The dependent variable is a dummy variable for the 60-day window before the policy announcement. The key independent variable is an indicator variable, Official, which equals one if the purchaser is a government official and zero otherwise. Other variables are as defined in the note to Table 1. Robust standard errors are reported in the parentheses below the parameter estimates. ***, ** and * represent the significance levels at 1%, 5% and 10%, respectively.

	(1)	(2)	(3)
Official	0.0327**	0.0366**	0.0345**
	(0.0157)	(0.0165)	(0.0165)
Age		2.5745***	2.6769***
		(0.5260)	(0.5303)
Age Squared		-0.3643***	-0.3765***
		(0.0774)	(0.0780)
Income		-0.0070	-0.0151
		(0.0335)	(0.0334)
Income Squared		-0.0001	0.0006
		(0.0016)	(0.0016)
Female		0.0054	0.0057
		(0.0069)	(0.0069)
Year Working		0.0006	0.0007
		(0.0014)	(0.0014)
Year Current Working		0.0007	0.0005
		(0.0008)	(0.0008)
Homeowner		0.0155*	0.0206**
		(0.0086)	(0.0088)
Size			-0.0003**
			(0.0001)
Unit Price			0.0000
			(0.0000)
Loan-to-Value			0.0419
			(0.0264)
Loan Term			0.0004
			(0.0006)
Professional Designation Fixed Effects		Yes	Yes
Education Level Fixed Effects		Yes	Yes
District Fixed Effects			Yes
Observations	13,029	12,936	12,936
R-squared	0.0004	0.0136	0.0189

Table 3: Other Years

The table shows the OLS estimation results of the model, $Window\ 60_{i,j,t} = \alpha + \beta Official_i + \gamma_1 X_{i,t} + \gamma_2 Z_{j,t} + \varepsilon_{i,j,t}$ on transactions occurred during the 365-day windows sourounding April 30 of 2008, 2009, 2011, and 2011, respectively. The dependent variable is a dummy variable for the 60-day window before April 30 of each year. The key independent variable is an indicator variable, Official, which equals one if the purchaser is a government official and zero otherwise. Other variables are as defined in the note to Table 1. Robust standard errors are reported in the parentheses below the parameter estimates. ***, ** and * represent the significance levels at 1%, 5% and 10%, respectively.

	2008 (1)	2009 (2)	2011 (3)	2012 (4)
Official	-0.0058	-0.0034	0.0045	-0.0128**
	(0.0103)	(0.0086)	(0.0105)	(0.0062)
Buyer Characteristics	Yes	Yes	Yes	Yes
Property Characteristics	Yes	Yes	Yes	Yes
Observations	5,435	12,881	11,516	25,156
R-squared	0.0163	0.0169	0.0185	0.0159

Table 4: New Housing Unit Sales

The table shows the OLS estimation results of the model, $Window\ 60_{i,j,t} = \alpha + \beta Official_i + \gamma_1 X_{i,t} + \gamma_2 Z_{j,t} + \varepsilon_{i,j,t}$ on new housing units sales. The dependent variable is a dummy variable for the 60-day window before the policy announcement. The key independent variable is an indicator variable, Official, which equals one if the purchaser is a government official and zero otherwise. Other variables are as defined in the note to Table 1. Robust standard errors are reported in the parentheses below the parameter estimates. ***, ** and * represent the significance levels at 1%, 5% and 10%, respectively.

	(1)	(2)	(3)
Official	0.0154	0.0015	0.0021
	(0.0101)	(0.0109)	(0.0108)
Buyer Characteristics		Yes	Yes
Property Characteristics			Yes
Observations	11,774	11,616	11,613
R-squared	0.0002	0.0112	0.0358

Table 5: Non-officials Holding high Positions

The table shows the OLS results estimating $Window\ 60_{i,j,t} = \alpha + \beta High\ Position_i + \gamma_1 X_{i,t} + \gamma_2 Z_{j,t} + \varepsilon_{i,j,t}$. The dependent variable is a dummy variable for the 60-day window before the policy announcement. The key independent variable is an indicator variable, Official, which equals one if the purchaser is a government official and zero otherwise. Other variables are as defined in the note to Table 1. Robust standard errors are reported in the parentheses below the parameter estimates. ***, ** and * represent the significance levels at 1%, 5% and 10%, respectively.

	(1)	(2)	(3)
High Position	-0.0097	-0.0153	-0.0135
	(0.0157)	(0.0160)	(0.0161)
Buyer Characteristics		Yes	Yes
Property Characteristics			Yes
Observations	12,371	12,278	12,278
R-squared	0.0000	0.0133	0.0182

Table 6: Application Time Windows

This table reports the OLS estimation results for apply time windows surrounding the policy announcement. The dependent variables are *Apply 30*, *Apply 30-60*, *Apply 60-90*, and *Apply 90-120*. The key independent variable is an indicator variable, Official, which equals one if the purchaser is a government official and zero otherwise. Other variables are as defined in the note to Table 1. Robust standard errors are reported in the parentheses below the parameter estimates. ***, ** and * represent the significance levels at 1%, 5% and 10%, respectively.

	Apply 30	Apply 30-60	Apply 60-90	Apply 90-120
	(1)	(2)	(3)	(4)
Official	-0.0171	-0.0338***	0.0265**	0.0165
	(0.0176)	(0.0128)	(0.0135)	(0.0152)
Buyer Characteristics	Yes	Yes	Yes	Yes
Transaction Characteristics	Yes	Yes	Yes	Yes
Observations	10,115	10,115	10,115	10,115
R-squared	0.0167	0.0206	0.0126	0.0150

Table 7: Loan Processing Time

The table shows the OLS estimation results of the model, $Process\ Time = \alpha_t + \beta_1 Official_i \times Apply\ 60 - 90 + \beta_2 Official_i + \gamma_1 X_{i,t} + \gamma_2 Z_{j,t} + \varepsilon_{i,j,t}$. The dependent variable, $Process\ Time$, loan processing time. $Window$ 60 is a dummy variable for the 60-day window before the policy announcement. Official is a dummy variable that equals one if the purchaser is a government official and zero otherwise. Other variables are as defined in the note to Table 1. Robust standard errors are reported in the parentheses below the parameter estimates. ***, ** and * represent the significance levels at 1%, 5% and 10%, respectively.

	(1)	(2)	(3)
Official × Apply 60-90	-6.1614**	-6.3103**	-6.4345**
	(2.5501)	(2.5959)	(2.5132)
Official	-0.4733	-0.9636	-0.9090
	(0.6664)	(0.7091)	(0.7062)
Application Time Fixed Effects	Yes	Yes	Yes
Individual Characteristics		Yes	Yes
Transaction Characteristics			Yes
Observations	13,008	12,914	12,914
R-squared	0.2161	0.2343	0.2448

Table 8: Transaction Price

The table shows the OLS estimation results of the model, $Log\ Unit\ Price = \alpha_t + \beta_1 Official_i \times Apply\ 60 - 90 + \beta_2 Official_i + \gamma_1 X_{i,t} + \gamma_2 Z_{j,t} + \varepsilon_{i,j,t}$. The dependent variable, $Log\ Unit\ Price$, is the natural logarithm of the price per square meter. $Window\ 60$ is a dummy variable for the 60-day window before the policy announcement. Official is a dummy variable that equals one if the purchaser is a government official and zero otherwise. Other variables are as defined in the note to Table 1. Robust standard errors are reported in the parentheses below the parameter estimates. ***, ** and * represent the significance levels at 1%, 5% and 10%, respectively.

	(1)	(2)	(3)
$Official \times Window60$	0.0552	0.0596	0.0667*
	(0.0460)	(0.0401)	(0.0392)
$Official$	-0.0638***	-0.0254	0.0061
	(0.0215)	(0.0203)	(0.0203)
Transaction Time Fixed Effects	Yes	Yes	Yes
Individual Characteristics		Yes	Yes
Transaction Characteristics			Yes
Observations	13,008	12,914	12,914
R-squared	0.2161	0.2343	0.2448

Table 9: Robustness Checks

This table presents two sets of robustness tests results, with Panel A for results including lower-ranked officials, Panel B for marginal effects from the Probit model, and Panel C for a shorter window (91 days before and after the policy announcement) estimation. The dependent variable is a dummy variable for the 30-day window before the policy announcement. The key independent variable is an indicator variable, Official, which equals one if the purchaser is a government official and zero otherwise. Other variables are as defined in the note to Table 1. Robust standard errors are reported in the parentheses below the parameter estimates. ***, ** and * represent the significance levels at 1%, 5% and 10%, respectively.

Panel A: Including lower-ranked officials

	(1)	(2)	(3)	(4)	(5)	(6)
All Official	0.0201**	0.0231***	0.0213**			
	(0.0081)	(0.0085)	(0.0086)			
Official				0.0354**	0.0407**	0.0386**
				(0.0158)	(0.0167)	(0.0166)
Low Official				0.0154*	0.0183**	0.0166*
				(0.0090)	(0.0092)	(0.0093)
Buyer Characteristics		Yes	Yes		Yes	Yes
Property Characteristics			Yes			Yes
Observations	13,029	12,936	12,936	13,029	12,936	12,936
R-squared	0.0005	0.0138	0.0191	0.0006	0.0139	0.0192

Panel B: Probit model results

	(1)	(2)	(3)
Official	0.0309***	0.0347***	0.0322**
	(0.0142)	(0.0149)	(0.0149)
Individual Characteristics		Yes	Yes
Transaction Characteristics			Yes
Observations	13,029	12,855	12,855
Pseudo R-squared	0.0004	0.0143	0.0204

Panel C: Shorter window results

	(1)	(2)	(3)
Official	0.0724**	0.0790**	0.0690**
	(0.0303)	(0.0322)	(0.0322)
Individual Characteristics		Yes	Yes
Transaction Characteristics			Yes
Observations	5,552	5,492	5,492
R-squared	0.0011	0.0246	0.0402

Table 10: The Second Housing Purchase Restriction Policy

The table shows the OLS estimation results of our baseline model, $Window\ 60_{i,j,t} = \alpha + \beta Official_i + \gamma_1 X_{i,t} + \gamma_2 Z_{j,t} + \varepsilon_{i,j,t}$. The dependent variable is a dummy variable for the 60-day window before the second policy announcement. The key independent variable is an indicator variable, Official, which equals one if the purchaser is a government official and zero otherwise. Other variables are as defined in the note to Table 1. Robust standard errors are reported in the parentheses below the parameter estimates. ***, ** and * represent the significance levels at 1%, 5% and 10%, respectively.

	(1)	(2)	(3)
Official	-0.0380*	-0.0094	-0.0060
	(0.0224)	(0.0232)	(0.0233)
Individual Characteristics		Yes	Yes
Transaction Characteristics			Yes
Observations	11,195	11,076	11,076
R-squared	0.0002	0.0153	0.0210