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ABSTRACT

This paper examines the impact of a policy change in Tobin's tax on housing market speculators. The policy intervention effectively raised the transaction cost in the market segment with a high presence of speculators. Relative to the unaffected control sample, we find that the rise in transaction cost substantially reduced speculative trading activities in the treatment sample. However, it significantly raised its price volatility and reduced the price informativeness. We further show that the unintended consequences are likely due to a relatively greater withdrawal by informed speculators than by destabilizing speculators after the transaction cost increase.

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1. Introduction

The 2008 Global Financial Crisis, triggered by the collapse of house prices in the U.S., brings a renewed interest on the role of speculators in fuelling housing markets' boom and bust. Earlier, Case and Shiller (1989) suggest that the housing market does not appear to be efficient. Some explicitly argue that home buyers are investors with backward-looking expectations; their behaviour helps explain the surge of price-rent ratios in the housing boom periods (Case, Quigley, and Shiller, 2003; Mayer and Sinai, 2007; Piazzesi and Schneider, 2009). Based on a survey sample in four U.S. cities, Case, Shiller, and Thompson (2012) report that home price expectations, which reached abnormal levels relative to the mortgage rate at the peak of the boom and have declined sharply since, were highly correlated to the past price movements of the housing market. Cheng, Raina, and Xiong (2013) find that mid-level managers in securitized finance business continue to speculate on house prices in their own home purchases in the boom period. Using transaction-level data, recent studies find supportive evidence of inexperienced housing speculators chasing short-term trends (Bayer, Geissler and Roberts, 2011) leading to price overreaction (Fu and Qian, 2013). Haughwout, Lee, Tracy, and Klaauw (2011) use unique credit report data to show the important role speculative investors play in contributing to the rise and fall of the U.S housing market in the recent crisis.

Indeed, behavioural finance suggests that "noise traders", broadly defined as investors who trade for non-fundamentals-related reasons, drive prices away from fundamentals and cause excess volatility (Cutler, Poterba, and Summers, 1990, 1991; De Long, Shleifer, Summers; and Waldmann, 1990a, 1990b; Shleifer and Summers, 1990; Hong and Stein, 1999). This naturally calls for policy interventions to curb destabilizing speculation. A transaction tax, often referred to as Tobin's tax, is viewed as an effective way to dissuade speculators, reduce volatility, and thus promote price stability (Tobin, 1978; Stiglitz, 1989; Summers and Summers 1989).

Opinions, however, are divided. Friedman (1953) argues that rational speculators help stabilize prices. The financial economics literature cautions that a transaction tax can deter informed traders, who are essential players in promoting informational efficiency and price stability in the market (Schwert and Jones, 1993). Subrahmanyam (1998) shows theoretically that, in the context of the stock market, a transaction tax can increase stock price volatility by

discouraging informed traders from acquiring information. Clearly, transaction tax is a double-edged sword – it can deter both informed and uninformed speculation.

Nevertheless, transaction tax has been utilized multiple times in recent years in order to "cool" the housing market, particularly in Asia (e.g., Singapore and Hong Kong). Anecdotally, the policy might indeed slow down price inflation in these markets. However, the debate over the effectiveness of such a tax suggests that we cannot be sure of the impact on volatility and price informativeness, which are important considerations. Investigation of these transaction tax effects in the housing markets is very sparse. Empirical evidence on these impacts of a Tobin's tax is usually based on financial market data, which does not easily generalize to the housing market. Moreover, there is little empirical information on the differential deterrence impact of transaction tax on noise vs. informed speculators.

In this paper, we study the impact of a policy induced rise in transaction cost (a Tobin's tax) on housing market speculators. Our study takes advantage of a natural experiment due to a policy intervention in Singapore in December 2006. Singapore has a highly developed economy and a well-functioning real estate market; data of all housing market transactions are publicly available with reliable quality.[1] More importantly, the nature of the policy intervention particularly suits the objective of our study—identifying the effect of Tobin's tax on housing market speculators and the associated mechanism. Specifically, aiming to curtail housing speculation, the policy raises the transaction cost in a sub-market with a high presence of speculators. With the unaffected submarket as a control group and using difference-in-differences regressions, we can explore the transaction tax's impact on trading volume, speculative trade, price volatility and informativeness while controlling for the general market conditions. Moreover, within the treatment group we can gauge the cross-sectional variation in the relative presence of informed vs. noise speculators. Thus, we can differentiate the policy's impact on price volatility according to the prevalence of informed vs. less informed speculators.

[1] Singapore is often ranked among top cities in global real estate investment ranking. For example, it was ranked 5th in the world in 2011 in terms of direct real estate activity according to Jones Lang LaSalle, just behind London, New York, Paris and Tokyo.

Our findings are as follows. (i) The heightened transaction cost is associated with a significant drop in trading activity; in particular, speculators are deterred. The decrease in speculative trading in the post policy year is equivalent to 75% of the pre-policy level. (ii) The decline in trading is associated with an 18% rise in price volatility. (iii) These results are particularly prominent in locations that attract informed speculators. (iv) Consequently, the affected market exhibits a decline in price informativeness. Our findings are robust against alternative specifications of the post-policy window and volatility measures as well as against the potential measurement error in our speculator identification. In addition, our results do not appear to be driven by an anticipation of the policy change. Amidst the increasing evidence on speculators' role in the recent crisis, our evidence nevertheless cautions against the use of a transaction tax in curbing speculators in housing markets: heightened transaction tax might deter the "good" speculators disproportionately more, leading to unintended consequences.

While our empirical work is about the effect of transaction tax on housing market transactions and price volatility, our result could be broadly related to the literature on transaction tax in other financial markets. After the global financial crisis, implementation of a financial transaction tax draws much attention to regulators in many countries.[2] In the literature, Umlauf (2003) shows that a transaction tax in the 1980s did not reduce price volatility in the Swedish stock market. Others (e.g., Roll, 1989; Jones and Seguin, 1997; Hau, 2006) show that raising transaction cost leads to either no change or a rise in volatility. Furthermore, Bloomfield, O'Hara and Saar (2009) find in a controlled experiment that such a tax deters informed and uninformed traders equally. The economic mechanism documented in our paper, albeit in the much less liquid and higher transaction cost housing market, complements the literature suggesting that transaction tax could end up discouraging informed speculators disproportionately more than others resulting in lower price stability and informativeness.

[2] On December 11th 2009, Financial Times reported that "European Union leaders urged the International Monetary Fund on Friday to consider a global tax on financial transactions in spite of opposition from the U.S. and doubts at the IMF itself". The U.S. Senator Tom Harkin and Representative DeFazio followed the pursuit and proposed bills in November 2011 to impose a transaction tax on financial firms. France becomes the first European country to impose a transaction tax on August 1st 2012.

The rest of the paper proceeds as follows. Section 2 discusses the policy experiment and empirical methodology. The data and sample statistics are described in Section 3 and the empirical findings are reported in Section 4. Section 5 discusses alternative interpretations and performs additional robustness checks. Second 6 concludes.

2. Empirical design and methodology

2.1. Market background and policy experiment

In Singapore, private condominium properties (known locally as non-landed properties) in new development projects are launched for sale before project completion (typically before the commencement of construction).[3] Like properties in completed private condominium projects, the ownership of these uncompleted properties, called presale contracts, can be freely traded and are sought later by homebuyers as well as by investors[4] (The Appendix provides additional details of the residential market background in Singapore).

The presale market is more attractive to short-term speculators than the spot market for completed properties because a presale contract has a lower holding cost than a completed property. Figure 1 highlights the difference between owning an uncompleted property vs. owning a completed property; the former does not require full payment until completion, management fees, maintenance costs, and property taxes. Because of these differences, speculators in the presale market have a strong incentive to exit the market before project completion.

[Insert Figure 1 about here]

Short-term speculators are called *flippers* in our sample — those who buy and subsequently sell before project completion. In our sample, speculators have a short investment horizon: they hold their investments for about 24 months on average, less than half the average holding time for a spot market purchaser (i.e., those who buy completed properties). Admittedly, flipping could be affected by unexpected changes in price trends or unexpected changes in

[3] Condominium residential projects in Singapore range in size from a few dozen units to over a thousand units and their construction period lasts around 3 years.

[4] The buying and selling procedures, including the incurrence of tax and various fees, are typically the same for presale and spot market transactions.

personal financial circumstances so that identifying flippers as short-term speculators can be noisy. We discuss the robustness of our speculator identification in section 5.

The policy intervention we study is the Singapore government's announcement on 15 December 2006, with immediate effect, to withdraw a stamp duty payment deferral in the presale market. Home buyers in Singapore typically pay a stamp duty (i.e., a transaction tax) of 3% of the full transaction price at the time of purchase. Previously, as a part of the various policies to counter the impact of the economic slowdown triggered by the Asian Financial crisis, the government in June 1998 gave concession for presale buyers to defer stamp duty payment until project completion or until the property was sold before completion.[5] The concession encourages short-term speculation because it allowed speculators to finance their stamp duty from the sale proceeds when they eventually sell their properties before project completion. By the same token, the withdrawal of the deferral raises the upfront purchase cost for these speculators, effectively raising their transaction costs.[6] The presence of the parallel affected presale and unaffected spot markets for condominium properties offer an opportunity to apply a difference-in-differences approach to identifying the policy's impact.

It is important to note that the policy does not directly affect the monetary cost for house purchase (except for the time value of money). However, the requirement to pay 3% stamp duty in cash at the time of purchase is a significant up front capital constraint for investors. This will reduce investors' incentive to purchase in the presale market, as previous research shows that the demand for housing is sensitive to cash-on-hand (Adams, Einav, and Levin, 2009; Ben-David, 2011). In our context, compared to the 10-20% down payment requirement and zero capital gains tax in Singapore, the 3% buyer stamp duty represents a significant *up front* capital constraint for investors in the presale market.[7] Moreover, the policy could be interpreted as a signal that the government intended to discourage short term speculation.

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[5] The government undertook other measures to stimulate the economy after the 1997 Asian Financial Crisis. Therefore, we do not use the introduction of the policy to study the impact of transaction tax reduction.

[6] Old pre-sale contracts are "grand-fathered" so that the holders did not have to immediately pay stamp-duties until project completion or re-selling of their contracts.

[7] Brokerage commission is typically 1-2% of the transaction price in Singapore and only sellers incur this transaction cost. Therefore, the 3% buyer stamp duty is the single and an economically significant item of monetary transaction cost buyers pay at the time of purchase.

2.2. Empirical methodology

Our empirical analysis involves several steps. Using Dec 2006 as the event month, we first investigate the impact of the withdrawal of the stamp duty deferral on project turnover and particularly on turnover by to flippers. We then study the policy's impact on price volatility. We define '*project turnover*' as the number of transactions in a condominium project in a given month scaled by the project size (the total number of units in that project). '*Speculative turnover'* is the purchases by flippers, who sold the purchased property before project completion, scaled by the project size.

We estimate price volatility based on observed transaction prices. To remove the price variability due to heterogeneous property attributes, we first adjust the raw transaction prices for the market price trend, project fixed effects, and property unit hedonic attributes using the hedonic pricing regression (Rosen, 1974); the pricing equation and the estimates are reported in the Appendix. The regression residuals, ε_{ij}^{t}, measure prices specific to each transaction of property unit *j* in condominium project *i* in month *t* after adjusting for market conditions and hedonic factors. Averaging over *j*, $\bar{\varepsilon}_{i}^{t}$, in month *t* reflects the market- and hedonic characteristics-adjusted price in project *i* in the month. The monthly change in this average is used to represent project-specific monthly return. We estimate project *i*'s price volatility in month *t* according to the range of the pricing errors within the project, *i.e.*, the difference between the highest and the lowest ε_{ij}^{t} within project *i* in month *t*.[8] A missing value is assigned for project-months with fewer than two transactions.

We seek to identify the policy's effects on the presale market's trading activity and price volatility relative to the spot market through difference-in-differences analyses as in the following generic form:

$$y_{it} = \alpha + \beta_1 WD + \beta_2 Presale_{it} + \beta_3 WD \times Presale_{it} + \beta_4\, controls_{it} + \mu_{it} \qquad (1)$$

[8] Existing studies have shown that the price range estimator is a more efficient estimator of the volatility (e.g., Parkinson, 1980).

where the dependent variable y_{it} represents monthly project turnover, or speculative turnover, or measures of price volatility in project *i* in month *t*; *Presale*, is a binary variable equal to 1 for projects in the presale market and 0 for projects in the spot market, and *WD* has a binary value 0 before the policy intervention and 1 afterward. Thus, by specification the presale market is the treatment group and the spot market is the control group. The coefficient β_1 measures any general changes in property market conditions correlated with the policy intervention. β_2 measures the effect associated with the treatment group before the policy change. The coefficient β_3 is the main variable of interest – it measures the differential effect of the policy change on the treatment group relative to the control group. The macroeconomic conditions as well as the policy shock may affect all similar condominium projects at the same time, e.g., those with the same type (presale or spot) and in the same location. Therefore, we cluster the standard errors to allow the regression residuals to be correlated within the same location (i.e., central vs. non-central region)[9] among all presale (or spot) condominium projects in a given month.[10]

3. Data and Sample Statistics

3.1 Data sources

The data for this study are obtained from Urban Redevelopment Authority (URA) REALIS database, which reports all property transactions lodged with the Singapore Land Authority (SLA). Our sample excludes transactions in private non-landed projects with fewer than 40 units (to avoid illiquidity concerns) and properties bought out for redevelopment (en bloc sales). The entire available sample, comprising more than 181,000 transactions observed between 1995:01 and 2010:10 in 854 non-landed projects, is employed to estimate the hedonic pricing model. Approximately 55% of all transactions are presale transactions (of uncompleted properties in new condominium development projects). The 854 projects range in size from 40 to 1,232 property units, with an average size of 184 units. A project's completion is dated by the receipt of a Temporary Occupancy Permit (TOP) from the government.

[9] In Singapore, the central location is the prime region for residential real estate, and condominium projects in the central location differ from the non-central projects in both project characteristics and investment demand.

[10] As a further robustness check (e.g., to account for autocorrelation of dependent variables), we use the bootstrap method to compute standard errors (Bertrand, Duflo, and Mullainathan, 2004). To save space, we do not report the results but the inference of our main findings remains to hold.

For each transaction, we observe the following: transaction date, transaction price, transaction type (new sale by developer, resale of presale contracts before completion, or resale after completion), buyer attributes (whether previously residing in a government Housing Development Board flat or at a private residential address), property attributes (project identity, building block, floor level, and living area) and project attributes (project size, location by postal district, completion date and land title[11]). We also obtain the monthly CPI and interest rate from the SingStat Time Series compiled by the Singapore Department of Statistics. We deflate the transaction price using the published CPI. Interest rates are used to estimate the equilibrium compensating price differential between spot and presale prices.

3.2 Comparability of the treatment (presale) and control (spot) samples

Before conducting the difference-in-differences analysis of the policy impact, we compare the spot market (control group) and presale market (treatment group) in several respects. In general, properties in the two markets are similar although units in the presale markets tend to be somewhat smaller and more expensive. Table I shows that the average size of the transacted unit between 1995 and 2010 is 1,377 square feet in the entire market and 1,302 square feet for the presale market. The average transaction price is US$505 per square foot (in real terms) for the spot market, and US$565 in the presale market.[12] Projects size (i.e., the number of units in a condominium project) in presale markets also tends to be larger (149 in the spot sample versus 204 in the presale sample).

[Insert Table I about here]

We further evaluate the comparability of the spot and presale markets in the pre-policy period. Figure 2 shows the kernel density plots, based on the Epanechnikov kernel function[13], for several measures at the monthly frequency for both markets during the one-year period before the policy intervention (i.e., 2005:11-2006:10). The similarity of the distributions in project size, geographic location of condominium projects, and average monthly transaction price indicate that

[11] In Singapore, land title can be permanently, a 999 years lease, or a 99 years lease.
[12] For the convenience of readers, we report the price information in the text in US dollars by using the concurrent exchange rate. The analyses in the paper, for example volatility measures, are based on the price in local currency.
[13] The result is robust to different choices of the kernel density functions and the bandwidth.

the treatment (presale) and control (spot) groups are balanced in distribution along those observables in the pre-policy period. We also compare the distribution of the project-level price volatility, one of our key policy outcome variables, between the two markets in the year before the policy intervention. Our range estimates of the (monthly) price volatility average 15% and 12% for the presale and spot samples, respectively. The kernel density plot shows that the distributional patterns are well matched between the two samples.

[Insert Figure 2 about here]

3.3. The sample statistics around the policy intervention

Table II provides the summary statistics for trading activity one month before (2006:11) and one month after (2007:01) the policy intervention. The mean turnover (the number of monthly transactions scaled by the project size) for the spot market is 0.77% in November 2006 and 0.71% in January 2007. The change in trading activity before and after the policy intervention is only -0.07% and is statistically insignificant, consistent with that the withdrawal of the tax concession does not affect the spot market.

[Insert Table II about here]

In contrast, the mean turnover for presale projects experienced a significant post-intervention drop of 4.3% (statistically significant at the 1% level), an almost two thirds reduction relative to the pre-intervention average turnover. Moreover, speculative turnover drops 1.66%, a 70% reduction relative to the pre-intervention level (statistically significant at the 1% level). The drop in turnover is unlikely associated with seasonal variation in trading activities between calendar months. Panel C of Table II shows that the difference between November project turnover and January project turnover from 1995 to 2010 (excluding 2006:11 and 2007:01) is statistically and economically indistinguishable from zero in both the presale and spot markets.

4. Empirical Analysis

4.1. Impact on speculative trading

We study the trading response to the withdrawal of the stamp duty deferral (*WD*) using the difference-in-differences method in a regression framework. To control for heterogeneity

across condominium projects, we include as explanatory variables *project size* and *Central* location dummy. We also include the average turnover between 2006:08 and 2006:10, *pre-policy turnovers*, to control for differences in trading across projects before the policy intervention. In the regression framework, we analyse the trading and volatility response in the 6-months period after the policy shock to identify the shorter-term effect, and study the longer term policy impact using a 12-month post-policy window. Housing transactions often need considerable lead planning time so that trading may have an apparent mechanical momentum. The policy impact might thus take some months to fully show up in market data. In addition, the exercise allows us to examine the persistence of the policy effects, if any.

The first three columns of Table III report the regression analyses of project turnovers one month before and six months after the policy intervention in December 2006. The first column shows that monthly project turnover is in general 5.5 percentage points higher in the presale market than in the spot market before the policy, consistent with the summary statistics shown in Table I. The critical result is that the presale market sees a sharper decline in turnover after the policy intervention (indicated by the coefficient of *WD*×*Presale*). On average, the turnover for presale projects drops 3.5 percentage points more than that in spot market projects, and the difference is statistically significant at the 1% level. The estimated 3.5 percentage point drop in trading implies a reduction of total trading by 51% (compared to 6.9 percentage points in November 2006 as shown in Table II) in the presale market during the six months after the policy shock.

[Insert Table III about here]

In Column 2 of Table III, we investigate the impact of the concession withdrawal on identified speculative trade in the presale market. The speculative turnover in the treatment sample, compared to the change in project-level turnover in the control sample, declines by 1.6 percentage points one month after the policy intervention. The effect is statistically significant at 1% and is equivalent to a 67% reduction in speculative trading (compared to 2.4 percentage points in November 2006 as shown in Table II) in the presale market, an effect that is larger than the average trading reduction (Column 1).

Column 3 of Table III shows that the drop in total turnover in the presale market after the policy intervention is more pronounced in projects with a high presence of speculators just before the policy. We note that before the policy change speculative turnover is strongly serially correlated (the monthly autocorrelation coefficient is 0.33). We therefore use the average speculative turnover ("*pre-policy speculative trading*") for each presale project between 2006:08 and 2006:10 (which are two to four months prior to the policy changes) to proxy for the presence of short-term speculators and interact it with the *WD* dummy. The results in Column 3 show first that locations that used to have high speculative trade indeed have higher total turnover. Very importantly, they confirm that the decrease in trading activity in the presale market is increasing in the presence of speculators. The presale condominium project associated with 1 percentage point greater speculative turnover before the policy experiences an additional 0.34 percentage point decrease in the total trading activity after the policy intervention. This result further suggests that the policy has a particularly strong impact on short-term speculators.

We further examine the longer term policy effect by extending the event window to twelve months after the policy implementation. Columns 4 to 6 show results similar to those obtained using a six-month post-event window. Interestingly, the magnitudes of reduction in total turnover or speculative turnover become larger in the 12 month period. We perform further analysis by separating the trading response in the first 6 months from that in the second 6 months in one single regression, and formally compare the size of the trading response using F-tests (see Appendix for details). The results indicate that the average monthly trading response (measured by total turnover and speculative turnover) is stronger in the second 6 months, and the effect is statistically significant at the 5% level. These results suggest that the effect of transaction tax on housing markets comes with a lag but is reasonably persistent.

4.2. Impact on price volatility

Having shown that the policy effectively reduces turnover in presale projects, we now investigate its impact on price volatility. We estimate project level price volatility by the pricing error range within individual projects. Again, we study the response in price volatility six months and twelve months after the implementation of the policy.

Marsh and Rosenfeld (1986) show that the range estimator is biased downward for thin trading. We therefore include monthly transaction volume as a control variable in our regressions. To the extent that the policy reduces transaction volume in presale projects, the resultant downward bias will be against finding policy induced volatility increase in presale projects. We will discuss in more detail on the robustness of our volatility measure in section 5.

[Insert Table IV about here]

Table IV reports our difference-in-differences regressions comparing the impact of the policy change in price volatility between the presale (treatment) and spot (control) markets. Columns 1 and 2 report the comparisons within the six months after the policy intervention and Columns 3 and 4 report the within twelve months comparisons.

Column 1 in Table IV shows that there is no significant difference in the change in price volatility between the presale and spot markets over the six months after the policy intervention. Column 3 shows that the change in price volatility in the presale market is significantly greater than in the spot market over the twelve months after the policy intervention. Compared to the average monthly price volatility in the pre-policy period (15%), price volatility increases by 18% in the presale projects. Note that the policy intervention, as shown in Table III, is associated with notable decline in turnover in the pre-sale market but not in the spot market.

To capture the role played by speculative traders, we again introduce as an explanatory variable the proxy for the prevalence of speculative trade: "*pre-policy speculative trading"* (which is the average speculative turnover in a presale project during the three months before the policy (2006:08-2006:10)). Both Columns 2 and 4 show that the proxy attracts a negative and significant coefficient; that is, a greater presence of speculative traders is associated with a lower price volatility before the policy intervention. In Column 2, the cross term between the proxy and the dummy indicating the post event months is positive but insignificant while in Column 4 it is significantly positive. Thus, markets that used to attract more speculative traders experience a greater rise in price volatility after the policy intervention.

We examine why volatility increase is stronger and statistically significant only in the longer term. First, we note that our volatility estimates are downward biased, so the true volatility

response could very well be positive and significant for both post-event windows. Moreover, we study the volatility response in the first 6 months and in the second 6 months in one single regression (see Appendix for details). F-tests suggest that, during the 12 month period after the policy shock, volatility increase is much higher in the second 6 months than in the first 6 months, and the difference is statistically significant at the 1%. This is consistent with the finding on the dynamics of the trading response. The (speculative) trading activity drops more strongly in the second 6 months after the policy shock, during which period we also observe the stronger volatility increase. Taken together, these results suggest first that the policy effect takes time to show up in the housing market. Yet, over time the decrease in trading after the policy intervention, particularly the withdrawals of speculators, causes the price volatility to increase in the affected presale market.

4.3. Impact on informed vs. noise speculators

Findings in Table III and Table IV present evidence that while a transaction tax deters speculative trade, it may raise rather than reduce price volatility. Note that if the policy had deterred noise speculators, the price volatility would have declined. Perhaps, the policy also deters informed speculators.

To evaluate the possibility, we exploit the potential variations in the relative presence of informed speculators vs. noise traders across presale projects in our treatment sample. There is an asymmetry in informed arbitrage trade in housing markets: informed speculators can benefit from their information advantage by buying under-priced housing units but cannot easily do so by shorting over-priced housing units. This asymmetry implies that there is a higher proportion of informed speculators in previously under-priced than previously overpriced projects. The cross-sectional variation enables us to disentangle the policy's impact on trading volume and price volatility via discouraging informed versus discouraging noise speculators.

To identify under- and over-priced presale projects (as a proxy of the presence of informed speculators), we conduct the following. First, we obtain $\bar{\varepsilon}_i^t$, the average for each month a presale project's unit level hedonic pricing residuals ε_{ij}^t. Based on the distribution of these

averages in month *t*, a project *i* is "underpriced" in month *t* if $\bar{\varepsilon}_i^t$ is in the bottom 30% of the distribution and "overprice" if it is in the top 30%.[14]

Compared to other presale projects (in particular the over-priced presale projects), underpriced presale projects tend to be larger in size and are farther away from project completion. Within our entire sample between 1995 and 2010, underpriced projects tend to attract more speculators in the subsequent month, *cerates paribus*. Price recovery seems to be slow; on average 26% of the underpriced projects remain in the bottom 30% price distribution six months later. However, those underpriced projects with more speculators (i.e., their speculative turnovers are in the top 30% among all underpriced projects) are 20% less likely to stay in the bottom tercile of the price distribution six months later. This adds credence to the hypothesis that underpriced projects are likely to attract a greater proportion of informed investors.

In Table V, we examine the differences in responses to the policy change among presale projects that were under- or over-priced in October 2006, the month immediately before our event window (2006:11 and 2007:1 to 2007:6). Our focus is on the changes in speculative turnovers and price volatility. To facilitate interpretation, we keep for the treatment group (the presale projects) only the under- and over-priced presale projects and compare them to the control group (spot market projects).

[Insert Table V about here]

Columns 1 and 2 of Table V show that in November 2006, the month before the policy intervention, the projects under-priced in Oct 2006 have a relatively higher speculative turnover than the overpriced presale projects as suggested by the comparison between the coefficients on *Underpricing Dummy* and *Overpricing Dummy* (an F-test of their difference is significant). This is consistent with the idea that underpriced projects attract informed speculators. Most interestingly, being in one of the "previously" underpriced presale projects is associated with an additional 0.8-1.1 percentage points decrease in speculative turnover, after the policy intervention,

[14] We have also experimented with the 20% and 80% cut off in identifying the underpriced and overpriced presale projects, and obtain the same results. Furthermore, to address the look-ahead bias, we also use an alternative underpricing measure based on the project-specific price, ε_{ij}^t, estimated from the transaction data before November 2006 (out-of-sample). Results in Table V remain qualitatively the same.

than being in one of the previously overpriced presale projects. The F-test for the difference is significant. Compared to the average monthly speculative turnover of 2.4% among all presale projects in the month before the policy change, this effect is economically large. On the basis that informed speculators are more prevalent in underpriced projects and that noise speculators are concentrated in overpriced projects, these results suggest that the transaction tax may have a stronger deterrence effect on informed traders than on noise traders.

Our result can be consistent with that presale projects underpriced in 2006:10 had attracted speculators in 2006:11 and thus fewer speculators in 2007:01 and afterwards as the adjustment process continues. We do not believe this is the driver of our regression result. First, as we noted earlier, price correction is sluggish; about 26% of underpriced projects remain underpriced over six months. Second, we perform a placebo regression test. In exactly the same way as before we identify underpriced and overpriced presale projects in October for all other years (1995-2010 except 2006). We designate the subsequent November as the pseudo-pre-policy month; for projects underpriced in October, speculative turnover typically increases in November. We also designate the subsequent January to June as the pseudo-post-policy months. Using speculative turnover as the treatment sample and spot market turnover as the control sample, we replicate the regression in Table V Col. 1. We find that speculative turnover in underpriced projects is neither statistically nor economically significantly changed in the post-policy months (i.e., the cross term between WD and underpricing is only slightly positive and insignificant). The cross term between WD and overpriced presale projects behaves similarly. These observations support our interpretation of the result in Table V: the policy change, rather than other factors, reduces the participation of speculators, especially informed speculators in the market.

Next, we study the price volatility response after the policy implementation among the underpriced and overpriced projects in the presale market. Note that a bigger drop in informed speculators should be associated with a higher volatility increase. Results in Columns 3 and 4 of Table V, Panel A show revealing evidence. First, on average, presale projects underpriced in 2006:10 in the treatment sample exhibit lower price volatility in the month before the policy intervention than the control sample (2006:11), in line with the expectation that informed traders aid pricing efficiency in these projects. Second, during the six and twelve months after the policy

intervention, these underpriced projects experienced a price volatility increase that is 11.5-11.8 percentage points greater than the overpriced projects (the F-test rejects the hypothesis of zero difference at the 1% level). Taken together, these results are consistent with the view that the transaction tax disproportionately deters informed speculators and exacerbates price volatility.

We conjecture that the policy has a stronger deterrence effect on rational informed investors than noise traders because informed investors are more sensitive to the change in expected return due to the policy change. The policy shock effectively increases the transaction cost for presale investors by tightening the capital constraint, which equally affected informed and noise investors. However, the policy shock might also indicate an increased likelihood of further government intervention to dampen housing market growth. Rational informed investors are more responsive to such a change in expected speculative returns than noise traders.

4.4. Impact on price informativeness

If the policy change, which raises speculators' transaction cost, indeed disproportionately deters informed speculator, price will become less informative. We now investigate this possibility.

Given the low transaction frequency in the real estate market, we are not able to use the conventional price informativeness measures. Kyle (1995) suggests that a higher level of information asymmetry implies a greater impact of trades on prices. In our context, the withdrawal of informed speculators after the policy change would result in lower information acquisition and greater information asymmetry in the market. Consequently, the same turnover, on average, should have a bigger impact on the price movement compared to the pre-policy period. We exploit this economic intuition to construct an empirical price informativeness proxy. To operationalize, we follow Amihud (2002) and use the absolute monthly project-specific return (i.e., the monthly change in the average of project i's project-specific price $\bar{\varepsilon}_i^t$) divided by

monthly project turnover to measure negative price informativeness (Amihud measure, hereafter).[15]

To validate the measure, we examine whether the presence of speculators is associated with a lower Amihud measure (more informative prices) for presale projects over the entire sample period in our dataset (1995:01–2010:10). In an unreported analysis, we run a cross-sectional regression of presale projects' Amihud measures (854 of them) on the monthly share of speculative purchases (as a proportion of monthly transactions in a project), the monthly turnover rate of each project, and other control variables. We find a strong negative relation between the Amihud measure and the average share of speculative purchases.

[Insert Table VI about here]

Table VI reports the change in price informativeness before and after the policy intervention across the condominium projects, showing the policy effect over a six-month and twelve-month period, respectively. In both Columns 1 and 2, the coefficients of the presale market dummy are significantly negative, suggestive of more informative prices in the presale market in general. The Amihud measure for the presale projects significantly increased relative to the spot market projects during the six-month and twelve-month period after the policy intervention that targeted only speculators in the presale market. The results confirm that the relative price informativeness in the presale market is reduced (resulting in a higher Amihud measure) after the policy intervention.

Overall, these results are broadly consistent with the notion that informed traders are well represented among speculators and their withdrawal from the market results in a loss of price informativeness. A transaction tax, thus, appears to be a double-edged sword, deterring both noise traders and informed traders. It may well bring about the inadvertent effect of exacerbating price volatility by disproportionately deterring the latter.

5. Robustness

[15] Following Amihud (2002), we winsorize the measure at the top and bottom 1% tail, and our results remain robust without the winsorizing.

Our analysis is built on two critical proxies: a proxy for speculative trade and a proxy for price volatility. We therefore first discuss the robustness of our main results with respect to these proxies. Another concern is whether investors anticipated the timing of the policy intervention and how it may affect our results. To save space, while we present the key robustness test results below, we report results of most robustness checks in the Internet Appendix.

5.1. Potential error in the measurement of speculative activities

We proxy for speculative trading by flipper purchases; that is, purchases in presale projects that were eventually sold before project completion. While such a transaction can be speculative, it is possible that genuine homebuyers and long-term investors can exit the market before project completion due to unexpected changes in family and financial circumstances cause re-selling of a property before completion (TOP). Such happenstances, however, are random across time and project location and should not systematically affect our results.

We are, however, concerned with projects in which speculators may be systematically under- or over-represented by the observed flippers. For example, genuine presale homebuyers may sell to take a profit when the market experiences a large and unexpected positive rise in demand before project completion. Likewise, an intentional speculator can be forced to hold an investment beyond completion to avoid realizing losses due to unexpected market downturn.

To address the problem, we restrict the analysis to a subsample of projects that were completed during stable market conditions, for example, we should exclude periods in an extreme economics situation, such as the run up period before the 2008 financial crisis and the dramatic decline after the crisis. Operationally, we exclude projects whose last three months before completion fell in the period when the benchmark price index was in the top or bottom 30% in between 1995 and 2010 (reported in the Internet Appendix) and repeat the regressions in Table III. We find consistent results: the policy intervention discouraged speculative trading in the presale market.

5.2. Robustness of the volatility analysis

Error in our volatility measurement would be large when the transaction frequency is low. This concern is specifically relevant for presale projects, which experience a significant decline

in trading activities after the policy intervention. We partly mitigate the estimation bias in the coefficient estimate by including the number of transactions as a control variable in the price volatility regressions (Table IV).

Larger measurement errors in project-months with fewer transactions also imply that the regression residuals are heteroskedastic. We further address this concern using a weighted least squares specification in the volatility analysis. In the first stage, we run the OLS regression as shown in Tables III, IV and V, and obtain the regression residuals. We regress the square of the residual on the number of transactions, the number of transactions squared, and project size. Then we re-estimate the second-stage OLS with the predicted residual squared as the regression weight. This specification corrects for the heteroskedasticity. Consistent with previous findings, there is a strong and significant increase in project price volatility in the presale market during the six-month and twelve-month horizon after the policy intervention (reported in the Internet Appendix).

We also partition the sample by project size to perform the price volatility analysis on more homogeneous sub-samples of condominium projects; the dividing size is 204 units (mean in our presale sample). Projects of similar size are more comparable in transaction volume. We perform the above weighted least squares regression on project price volatility as in Table IV. The price volatility increase is stronger and more significant among the larger condominium projects (see Internet Appendix). As there tends to be fewer transactions, on average, in smaller projects, this finding further suggests that our previous result is not due to errors in measuring price volatility, especially in the project-months with a low transaction volume.

Next, we note that our volatility measures are derived from the pricing residuals in the hedonic pricing regression, for each project the price volatility is the range of the residuals. However, a greater range may reflect a poorer fit of the hedonic pricing model rather than the true price volatility. For example, completed properties may have more heterogeneous value-relevant information, such as maintenance and unit condition, that are not captured in the hedonic regression. The result is more heterogeneous price residuals in the spot market (relative to the presale market) transactions.

Table A_I in the Appendix shows the results for the hedonic pricing regression for both the presale market and the spot market. Our hedonic pricing model explains, on average, 94% of

the variation in transaction prices. The presale and spot transaction prices are equally well explained by the pricing model: the R-squared is 94.1% for the presale market transactions and 94.4% for the spot market transactions. The negligible difference suggests that nuances in the behavior of the estimated price volatility for the presale and spot market are likely not driven by uneven fit of the underlying hedonic price regression. We perform an additional robustness check on the price volatility analysis by dropping the spot market projects that are more than 20 years old. Our main results continue to hold when the more recent spot market project are used as the control group.

We further check the robustness of our volatility results by using an alternative volatility measure. A natural choice is the return volatility, defined on a rolling basis as the standard deviation of the monthly project-specific return for the most recent six months. Again we find that return volatility is smaller in the presale market before the policy intervention; and it significantly increases as well after the policy intervention (see the Internet Appendix). We also apply the weighted least squares approach and the results are qualitative the same.

5.3. Is the timing of the policy intervention anticipated by market participants?

An alternative interpretation of our main results is that investors anticipated the timing of the policy intervention and rationally advanced their investment before the transaction tax takes effect, resulting in subsided speculative activities immediately after the policy intervention. We first note that the reduction in trading and the increase in volatility after the policy intervention are not transitory; the effects remain strong for a period of six to twelve months. When we exclude the month immediately after the policy is enacted (Jan 2007) and repeat our turnover and volatility regressions, our results remain as significant both statistically and economically. It is therefore unlikely that our findings are driven by a temporary elevation of speculation during the month before the policy intervention. This is not surprising–after all, the withdrawal of the buyer stamp duty deferral concession was announced with immediate effect without prior public debate and it is unlikely for the date of withdrawal to be anticipated.[16]

[16] We also searched for news reports related to the buyer stamp duty in the major newspapers in Singapore, and we find no coverage of any discussion of the policy intervention prior to the government announcement.

6. Conclusion

Our study provides the first piece of evidence on the impact of a Tobin's tax on speculative trading activities and price volatility in housing markets. We explore a policy intervention in the private residential market in Singapore, where condominium properties are traded both in the (forward) presale market and in the (spot) resale market; the former is particularly attractive to speculators. The Tobin's tax policy change raises a buyer's transaction cost in the presale market but does not directly affect the spot market, which serves as a control group. Difference-in-differences regression analyses show that the transaction cost increase significantly deters speculative trading and raises price volatility in the presale projects.

Further investigation into sub-markets reveals that the increase in transaction cost deters informed traders more than other traders. Because of difficulties in short-selling presale contracts, underpriced presale market segments attract informed speculators but overpriced presale market segments do not. We find that while the policy intervention generally reduces trading in presale projects, the drop is more pronounced in presale projects that are relatively underpriced shortly before the intervention. In addition, the increase in price volatility is concentrated in these underpriced presale projects. In contrast, previously overpriced presale projects experiences a smaller reduction in speculative trading and little volatility change afterwards.

These findings indicate that informed traders are more sensitive to the heightened transaction tax than other traders. Accordingly, we find that after the increase in transaction tax price informativeness drops in the affected presale housing market. All of our findings are robust against alternative measures of volatility and speculative activity.

Overall, our findings caution against increasing transaction tax to stabilize housing markets. While admitting that housing markets are different from high trading frequency asset markets, we speculate that our results may be broadly relevant for the global debate on the transaction tax in financial markets. As far as the objective of promoting asset price stability and informativeness is concerned, these taxes and regulatory measures are not necessarily effective and could even be counter-productive. In most cases, speculative trades comprise informed and noise trading. A transaction tax deters both; our results raise the possibility that the former may be more affected than the latter.

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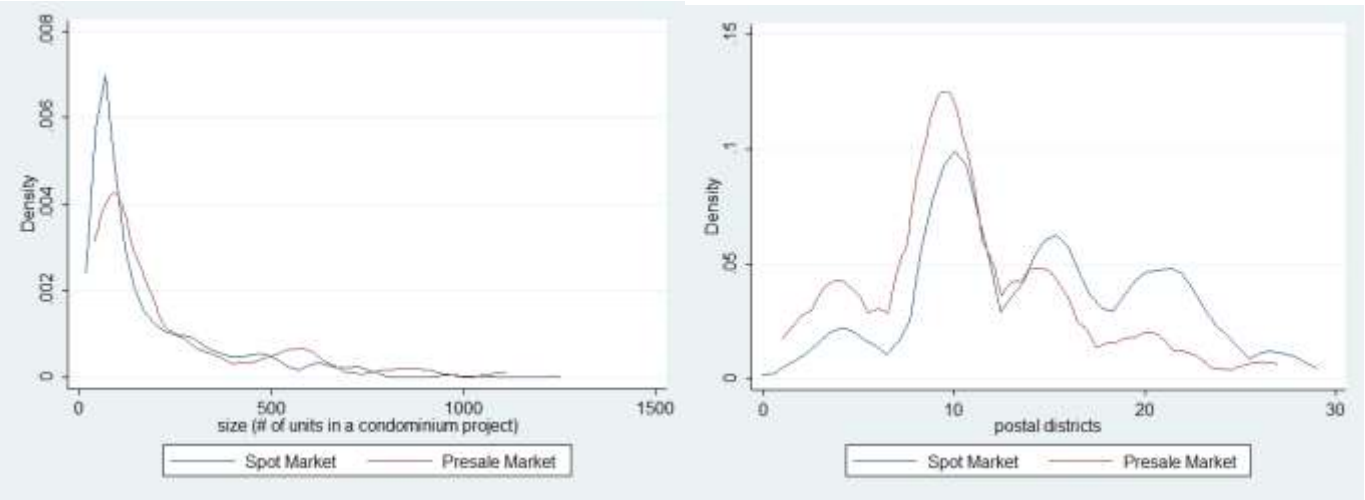
Figure 1: Residential project life cycle and self-selection of speculators

The chart below highlights the differences between the ownership of a presale contract (synthetic ownership) and of a completed property (real ownership). A project completion date is defined as the date a project receives a Temporary Occupancy Permit (TOP) from the government.

Timeline	Project launch for sale → Project completion (TOP date)	
Ownership status	Synthetic	Real
Construction status	Uncompleted	Completed
Transaction type	Presale (forward market)	Resale (spot market)
Investment amount	Down payment: $(1-\gamma) \times$ Price, $0<\gamma<1$	Full price (or with mortgage)
Holding expenses	Interest on down payment (if any)	Full interest + maintenance costs and management fee + property tax
Ownership benefits:		
Speculators	High liquidity and financial leverage	
Long term investor / user		(Imputed) income

Figure 2: Comparability of presale (treatment) and spot (control) groups: kernel density plots

The figure shows the kernel density plots (based on the Epanechnikov kernel function) for project size, project location distribution (among 28 postal districts), average transaction price (per square foot, in real U.S. dollars) in a month, and monthly project price volatility (definition in Table III) in the presale (treatment) and spot (control) group during the one-year period leading to the policy (2005:11-2006:10).



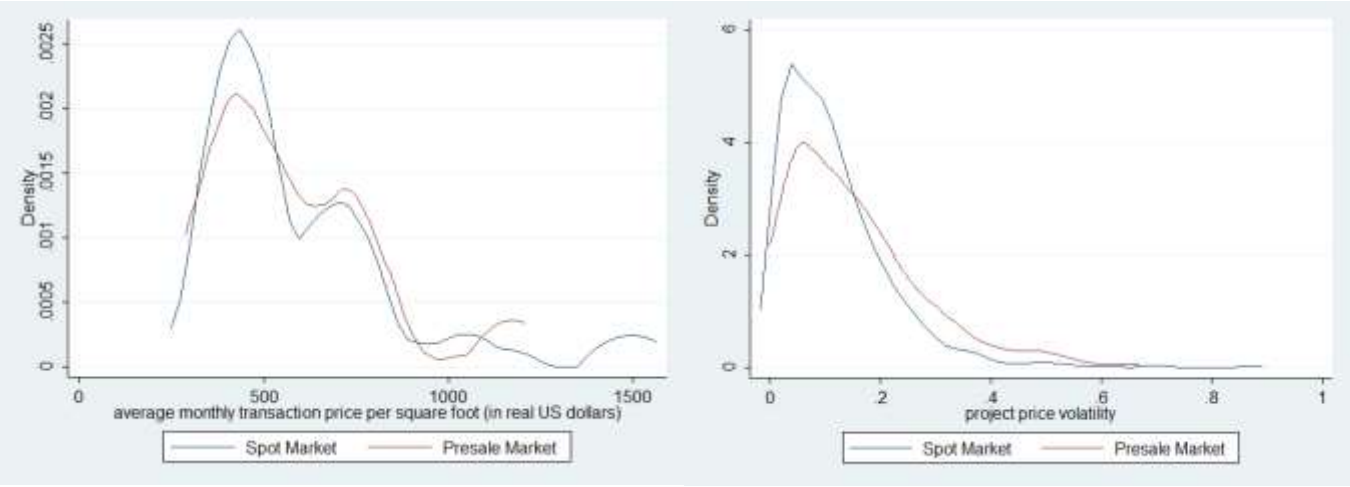


Table I: Entire sample statistics of the spot market and the presale market

This table reports the summary statistics of the full sample of housing transactions for both the spot and presale markets in Singapore from 1995:01 to 2010:10.

Panel A: Transaction-level statistics				
Spot sample	Mean	Std Dev.	Min	Max
Real Price (per sqft) (USD)	505	253	88	3,143
Unit Area (sqft)	1,471	626	280	13,046
Project Size (# of units)	149	157	40	1,232
Floor Level	8	7	1	68
Age (Months after TOP)	115	87	0	555
Observations	80,768			
Presale sample	Mean	Std Dev.	Min	Max
Real Price (per sqft) (USD)	565	282	66	3,308
Unit Area (sqft)	1,302	478	344	11,011
Project Size (# of units)	204	202	40	1,232
Floor Level	9	8	1	69
Months to TOP	24	12	1	108
Observations	100,704			

Table II: Trading activity around the policy event: summary statistics

This table compares the real estate transaction turnover in projects of the spot market (unaffected, control sample) and the presale market (affected, treatment sample) around the buyer stamp duty deferral withdrawal (2006:12). Panel A shows the mean and median statistics of the trading activity of the spot market, while Panel B shows the mean and median statistics of the total as well as speculative trading activity in the presale market. Panel C compares the time series averages of the mean monthly project turnover between November and January for the presale and spot market, respectively. We average the mean project turnover for November (and January) from 1995-2010 (excluding 2006:11 and 2007:01). *Monthly Project turnover* is defined as the number of transactions in a month divided by the condominium size. *Monthly Speculative turnover* is defined as the number of purchases in a month which get sold before the condominium is completed, divided by the condominium size. We also report the statistical significance of the mean differences based on a t-test, and the statistical significance of the median differences based on a Wilcoxon signed-rank test, with ***, **,* denoting 1%, 5%, and 10% significance respectively.

Panel A

Spot Market (controlled sample)

	Nov-06		Jan-07		Difference	
	Mean	Median	Mean	Median	Mean	Median
Monthly Project Turnover	0.77%	0.00%	0.71%	0.00%	-0.07%	0.00%

Panel B

Presale Market (treatment sample)

	Nov-06		Jan-07		Difference	
	Mean	Median	Mean	Median	Mean	Median
Monthly Project Turnover	6.90%	2.38%	2.61%	1.61%	-4.30%***	-0.76%***
Monthly Speculative Turnover	2.40%	0.73%	0.74%	0.00%	-1.66%***	0.00%

Panel C

Cyclicality of Trading in Presale and Spot Markets	Nov(except 06)		Jan (except 07)		Difference	
	Mean	Median	Mean	Median	Mean	Median
Monthly Project Turnover—Presale Market	0.41%	0.00%	0.40%	0.00%	-0.01%	0.00%
Monthly Project Turnover—Spot Market	1.96%	0.21%	2.07%	0.15%	0.11%	-0.06%

Table III: Policy impact on (speculative) turnover

This table presents the result of the regression analysis of the policy impact on (speculative) turnover. The first three columns report the turnover responses in the 6-month post-policy window (2006:11-2007:06), and the second three columns present results 12 months after the policy (2006:11-2007:12). The month when the policy came into effect (i.e., 2006:12) is excluded. *Turnover* is defined as the number of transactions for a condominium project in a month divided by project size. *Turnover** is equal to *Turnover* on presale market: it is speculative turnover, i.e., the number of presale purchases sold before project completion divided by size. *WD* is a dummy that is equal to one if it is after the buyer stamp duty deferral withdrawal. *Presal*e is a dummy that is equal to one for presale condominium projects. *Pre-policy turnover* is the average turnover of a project in the three months before the event window (2006:08-2006:10). *Pre-Policy Speculative Trading* is the average speculative turnover in a presale project during the three months before the policy (2006:08-2006:10). *Size* is the number of units in a project. *Central* is equal to one if the project is in the Central (core) region of Singapore. Standard errors are clustered to allow correlated outcomes within condominium projects in the presale (or control) market in the same location (central vs. non-central) at a given month. Standard errors are included in parentheses, and ***, **, * indicate 1%, 5%, and 10% significance, respectively.

	6 months after the policy (2006:11-2007:06)			**12 months after the policy (2006:11-2007:12)**		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	**Turnover**	**Turnover***	**Turnover**	**Turnover**	**Turnover***	**Turnover**
WD	0.004***	0.004***	0.002	0.002	0.002	0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Presale	0.055***	0.012***		0.057***	0.013***	
	(0.00)	(0.00)		(0.00)	(0.00)	
WD × Presale	-0.035***	-0.016***		-0.042***	-0.018***	
	(0.01)	(0.00)		(0.01)	(0.00)	
Pre-Policy Speculative Trading			0.171**			0.241***
			(0.07)			(0.04)
WD × Pre-Policy Speculative Trading			-0.342***			-0.377***
			(0.03)			(0.03)
Pre-policy Turnover	0.072*	0.055***	0.081**	0.041**	0.035***	0.052**
	(0.04)	(0.02)	(0.03)	(0.02)	(0.01)	(0.02)
Project Size	0.000	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Central	0.004**	0.004***	0.004***	0.002	0.002	0.002
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.004***	0.004***	0.005***	0.006***	0.006***	0.007***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	5,467	5,467	5,467	10,153	10,153	10,153
R-squared	0.16	0.03	0.03	0.12	0.02	0.01

Table IV: Policy impact on price volatility

This table presents the results on the policy impact on the project price volatility. *Price Volatility* is defined as the difference between the highest and lowest (log) transaction price (after adjusting for market and hedonics) for each project in a month. We require the number of transactions in the project to be at least two for the variable to be well defined. Condominium projects in the presale and spot markets are included. The first two columns report the six-month impact (2006:11-2007:06), and the second two columns present results twelve months after the policy (2006:11-2007:12). The month when the policy came into effect (2006:12) is excluded. *WD* is a dummy that is equal to one if it is after the buyer stamp duty deferral withdrawal. *Presale* is a dummy that is equal to one for presale condominium projects. *Pre-Policy Speculative Trading* is the average speculative turnover in a presale project during the three months before the policy (2006:08-2006:10). *Transaction Volume Control* is equal to the number of transactions in the project-month and is used to control for the small sample bias introduced in calculating the price volatility measure. Other control variables include pre-policy turnover, project size and Central region dummy (see Table III). Standard errors are clustered to allow correlated outcomes within condominium projects in the presale (or control) market in the same location (central vs. non-central) at a given month. Standard errors are included in parentheses, and ***, **, * indicate 1%, 5%, and 10% significance, respectively.

	6 months after the policy (2006:11-2007:06)		**12 months after the policy (2006:11-2007:12)**	
	(1)	(2)	(3)	(4)
Variable	**Price volatility**	**Price volatility**	**Price volatility**	**Price volatility**
WD	0.038***	0.039***	0.031***	0.036***
	(0.01)	(0.01)	(0.01)	(0.01)
Presale	-0.012		-0.016	
	(0.02)		(0.01)	
WD × Presale	0.002		0.027*	
	(0.02)		(0.02)	
Pre-policy speculative trading		-0.614**		-0.545**
		(0.26)		(0.25)
WD × Pre-policy speculative trading		0.140		0.395*
		(0.23)		(0.22)
Pre-policy Turnover	0.006	0.151	-0.044	0.006
	(0.04)	(0.11)	(0.04)	(0.04)
Project Size	0.000***	0.000***	0.000***	0.000***
	(0.00)	(0.00)	(0.00)	(0.00)
Central	0.044***	0.044***	0.041***	0.044***
	(0.01)	(0.01)	(0.01)	(0.01)
Transaction Volume Control	0.005***	0.004***	0.005***	0.005***
	(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.031***	0.028***	0.035***	0.031***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	2,170	2,170	3,530	3,530
R-squared	0.22	0.22	0.23	0.23

Table V: Policy impact on trading and volatility in the presale (treatment) market: underpriced vs. overpriced projects

This table presents the result of the regression analysis of the policy impact among the overpriced and underpriced projects in the presale market relative to the change in the spot (control) market. We argue the presence of informed speculators is proportionately higher in underpriced presale projects than in overpriced presale projects. Panel A shows the results of the average effect in the 6 or 12 months after the policy change. The *Underpricing* dummy is one if a presale project has an average project-specific price that is in the bottom 30% distribution among all presale projects in 2006:10, and the *Overpricing* dummy is one if a presale project has an average project-specific price in the top 30% distribution among all presale projects in 2006:10. *Turnover** is equal to the number of speculative purchases (divided by size) for presale projects, and is equal to the number of purchases (i.e., Turnover) divided by size for spot projects. *WD* is a dummy that is equal to one if a month is after the buyer stamp duty deferral withdrawal (i.e., 2007:01). ***WD_first 6 months*** is equal to one if the month falls in the first 6 months after the buyer stamp duty deferral withdrawal (2007:01-2007:06), and ***WD_second 6 months*** is equal to one if the month is in the second 6 months after the policy change (2007:07-2007:12)We include the same set of control variables as in Table III and IV. Columns (1) and (2) present results on the policy impact on (speculative) trading, and Columns (3) and (4) present results on the policy impact on price volatility. For a cleaner interpretation, the month in which the policy is enforced (2006:12) is excluded. Standard errors are clustered to allow correlated outcomes within condominium projects in the presale (or control) market in the same location (central vs. non-central) at a given month. They are included in parentheses, and ***, **, * indicate 1%, 5%, and 10% significance, respectively.

VARIABLES	(1)	(2)	(3)	(4)
	Turnover*		**Price volatility**	
	6 months	**12 months**	**6 months**	**12 months**
WD	0.003***	0.001	0.034***	0.029***
	(0.00)	(0.00)	(0.01)	(0.01)
Underpricing Dummy	0.017***	0.018***	-0.061***	-0.049**
	(0.00)	(0.00)	(0.02)	(0.02)
WD x Underpricing Dummy	-0.020***	-0.021***	0.065***	0.089***
	(0.00)	(0.00)	(0.02)	(0.03)
***Overpricing* Dummy**	0.006*	0.009**	0.036***	0.040***
	(0.00)	(0.00)	(0.01)	(0.01)
***WD x Overpricing* Dummy**	-0.009**	-0.013***	-0.050***	-0.029**
	(0.00)	(0.00)	(0.02)	(0.01)
Other Controls	Yes	Yes	Yes	Yes
Observations	5,121	9,555	1,971	3,227
R-squared	0.05	0.02	0.25	0.25

Table VI: Does price informativeness decrease after the policy?

In this table, we investigate whether price informativeness decreases after the policy change in 2006:12. The dependent variable (*Amihud* measure) is defined as the absolute value of project return in a given month divided by the project turnover. We winsorize the Amihud measure at the top and bottom 1% tails to control for outliers. Condominium projects in the presale and spot markets are included. The first column reports the six-month impact (2006:11-2007:06), and the second column presents results twelve months after the policy (2006:11-2007:12). For a cleaner interpretation, the month in which the policy is enforced (2006:12) is excluded. Other control variables include pre-policy project turnover, pre-policy project-specific return, project size and Central region dummy. Please refer to Table IV in the paper for definitions of the other independent variables. Standard errors are clustered to allow correlated outcomes within condominium projects in the presale (or control) market in the same location (central vs. non-central) at a given month. Standard errors are included in parentheses, and ***, **, * indicate 1%, 5%, and 10% significance, respectively.

	6 Months after the policy (2006:11-2007:06)	**12 Months after policy (2006:11-2007:12)**
	(1)	(2)
Variable	Amihud	Amihud
WD	-1.251	-0.210
	(0.80)	(0.95)
Presale	-5.493***	-5.672***
	(0.86)	(0.92)
WD×Presale	2.342**	3.132**
	(0.94)	(1.22)
Constant	8.746***	7.821***
	(0.93)	(1.11)
Controls	Yes	Yes
Observations	3,233	5,516
R-squared	0.05	0.03

Appendix: The Private Residential Market and Presale in Singapore

Residential properties in Singapore can be broadly divided into three types: (i) HDB flats, sold by the government's Housing Development Board (HDB) at subsidized prices to eligible citizens below stipulated income levels; (ii) non-landed private properties (condominium flats), not subject to ownership restrictions, and (iii) landed private properties (detached, semi-detached, and terrace houses), which normally can be owned by residents only. In 2009, HDB homes accounted for about 78% of the 1.13 million residential properties in Singapore, and the non-landed private properties for about 16%. The presale activity examined in this paper pertains to the market for non-landed private properties, which are sought after by high-income Singaporean households and expatriates, aspiring home upgraders from HDB homes, as well as by domestic and international investors. Geographically the residential market can be divided into the Central and Non-Central regions; the former is considered the prime location catering to the demand for high-end private homes.

Non-landed residential projects, ranging in size from a few dozen property units to over a thousand units, are often launched for presale before the commencement of construction (see project life cycle timeline in Figure 1). A presale resembles a forward contract, whereby the developer undertakes to deliver the project according to specification within a specified time period (usually around three years from the commencement of construction) and the buyer is obliged to pay a predetermined price at the time of purchase. Presale contracts are tradable, and transactions are conducted through private negotiations (typically through brokers) as in the spot market. Presale contracts are as liquid as the completed property units. Over the past 15 years, on average the annual dollar trading volume in the presale market is USD 7 billion. The monthly (dollar) transaction volume of uncompleted units is as large as (and often exceeds) that of completed units most of the time .

The standard presale contract offered by developers involves a progressive payment scheme (PPS). The buyer pays a fee of 5% to 10% of the property price to book the property for purchase. Within eight weeks, the buyer signs the sale and purchase agreement (SPA) and makes a down payment of up to 20% of the property price (less any booking fee already paid). Progressive payments equal to 40% of the price will subsequently be made during construction. The building project is deemed completed when a temporary occupancy permit (TOP) is issued by the Commissioner of Building Control, at which time a further 25% of the payment is due. The final 15% of the price is due upon the transfer of legal title and issuance of the certificate of statutory completion (CSC). Presale proceeds are typically kept in escrow to be released to the developer according to construction progress. In contrast to other presale markets (e.g., China), developers in Singapore almost never default or abandon a project prematurely (even during the

market distress period). In case of buyer default upon project completion, which has been rare, developers have recourse against the defaulting buyers for the loss incurred.

As shown in Figure 1, the life cycle of a project is represented by a timeline with a reference point at the project completion date (or TOP date). Let T denote the TOP date. Transactions before TOP are referred to as presale (forward) transactions, and those after TOP are resale (spot) transactions. In Singapore, a presale differs from a spot market transaction in three important respects. First, it allows the buyer and the developer to lock in at time $t<T$ a fixed price for the property to be delivered at time T. Second, it offers the buyer an interest-free leveraged position in the property by allowing delayed payment of a substantial portion of the price. Last, the presale mechanism allows developers or homebuyers to hedge future price risks. Given that developer and buyer defaults are very rare in Singapore, the value of a presale contract has three key components: the discounted value at t of a future spot price at T, a hedging premium arising from price risk sharing between the developer and the investor, and the interest saving derived from the interest-free leverage.

Let p_{ij} be the (log) value associated with the hedonics of the property unit j in the condominium project i and $p(t)$ be the log spot price index. Further, let ρ and σ^2, respectively, be the expected rate of appreciation and the instantaneous diffusion variance of the spot price between t and T. According to Liu, Edelstein, and Wu (2011), the hedging premium, denoted by h, increases with the uncertainty of spot price at T, $\sigma^2(T-t)$, when the developer is risk neutral and the buyer is risk averse.; $h = h(\sigma^2(T-t))$. Without financial leverage, the value of the presale contract at $t<T$ would be $\exp(p_{ij}+p(t)+\rho(T-t)-(r+\lambda)\cdot(T-t))+h(\sigma^2(T-t)))$, where r is the interest rate and λ is the real estate risk premium. Suppose the presale contract requires an upfront down payment equal to $1-\gamma$ fraction of the forward price, with the remaining $\gamma<1$ portion due at T. In other words, the presale investor gets interest-free financing with a loan-to-value ratio of $\gamma<1$ between time t and T. In the log term (with the use of the first-order Taylor expansion), we obtain $V_{ij}(t,T)$, the market value at time t of the forward contract for property j in the condominium project i to be delivered at TOP date T, as:

$$\ln\left(V_{ij}(t,T)\right) = p_{ij} + p(t) - d \times (T-t) + h(\sigma^2(T-t)) + \gamma \times r \times (T-t), \quad \text{(A.1)}$$

where $d \equiv r + \lambda - \rho$ denotes rental yield (according to the Gordon valuation formula). The last three components of Eq. (A.1), which are all proportional to time to TOP, $T-t$, represent adjustments for presale. The presale adjustments decrease with $T-t$ at rate d due to foregone rental income but increase with $T-t$ in proportion to r and σ^2 due to interest saving and buyer risk aversion respectively.

Since Eq. (A.1) serves as a pricing benchmark in this study, we do not have to identify its structural parameters. Hence we use a semi-parametric specification to control for the time-varying

presale price adjustments. The transaction price per square foot of a presale property unit j in development project i, P_{ij}, can be expressed as the fundamental value V_{ij} plus a price discovery error $\varepsilon_{ij}(t)$:

$$\ln\left(P_{ij}(t,T)\right) = \ln(V_{ij}(t,T)) + \varepsilon_{ij}(t)$$
$$= p_{ij} + p(t) + \tau(T-t)\theta_{central} + \tau(T-t)\theta_t + Developer\_sale + \varepsilon_{ij}(t) \quad \text{(A.2)}$$

where p_{ij} represents hedonic adjustment (including project i fixed effect, and unit j characteristics such as floor area, and floor level), $p(t)$ is the log spot price index. $\tau(T-t)$ equals zero when $T-t \leqslant 0$ and is linear in $(T-t)$ when $T-t > 0$. θ_t denote calendar month fixed effects to control for time-varying components of the presale adjustments, and $\theta_{Central}$ is the Central Region fixed effect to account for potential difference in required rental yield between the two regions. *Developer_sale* is a dummy equal to 1 if the sale is by the developer, to control for any sale incentive offered by the developer. $\varepsilon_{ij}(t)$ is the price residual, the average of which within the project i in each period gives the project-specific price $\varepsilon_i(t)$. Note that when $T-t \leqslant 0$ (i.e., after the project is completed), Eq. (A.2) reduces to the standard hedonic price index model widely used in real estate market studies (Rosen, 1974).

First, we estimate the spot market benchmark price indexes (adjusted for CPI trend) for the Central and Non-Central Regions by applying Eq. (A.2) (in this case the standard hedonic pricing model) to transactions in completed projects ($t>T$). We obtain both the market benchmark indexes and the project-specific pricing error ($\varepsilon_{ij}(t)$) in the spot market (relative to the market trend and adjusting for hedonic attributes). The estimated benchmark indexes are plotted in Panel A of Figure A_1 (which can be compared to the average transaction price shown in Panel B of Figure A_1). Second, we estimate the presale pricing model using the presale transactions adjusted for the spot benchmark price trends estimated in the first step. The estimates are reported in Table A_I, Panel B and are used to compute project-specific pricing errors ($\varepsilon_{ij}(t)$) during presale.

[Insert Figure A_1 about here]

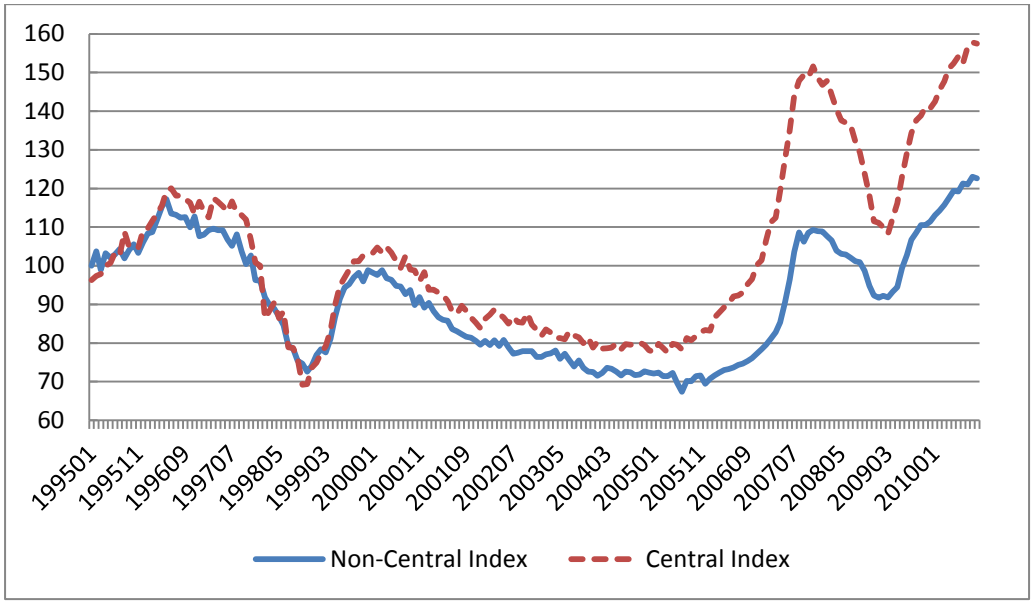
[Insert Table A_I about here]

To assess robustness of the hedonic estimates, we carry out the estimation by dividing the sample into earlier (<2004) and later (> = 2004) subsamples, and by restricting our estimation to projects larger than the cross-sectional mean. The estimated price indices and hedonic coefficients are very similar to those in Panel A of Table A_I. These results are not reported but are available from the author upon request.

Figure A_1: Benchmark price indices and regional average prices

Panel A shows the estimated price index using all resale (spot) transactions from 1995 to 2010, according to Eq. (A.2); the index values are the region-specific month dummies from the regression reported in Panel A of Table A_I. The index is set to be 100 for Non-Central region in 1995:01. Panel B shows the CPI-adjusted monthly average transaction price among all transactions per square foot (denoted in USD).

Panel A: Estimated Price Index



Panel B: Monthly Average Transaction Price

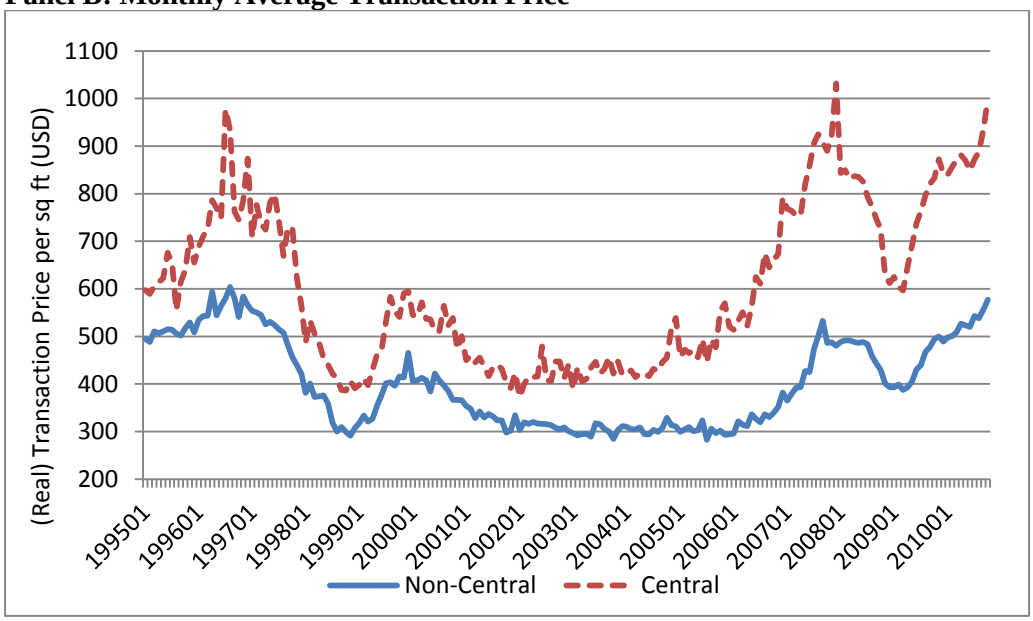


Table A_I: OLS estimates of benchmark price equations

This table reports the estimated coefficients for hedonics of our pricing model (Eq. A.2). Panel A presents results on the pricing model estimation for the spot transactions, and Panel B report results on the pricing model estimation for the presale transactions. The log region-specific spot price index used to adjust the dependent variable in Panel B equals the region-specific calendar month fixed effects estimated in Panel A. Standard errors are clustered at the project level. P-values are reported in the parenthesis and **,* represent 1% and 5% significance respectively.

Panel A: Spot market sample (transaction in completed projects, t>T)		**Panel B: Presale market sample (transaction in uncompleted projects, t ≤T)**	
Dependent variable	ln(price psf)	Dependent variable	ln(price psf)−ln(region-specific spot price index)
ln(Floor_level)	0.040** (0.00)	*ln(Floor_level)*	0.038** (0.00)
ln(Floor_level) ×*Central*	0.001 (0.49)	*ln(Floor_level)* ×*Central*	0.019** (0.00)
Ground_floor	0.024** (0.00)	*Ground_floor*	-0.018** (0.00)
Ground_floor ×*Central*	-0.012** (0.01)	*Ground_floor* ×*Central*	0.026** (0.00)
ln(Living_area)	-0.274** (0.00)	*ln(Living_area)*	-0.217** (0.00)
ln(Living_area) ×*Central*	0.107** (0.00)	*ln(Living_area)* ×*Central*	0.115** (0.00)
ln(Building_age)	-0.127** (0.00)	*Time_to_completion*	0.002** (0.00)
ln(Building_age) ×*Central*	-0.018** (0.01)	*Time_to_completion* ×*Central*	0.001** (0.00)
FE	Building blocks	*Developer_sale*	-0.031** (0.00)
FE	Calendar month for Central	*FE*	Building blocks
FE	Calendar month for Non-Central	*FE*	Time_to_completion× (Calendar month fixed effects)
Observations	80,751	Observations	100,704
R-squared	0.944	R-squared	0.941

Internet Appendix

(Not Intended for Publication)

Internet Appendix Table I. Dynamics of Speculative Turnover and Volatility Response

This table shows results that decompose the speculative and volatility response in all presale projects (treatement sample) in the 12 month post-policy period into the response in the first 6 months and the second 6 months. *WD_first 6 months* is equal to one if the month falls in the first 6 months after the buyer stamp duty deferral withdrawal (2007:01-2007:06), and *WD_second 6 months* is equal to one if the month is in the second 6 months after the policy change (2007:07-2007:12). Column (1) shows the decomposition result of the finding in column (5) in Table III, and column (2) shows the decomposition result of the finding in column (3) in Table IV. We use the same specification and include the same set of control variables as in Table V. Standard errors are clustered to allow correlated outcomes within condominium projects in the presale (or control) market in the same location (central vs. non-central) at a given month. They are included in parentheses, and ***, **, * indicate 1%, 5%, and 10% significance, respectively.

VARIABLES	(1) **Turnover*** 12 months	(2) **Price volatility** 12 months
WD_first 6 months	0.004***	0.037***
	(0.00)	(0.01)
WD_second 6 months	-0.000	0.022***
	(0.00)	(0.01)
Presale	0.013***	-0.016
	(0.00)	(0.01)
WD_first 6 months x Presale	-0.016***	0.006
	(0.02)	(0.02)
WD_second 6 months x Presale	-0.020***	0.061***
	(0.00)	(0.02)
Other Controls	Yes	Yes
Observations	10,153	3,530
R-squared	0.04	0.24

Internet Appendix Table II. Trading response to BSDDW using an alternative speculator measure

This table investigates the robustness of the policy impact on speculators with a subsample analysis. We remove the presale projects that are completed in more extreme market conditions (i.e., either of the three months before completion occurs at a time where market index is in the top or bottom 30% of the time-series distribution). The sample period is the one month before and one month after the policy event (2006:11-2007:01), and results also hold using a longer post-event window. For a cleaner interpretation, the month in which the policy is enforced (2006:12) is excluded. Refer to Table III for definitions of the variables. Standard errors are clustered to allow correlated outcomes within condominium projects in the presale (or control) market in the same location (central vs. non-central) at a given month. Standard errors are included in the parenthesis, and ***, **, * indicate 1%, 5% and 10% significance respectively.

VARIABLES	(1) Turnover	(2) Turnover*
WD	-0.001	-0.001
	(0.00)	(0.00)
Presale	0.022***	-0.000
	(0.00)	(0.00)
WD × Presale	-0.024***	-0.009***
	(0.00)	(0.00)
Pre-policy Turnover	0.205***	0.081***
	(0.02)	(0.01)
Project Size	0.000	0.000
	(0.00)	(0.00)
Central	0.003***	0.003***
	(0.00)	(0.00)
Constant	0.004***	0.005***
	(0.00)	(0.00)
Observations	1,461	1,461
R-squared	0.20	0.05

Internet Appendix Table III: Robustness checks for price volatility analysis

This table presents more specifications on the analysis of the policy impact on the project price volatility. Panel A shows the 6-month and 12-month price volatility result using weighted least squares (to correct for heteroskedasticity), where the weights are the predicted residual square from the first-stage OLS on the monthly transaction volume, volume squared, and project size. Panel B shows the 6-month price volatility result by project size. Column (1) of Panel B presents results for projects with size between 40 and 204 units, and Column (2) shows the impact for projects with more than 204 units. Standard errors in Panel B are clustered to allow correlated outcomes within condominium projects in the presale (or control) market in the same location (central vs. non-central) at a given month. Please refer to Table III in the paper for detailed variable definitions. Other control variables are the same as in Table IV. Standard errors are included in the parenthesis, and ***, **, * indicate 1%, 5%, and 10% significance, respectively.

Panel A: Weighted least squares

Time horizon after the policy intervention:	6 months (2006:11-2007:06)	12 months (2006:11-2007:12)
	(1)	(2)
VARIABLES	Price volatility	Price volatility
WD	0.041***	0.033***
	(0.01)	(0.01)
Presale	-0.012	-0.007
	(0.02)	(0.02)
WD × *Presale*	0.019	0.040**
	(0.02)	(0.02)
Controls	Yes	Yes
Observations	2,170	3,530
R-squared	0.24	0.28

Panel B: By project size (with 6 months after the policy intervention, 2006:11-2007:06)

	Small projects (40-200 units)	Large projects (>200 units)
	(1)	(2)
VARIABLES	Price volatility	Price volatility
WD	0.036***	0.038***
	(0.01)	(0.01)
Presale	-0.005	-0.055*
	(0.01)	(0.03)
WD × *Presale*	-0.023*	0.057**
	(0.01)	(0.02)
Controls	Yes	Yes
Observations	957	1,213
R-squared	0.12	0.27

Internet Appendix Table IV: Policy impact on rolling return volatility

This table shows the policy impact on price volatility using an alternative return volatility measure as the dependent variable. Return volatility is defined as, on a rolling basis, the standard deviation of the monthly project-specific return over the most recent six months. We require the number of transactions in the project to be at least one for the variable to be well defined. Condominium projects in the presale and spot markets are included. For a cleaner interpretation, the month in which the policy is enforced (2006:12) is excluded. *Volatility Bias* is equal to $\frac{1}{6}\sum_{t=1}^{6}(\frac{1}{n_{kt}}+\frac{1}{n_{kt-1}})$, where n_{kt} denotes the number of transactions for project k in month *t (*the proof is available upon request). It is used to control for the bias introduced in calculating the return volatility measure when there are few transactions in a project-month. Other unreported control variables include the lagged return volatility (up to five months), pre-policy project turnover, pre-policy project-specific return, project size and *Central* dummy. Please refer to Table III in the paper for definitions of the other independent variables. We compute Newey-West (with five lags) standard errors to control for autocorrelation. Standard errors are included in the parenthesis, and ***, **, * indicate 1%, 5%, and 10% significance, respectively.

Time horizon after the policy intervention:	6 months (2006:11-2007:06)	12 months (2006:11-2007:12)
	(1)	(2)
Variables	Rolling volatility	Rolling volatility
WD	-0.000	0.001
	(0.00)	(0.00)
Presale	-0.006***	-0.007***
	(0.00)	(0.00)
WD× Presale	0.005**	0.007***
	(0.00)	(0.00)
Volatility Bias	0.007***	0.006***
	(0.00)	(0.00)
Constant	0.005**	0.008***
	(0.00)	(0.00)
Controls	Yes	Yes
Observations	2,974	5,203