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Speculative Investors and Transactions Tax: Evidence from the Housing Market

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This paper examines the impact of a policy change in transaction tax on speculators. The policy intervention took place in Singapore's housing market; it effectively raised the transaction cost in a segment favored by short-term speculators. Relative to the unaffected control sample, we find that the rise in transaction cost, equivalent to a two- to three-percentage-point increase in transaction tax, reduced speculative trading in the treatment segment by 75% and raised its price volatility by 18%. It also significantly reduced price informativeness. We further show that the results are likely due to a relatively greater withdrawal by informed speculators than by destabilizing speculators following the transaction cost increase.

Keywords: transaction tax; volatility; speculators; informed traders; noise traders; housing market
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1. Introduction

After the global financial crisis, the Tobin tax as a form of financial transaction tax has drawn much attention. On December 11, 2009, the *Financial Times* reported that "European Union leaders urged the International Monetary Fund [IMF] on Friday to consider a global tax on financial transactions in spite of opposition from the U.S. and doubts at the IMF itself" (Barber and Parker 2009). U.S. Senator Tom Harkin and Representative Peter DeFazio pursued the issue and proposed bills in November 2011 that would impose a transaction tax on financial firms. France became the first European country to impose a transaction tax on August 1, 2012. In real asset markets, many countries and regions in Asia also significantly increased the transaction tax of property transactions in 2010, 2011, and 2012.

The allegedly key economic appeal of a transaction tax is that it may curb speculation and reduce "excess" volatility. Indeed, behavioral finance suggests that "noise traders," broadly defined as investors who trade for nonfundamentals-related reasons, drive prices away from fundamentals and cause excess volatility (Cutler et al. 1990, 1991; De Long et al. 1990a, b; Shleifer and Summers 1990; Hong and Stein 1999). This naturally calls for policy interventions to curb destabilizing speculation. A transaction 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 Seguin 1993). Subrahmanyam (1998) shows theoretically that, in the context of a stock market, a transaction tax can increase stock price volatility by discouraging informed traders from acquiring information. Clearly, the transaction tax is a double-edged sword—it can deter both informed and uninformed speculation.

These varied theoretical predictions suggest that the effect of transaction tax could be an empirical matter, depending on its impact on informed versus less informed speculators. Unfortunately, direct empirical evidence is limited. Umlauf (1993) shows that price volatility did not decline in the Swedish stock market after the introduction of a transaction tax in the 1980s, in contrast to the view of the proponents of the tax. A small number of studies on transaction-cost regime shift and stock market volatil-

ity (e.g., Roll 1989, Jones and Seguin 1997, Hau 2006) show that a transaction cost increase leads to either no change or a rise in volatility. Other more recent studies find similar results as well in various market settings (e.g., Chou and Wang 2006, Habermeier and Kirilenko 2003, Phylaktis and Aristidou 2007, Su and Zheng 2011, Deng et al. 2014). Overall, there is little empirical evidence on the differential deterrence impact of transaction tax on noise versus informed speculators: the one paper that makes that distinction, Bloomfield et al. (2009), conducts a controlled lab experiment and shows that such a tax deters informed and uninformed traders equally.

We fill the empirical gap by examining an unanticipated policy intervention that raised the transaction cost in Singapore's housing market in December 2006. The policy intervention created a natural experiment that allows us to identify the differentiated impact of elevated transaction cost on informed and uninformed speculation and thus on price volatility. The setting is attractive for several reasons. First, the unanticipated policy change was introduced to cool down house price appreciation and thus is arguably exogenous to our focus—price volatility. Second, the policy raised the transaction cost only in a submarket favored by speculators. The policy-induced increase in transaction tax is equivalent to about two to three percentage points (the calculation is shown in the Internet appendix, available as supplemental material at https://doi.org/10.1287/mnsc.2015.2268). Using the unaffected (but otherwise similar) submarket as a control group, we can use difference-in-differences regressions to reveal the transaction tax's impact on trading volume, speculative trade, price volatility, and price informativeness. Third, and very importantly, we can differentiate the relative presence of informed and noise speculators among affected submarkets. Housing markets have higher transaction costs and lower liquidity than most financial markets, and thus house prices adjust more slowly than financial asset prices. Therefore, using hedonic price regressions, we can identify temporarily over- and underpriced submarkets in housing. Because of short-selling constraints, informed investors are less (more) represented in overvalued (undervalued) housing segments. These empirical features provide us a means to identify the nuanced effect of elevating transaction cost on informed versus noise speculation and price volatility.

Our findings are as follows: (i) The heightened transaction cost is associated with a significant drop in speculative trading that is equivalent to 75% of the prepolicy level. (ii) The decline in trading is associated with an 18% rise in price volatility. Moreover, price volatility increases in underpriced submarkets and decreases in overpriced submarkets. Thus heightened transaction costs discourage both informed and noise traders but lead to opposite changes in price volatility in submarkets with different appeal to informed traders prior to the policy intervention. Overall, however, the former retreats more than the latter, leading to a net increase in price volatility. (iii) The affected market exhibits a decline in price informativeness.

Our findings are robust against alternative specifications of the postpolicy window and volatility measures as well as against potential measurement errors in our speculator identification. The evidence cautions against the use of a transaction tax in curbing speculators: a heightened transaction tax might deter the "good" speculators disproportionately more, leading to unintended consequences such as lower price stability and informativeness.

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 §3, and the empirical findings are reported in §4. Section 5 discusses alternative interpretations and performs additional robustness checks. Section 6 concludes.

2. Empirical Design and Methodology

2.1. Market Background and Policy Experiment

In Singapore, private condominium properties (known locally as nonlanded properties) in new development projects are launched for sale before project completion and typically before the commencement of construction.[1] These new projects are mostly situated in built-up areas and hence share similar location and building attributes as completed private condominium projects. The ownership of these not-yet-completed properties can be freely traded and are sought after by homebuyers as well as by investors. (The Internet appendix provides additional details of the residential market background in Singapore.)[2] From here on, we refer to the market for uncompleted condominiums as "presale" and the market for completed condominiums as "spot."

The presale market is more attractive to short-term speculators than is the spot market for completed properties, because the holding cost for an uncompleted property is lower than that of a completed property. As in Fu and Qian (2014), Figure 1 highlights the difference; the former does not require full-price payment until completion and does not

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

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

Figure 1 Residential Project Life Cycle and Self-Selection of Speculators

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

Notes. 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 from the government.

require the payment of management fees, maintenance costs, and property taxes. Also, because holding costs increase significantly upon project *completion*, speculators in the presale market have incentives to exit the market before completion.

Therefore, we define presale purchases subsequently sold before project completion as short-term speculative trades and those engaged in such round-trip transactions *flippers* (short-term speculators). Although a flipper may directly buy from a developer or from a previous presale contract holder, market transaction data allow us to cleanly identify purchase-to-sale trips completed before project completion. Short-term speculative trades and all other purchases constitute the overall trading in the presale market.

In our sample, flippers indeed 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). In a previous study using the same data, Fu and Qian (2014) find that flipper purchases are more represented in projects that recently experienced higher price appreciation, a strong indication that flippers are linked to short-term speculative motives. Admittedly, flipping could be affected by unexpected changes in price trends or unexpected changes in personal financial circumstances so that identifying flippers as short-term speculators can be noisy. In addition, we do not identify speculators in the spot market by construction. We discuss the robustness of our speculator identification in §5.

The policy intervention we study is Singapore government's announcement on December 15, 2006, with immediate effect, to withdraw a stamp duty deferral in the presale market. Homebuyers 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, in June 1998, as a part of the various policies to counter the impact of the economic slowdown triggered by the Asian financial crisis, the government gave concession for presale buyers to defer stamp duty payment until project completion or until the property was sold before completion.[3] The concession encourages short-term speculation because it allows 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 speculators, effectively raising their transaction cost.[4] The presence of the parallel affected presale and unaffected spot markets for condominium properties offers an opportunity to apply a difference-in-differences approach to identifying the policy impact.

The policy change—the requirement to pay 3% stamp duty in cash at the time of purchase—significantly raises the presale market buyers' upfront capital commitment. Compared with the 10%–

[3] 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.

[4] Old presale contracts are "grandfathered" so that the holders do not have to immediately pay stamp duties until project completion or reselling of their contracts.

20% down payment requirement and zero capital gains tax in Singapore, this increase is economically significant.[5] It 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 et al. 2009, Ben-David 2011). Moreover, the policy could be interpreted as a signal that the government intended to discourage short-term speculation. A simple calculation elaborated in the Internet appendix suggests that the withdrawal of the stamp duty concession is equivalent to a two- to three-percentage point (or 66%–100%) increase in the level of the stamp duty tax.

2.2. Empirical Methodology

Our empirical analysis involves several steps. Using December 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 flippers. We then study the policy impact on price volatility. We define *project turnover* as the number of transactions in a condominium project in a given month normalized by the project size (the total number of units in the project). *Speculative turnover* is the purchases by flippers normalized by the project size.

We estimate price volatility based on observed transaction prices. To remove price variability stemming from heterogeneous property attributes, we first adjust the natural log of raw transaction prices per square foot for market price trend, project fixed effects, and property unit–hedonic attributes using the hedonic price regression (Rosen 1974); the pricing equation and the estimates are reported in the Internet appendix. Simply put, the regression residuals, ε_{ij}^t, measure transaction prices specific to property unit j in condominium project i in month t after adjusting for market conditions and hedonic factors. Averaged over j, $\bar{\varepsilon}_i^t$ reflects the market- and hedonic characteristics-adjusted price in project i in the month t. 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 $\{\varepsilon_{ij}^t\}$, i.e., the difference between the highest and the lowest ε_{ij}^t within project i in month t. A missing value is assigned for project-months with fewer than two transactions.[6]

By construction, the within-project range of $\{\varepsilon_{ij}^t\}$ measures project-specific price volatility, since we remove macroeconomic trend and detailed building-level attributes. The measure reflects new information arrival about asset fundamentals (such as location appeal, builder reputation, the development of competing projects, or complementary infrastructures) or noise induced by uninformed trade, or both. The hedonic factor adjustments as well as the uniformity and steady nature of house prices in similar neighborhoods imply that information-driven change in price volatility should be common in both presale (treatment) and spot (control) projects. As a result, the incremental change after the policy intervention in the volatility measure in the treatment group, relative to volatility change in the unaffected spot projects in a similar neighborhood, is likely attributable to the policy's effect on short-term speculative trading.

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

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

where the dependent variable y_{it} represents monthly project turnover, 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 (the treatment group) and 0 for projects in the spot market (the control group), and *WD* has a binary value of 0 before the policy intervention and one afterwards. 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. Since the macroeconomic conditions as well as the policy shock may simultaneously affect all similar condominium projects (e.g., presale and spot projects in the same location), we cluster the standard errors to allow the regression residuals to be correlated within the same location (i.e., central versus noncentral region)[7] among all presale or spot market condominium projects in a given month.[8]

[5] 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 economically significant item of monetary transaction cost buyers pay at the time of purchase.

[6] Existing studies have shown that the price range estimator is a more efficient estimator of the volatility (e.g., Parkinson 1980). Our results are robust to a more conventional volatility measure (e.g., standard deviation of monthly returns), which we discuss in greater detail in §5.2.

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

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

3. Data and Sample Statistics

3.1. Data Sources

The data for this study are obtained from the Urban Redevelopment Authority (URA) REALIS database, which reports all property transactions lodged with the Singapore Land Authority. Our sample excludes transactions in private nonlanded projects with fewer than 40 units (to avoid illiquidity concerns) and properties bought out for redevelopment (en bloc sales). The resultant sample, comprising more than 181,000 transactions observed between January 1995 and October 2010 in 854 nonlanded 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.

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[9]). We also obtain the monthly consumer price index (CPI) and interest rate from the SingStat Time Series database 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 1 shows that the average size of the transacted unit between 1995 and 2010 is 1,471 square feet in the spot 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.[10] The project 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).

Table 1 Entire Sample Statistics of the Spot Market and the Presale Market

	Mean	Std. dev.	Min	Max
Spot sample				
Real price (per sq ft) (USD)	505	253	88	3,143
Unit area (sq ft)	1,471	626	280	13,046
Project size (no. 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				
Real price (per sq ft) (USD)	565	282	66	3,308
Unit area (sq ft)	1,302	478	344	11,011
Project size (no. of units)	204	202	40	1,232
Floor level	9	8	1	69
Months to TOP	24	12	1	108
Observations	100,704			

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

We further evaluate the comparability of the spot and presale markets in the prepolicy period. Figure 2 shows the kernel density plots, based on the Epanechnikov kernel function,[11] for several measures at the monthly frequency for both markets during the one-year period before the policy intervention (i.e., November 2005 through October 2006). The similarity of the distributions in project size, geographic location of condominium projects, and average monthly transaction price indicates that the treatment (presale) and control (spot) groups are balanced in distribution along those observables in the prepolicy 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.

3.3. The Sample Statistics Around the Policy Intervention

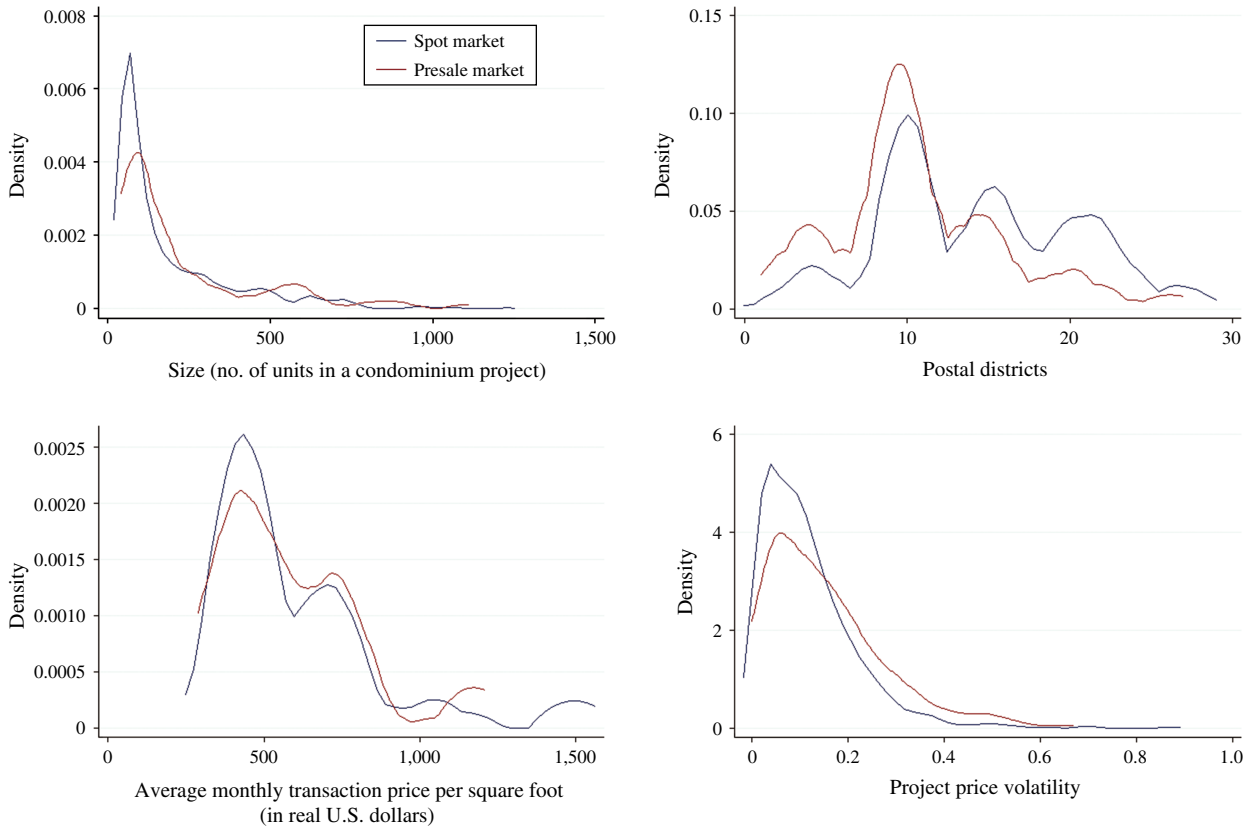
Table 2 provides the summary statistics for trading activity one month before (November 2006) and one month after (January 2007) the policy intervention. The mean turnover for the spot market is 0.77% in November 2006 and 0.71% in January 2007. The change of −0.07 percentage points is statistically

[9] There are three types of land titles for private residential developments in Singapore: freehold, 999-year lease, and 99-year lease.

[10] For the convenience of readers, we report the price information in the text in U.S. dollars by using the concurrent exchange rate. The analyses in the paper (for example, volatility measures) are based on local currency prices.

[11] The result is robust to different choices of the kernel density functions and the bandwidth.

Figure 2 (Color online) Comparability of Presale (Treatment) and Spot (Control) Groups: Kernel Density Plots



Notes. 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 (defined in Table 3) in the presale (treatment) and spot (control) group during the one-year period leading to the policy (November 2005 through October 2006).

insignificant, consistent with the fact that the withdrawal of the tax concession does not affect the spot market.

By contrast, the mean turnover for presale projects experienced a significant postintervention drop of 4.3 percentage points (statistically significant at the 1% level), an almost two-thirds reduction relative to the preintervention average turnover. Moreover, speculative turnover by flippers drops 1.66 percentage points, a 70% reduction relative to the preintervention 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 2 shows that the difference between November project turnover and January project turnover from 1995 to 2010 (excluding November 2006 and January 2007) is statistically and economically indistinguishable from zero in both the presale and spot markets.

4. Empirical Analysis

4.1. Impact on Speculative Trading

We use difference-in-differences regressions to study the trading response to the withdrawal of the stamp duty deferral (*WD*). To control for heterogeneity across condominium projects, we include as explanatory variables project size, project age, and a central location dummy. (In Singapore, projects in the Central Region command a premium.) We also include the average turnover between August and October 2006, *prepolicy turnovers*, to control for differences in trading across projects before the policy intervention.[12]

[12] Since we need to use recent transaction information (e.g., between August and October 2006) to identify the pretreatment speculative activity intensity (as well as the underpriced and overpriced project classification in later analyses), in our difference-in-differences regressions, we include only November 2006 as the pretreatment period to avoid in-sample bias. However, our main specification in Table 3 (columns (1) and (2) and columns (4) and (5)) and Table 4 (columns (1) and (3)) are robust to inclusion of a longer pretreatment period in the estimation. Furthermore, we plot the time series of monthly project trading turnover in the presale and spot market using a longer pretreatment window (Figure IA.2 in the Internet appendix). The trading activities in the presale (treatment) and the spot (control) market follow a similar trend before the policy announcement in December 2006, with a slightly visible upward trend in presale market turnovers, after which the gap shrinks visibly. This provides additional auxiliary evidence supporting the validity of our difference-in-differences specification.

Table 2 Trading Activity Around the Policy Event: Summary Statistics

	November 2006		January 2007		Difference	
	Mean (%)	Median (%)	Mean (%)	Median (%)	Mean (%)	Median (%)
	Panel A: Spot market (controlled sample)					
Monthly project turnover	0.77	0.00	0.71	0.00	−0.07	0.00
	Panel B: Presale market (treatment sample)					
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
	November (except 2006)		January (except 2007)		Difference	
	Mean (%)	Median (%)	Mean (%)	Median (%)	Mean (%)	Median (%)
	Panel C: Cyclicality of trading in presale and spot markets					
Monthly project turnover—Spot market	0.41	0.00	0.40	0.00	−0.01	0.00
Monthly project turnover—Presale market	1.96	0.21	2.07	0.15	0.11	−0.06

Notes. 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 (December 2006). Panel A shows the mean and median statistics of the trading activity of the spot market, and 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 markets, respectively. We average the mean project turnover for November (and January) from 1995 to 2010 (excluding November 2006 and January 2007). *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 that 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.

***, **, and * indicate 1%, 5%, and 10% significance, respectively.

In the regression framework, we analyze the trading and volatility responses in the six-month period after the policy shock to identify the shorter-term effect. We also study a longer-term policy impact using a 12-month postpolicy window. Housing transactions often need considerable lead 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 3 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, 4.9 percentage points higher in the presale market than in the spot market before the policy, consistent with the summary statistics shown in Table 2. The critical result is that the presale market sees a sharper decline in turnover after the policy intervention (indicated by the coefficient of $WD \times Presale$). On average, the turnover for presale projects drops by 3.5 percentage points more than that in spot market projects in the six months after the policy shock, and the difference is statistically significant at the 1% level. This estimated extra drop in trading implies a reduction of total trading by 51% (compared with 6.9 percentage points in November 2006, as shown in Table 2) in the presale market.

In column (2) of Table 3, we investigate the impact of the concession withdrawal on identified speculative trading in the presale market. The speculative turnover in the treatment sample, compared with the change in project-level turnover in the control sample, declines by 1.6 percentage points during the six months after the policy intervention. The effect is statistically significant at 1% and is equivalent to a 67% reduction in speculative trading (compared with 2.4 percentage points in November 2006 as shown in Table 2) in the presale market, an effect that is larger than the average trading reduction (column (1)).[13]

Column (3) of Table 3 shows that the drop in total turnover in the presale market after the policy intervention is more pronounced in projects with a higher 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 (*Prepolicy speculative trading*) for each presale project between August and October 2006 (i.e., two to four months prior to the policy change) 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 more active speculative trading indeed have higher total turnover. Very importantly, they also confirm that the decrease in trading activity in the presale

[13] We also consider an alternative measure of speculative activity—the share of speculative purchases in the overall monthly trading activity for a presale project. Then we compare the speculative purchase share for presale projects before and after the policy introduction. In unreported results, we find that the fraction of speculative purchases drops significantly among presale projects (by almost 30% by the end of December 2007).

Table 3 Policy Impact on (Speculative) Turnover

	Six months after the policy (Nov 2006–June 2007)			Twelve months after the policy (Nov 2006–Dec 2007)		
Dependent variable =	(1) *Turnover*	(2) *Turnover**	(3) *Turnover*	(4) *Turnover*	(5) *Turnover**	(6) *Turnover*
WD	0.004***	0.004***	0.002**	0.002*	0.002*	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Presale	0.049***	0.005***		0.051***	0.007***	
	(0.004)	(0.002)		(0.004)	(0.002)	
WD × *Presale*	−0.035***	−0.016***		−0.042***	−0.018***	
	(0.004)	(0.002)		(0.005)	(0.003)	
Prepolicy speculative trading			0.166***			0.236***
			(0.059)			(0.038)
WD × *Prepolicy speculative trading*			−0.342***			−0.377***
			(0.027)			(0.024)
Prepolicy turnover	0.057	0.040**	0.050*	0.027	0.022**	0.026*
	(0.034)	(0.015)	(0.027)	(0.019)	(0.008)	(0.015)
Project size	−0.000	0.000	0.000	0.000	0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Project age	−0.001***	−0.001***	−0.000***	−0.000***	−0.000***	−0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Central	0.004***	0.005***	0.004***	0.003*	0.003*	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	0.011***	0.010***	0.010***	0.011***	0.011***	0.011***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	5,467	5,467	5,467	10,153	10,153	10,153
R^2	0.186	0.095	0.077	0.148	0.077	0.055

Notes. 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 postpolicy window (November 2006-through June 2007), and the second three columns present results 12 months after the policy (November 2006 through December 2007). The month when the policy came into effect (i.e., December 2006) 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* for the spot market, but for the presale market, it is speculative turnover, i.e., the number of presale purchases that eventually sold before project completion divided by project size. *WD* is a dummy that is equal to 1 if it is after the buyer stamp duty deferral withdrawal. *Presale* is a dummy that is equal to 1 for presale condominium projects. *Prepolicy turnover* is the average turnover of a project in the three months before the event window (August 2006 through December 2006). *Prepolicy speculative trading* is the average speculative turnover in a presale project during the three months before the policy (August 2006 through October 2006). *Project size* is the number of units in a project. *Project age* is the number of months since project completion. *Central* is equal to 1 if the project is in the central (core) region of Singapore. Standard errors (reported in parentheses) are clustered to allow correlated outcomes within condominium projects in the presale (or control) market in the same location (central versus noncentral) at a given month.

***, **, and * indicate 1%, 5%, and 10% significance, respectively.

market is greater in the presence of speculators. The presale condominium project associated with one-percentage-point-greater speculative turnover before the policy experiences an additional 0.342-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 12 months after the policy implementation. Columns (4)–(6) show results similar to those obtained using a 6-month postevent window. Interestingly, the magnitudes of reduction in total turnover and in speculative turnover become larger in the 12-month period. For example, the decrease in speculative turnover in the presale projects in the 12-month period is equivalent to a 75% reduction compared with the level in November 2006. We perform further analysis by separating the trading response in the first six months from that in the second six months in one single regression and formally compare the size of the trading response using *F*-tests (see Table IA.2 in the Internet appendix for details). The results indicate that the average monthly trading response (measured by both the total turnover and the speculative turnover) is stronger in the second six 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. Note that the trading activity in the spot market experiences no change during the 12-month postpolicy window: the coefficient for *WD* is both statistically and economically insignificant. This suggests that after the policy intervention, investors or speculators have not significantly switched to the spot (control) market.

4.2. Impact on Price Volatility

We now investigate the policy's impact on price volatility measured by the monthly range of pricing errors within individual projects. Again, we study price volatility response in the 6-month and 12-month postpolicy intervention windows.

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 §5.

Table 4 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 6-month window after the policy intervention, and columns (3) and (4) report the 12-month window comparisons.

Column (1) in Table 4 shows that price volatility increases in both the presale and spot market over the six months after the policy intervention, but there is no significant difference in the changes. However, column (3) shows that the increase in price volatility in the presale market is significantly greater than that in the spot market over the 12 months after the policy intervention. The price volatility increase (2.7%) is equivalent to 18% of the average monthly price volatility in the prepolicy period (which is 15%).

To capture the role played by speculative traders, we again introduce as an explanatory variable to proxy for the prevalence of speculative trade, *prepolicy speculative trading* (which is the average speculative turnover in a presale project during the three months before the policy (August 2006 through October 2006)). 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 postevent months is positive but insignificant, whereas 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 postevent windows. Moreover, when we study the volatility response in the first six months and in the second six months in one single regression (see Table IA.2 in the Internet appendix for details), *F*-tests suggest that volatility increase is much higher in the second six months than in the first six months, and the difference is statistically significant at the 1% level. This is consistent with the finding on the dynamics of the trading response. The (speculative) trading activity drops more strongly in the second six months after the policy shock, during which period we also observe the stronger volatility increase. Taken together, these results suggest 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 Tables 3 and 4 present evidence that although 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 more than it does noise traders.

To evaluate the possibility, we exploit the potential variation in the relative presence of informed speculators versus 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 underpriced housing units but cannot easily do so by shorting overpriced housing units. This asymmetry implies a higher proportion of informed speculators in previously underpriced 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 overpriced presale projects (as a proxy of the presence of informed speculators), we conduct the following. First, we obtain $\bar{\varepsilon}_i^t$, the average of presale project's unit-level hedonic pricing residuals in month i (ε_{ij}^t). Based on the distribution of these averages in month t, we call a project i "underpriced" in month t if $\bar{\varepsilon}_i^t$ is in the bottom 30% of the distribution and "overpriced" if it is in the top 30%.[14]

Compared with other presale projects (in particular, the overpriced presale projects), underpriced presale

[14] We have also experimented with the 20% and 80% cutoffs in identifying the underpriced and overpriced presale projects, and we 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 5 remain qualitatively the same.

Table 4 Policy Impact on Price Volatility

	Six months after the policy (Nov 2006–June 2007)		Twelve months after the policy (Nov 2006–Dec 2007)	
Dependent variable =	(1) *Price volatility*	(2) *Price volatility*	(3) *Price volatility*	(4) *Price volatility*
WD	0.038***	0.039***	0.031***	0.036***
	(0.006)	(0.005)	(0.005)	(0.005)
Presale	−0.011		−0.016	
	(0.015)		(0.014)	
WD × *Presale*	0.002		0.027*	
	(0.016)		(0.015)	
Prepolicy speculative trading		−0.618**		−0.539**
		(0.268)		(0.248)
WD × *Prepolicy speculative trading*		0.151		0.385*
		(0.232)		(0.218)
Prepolicy turnover	0.008	0.165	−0.045	0.056
	(0.038)	(0.101)	(0.035)	(0.095)
Project size	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Project age	0.000	0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Central	0.044***	0.044***	0.041***	0.042***
	(0.010)	(0.010)	(0.008)	(0.008)
Transaction volume control	0.005***	0.004***	0.005***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Constant	0.030***	0.026**	0.036***	0.032***
	(0.008)	(0.010)	(0.007)	(0.008)
Observations	2,170	2,170	3,530	3,530
R^2	0.223	0.223	0.230	0.230

Notes. 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 6-month impact (November 2006 through June 2007), and the second two columns present results 12 months after the policy (November 2006 through December 2007). The month when the policy came into effect (December 2006) is excluded. *WD* is a dummy that is equal to 1 if it is after the buyer stamp duty deferral withdrawal. *Presale* is a dummy that is equal to 1 for presale condominium projects. *Prepolicy speculative trading* is the average speculative turnover in a presale project during the three months before the policy (August 2006 through October 2006). *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 prepolicy turnover, project size, project age, and the Central Region dummy (see Table 3). Standard errors (reported in parentheses) are clustered to allow correlated outcomes within condominium projects in the presale (or control) market in the same location (central versus noncentral) at a given month.

***, **, and * indicate 1%, 5%, and 10% significance, respectively.

projects tend to be larger and farther away from project completion. Within our entire sample between 1995 and 2010, underpriced projects tend to attract more speculators in the subsequent month, ceteris 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 speculators drawn to the underpriced projects are likely informed investors.

In Table 5, we examine the different responses to the policy change among presale projects that were under- or overpriced in October 2006, the month immediately before our event window (November 2006 and January to June 2007). Our focus is on the changes in speculative turnovers and price volatility. Thus, in our difference-in-differences regressions, we limit the treatment group (the presale projects) to only the under- and overpriced presale projects and compare them to the control group (all spot market projects).

Columns (1) and (2) of Table 5 show that in November 2006, the month before the policy intervention, the projects underpriced in October 2006 have a relatively higher speculative turnover than the overpriced presale projects, as indicated by the coefficients on *Underpricing* and *Overpricing* (an *F*-test of their difference is significant). This is consistent with the idea that

Table 5 Policy Impact on Trading and Volatility in the Presale (Treatment) Market: Underpriced vs. Overpriced Projects

	(1)	(2)	(3)	(4)
Dependent variable =	*Turnover**		*Price volatility*	
	6 months	12 months	6 months	12 months
WD	0.003***	0.002	0.034***	0.029***
	(0.001)	(0.001)	(0.005)	(0.005)
Underpricing	0.004**	0.006**	−0.055***	−0.046*
	(0.002)	(0.002)	(0.017)	(0.023)
WD × *Underpricing*	−0.014***	−0.015***	0.060***	0.079***
	(0.002)	(0.003)	(0.021)	(0.023)
Overpricing	−0.003	−0.001	0.042***	0.044***
	(0.003)	(0.003)	(0.007)	(0.010)
WD × *Overpricing*	−0.004	−0.006	−0.057***	−0.035***
	(0.004)	(0.004)	(0.014)	(0.012)
Other controls	Yes	Yes	Yes	Yes
Observations	5,217	9,712	2,003	3,285
R^2	0.093	0.069	0.254	0.250

Notes. 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 1 if a presale project has an average project-specific price that is in the bottom 30% distribution among all presale projects in October 2006, and the *Overpricing* dummy is 1 if a presale project has an average project-specific price in the top 30% distribution among all presale projects in October 2006. *Turnover** is equal to the number of speculative purchases divided by project size for presale projects, and it is equal to the number of purchases (i.e., turnover) divided by size for spot projects. *WD* is a dummy that is equal to 1 if a month is after the buyer stamp duty deferral withdrawal (i.e., January 2007). We include the same set of control variables as in Tables 3 and 4. 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 (December 2006) is excluded. Standard errors (reported in parentheses) are clustered to allow correlated outcomes within condominium projects in the presale (or control) market in the same location (central versus noncentral) at a given month.

***, **, and * indicate 1%, 5%, and 10% significance, respectively.

underpriced projects attract more speculators, presumably the informed type.

Most interestingly, the speculative turnover experienced a significant decrease primarily in the underpriced presale projects after the policy introduction, and the decrease in speculative turnover in the overpriced presale projects is both statistically and economically insignificant. Specifically, a typical "previously" underpriced presale project experienced an additional 0.9- to 1.00-percentage-point decrease in speculative turnover after the policy intervention, compared with a typical previously overpriced presale project (i.e., *WD* × *Underpricing* − *WD* × *Overpricing*). The *F*-test for the difference is significant. Compared with 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 relatively more concentrated in overpriced projects, these results suggest that the transaction tax policy not only affects the level of speculative trading but also has compositional implications: it likely exerts a stronger deterrence effect on informed traders than on noise traders.

Alternatively, our result can be consistent with the interpretation that presale projects underpriced in October 2006 attract speculators in November 2006 and thus leave fewer speculators in January 2007 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-prepolicy month. 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 repeat the regression in Table 5, column (1). We find that speculative turnover in underpriced projects is neither statistically nor economically significantly changed in the pseudo-postpolicy 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 5: 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 presale projects. Note that a bigger drop in informed speculators should be associated with a greater volatility increase. On the contrary, a bigger drop in noise speculators should be associated with a greater volatility decrease.

Results in columns (3) and (4) of Table 5 show revealing evidence. First, presale projects underpriced (overpriced) in October 2006 exhibit lower (greater) price volatility in the month before the policy intervention than the control sample (November 2006). These observations are in line with the expectation that informed traders mitigate price volatility, whereas noise traders do the opposite.

Second, during the 6 and 12 months after the policy intervention, these underpriced projects experienced a significant price volatility increase, whereas the opposite is true in overpriced projects. Indeed, underpriced

projects experienced a price volatility increase that is 11.4–11.7 percentage points greater than the overpriced projects (the *F*-test rejects the hypothesis of zero difference between $WD \times Underpricing$ and $WD \times Overpricing$ 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. In addition, the strong volatility response in the cross section of presale projects also helps explain the generally weaker volatility effect in the full sample (see Table 4), since the average volatility change masks the heterogeneity of the policy impact among different types of speculative traders.

We conjecture that the policy has a stronger deterrence effect on rational informed investors than on noise traders, because informed investors are more sensitive to the reduction in expected returns as a result of the policy change. The policy shock effectively increases the transaction cost for presale investors and tightens the capital constraint, which equally affects 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 speculators, 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 such as the probability of informed trading (that are highly dependent on the market structure and trading mechanism). Kyle (1995) suggests that a higher level of information asymmetry implies a greater impact of trading on prices. We apply this general economic intuition to 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 induce a greater price response compared with the prepolicy period. This construct is less sensitive to the specific trading mechanism and frequency under which the underlying asset is transacted. 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 (hereafter referred to as the Amihud measure).[15]

Table 6 Does Price Informativeness Decrease After the Policy?

Dependent variable =	Six months after the policy (Nov 2006–June 2007) (1) *Amihud*	Twelve months after policy (Nov 2006–Dec 2007) (2) *Amihud*
WD	−1.145	−0.035
	(0.884)	(1.073)
Presale	−2.337**	−2.333**
	(0.907)	(1.040)
WD × *Presale*	2.158*	2.834**
	(1.065)	(1.333)
Constant	6.033***	4.888***
	(1.030)	(1.201)
Controls	Yes	Yes
Observations	3,233	5,516
R^2	0.082	0.061

Notes. In this table, we investigate whether price informativeness decreases after the policy change in December 2006. The dependent variable, the 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 6-month impact (November 2006 through June 2007), and the second column presents results 12 months after the policy (November 2006 through December 2007). For a cleaner interpretation, the month in which the policy is enforced (November 2006) is excluded. Other control variables include prepolicy project turnover, prepolicy project-specific return, project size, project age, and the central region dummy. Refer to Table 3 for definitions of the other independent variables. Standard errors (reported in parentheses) are clustered to allow correlated outcomes within condominium projects in the presale (or control) market in the same location (central versus noncentral) in a given month.

***, **, and * indicate 1%, 5%, and 10% significance, respectively.

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 data set (January 1995 through October 2010). In an unreported analysis, we run a cross-sectional regression of presale projects' Amihud measures (854 in total) 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.

Table 6 reports the change in price informativeness before and after the policy intervention across the condominium projects, showing the policy effect over a 6-month and 12-month period, respectively. In both columns (1) and (2), the coefficients of the presale market dummy are significantly negative, sug-

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

gestive 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 6-month and 12-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. Thus, a transaction tax 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.

4.5. Real Effect

In this section, we study the potential real effects of the transaction tax policy on the presale market. Note that the policy might signal potential future government "cooling" policies and dampen expectations of future prices leading to buyers delaying their house purchases. All these might increase developers' difficulty in financing new condominium projects or simply discourage them from initiating projects. In response, the housing starts in the condominium market may decrease. To study this possibility, we collect data from Urban Development Authority and plot the quarterly aggregate housing starts in the private-sector nonlanded (i.e., condominium) market (see Figure 3).[16]

Figure 3 shows that the number of starts in the condominium market experienced an immediate and economically significant drop of about 60% in the first quarter after the policy announcement (i.e., 2007Q1), relative to the prepolicy quarter. However, this effect appears to be transient: the housing starts started increasing again from 2007Q2, which exceeds the prepolicy level in the second half of 2007. The reversal is plausibly related to the strong economic growth both domestically and globally in 2007: indeed, the overall housing market price continues to grow in the period (see Figure IA.1 in the Internet appendix). In other words, strong aggregate economic factors potentially mitigate the policy's negative real impact.

[16] Given the data restriction, we are not able to obtain the housing starts information at a more micro level (e.g., by geography or by developer, or at a higher frequency level). This makes a rigorous statistical analysis difficult: the data do not allow proper identification (such as the counterfactuals) or enough power to accurately identify the real effects. Nevertheless, it is helpful to plot the aggregate time trend in the private-sector nonlanded housing starts.

5. Robustness

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, we present the key results below but report all 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. Although such a transaction can be speculative, it is possible that genuine homebuyers and long-term investors can exit the market before project completion because of unexpected changes in family and financial circumstances. Such happenstances, however, are random across time and project location and should not systematically affect our results.

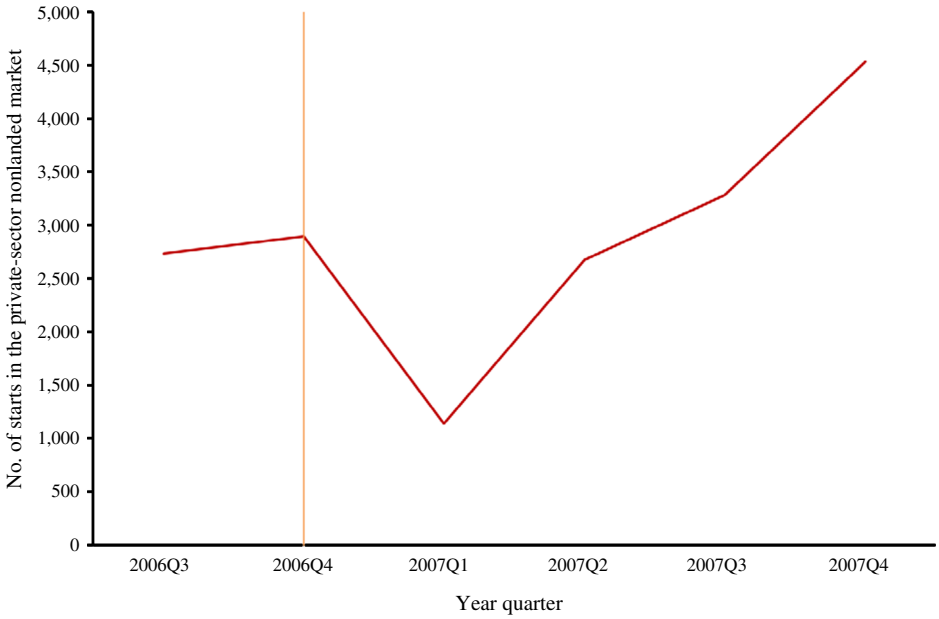
However, we are concerned with projects in which speculators may be systematically under- or overrepresented by the observed flippers. For example, genuine presale homebuyers may sell to take a profit when the market experiences a large and unexpected positive rise in demand before project completion.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

To address the problem, we restrict the analysis to a subsample of projects completed during stable market conditions. For example, we should exclude projects completed in an extreme economic situation, such as during the run-up period before the 2008 financial crisis and the dramatic decline after the crisis. Operationally, we exclude projects from our sample if their last three months before completion fell in periods when the overall market price level was in the top or bottom 30% of the time series distribution between 1995 and 2010.[17] Then we repeat the regressions in Table 3. We find consistent results: the policy intervention discouraged speculative trading in the presale market.

The above robustness check does not fully deal with the misclassification that some real "homebuyers" purchase multiple units in multiple locations in the presale market to hedge against changes in their preferences or delay in project completion time. They eventually sell unwanted units before project completion after they have firmed up their location

[17] The overall market price level is measured by a benchmark price index, estimated from Equation (IA.1) and reported in the Internet appendix.

Figure 3 (Color online) Aggregate Housing Starts in the Condominium Market



Notes. This figure plots the aggregate quarterly number of housing starts in the private-sector nonlanded market in Singapore from 2006Q3 to 2007Q4. The data are obtained from the URA REALIS database.

preference or have found a firm project completion time that matches well with their intended move-in time. In our sample, these real buyers may be classified as flippers. This overrepresentation of flippers does not necessarily weaken the likelihood of finding our results, because a rise in transaction cost reduces the attractiveness of the hedging strategy. Although we cannot categorically exclude these possibilities, we believe these types of real homebuyers are rare, because these hedging strategies are very costly.[18]

We do not distinguish short-term speculators in the spot market, though their presence cannot be ruled out. What is important to our difference-in-differences analysis, however, is that the short-term speculators in the spot market are not affected by the policy intervention we study. There remains a possibility that short-term speculators in the presale market move to the spot market after the policy intervention, since the imposition of stamp tax changed the relative cost of investing in the two market segments. If so, we would observe a noticeable increase in turnover activity in the spot market after the policy announcement. However, in Table 3, we see a very small increase in trading activity in the spot market (coefficient on *WD*), casting doubt on the empirical relevance of this conjecture. Therefore, this particular concern is unlikely to affect our main findings and conclusions.

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 (see Table 4).

Measurement errors in the dependent variable 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 ordinary least squares (OLS) regression as shown in Tables 3–5, and we 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 reestimate the second-stage OLS with the predicted residual squared as the regression weight. This specification corrects for the heteroskedasticity. We obtain results qualitatively identical to what we have reported.

We also partition the sample by project size to perform the price volatility analysis on more homogeneous subsamples; the dividing size is 204 units (the mean in our presale sample). Projects of similar size

[18] Consider a foreign individual moving to Singapore wanting to ascertain a place to stay upon arrival. The person can rent. The rental cost of a $5 million condo is about $120,000 in 2006–2007. To buy multiple compatible units, the person faces price risks. A 2.5% drop in price will be more than enough to pay rental. In addition, the buyer faces legal fees and a $150,000 stamp duty when selling. Simple economic calculation says that a rational person is unlikely to adopt such a hedging strategy.

are more comparable in transaction volume. We perform the above weighted least squares regression on project price volatility as in Table 4. The price volatility increase is stronger and more significant among the larger condominium projects.

Next, we note that our volatility measures are derived from the pricing residuals in the hedonic pricing regression—the range of the residuals measures the price volatility for each project. 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. It results in more heterogeneous price residuals in the spot market (relative to the presale market) transactions.

Table IA.1 in the Internet appendix shows the results for the hedonic pricing regression for both presale and spot markets. 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^2 values are 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 markets 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 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. We also apply the weighted least squares approach, and the results are qualitatively the same.

Last, we discuss the hypothesis whether the observed volatility response can be (partly) due to a supply-side inventory effect. Specifically, it may be that speculators, without an intention to live in the properties, flood the market after the introduction of the policy, making it more costly for them to sell. This supply-driven effect will mechanically increase the impact of the tax change on the price volatility among units within uncompleted buildings. However, this specific channel would imply a downward pressure on the price growth after the policy introduction. Examining the price growth response in the presale market relative to the change in the spot market using the same difference-in-differences specification, we find that presale projects experienced no price drop after the policy announcement in either the 6-month or the 12-month postpolicy period. Furthermore, we refer to the nuanced results in the cross-sectional heterogeneity in volatility response to differentiate our interpretation from the described argument. As Table 5 shows, underpriced and overpriced presale projects have distinct volatility response to the policy: the price volatility (or price dispersion) increases significantly in underpriced projects but decreases in the overpriced projects. The mechanical inventory effect cannot explain this heterogeneous volatility response unless speculators rush to sell primarily in the underpriced projects, which is counterintuitive. (One would expect the opposite: speculators in overpriced projects are more likely to be impatient as there is on average smaller room for further price appreciation.) Taken together, our main findings on the volatility response are unlikely the result of such a mechanical effect.

5.3. Is the Timing of the Policy Intervention Anticipated by Market Participants?

An alternative interpretation of our main results is that some 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 price volatility after the policy intervention are not transitory; the effects remain strong for a period of 6 to 12 months. To be conservative, we repeat our turnover and volatility regressions excluding the month immediately after the policy is enacted (January 2007). Our results remain 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.[19]

6. Conclusion

The empirical challenge in studying the effect of transaction tax lies in its deterrence of both speculative and nonspeculative investors. In addition, among

[19] 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.

speculators, it deters both informed and noise speculators. Our study provides the first piece of evidence that a transaction tax deters speculative trading activities and raises price volatility because it discourages informed speculators more than noise speculators.

We take advantage of a policy intervention in Singapore's private residential housing markets in December 2006. The setting offers many empirical advantages. First, the unexpected policy intervention raised the transaction cost, equivalent to a two- to three-percentage-point increase in transaction tax, in one market segment (presale market) that particularly attracts speculators but not that of the other market segment (spot market). Indeed, the policy was intended to mitigate house price appreciation, and price volatility was not the focus. Results based on the difference-in-differences identification show that the transaction cost increase significantly deterred speculative trading (by 75%) and raised price volatility (by 18%) in the affected segment.

Second, because house prices adjust more slowly than regular financial asset prices, we are able to identify temporarily over- and underpriced projects. Because of difficulties in short-selling presale contracts, underpriced presale projects attract informed speculators, but overpriced ones do not. Instead, the latter tends to attract disproportionally more noise speculators than the former. We find that although the policy intervention generally reduced trading in all presale projects, the drop was more pronounced in the projects underpriced just before the intervention. In addition, the increase in price volatility was concentrated in these projects. By contrast, the presale projects overpriced just before the intervention experienced a smaller reduction in speculative trading and little volatility change afterward. These findings indicate that informed traders are more sensitive to the heightened transaction tax than other traders. Accordingly, we find that price informativeness dropped in the affected presale housing market. All our findings are robust against alternative measures of volatility and speculative activity.

Overall, our findings caution against raising transaction tax to mitigate asset price volatility. Although housing markets are different from high-trading-frequency asset markets, our results may be broadly relevant for the global debate on the role of transaction tax in financial markets and on curbing excess volatility. In most cases, speculative trades comprise informed and noise trading, and a transaction tax potentially deters both. Our results show that the former may be more affected than the latter, leading to increase rather than decrease in overall price volatility.

These results also directly contribute to the debate over the role of speculators in the recent housing crisis. Recent studies find that housing market speculators helped contribute to the rise and fall of the U.S. housing prices that deviate from the fundamental in the recent crisis (Mayer and Sinai 2007, Haughwout et al. 2011, Cheng et al. 2014, Chinco and Mayer 2016). Our evidence suggests that we should exercise caution about taxing housing speculators, given a potentially differential effect in discouraging the informed housing speculators.

Supplemental Material

Supplemental material to this paper is available at https://doi.org/10.1287/mnsc.2015.2268.

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