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NO-BUBBLE CONDITION: MODEL-FREE TESTS IN HOUSING MARKETS

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We test for the existence of housing bubbles associated with a failure of the transversality condition that requires the present value of payments occurring infinitely far in the future to be zero. The most prominent such bubble is the classic rational bubble. We study housing markets in the United Kingdom and Singapore, where residential property ownership takes the form of either leaseholds or freeholds. Leaseholds are finite-maturity, pre-paid, and tradeable ownership contracts with maturities often exceeding 700 years. Freeholds are infinite-maturity ownership contracts. The price difference between leaseholds with extremely-long maturities and freeholds reflects the present value of a claim to the freehold after leasehold expiry, and is thus a direct empirical measure of the transversality condition. We estimate this price difference, and find no evidence of failures of the transversality condition in housing markets in the U.K. and Singapore, even during periods when a sizable bubble was regularly thought to be present.

KEYWORDS: Asset pricing, real estate, rational bubbles, transversality condition.

0. INTRODUCTION

THE EXISTENCE OF BUBBLES IN ASSET MARKETS is one of the fundamental debates in economics and finance, yet challenges to designing appropriate tests for bubbles have prevented an empirically driven resolution to this debate. In this paper, we propose a new model-free and direct empirical test for the existence of bubbles associated with failures of the transversality condition, the most prominent of which is the classic rational bubble. Our test overcomes many of the challenges that pervade the existing empirical literature.

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Consider an asset that pays dividend D_t in each period, and denote its price at time t by P_t. If $\xi_{t,t+1}$ is a valid stochastic discount factor for this asset, the price today equals the present discounted value of the price and the dividend tomorrow:

$$P_t = E_t\big[\xi_{t,t+1}(P_{t+1} + D_{t+1})\big]. \tag{1}$$

Applying a recursive argument and the law of iterated expectations, we obtain

$$P_t = \sum_{s=1}^{\infty} E_t[\xi_{t,t+s} D_{t+s}] + B_t, \quad B_t \equiv \lim_{T\to\infty} E_t[\xi_{t,t+T} P_{t+T}], \tag{2}$$

where $\xi_{t,t+s} \equiv \prod_{j=0}^{s-1} \xi_{t+j,t+j+1}$ is the stochastic discount factor between periods t and $t+s$. The price of the asset is decomposed into its fundamental value (i.e., the present discounted value of dividends), and a bubble component, B_t. If present, the bubble evolves according to $B_t = E_t[\xi_{t,t+1} B_{t+1}]$, with $B_0 > 0$. Theories about classic rational bubbles provide sharp null and alternative hypotheses: $B_t = 0$ if there is no bubble and the transversality condition is not violated, and $B_t > 0$ if there is a bubble because it implies a positive price today for a claim that postpones making a payment indefinitely and, therefore, has zero fundamental value.[2]

The classic rational bubble has a longstanding tradition in the theoretical literature, with seminal papers by Samuelson (1958), Diamond (1965), Blanchard and Watson (1982), Tirole (1982, 1985), and Froot and Obstfeld (1991). It has since become the workhorse model of bubbles in macroeconomics (e.g., Caballero and Krishnamurthy (2006), Arce and López-Salido (2011), Martin and Ventura (2012, 2014), Farhi and Tirole (2012), Doblas-Madrid (2012), Giglio and Severo (2012), Galí (2014), Galí and Gambetti (2015), Caballero and Farhi (2014)).

Despite the role of classic rational bubbles in theoretical models, empirical evidence on their existence has remained elusive. A simple test for classic rational bubbles is to verify whether claims to payments at infinite maturity do, in fact, have zero present value. This direct test, however, has been impossible to conduct so far because we normally do not observe traded claims to payments that only occur at even approximately infinite maturity. Due to the challenges with directly measuring the price of very long-run financial claims, researchers have resorted to indirect, model-dependent tests of bubbles, thus incurring the *joint hypothesis problem*: every test of a bubble is a joint test of the presence of

[2]We focus on the case $B_t > 0$, rather than $B_t < 0$, because a negative bubble can be ruled out in theory if there is free disposal of rents. Nonetheless, our test would detect negative bubbles if they were present.

the bubble in the data and the validity of the model applied by the econometrician.[3]

Given the difficulties in empirically testing for classic rational bubbles, the literature has explored the theoretical conditions under which these bubbles can arise in equilibrium. Brock (1982), Tirole (1982), Milgrom and Stokey (1982), and Santos and Woodford (1997) argued that these conditions are somewhat special since the presence of a bubble is inconsistent with the optimization problem of an infinitely lived representative agent, inconsistent with backward induction in a finite horizon setting, inconsistent with the "no-trade theorems" in settings with asymmetric information and common priors, and inconsistent with finiteness of the present value of the economy's output or endowment. These theoretical challenges have inspired a literature demonstrating that classic rational bubbles can occur in economies with combinations of overlapping generations, incomplete markets, and financial frictions (Tirole (1985), O'Connell and Zeldes (1988), Kocherlakota (1992, 2008), Farhi and Tirole (2012), Doblas-Madrid (2012)). This literature reaffirmed the theoretical plausibility of the classic rational bubble as an equilibrium phenomenon. It seems unlikely, therefore, that the debate over the existence of these bubbles can be settled on purely theoretical grounds; on the contrary, it is an inherently empirical question.

We advance this debate by providing a new, direct test of classic rational bubbles. Our test exploits a unique feature of housing markets in the United Kingdom and Singapore, where property ownership takes the form of either very long-term leaseholds or freeholds. Leaseholds are *finite-maturity*, pre-paid, and tradeable ownership contracts, often with initial maturities of 999 years, while freeholds are *infinite-maturity* ownership contracts. The price difference between leaseholds with extremely-long maturities (e.g., more than 700 years) and freeholds for otherwise identical properties captures the present value of a claim to the freehold at lease expiry, and thus closely approximates the price of the bubble claim, B_t. We estimate the price difference between freeholds and extremely-long leaseholds to obtain a direct estimate of the price of the bubble claim, and test whether it is indeed positive. This test exploits the fact that rational bubbles can only be attached to an infinite-maturity asset such as a freehold, but not to a finite-maturity asset such as a leasehold. Our test has the advantage of being both *model-free* and *direct*. It is model-free in the sense that all structural models agree that the fundamental value of the claim B_t is zero. It is direct because we test the very condition that defines the bubble, the no-bubble condition, rather than deriving and testing indirect, model-implied necessary or sufficient conditions for the existence of a bubble.

[3]Both sides of the debate on the efficient market hypothesis agree on this fundamental difficulty. Shleifer (2000) remarked: "The dependence of most tests of market efficiency on a model of risk and expected return is Malkiel and Fama's (1970) deepest insight, which has pervaded the debates in empirical finance ever since."

Our empirical analysis is based on proprietary information on the universe of property sales in the U.K. and Singapore between 1995 and 2013. These data contain information on transaction prices, leasehold terms, and property characteristics such as location and structural attributes. We estimate the price of the bubble claim by comparing the prices of leaseholds with maturities between 700 years and 999 years to the prices of freeholds across otherwise identical properties. We use hedonic regression techniques to control for possible heterogeneity between leasehold and freehold properties. We find that extremely-long leaseholds are valued identically to otherwise similar freeholds. Our results, therefore, show no violation of the no-bubble condition in these markets. This is true on average, as well as for time periods and geographic regions with a higher ex ante probability of finding a bubble, such as regions with high house price–income ratios.

We address a number of potential challenges to our methodology of directly testing for classic rational bubbles in these housing markets.[4] One concern is that freeholds might be inferior on unobservable property characteristics, and that their price parity with leaseholds is therefore masking a bubble. We show that this is not the case by documenting that annual rents are identical across leasehold and freehold properties. Since relevant differences in property characteristics should be reflected in these rents, the flow utility from inhabiting either type of property has to be the same. This finding is not surprising since properties with extremely-long leaseholds and freeholds located in the same geography are essentially identical on all observable characteristics, making it unlikely that they would differ substantially on unobservable characteristics. We also show that freehold and leasehold contracts are similarly liquid, and have a similar "time-on-market" when listed for sale, addressing concerns that differences in liquidity might mask a bubble.

We also discuss a number of institutional features of these housing markets that might affect our interpretation. We first show that, institutionally, neither the assignment of redevelopment rights nor the assignment of maintenance costs should have a quantitatively important effect on the relative value of extremely-long leaseholds and freeholds. Consistent with this, we find that the absence of a significant price difference is stable across regions and properties with differentially valuable redevelopment options and different practices of assigning maintenance responsibilities. We also show that taxes, lease extensions, concerns about property rights, and the differential timing of sales and originations of different types of contracts do not affect our estimated price differences. In addition, the fact that we obtain the same results in two markets

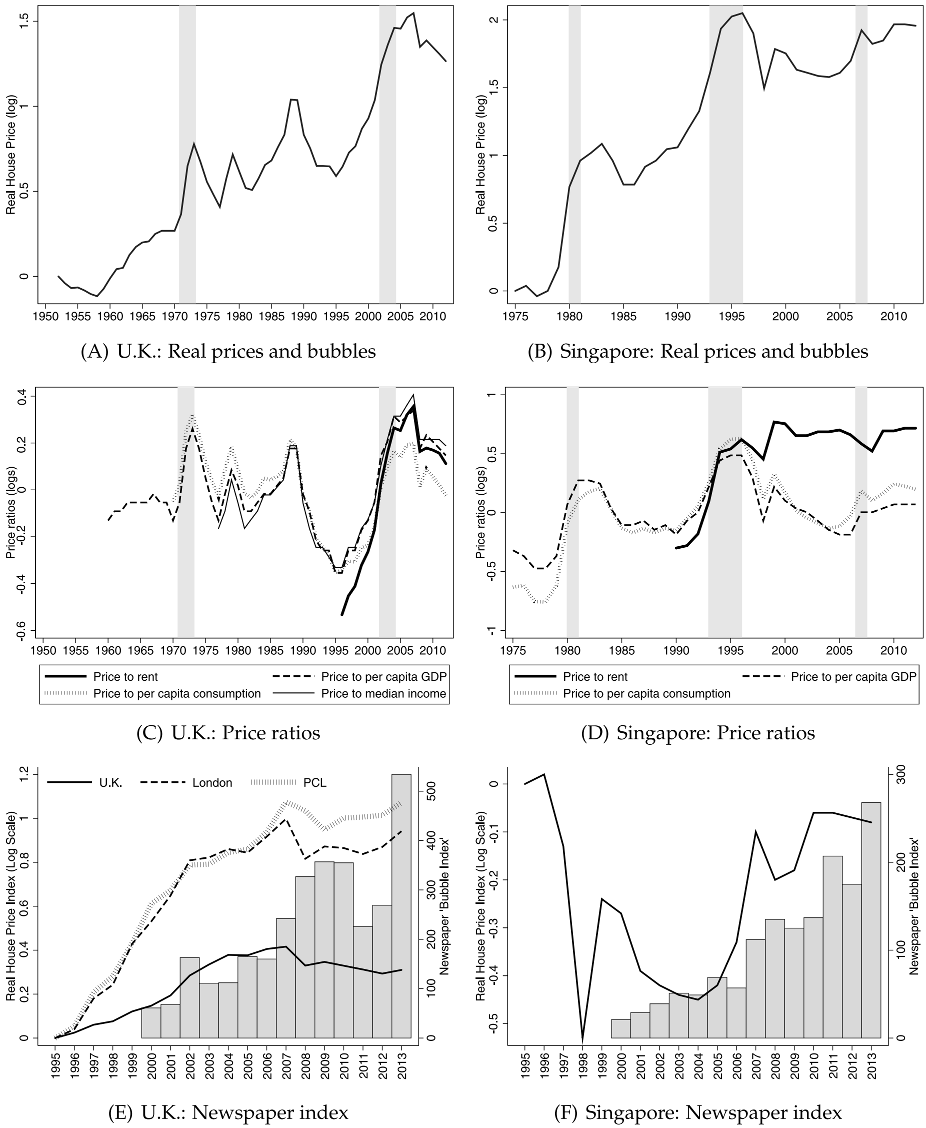
[4]Potential challenges to our test have to conjecture (i) the possibility that a bubble is present, thus making the freehold more valuable than the extremely-long leasehold, but that (ii) some confounding factor increases the price of the extremely-long leasehold precisely by the amount of the bubble. The two contracts would then trade at the same price, as we estimate in the data, which would mimic an equilibrium without a bubble.

with different institutional and economic environments, the U.K. and Singapore, minimizes the concern that our no-bubble result is due to market-specific institutional frictions. We conclude that no institutional features of extremely-long leaseholds significantly affect their value relative to freeholds.

The theory of bubbles is vast and richly varied, with different authors associating different phenomena with the term "bubble." Section 1.3 provides more details on the nomenclature adopted in this paper and the exact scope of our test. The takeaway is that we can rule out any bubble associated with a failure of the transversality condition. The most prominent such bubble is the classic rational bubble described above,[5] but failures of the transversality condition can also arise, for example, in the myopic-rational-expectations equilibrium of Tirole (1982), and in economies with differences in beliefs à la Harrison and Kreps (1978). We focus on bubbles that require a failure of the transversality condition because they are a workhorse model of bubbles, especially in macroeconomics, and because we can offer a clean test for such bubbles. These are not the only models of bubbles, and our paper and test methodology are silent on the possible presence of bubbles that can occur in finite-horizon economies or on finite-maturity assets.

Our empirical analysis focuses on housing markets in the United Kingdom and Singapore, because the institutional setup provides a clean test of classic rational bubbles; we are silent on the possibility of bubbles in other asset classes and time periods. In addition, housing has been the subject of bubble-related attention in recent years. Figure 1 shows the behavior of house prices in the U.K. and Singapore, both in levels and relative to measures of fundamentals such as income and rents. Both countries experienced episodes of strong increases in real house prices, as well as in price-income and price-rent ratios during our sample. Motivated by such evidence, the academic literature has speculated about the presence of bubbles in housing markets during our sample period. For example, Martin and Ventura (2012), Galí (2014), and Galí and Gambetti (2015) motivated their rational bubble model with the recent boom-bust pattern in house prices, and Kocherlakota (2009), Arce and López-Salido (2011), Basco (2014), and Miao, Wang, and Zhou (2014) provided models of classic rational bubbles to explain recent house price movements. In addition, we show that existing time-series tests for classic rational bubbles, such as Phillips, Shi, and Yu (2014), suggest the presence of such bubbles in both countries during our sample. We therefore conclude that our focus on the housing markets in Singapore and the U.K. during a period of boom-bust cycles provides a setting with a good ex ante chance of detecting a bubble; however, our results show that no classic rational bubble was actually present.

[5]We use the term "classic" to denote the literature on rational bubbles in the style of Tirole (1985). Other authors have derived bubbles that they also call rational bubbles, but that can occur in finite-horizon economies (see Conlon (2004), Doblas-Madrid (2014)). These latter bubbles are not the subject of our test.



(A) U.K.: Real prices and bubbles

(B) Singapore: Real prices and bubbles

(C) U.K.: Price ratios

(D) Singapore: Price ratios

(E) U.K.: Newspaper index

(F) Singapore: Newspaper index

FIGURE 1.—U.K. and Singapore—house prices and bubble index. *Note*: The top row shows the log real house prices in the U.K. and Singapore, normalized to 0 at the beginning of the sample. The middle row shows various log scaled price measures in the two countries. Each series is shifted by a constant to improve readability. Shaded areas show the periods in which the Phillips, Shi, and Yu (2014) test detects a classic rational bubble (see Appendix A.2.2.2). The bottom row reports the real house prices in the U.K., London, Prime Central London (PCL), and Singapore, together with a "bubble index" that counts how often real estate bubbles are mentioned in each country's newspapers. The "bubble index" can only be constructed since 2000. See Appendix A.1.1 for details on the construction of these series.

Determining the empirical relevance of bubbles associated with failures of the transversality condition can provide guidance as to which types of bubbles theoretical models should focus on. It is often argued that the prevalence of the classic rational bubble in macroeconomics is due to its theoretical tractability, rather than to its realism. However, this modeling choice is not innocuous: for example, we show in Section 4 that while classic rational bubbles grow faster with higher interest rates, the resale-option bubble at the core of Harrison and Kreps (1978), Scheinkman and Xiong (2003), and Simsek (2010), which our tests are silent on, shrinks when interest rates increase. Such properties play an important role in the debate around the effects of central banks "leaning against the wind" by raising interest rates to combat asset-price bubbles (Allen and Gale (2004), Bernanke (2010), Galí (2014)). More generally, since we find no evidence for the classic rational bubble that is commonly used in the macroeconomics literature, it will be important going forward to understand how the positive and normative conclusions of this literature would change in the presence of a (possibly irrational) finite-horizon bubble.

1. INSTITUTIONAL SETTING AND EMPIRICAL TEST

We next document that, during our sample period, the housing markets in the U.K. and in Singapore featured many characteristics commonly associated with asset-price bubbles. We then describe the relevant institutional setup in these housing markets, and formalize our empirical test for classic rational bubbles.

1.1. *Existing Econometric Evidence Suggestive of Bubbles*

In Figure 1, we explore the time series of prices and fundamentals in the U.K. and Singapore housing markets, and show that they display many of the features of asset-price bubbles.[6] In the top row of Figure 1, we plot the log of real house prices in the two countries. The most recent boom in U.K. house prices started in the mid-1990s, but was preceded by several other boom-bust episodes, for example in the mid-1980s. Similarly, Singapore experienced several run-ups and collapses in house prices since the 1970s, for example around the 1997 Asian financial crisis. In both countries, real house prices grew quickly and reached elevated levels in the global housing boom years of 2000–2007; for example, real house prices increased by 86% in the U.K. during this period.

The middle row of Figure 1 shows that house prices relative to fundamentals also exhibited patterns consistent with the presence of a bubble. We plot

[6]Appendix A.1 of the Supplemental Material (Giglio, Maggiori, and Stroebel (2016)) provides details on the construction of all data series and their sources.

the log of scaled prices, $\log(price_t/f_t)$, for different measures of fundamentals, f_t: rents, median income per capita, GDP per capita, and household consumption per capita. During our sample, there were several episodes in which prices increased relative to fundamentals. For example, between 1995 and 2005, the price-rent ratio in the U.K. more than doubled; prices also rose relative to our other measures of fundamentals. The price-rent ratio in Singapore showed similar run-ups in prices relative to fundamentals in the mid-1990s. Such price increases, in particular relative to fundamentals, are often interpreted as signs of housing bubbles in the academic literature (see, e.g., Case and Shiller (2003), Himmelberg, Mayer, and Sinai (2005), Caballero and Krishnamurthy (2006), Wheaton and Nechayev (2008), Piazzesi and Schneider (2009), Glaeser, Gottlieb, and Gyourko (2010), Mayer (2011), Galí (2014), Nathanson and Zwick (2014)).

The bottom row of Figure 1 shows that non-academic market participants and observers also considered these price movements as evidence for a bubble. We plot a "bubble index," constructed by counting the number of references to "real estate bubbles" in major national newspapers. In both countries, fast increases in house prices were accompanied (and partly followed) by a large increase in references to housing bubbles in national newspapers. Panel E also zooms in on the movements in U.K. house prices during our sample period 1995–2013, not only at the country level, but also in the areas of London and Prime Central London, where price run-ups were even larger.

Previous formal tests for classic rational bubbles have focused on *indirect* measures of failures of the no-bubble condition, most notably by testing the cointegration between prices and some transformation of current dividends (Diba and Grossman (1988)). These tests exploit the fact that the presence of a bubble leads to a potentially explosive path (i.e., integrated of a higher order) for prices that is not reflected in dividends, thus inducing a non-stationary price-dividend ratio. The most recent advancements of this class of tests by Phillips, Wu, and Yu (2011) and Phillips, Shi, and Yu (2014) addresses the concern of Evans (1991) that, in small samples, periodically collapsing bubbles might look more stationary than their true data generating process. They do so by allowing for integration tests on subsamples of the data, while appropriately adjusting the test statistics. In fact, recent work has applied such explosive root tests to housing markets, and has found evidence in favor of the existence of classic rational bubbles: Garino and Sarno (2004) focused specifically on the U.K. and Jiang, Phillips, and Yu (2014) focused on Singapore, while Pavlidis, Yusupova, Paya, Peel, Martinez-Garcia, Mack, and Grossman (2013) and Engested, Hviid, and Pedersen (2015) provided evidence of classic rational bubbles in house prices for many countries, including the United Kingdom.

In this paper, we use our novel data and methodology to test for classic rational bubbles in both aggregate house price series, as well as by focusing on specific subsamples and subperiods that were ex ante more likely to contain a bubble. In choosing these samples in Section 2.4, we rely both on cross-sectional and time-series dispersion of house price growth and price-fundamental ratios described above, and on statistical evidence for bubbles detected by the *existing* time-series tests. In the U.K. between 1952 and 2014, the Phillips, Shi, and Yu (2014) test identifies 1971–1973 and 2002–2004 as bubble episodes with a 5% confidence level.[7] In Singapore, the test identifies three bubble episodes between 1975 and 2014: 1980–1981, 1992–1996, and 2007. The top and middle rows of Figure 1 shade these periods. We also perform this test separately on London, Prime Central London, and on 100 subregions of the U.K. identified by their postcode areas. In all of these subsamples, one or more bubbles are identified by the existing time-series tests, mostly concentrated around the period 2002–2004; however, the test also identifies additional local bubbles that affect some regions but not all of them, and which therefore do not appear in the aggregate. Appendix A.2 of the Supplemental Material reports the results of the tests for each of these subsamples.[8]

1.2. *Institutional Setting*

Residential real estate ownership in both the U.K. and in Singapore comes in two forms: infinite-maturity ownership, called a freehold, and long-duration, finite-maturity ownership, called a leasehold. A leasehold is a grant of exclusive possession of the property for a clearly defined, finite period of time during which the tenant can exclude all other people from the property, including the freeholder (Burn, Cartwright, and Cheshire (2011)).[9] In the U.K., common initial lease lengths are 99, 125, 150, 250, and 999 years. In Singapore, initial lease lengths are either 99 or 999 years. During the life of the lease, the

[7]Of the many possible tests for explosive roots, we implement the most recent advancements in this literature by Phillips, Shi, and Yu (2014). Appendix A.2 of the Supplemental Material provides details on the implementation of these tests. The test statistic for bubble detection is not significant in 2003, but is significant in both 2002 and 2004.

[8]While these tests find statistical evidence for classic rational bubbles in some periods in our sample, they could also be classifying very fast, temporary changes in prices due to time-varying but persistent discount rates as bubbles (see Cochrane (1992)). Our test, on the other hand, by comparing two almost identical assets, the leasehold and the freehold, nets out the effect of changes in the discount rates applied by households.

[9]This contract structure is not unique to the U.K. and Singapore. The real estate literature has studied the pricing of leasehold and freehold contracts in a variety of settings and countries (e.g., Capozza and Sick (1991), Wong, Chau, Yiu, and Yu (2008), Iwata and Yamaga (2009), Tyvimaa, Gibler, and Zahirovic-Herbert (2014), Bracke, Pinchbeck, and Wyatt (2014), Gautier and van Vuuren (2014)). None of these papers focused on the implications for studying bubbles. Giglio, Maggiori, and Stroebel (2015) and Giglio, Maggiori, Stroebel, and Weber (2015) also exploited this institutional setup, but used it to address a different economic question: the estimation of very long-run discount rates. Due to the different economic question, their use of the data also differs from the present paper. While they focused on shorter (0–300 year) leaseholds on flats, we focus on extremely-long (700+ years) leaseholds and freeholds on houses. We also present a number of tests analyzing the cross-section and time series of potential price differences between leaseholds and freeholds, none of which were studied by Giglio, Maggiori, and Stroebel (2015).

lessee is entitled to rights similar to those a freeholder would have, including the right to mortgage and rent out the property. Leaseholds and freeholds are also treated equally for tax purposes. Unlike for commercial leases, the vast majority of the costs associated with a residential leasehold comes through the up-front purchase price; annual payments, the so-called "ground rents," are small to non-existent, and do not significantly affect the prices paid for leaseholds. Leasehold properties are traded in liquid secondary markets, where the buyer purchases the remaining term of the lease. The markets for freehold and leasehold properties are fully integrated, and the two types of contracts are advertised side-by-side by real estate agents and on online platforms. Once the lease expires, the ownership reverts back to the residual freeholder.

In the U.K., there is a broad set of residual freeholders, including large private corporations, aristocratic estates, the Church of England, Oxford and Cambridge Colleges, and the Royal Family. In Singapore, by far the largest residual freeholder is the government of Singapore, represented by the Singapore Land Authority (SLA). In Singapore, leaseholders have no statutory right to lease extensions or to acquire the underlying freehold interest, a process called enfranchisement. In the U.K., the Leasehold Reform Act 1967 has provided owners of houses with the right to extend the lease or enfranchise at market prices. Such transactions entail significant costs, including those for engaging a valuer and a solicitor, as well as the uncertainty and costs of a possible court trial. In Section 3, we discuss a number of factors that might differentially affect the flow utility of leasehold and freehold properties, such as potentially restrictive covenants, taxes, and the assignment of maintenance costs and redevelopment rights, and show that their quantitative effect on prices is small.

1.3. *Empirical Test*

The institutional setting of the U.K. and Singapore is uniquely suited to directly testing the transversality condition, $B_t \equiv \lim_{T\to\infty} E_t[\xi_{t,t+T}P_{t+T}] = 0$, since it allows us to estimate the present value of a claim to the freehold occurring at extremely-long horizons. Let P_t be the price of the freehold contract at time t, and P_t^T the price of the leasehold contract with maturity T at time t. A simple algebraic substitution, detailed in Appendix A.4 of the Supplemental Material, shows that $P_t - P_t^T = E_t[\xi_{t,t+T}P_{t+T}]$. Intuitively, the difference in value between a freehold and a T-maturity leasehold is the present value of the claim to the infinite stream of rents starting T years from today. This is also the value of the claim to the freehold T years from now.

We focus on leaseholds with maturities in excess of 700 years, a horizon sufficiently long to approximate well the infinity limit of the transversality condition:

$$P_t - P_t^T \approx B_t = \lim_{T\to\infty} E_t[\xi_{t,t+T}P_{t+T}] \quad \text{for} \quad T > 700 \text{ years.}$$

We test whether the transversality condition holds, by testing whether the price difference between extremely-long leaseholds and freeholds is zero. To make the interpretation of this difference easier, we normalize the price discount by the price of the freehold: $Disc_t^T \equiv \frac{P_t^T}{P_t} - 1$. Then, $-Disc_t^\infty$ is the fraction of the current price of the asset (the freehold) that is due to the classic rational bubble. We correspondingly formulate our null hypothesis of no classic rational bubbles as: $Disc_t^T = 0$ for $T > 700$ years. A violation of the null hypothesis would constitute evidence of a bubble. This kind of bubble would be predominantly associated with the classic rational bubble in the theoretical literature, but could also be evidence of a (smaller) set of bubbles with non-rational elements that generate a failure of the transversality condition, such as Tirole's (1982) myopic-rational-expectations bubble.[10]

While the finding that $B_t = 0$ would rule out the presence of the bubbles discussed above, it would not rule out the presence of all types of bubbles in our data. In particular, it would not rule out the presence of bubbles, many of which are popular in the behavioral literature, that can arise even in finite-horizon economies or on finite-maturity assets.[11] Indeed, our test exploits the theoretical restriction that while the classic rational bubble can occur on an asset that pays no dividends, such as money, it is essential that the asset has infinite maturity. The classic rational bubble cannot occur on an asset of arbitrarily long but finite maturity. This is because classic rational bubbles derive their value from agents' expectations of being able to resell the bubble claim at a sufficiently high price, with each agent expecting to sell the bubble to the next agent. The finite maturity of the asset breaks this loop, because no agent would want to hold the bubble in the last period before maturity; backward induction then makes it impossible for the bubble to be present in any earlier period. In our context, this means that the bubble could affect the price of a freehold, an infinite-maturity asset, but cannot affect the price of a leasehold, a finite-maturity asset. The finite-maturity nature of leaseholds is discussed in Appendix A.3.5 of the Supplemental Material. Based on this restriction, our empirical test requires two further identifying assumptions: that leasehold and freehold cash flows only differ in their maturity, and that maturities greater

[10]Tirole showed that in a setup with finitely many infinitely-lived agents, a bubble can emerge if strict rationality is relaxed, and formalized the concept of a myopic-rational-expectations equilibrium (used informally by Sargent and Wallace (1973), and others), as one where, in sequential trading, "*in each period [the agents] compare their current trading opportunities with the expected trading opportunities in the following period.*" Thus "*traders choose their trades on the basis of short-run considerations*," hence the myopia.

[11]See, for example, DeLong, Shleifer, Summers, and Waldmann (1990a, 1990b), Allen, Morris, and Postlewaite (1993), Barberis, Shleifer, and Vishny (1998), Abreu and Brunnermeier (2003), Barberis and Shleifer (2003), Scheinkman and Xiong (2003), Hong, Scheinkman, and Xiong (2006, 2008), DeMarzo, Kaniel, and Kremer (2008), Simsek (2010), Glaeser and Nathanson (2015), and Barberis, Greenwood, Lawrence, and Shleifer (2015). A number of these papers adopt infinite horizons for convenience, but the bubbles that they model would survive in finite-horizon and finite-maturity assets settings.

than 700 years are a close approximation to the infinity limit, that is, that the present value of rents more than 700 years in the future is essentially zero. Validating the first assumption is the key focus of our empirical estimation (Section 2) and robustness checks (Section 3). We next discuss the second assumption.

We find a horizon between 700 and 999 years to be sufficiently long to approximate the infinity limit of the no-bubble condition. We (informally) quantify the approximation error, by considering the Gordon (1962) growth environment where rents grow at rate g, and are discounted at a constant rate r. A simple derivation, provided in Appendix A.4 of the Supplemental Material, shows that $Disc_t^T = -e^{-(r-g)T}$. In the absence of a bubble, even a very low net discount rate $r - g$ of 1% would imply leasehold discounts as low as −0.09% and −0.001% at 700 and 900 years, respectively. A net discount rate of 1% is much lower than the values normally estimated in the literature, which has found average housing returns r to be above 6% and real rent growth g to be below 1%, leading to a more plausible calibration of $r - g$ above 5% (e.g., Flavin and Yamashita (2002)).[12] This confirms that our horizon is sufficiently long for the approximation to hold well, even in the presence of very low net discount rates.[13]

Finally, while it is expositionally convenient to employ the concept of a stochastic discount factor, $\xi_{t,t+T}$, the existence of which requires the law of one price to hold, our empirical tests do not strictly require even this mild restriction. Even if the law of one price was violated, both in the data and in a model of bubbles with frictions, a classic rational bubble would still be defined as a positive price for an asset that postpones cash flows indefinitely, and our test would correctly identify it in the real estate market.

2. TESTING THE NO-BUBBLE CONDITION

In this section, we present results from our empirical tests of the no-bubble condition. We first describe the data used in the analysis. We then provide aggregate results that pool transactions from all years. We also present more geographically and temporally disaggregated results, for which we focus on subsamples of the data that, based on observable characteristics, were more likely ex ante to contain a bubble.

Estimating the relative price of leaseholds and freeholds is challenging, because the underlying properties are heterogeneous assets, and we do not ob-

[12] Giglio, Maggiori, and Stroebel (2015) estimated long-run net discount rates for housing of 1.9%. Such a discount rate is consistent, in the absence of bubbles, with zero leasehold discounts at horizons greater than 700 years.

[13] Pástor and Veronesi (2003) pointed out that market valuations might appear bubbly, despite the absence of bubbles, if there is sufficient uncertainty about growth prospects of dividends, with the possibility of $r - g$ being close to 0 in the long run. This possible confounding effect is not a concern for our estimates, because we find valuations to be inconsistent with the presence of a classic rational bubble.

serve the same properties transacting both as leaseholds and as freeholds. Therefore, to estimate $Disc_t^{\infty}$, we need to compare prices across properties that are either freeholds or leaseholds. Since these properties could differ on important dimensions such as size and location, we need to control for these differences. To do so, we use hedonic regression techniques, which allow us to consider the variation in prices across contract types for different properties, while controlling for key characteristics of each property (see Rosen (1974)). Section 3 addresses possible concerns about confounding explanations for our results.

2.1. *U.K. Residential Housing Data*

We obtain transaction-level administrative data on all residential property sales in England and Wales between 1995 and 2013 from the U.K. Land Registry. The data include information on the price paid, property type, the full address, whether the transaction was for a freehold or a leasehold property, and information on lease characteristics such as origination date and lease length. For a large subset of properties, we also obtain proprietary information on property characteristics such as the number of bedrooms, bathrooms, and the size, age, and condition of the property. These are collected by Rightmove.co.uk from "for sale" listings and other data sources.[14] We observe a full set of hedonic characteristics for approximately 52% of the properties transacting since 1995.

We focus on houses in the U.K., because this market is dominated by freeholds and extremely-long leaseholds, whereas flats are mainly sold as shorter leaseholds. Our final sample contains about 7.6 million transactions between 1995 and 2013 for houses with a full set of hedonic characteristics. Extremely long leaseholds account for 4.7% of our transaction sample, freeholds account for 94.3%, and shorter leaseholds constitute the remaining transactions. Appendix Figure A.1 of the Supplemental Material plots a heatmap of the share of all transactions that are of extremely-long leaseholds across 3-digit postcodes.[15] A white postcode indicates an area with no extremely-long leasehold transaction; a black postcode indicates an area where at least 2% of transactions are of extremely-long leaseholds. While 1% or 2% may seem like a small percentage, given the large size of our data set it is large enough in absolute terms to provide us with good identification. While we find transactions of extremely-long leaseholds everywhere in the U.K., there is a clear concentration in the north of England (around Manchester and Liverpool), as well as the South-West. Importantly, there are also several areas in London with a sizable fraction of extremely-long leaseholds; some of our analysis will focus

[14]Rightmove.co.uk is the U.K.'s largest property portal, with more than 13 million unique monthly visitors.

[15]These postcodes, which are also called "postcode districts," constitute the level of geographic fixed effects in our analysis. On average, there are 24,700 inhabitants per postcode district.

TABLE I

SUMMARY STATISTICS[a]

	700+ Leaseholds			Freeholds						
	Mean (1)	Median (2)	Std (3)	Mean (4)	Median (5)	Std (6)	Δ (7)	SE (8)	ΔFE (9)	SE (10)
				Panel A: U.K.						
Log Price (£)	11.26	11.29	0.71	11.70	11.74	0.73	−0.44	0.03	−0.01	0.01
Bedrooms	2.74	3	0.79	3.01	3	0.87	−0.26	0.02	−0.10	0.01
Bathrooms	1.16	1	0.44	1.30	1	0.58	−0.14	0.01	−0.02	0.00
Age (years)	66.6	71	43.26	58.12	50	48.75	8.47	1.32	−2.75	0.92
Size (m^2)	101.5	93	55.52	112.8	100	58.17	−11.26	0.77	−5.04	0.51
N		353,309			7,167,253					
				Panel B: Singapore						
Log Price (SG$)	13.96	13.89	0.53	14.02	13.92	0.63	−0.06	0.04	0.02	0.06
Age (years)	4.63	0	7.49	5.20	0	8.80	−0.59	0.35	0.03	0.92
Size (m^2)	176.7	136.0	148.4	173.1	129.0	197.1	3.98	6.39	4.15	4.47
N		25,301			174,126					

[a]Table shows summary statistics for the U.K. transaction sample in Panel A, and for the Singapore transaction sample in Panel B. For each of our key hedonic characteristics, we show the mean, median, and standard deviation separately for extremely-long leaseholds and freeholds. We also show the average difference between extremely-long leaseholds and freeholds (Δ), and the average difference after controlling for the same level of fixed effects as in the hedonic pricing regression (3) (ΔFE). For the U.K., these are 3-digit postcode by transaction year by property type fixed effects; for Singapore, they are 5-digit postcode by transaction year by property type by title type fixed effects. We also report the sample size (N). All standard errors are clustered at the postcode level.

exclusively on this area, depicted in Appendix Figure A.2 of the Supplemental Material.

Since we focus on estimating the price difference between freeholds and leaseholds with 700+ years remaining maturity, we provide key summary statistics for these two contract types in the U.K. data. Panel A of Table I reports the mean, median, and standard deviation of the log price, the number of bedrooms and bathrooms, and the age and size of the property. Column 7 reports the average unconditional difference between the two contract types. Extremely-long leasehold properties are, on average, cheaper, smaller, and older than freehold properties. Column 9 shows the average difference in each characteristic between extremely-long leaseholds and freeholds, conditional on 3-digit postcode $\times$ transaction year $\times$ property type fixed effects. Once we control for spatial and temporal heterogeneity, any differences between extremely-long leaseholds and freeholds become economically trivial. Variation in the geographic distribution of leasehold and freehold properties is the key driver of the unconditional differences in property characteristics.[16]

[16]More evidence supporting this conclusion can be seen in Appendix Figure A.3 of the Supplemental Material, which plots residuals from a regression of property characteristics on 3-digit

2.2. *Singapore Residential Housing Data*

We obtain transaction-level administrative data for all private residential housing sales in Singapore from the country's Urban Redevelopment Authority. We observe approximately 379,000 arms-length transactions between 1995 and 2013. For each transaction, there is information on the sale price and date, the lease terms, and characteristics of the property. About 6.6% of our transaction sample consists of extremely-long leaseholds, 45.5% of freeholds; the remaining transactions are for shorter leaseholds. Panel B of Table I provides summary statistics on the characteristics of extremely-long leaseholds and freeholds in our Singapore transaction sample. Extremely-long leaseholds tend to be marginally cheaper than freeholds; they are also slightly larger and younger. The differences in observable characteristics and prices across freeholds and extremely-long leaseholds are economically small, whether or not we control for 5-digit postcode × transaction year × property type × title type fixed effects.[17]

2.3. *No-Bubble Condition: Aggregate Results*

The summary statistics presented in Sections 2.1 and 2.2 show that, conditional on being of the same property type, located in the same postcode, and sold at the same time, extremely-long leaseholds and freeholds transact at essentially identical prices in both the U.K. and Singapore. However, this could be either because there is no classic rational bubble, or because leasehold properties are more attractive, thus masking the presence of a bubble. To control for possible differences in observable property characteristics, we study the price difference between extremely-long leaseholds and freeholds by estimating the following hedonic regression separately for the U.K. and Singapore:

$$\log(Price_{i,p,h,t}) = \alpha + \beta ExtremelyLongLease_i + \gamma Controls_{i,t} + \phi_{p,h,t} + \varepsilon_{i,p,h,t}. \tag{3}$$

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postcode × transaction year × property type fixed effects separately for extremely-long leaseholds and freeholds. Panel A shows that, conditional on these fixed effects, leaseholds and freeholds have the same price distribution. Panels B–E show that the conditional distribution of hedonic characteristics is also very similar for extremely-long leaseholds and freeholds.

[17]Title type can be either "land" or "strata." Land title properties occupy land that is exclusive to the owner (e.g., detached houses), whereas a strata title comprises units in cluster housing (e.g., apartments). Owners of strata properties enjoy exclusive title only to the airspace of their unit. The land that the development is built on is shared by all the owners of the project. Appendix Figure A.4 of the Supplemental Material shows the distribution of residuals from a regression of the transaction price and each characteristic on 5-digit postcode × property type × title type fixed effects, separately for extremely-long leaseholds and freeholds. Conditional on these fixed effects, the distributions of the transaction price and the observable characteristics are nearly identical for freeholds and extremely-long leaseholds.

The unit of observation is a transaction of property i, of property type p, in postcode h, at time t. The variable $ExtremelyLongLease_i$ is an indicator of whether property i is sold as a leasehold with more than 700 years of remaining maturity. The excluded category in the regression is freeholds. The coefficient β captures the log price discount of extremely-long leaseholds relative to otherwise similar freeholds. We control for average prices in a property's geography by including property type by postcode by time of sale fixed effects, $\phi_{p,h,t}$.[18] For the U.K., we use 3-digit postcodes; for Singapore, we use 5-digit postcodes. These geographies correspond to areas that are both sufficiently large to have variation across contract type and sufficiently small for the housing stock to be relatively homogeneous. We also control for various characteristics of the property using standard hedonic variables.[19] Standard errors are clustered at the postcode level.

Table II shows results from regression (3) for the U.K. (columns 1–4) and Singapore (columns 5–8). Our preferred specifications, reported in columns 1 and 5, use the transaction year to account for time fixed effects. There is no significant difference between the prices of extremely-long leaseholds and freeholds in either country. While estimates are less precise for Singapore, where we observe fewer transactions, for the U.K. the price difference is a precisely estimated zero. This shows that, on average, there was no classic rational bubble in house prices in the U.K. and Singapore during our sample period.[20]

To understand the quantitative implications of our results, consider that our (statistically insignificant) point estimates imply that the bubble, if present,

[18]That is, if we observed transactions for two property types, in 10 regions, and spread over 10 time periods, we would include $2 \times 10 \times 10 = 200$ separate fixed effects. For U.K. houses, the property types are "detached," "semi-detached," and "terraced." For Singapore, the property types are "apartment," "condominium," "detached house," "executive condominium," "semi-detached house," and "terraced house." In Singapore, we also interact the fixed effects with whether the property is a "land" or a "strata" title.

[19]For both the U.K. and Singapore, we control for the size and age of the property in a flexible way. For property size in the U.K., we split the property size distribution into 50 equally sized groups, and include the corresponding dummy variables in the regression; for Singapore, we allow for 40 equally sized groups. Age is controlled for by adding a dummy variable for each possible integer value. In addition, for the U.K., we include as controls the number of bathrooms, the number of bedrooms, the property condition, whether there is parking, whether the property is new, and the type of heating. In Singapore, we include the number of units in the development. We also include transactions of shorter leaseholds (with less than 300 years remaining) in our sample, controlling for the lease length remaining at the time of transaction by including dummies for buckets of maturities as in Giglio, Maggiori, and Stroebel (2015). We include these shorter leaseholds in our sample—even though they are not the focus of this paper—to maximize power, since they help identify the hedonic coefficients and the fixed effects. All results are robust to excluding them.

[20]To confirm the quality of our data, Panels A to D of Appendix Figure A.5 of the Supplemental Material show the estimated coefficients on the key hedonic characteristics from the U.K. regression in column 1 of Table II. Panels A and B of Appendix Figure A.6 of the Supplemental Material show the same from the Singapore regression in column 5 of Table II. Larger properties, younger properties, and properties with more bedrooms and bathrooms all trade at higher prices.

TABLE II

EFFECT OF LEASE TYPE ON PRICES: AGGREGATE RESULTS[a]

	England & Wales				Singapore			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
700+ Year Leasehold	0.001	0.001	0.001	0.000	−0.013	−0.009	−0.007	−0.004
	(0.005)	(0.005)	(0.005)	(0.004)	(0.036)	(0.035)	(0.040)	(0.036)
Fixed Effects	PC × Y × Prop	PC × Q × Prop	PC × M × Prop	PC × Y × Prop	PC × Y × Prop × Title	PC × Q × Prop × Title	PC × M × Prop × Title	PC × Y × Prop × Title
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Restriction				Winsorized				Winsorized
R-squared	0.883	0.886	0.887	0.911	0.969	0.978	0.980	0.961
N	7,602,276				378,767			

[a]Table shows results from regression (3). The dependent variable is the log price paid for houses sold in England and Wales between 1995 and 2013 (columns 1–4), and for properties sold in Singapore between 1995 and 2013 (columns 5–8). In columns 4 and 8, the dependent variable is winsorized at the 5% level. For the U.K. sample, we include fixed effects at the 3-digit postcode by transaction date by property type (detached, semi-detached, terraced) level. In columns 1 and 4, the transaction date is the transaction year; in columns 2 and 3, it is the transaction quarter and month, respectively. We control for property size, the number of bedrooms and bathrooms, property age, property condition, whether there is parking, and the type of heating. Standard errors are clustered at the 3-digit postcode level. For the Singapore sample, we include fixed effects at the 5-digit postcode by transaction date by property type (apartment, condominium, detached house, executive condominium, semi-detached house, and terrace house) by title type (strata or land) level. In columns 5 and 8, the transaction date is the transaction year; in columns 6 and 7, it is the transaction quarter and month, respectively. We control for property age, property size, and the total number of units in the property. Standard errors are clustered at the 5-digit postcode level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

would have a magnitude of about 1.3% of the value of freeholds in Singapore. In the U.K., the price difference is even smaller, at 0.1% of the value of the freehold, and we can rule out a classic rational bubble that contributes more than 1.1% to the price of a freehold with 95% confidence. The level of such a bubble would be economically small, and it cannot explain the observed house price fluctuations. As discussed in Section 1.1, between 2000 and 2007, U.K. house prices increased by 86%. For a bubble worth 0.1% to explain this price increase, it would have had to grow by 86,000% during those seven years.[21]

To test the robustness of our results, we also use the transaction quarter and month to account for time fixed effects; the results, which are shown in columns 2 and 3 of Table II for the U.K. sample, and columns 6 and 7 for the Singapore sample, are robust to these variations. In addition, in columns 4 and 8, we winsorize the dependent variables at the 5% level, to ensure that our results are not obscured by outlier observations. As before, we detect no differences between the prices paid for extremely-long leaseholds and freeholds.

We labeled our tests as model-free in the sense that they do not require imposing a structural model to compute the fundamental value of the asset. Regression (3) does, however, impose an empirical model in computing how the hedonic characteristics are related to prices. Table II shows that our regression specification is robust to variations in the fixed effects. In addition, by including separate dummy variables for the various quantiles of the distribution of each hedonic variable, we allow for a flexible functional form of the dependence of prices on the hedonic variables. To confirm that our results are robust to different empirical specifications, we next report results of a propensity score matching estimation (Rosenbaum and Rubin (1983)). We first compute, for each observation, a propensity score that indicates the likelihood that the property is treated (i.e., is an extremely-long leasehold as opposed to a freehold), as a function of the characteristics of the property. This propensity score is obtained from a logit regression of the extremely-long leasehold indicator on the property's hedonic characteristics. We then match each leasehold transaction to the freehold transaction with the closest propensity score that occurred in the same month and postcode as the leasehold transaction. This is the freehold which, given its hedonic characteristics, had the closest ex ante probability of being an extremely-long leasehold. We then compare the average prices across pairs of matched freehold-leasehold transactions. The results

[21]We thank an anonymous referee for suggesting this calibration. Call $P_t = (P_t^f + B_t)$, where P_t is the observed price, P_t^f is the fundamental value, and B_t is the bubble term. Suppose that all observed price movements are attributed to the bubble term (i.e., the fundamental value does not change). The relative price growth between times 0 and t is then $(P_t - P_0)/P_0 = (B_t - B_0)/P_0 = \frac{B_t - B_0}{B_0}\frac{B_0}{P_0}$. Since $\frac{B_0}{P_0} = 0.001$, a growth rate of prices of 86% in seven years requires a growth rate of the bubble of 86,000% over the same period.

of our matching estimator are consistent with those of the hedonic regression analysis: the average log price difference between extremely-long leaseholds and freeholds in the U.K. is −0.001 (standard error 0.003) and 0.001 in Singapore (standard error 0.043). In both cases, the difference is economically and statistically indistinguishable from zero. We conclude that there is no evidence in our data supporting the presence of a classic rational bubble.

2.4. *No-Bubble Condition: Time-Series and Cross-Section*

The tests reported in the previous section show that the prices of extremely-long leaseholds and freeholds are very similar *on average*, thus rejecting the presence of a classic rational bubble *on average*. It is, however, possible that such a bubble was large only in some parts of our sample, and then collapsed. In this section, we therefore consider subsamples of transactions that maximize the possibility of detecting a classic rational bubble.

We first focus on transactions in years where the test of Phillips, Shi, and Yu (2014) suggests the presence of a classic rational bubble (see Section 1.1). Columns 1 and 2 of Table III, Panel A, show results from the U.K. (2002–2004) and Singapore (1995–1996, 2007), respectively. In those samples, extremely-long leaseholds are, if anything, priced slightly higher than freeholds, though the economic magnitude of those differences is small, and they are not generally statistically significant. This shows that there was no classic rational bubble in housing markets, even during years when existing time-series tests suggest that one was present. Relatedly, Panels A and B of Figure 2 report the price difference between extremely-long leaseholds and freeholds for the U.K. and Singapore, estimated separately for each year. While our statistical power decreases and standard errors increase, there is no evidence of a classic rational bubble in any year between 1995 and 2013. For the U.K., the point estimate of a bubble never accounts for more than 1.2% of house prices, and it is never statistically significant at the 5% level. These results show the absence of a classic rational bubble even during periods when the housing market looked "bubbly" on many of the indicators discussed in Section 1.1.

We next investigate the possibility that bubbles could have emerged only in some geographic areas.[22] Columns 3 and 4 of Table III, Panel A, focus on transactions in London as well as in Prime Central London (PCL), defined as the neighborhoods of Mayfair, Knightsbridge, Belgravia, Chelsea, and Kensington. Panel E of Figure 1 shows that these areas had above-average house price growth during our sample. The area of PCL is of particular interest for our analysis, because it experienced some of the biggest house price increases, features high price-rent ratios, and has attracted significant inflows of foreign

[22]Note that, in general, rational bubbles in real estate cannot be too localized, because the increases in prices in one area would eventually trigger a substitution with the housing stock in neighboring areas, thus leading to price increases in progressively more extended areas.

TABLE III

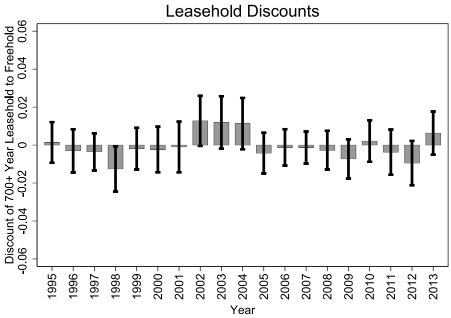
EFFECT OF LEASE TYPE ON PRICES: CROSS-SECTIONAL RESULTS[a]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Panel A				
700+ Year Leasehold	0.012*	0.011	−0.017	0.009	−0.009	0.002	0.015
	(0.006)	(0.053)	(0.026)	(0.062)	(0.056)	(0.031)	(0.055)
FE & Controls	✓	✓	✓	✓	✓	✓	✓
Sample	U.K. 2002-4	Singapore 1995, 1996, 2007	London	PCL	Singapore Central Region	London Bubble Quarters	PCL Bubble Quarters
R-squared	0.828	0.977	0.892	0.871	0.981	0.870	0.844
N	1,106,069	68,415	285,281	52,336	179,852	56,194	7,244
			Panel B				
700+ Year Leasehold	0.004	0.009	−0.001	0.006	−0.005	−0.005	0.004
	(0.006)	(0.006)	(0.004)	(0.005)	(0.012)	(0.008)	(0.006)
FE & Controls	✓	✓	✓	✓	✓	✓	✓
Sample	U.K. PC-Year Bubble	U.K. PC-Year Bubble 10%+ Long LH	U.K. PC-Year No Bubble	U.K. PC-Year No Bubble 10%+ Long LH	Top 20% Price/Inc U.K. 2004+	Top 20% Δ Price/Inc U.K. 2004-7	Bottom 20% TOM U.K. 2001+
R-squared	0.856	0.795	0.900	0.856	0.833	0.884	0.875
N	2,969,013	408,437	4,633,263	577,293	557,368	383,115	990,513

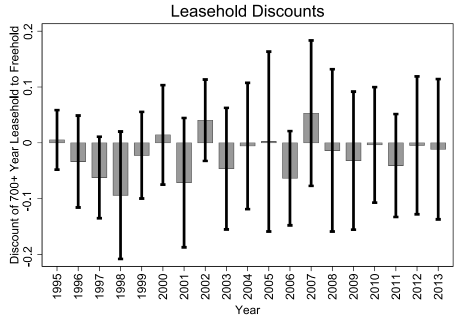
[a]Table shows results from regression (3). The dependent variable is the log price paid in arms-length housing transactions. In each column, we consider a particular sample where we might have expected the presence of a classic rational bubble. In Panel A, column 1, we focus on transactions in the U.K. in 2002, 2003, and 2004, and in column 2 on transactions in Singapore in 1995, 1996, and 2007. In column 3, we focus on transactions in London, and in column 4 on transactions in Prime Central London (PCL). In column 5, we focus on Singapore's Central region. In column 6, we focus on transactions in London for the periods 1999Q2–2000Q3, 2002Q3–2002Q4, and 2007Q1–2007Q3. In column 7, we focus on transactions in PCL for the periods 1999Q3–2000Q3 and 2007Q1–2007Q3. In Panel B, column 1, we focus on those U.K. postcode area-years where the Phillips, Shi, and Yu (2014) test finds rational bubbles; in column 2, we focus on the subset of those postcode area-years in postcode areas with at least 10% of all transactions for extremely-long leaseholds. In columns 3 and 4, we focus on the complement set of all remaining U.K. postcode area-years not tested in columns 1 and 2. In column 5, we focus on transactions in the U.K. that occurred in areas in the top 20% of the price-income ratio distribution. The price-income ratio is measured as of 2004, and the regression sample includes all years starting from that date. We measure the price-income ratio at the Middle Layer Super Output Area (MSOA) level, the most precise level at which average incomes are reported. In column 6, we consider transactions between 2004 and 2007, focusing on the MSOAs with the 20% largest price-income ratio growth over that period. In column 7, we focus on those MSOA-years in the bottom 20% of the time-on-market distribution. The time-on-market information is available since September 2001, and we use transactions since that year in our regression. For all U.K. samples, we include fixed effects at the 3-digit postcode by transaction year by property type level. We also control for property size, the number of bedrooms and bathrooms, property age, property condition, whether there is parking, and the type of heating. Standard errors are clustered at the 3-digit postcode level. For the Singapore sample, we include fixed effect at the 5-digit postcode by transaction month by property type by title type (strata or land) level. We control for property age, property size, and the total number of units in the property. Standard errors are clustered at the 5-digit postcode level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

money, suggesting that its housing stock is treated as a store of value in a manner consistent with rational bubble theory (see Farhi and Tirole (2012)).[23] Our

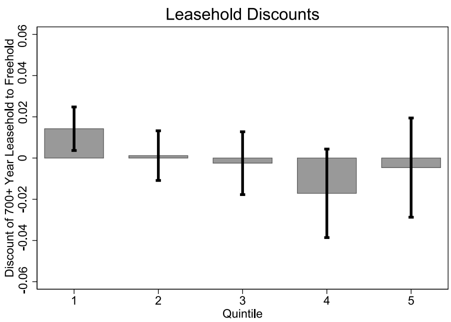
[23]In its Q4 2010 *Buy-To-Let Review*, the Association of Residential Letting Agents reports gross annual rental yields for PCL of 4.79%, compared with a national yield of 5.04%. By Q4 2012, the rental yield in PCL had fallen to 4.46%, while the national rental yield rose to 5.23%. Lower



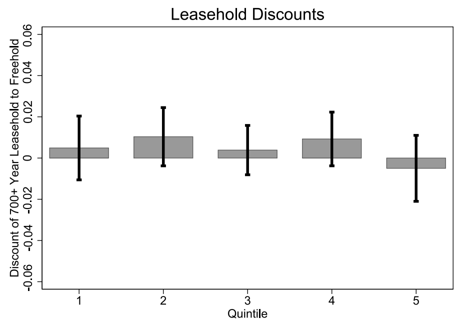
(A) U.K.: By year



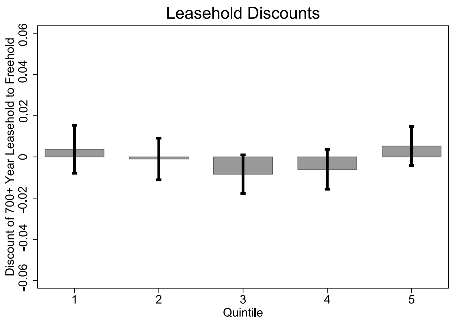
(B) Singapore: By year



(C) U.K.: By price-income ratio (2004)



(D) U.K.: By price-income ratio growth (2004-7)



(E) U.K.: By time-on-market

FIGURE 2.—Time-series and cross-section of bubble claim. *Note*: Figure reports estimates of the discount between 700+ year leaseholds and freeholds from regression (3), dividing the sample along time-series and cross-sectional dimensions. Panels A and B show the coefficients of the 700+ leasehold discount year-by-year for the U.K. and Singapore, respectively. Panels C through E report the coefficients of the 700+ leasehold discount, splitting Middle Layer Super Output Areas by quintiles of measures of the potential for a bubble: the price-income ratio in 2004 (Panel C), the growth of the price-income ratio between 2004 and 2007 (Panel D), and the time-on-market (Panel E). These measures of bubble potential are constructed as in columns 4–6 of Table III, Panel B. The bars indicate the 95% confidence interval of the estimate using standard errors clustered at the 3-digit postcode level in the U.K., and at the 5-digit postcode level in Singapore.

test finds no evidence of a classic rational bubble in either of these areas, because the price difference between extremely-long leaseholds and freeholds is small and indistinguishable from zero. In column 5, we focus on the "Central Region" in Singapore, which includes the Central Business District and many of the city-state's luxury units. In this subsample, extremely-long leaseholds are priced at a statistically insignificant 0.9% discount to freeholds.

We also analyze the evidence for the presence of classic rational bubbles in particular years in particular regions. Appendix Figure A.8 of the Supplemental Material plots time-series of house prices in London and PCL, shading periods where the Phillips, Shi, and Yu (2014) test suggests the presence of a classic rational bubble. In columns 6 and 7 of Table III, Panel A, we focus our test on transactions in London and in PCL during those years, and, again, find no evidence of a classic rational bubble. More generally, we run the Phillips, Shi, and Yu (2014) test on house price series from all 100 U.K. postcode areas with more than 20,000 transactions in our data. Appendix Figures A.9 to A.17 of the Supplemental Material show these house price series and shade the years in which a bubble is detected by the existing tests. Correspondingly, we report in column 1 of Table III, Panel B, our test results when the test is run jointly on all these shaded postcode area-years; in column 2, we further restrict the sample to the subset of these postcode area-years in which at least 10% of transactions were for extremely-long leaseholds. In both samples, our tests find that, if anything, extremely-long leaseholds trade at a premium to freeholds. In columns 3 and 4, we focus on the complement set of postcode area-years, the unshaded areas in Appendix Figures A.9 to A.17, and find that extremely-long leaseholds trade at a small and statistically insignificant price discount to freeholds, thus continuing to provide no evidence of the presence of a classic rational bubble.[24]

In columns 5 to 7 of Table III, Panel B, we split the U.K. along other dimensions that might suggest the presence of a bubble. We obtain data on price-income ratios for each Middle Layer Super Output Area (MSOA) in the U.K.

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rental yields, which correspond to higher price-rent ratios, suggest higher prices relative to fundamentals. Regarding the importance of foreign capital, Knight Frank (2013) reported that 49% of buyers in PCL between 2011 and 2013 were foreign residents; for the whole of London, this number was lower, but still significant at 20% of all buyers. See Appendix A.1.4 of the Supplemental Material for more documentation of foreign capital flows into the U.K. property market. For an analysis of foreign capital flows to London, see also Cheshire (2014), Badarinza and Ramadorai (2014), and Sá (2015).

[24]We also ran our test separately in each of the 217 postcode area-years in which we observe at least 1,000 transactions, and in which at least 10% of the transactions were for extremely-long leaseholds and 10% of transactions were for freeholds. Appendix Figure A.7 of the Supplemental Material shows histograms of the price discount or premium of extremely-long leaseholds relative to freeholds, and of the associated t-statistics. We find evidence of a price discount of extremely-long leaseholds with a t-statistic smaller than -1.96 in seven out of the 217 postcode-area years under study.

for the years 2004 and 2007 (see Appendix A.1.2 of the Supplemental Material for details). There are 7,201 unique MSOAs, each with between 2,000 and 6,000 households, and an average population size of 7,500. In areas with high price-income ratios, property prices are high relative to observed fundamentals, and are thus more likely to contain a bubble. Yet, column 5 shows that there was no classic rational bubble even in areas with the highest 20% price-income ratio in 2004. Column 6 similarly shows that there was no classic rational bubble in those MSOAs that experienced the largest increases in price-income ratios between 2004 and 2007.

Finally, we consider the time it takes to sell a house as a proxy for how “hot” a particular housing market is; this measure is interesting, since markets where houses sell very quickly are more conducive for prices to deviate from fundamentals (Novy-Marx (2009)). We obtain “for sale” listing information for about 1.8 million transactions of houses in the U.K. since September 2001 from Rightmove.co.uk. For these listings, we measure the time between the first listing and the eventual sale, that is, the “time-on-market,” and then calculate the average time-on-market for each MSOA-year. In column 7 of Table III, Panel B, we focus on transactions in MSOA-years in the lowest 20% of the time-on-market distribution. Even in these subsamples, we find no evidence for a classic rational bubble.

The cross-sectional analysis in Table III shows that there was no classic rational bubble in areas with very high price-income ratios, high growth rates of the price-income ratio, and low time-on-market, all of which represent measures of the likelihood of finding a bubble. In panels C, D, and E of Figure 2, we report the discounts of extremely-long leaseholds relative to freeholds across all quintiles of the distribution of these variables. The coefficients reported in columns 5 through 7 of Table III, Panel B, correspond to the top quintile (for measures of price-income ratio level and growth) or bottom quintile (for time-on-market) of each panel. We find no evidence of a classic rational bubble in any of the cross-sections we study; there is also no pattern in the relative pricing of extremely-long leaseholds and freeholds across our measures of the ex ante likelihood of finding a bubble.

3. FRICTIONS AND THE PRICING OF EXTREMELY-LONG LEASEHOLDS

We next address concerns that institutional features of housing markets in the U.K. and Singapore might influence the relative pricing of extremely-long leaseholds and freeholds, and would thus interfere with our test's ability to detect classic rational bubbles.

We first focus on the role of potentially unobserved differences in property characteristics and potentially different liquidity across contract types. We then consider whether any of the institutional features discussed in Section 1.2 significantly affect the relative pricing of extremely-long leaseholds and freeholds. We show that neither the assignment of redevelopment rights and maintenance costs nor concerns about the stability of property rights, the finite maturity of leasehold contracts, or the market-timing by freeholders affect our test for classic rational bubbles. In Appendix A.3 of the Supplemental Material, we show that the potential presence of ground rents and the tax framework do not significantly bias our tests.

3.1. *Unobserved Property Characteristics*

Despite our ability to control for many observable property characteristics in regression (3), one might worry that extremely-long leaseholds and freeholds could differ on unobservable property characteristics. If extremely-long leasehold contracts did indeed correspond to better properties, this could mask the presence of a bubble. We consider this to be unlikely, as we show in Section 2.1 that leasehold houses are marginally *worse* on observable characteristics; it is then unlikely that they are sufficiently *better* on unobservable characteristics to mask a possible bubble. However, to formally rule out this concern, we argue that if leasehold properties were truly better on unobservable characteristics, this should affect their rental value. In fact, a property's rental value should capture all property characteristics that affect its price, including those unobservable to the econometrician but observed by the renters, such as the degree of maintenance.

To test whether extremely-long leaseholds and freeholds differ on unobservable property characteristics, we obtained more than 100,000 rental listings for Singapore from iProperty, the country's leading real estate online portal. These listings cover the period 2010–2013. About 55,000 listings are for freehold properties, and 6,000 are for properties with extremely-long leaseholds. We repeat regression (3), but substitute the log of the annual rent as the dependent variable. As shown in columns 1 and 2 of Table IV, there is no difference in the annual rents between the two types of contracts.[25]

For the U.K., we scanned the leading online property portals, rightmove.co.uk and zoopla.co.uk, to obtain all 65,000 rental listings for houses that were live on March 30, 2015. As shown in column 3 of Table IV, after we control for observable property characteristics, we find no significant difference in annual rents for extremely-long leasehold and freehold houses.[26] These results

[25]To provide confidence in the quality of these rental price data, panels C and D of Appendix Figure A.6 show the coefficients on the key hedonic characteristics in the regression; as with transaction prices, larger and younger properties rent for higher annual amounts.

[26]Since Rightmove and Zoopla did not provide the exact address for rental listings (they chose not to do so, we believe, to prevent potential renters from contacting listing agents directly rather than through the site), the scraping of these data involved obtaining the address of each property by reverse-geocoding the location where a property marker was placed on Google Maps. See Appendix A.1.3 of the Supplemental Material for details. Since fewer property characteristics are available for rental listings, we can only control for the number of bedrooms and bathrooms, as well as a "furnished" indicator. As for the benchmark analysis, the coefficients on the control

TABLE IV

EFFECT OF LEASE TYPE ON RENTS AND TIME-ON-MARKET[a]

	log(Rent)			log(Time-On-Market)	
	(1)	(2)	(3)	(4)	(5)
700+ Year Leasehold	−0.006 (0.037)	−0.000 (0.044)	0.002 (0.032)	0.016*** (0.003)	0.017*** (0.004)
Fixed Effects	PC × Y × Prop × Title	PC × M × Prop × Title	PC × Prop	PC × Y × Prop	PC × M × Prop
Controls	✓	✓	✓	✓	✓
Sample	Singapore 2010–2013	Singapore 2010–2013	U.K. March 30, 2015	U.K. 2001–2013	U.K. 2001–2013
R-squared	0.884	0.902	0.875	0.078	0.371
N	101,932	101,932	64,797	1,812,830	1,812,830

[a]Table shows results from regression (3), with different dependent variables. The dependent variable in columns 1 and 2 is the log rental listing price for all properties listed “for rent” on iProperty.com.sg between 2010 and 2013. We include fixed effects at the 5-digit postcode by transaction date by property type (apartment, condominium, detached house, executive condominium, semi-detached house, and terrace house) by title type (strata or land) level. In columns 1 and 2, the transaction date is the transaction year and month, respectively. We also control for property age and property size. Standard errors are clustered at the 5-digit postcode level. The dependent variable in column 3 is the log rental listing price for all houses listed as “for rent” on Rightmove.co.uk and Zoopla.co.uk on March 30, 2015. We include fixed effects at the 3-digit postcode by property type (detached, semi-detached, terraced) level. We also control for the number of bedrooms and bathrooms, and whether the house is furnished. Standard errors are clustered at the 3-digit postcode level. In columns 4 and 5, the dependent variable is the log of “time-on-market” between first listing and sale, measured in days, for those houses listed on Rightmove.co.uk between September 2001 and 2013. We include fixed effects at the 3-digit postcode by transaction date by property type (detached, semi-detached, terraced) level. In columns 4 and 5, the transaction date is the transaction year and month, respectively. We also control for property size, the number of bedrooms and bathrooms, property age, property condition, whether there is parking, and the type of heating. Standard errors are clustered at the 3-digit postcode level. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

confirm that unobservable property characteristics cannot explain our result of zero price difference between extremely-long leaseholds and freeholds.

3.2. *Market Liquidity*

A second possible force that could bias our test against finding a bubble is if freehold properties were less liquid than extremely-long leaseholds in the resale market, and if they would therefore trade at a discount relative to these leaseholds in the absence of a bubble. To test whether this hypothesis confounds our test, we use the time-on-market measure constructed using the Rightmove “for sale” listings data described in Section 2.4 as the dependent variable in regression (3). As discussed, time-on-market provides a

variables, shown in panels E and F of Appendix Figure A.5 of the Supplemental Material, confirm the quality of the rental data.

useful proxy for the liquidity of the asset (see Genesove and Han (2012), Piazzesi, Schneider, and Stroebel (2013)). Columns 4 and 5 of Table IV show that extremely-long leaseholds sit on the market for an average of 2 days longer than freeholds. This difference is small relative to an overall average time-on-market of about 140 days. Differences in liquidity between the two contract types are therefore unlikely to confound our results. Consistent with this, Section 2.4 showed that the price differences between extremely-long leaseholds and freeholds is consistently small across areas with very different average liquidity.

3.3. *Institutional Differences Across Contract Types*

We next discuss institutional features of leasehold contracts that might affect the relative valuation of extremely-long leaseholds and freeholds. While most of these features would, if anything, reduce the value of a leasehold, and would therefore bias us in favor of finding a classic rational bubble, we actually show that they have no significant effect on the value of extremely-long leases. We focus on the institutional setting in the U.K., since the size of the country allows for cross-sectional tests to directly quantify the potential impact of particular institutional features. Since Singapore's legal system is based on English common law, many of the principles are the same across the two countries.

3.3.1. *Maintenance and Service Charges*

We first consider how the responsibility for conducting and paying for property maintenance affects the valuation of extremely-long leaseholds. Under English common law, the basic principle regarding the responsibility for property maintenance is that of *caveat lessee*, or "let the lessee beware," which is generally interpreted as excluding responsibility by the freeholder regarding the state of the property (Garner and Frith (2013)). The parties involved determine bilaterally whether the leaseholder or the residual freeholder is responsible for carrying out maintenance and repairs. However, independently of who is responsible for *conducting* the maintenance, for long leases the *cost* of the maintenance is always borne by the leaseholder (see Appendix A.3 of the Supplemental Material). In particular, when the contract places the responsibility for maintenance on the freeholder, the cost of "these tasks, however, must be paid for, and that burden will be placed on the leaseholder" (Garner and Frith (2013)). This is usually done through a "service charge," which is a payment by the leaseholder for services provided by the freeholder. These services include not only maintenance and repairs, but can also cover insurance of the building, provision of central heating, lifts, porterage, estate staff, lighting, and the cleaning of common areas. Generally speaking, leasehold contracts specify that leaseholders living in houses are responsible for their own maintenance, while leaseholders living in blocks of flats with many common areas pay a service charge in return for maintenance and repairs by the freeholder.[27]

In the presence of a service charge, one possible concern is whether that service charge corresponds to the private market cost of the services provided. In particular, one might worry that a freeholder could use the service charge to extract economic rents from "captive" leaseholders. This concern would bias our test toward (mistakenly) finding a bubble, because it lowers the value of a leasehold compared to a freehold. However, in practice, a freeholder's ability to extract rents is severely limited. First, the Commonhold and Leasehold Reform Act 2002 grants leaseholders the right to challenge the reasonableness of service charges with a government-run tribunal. Second, the Act provides a right for leaseholders to force the transfer of the landlord's management functions to a special "right to manage" company. This does not require the landlord's consent, and significantly limits her ability to extract unreasonable service charges (see also Appendix A.3.2 of the Supplemental Material).

Most professional freeholders appoint a management company to manage the properties and carry out repairs. A second concern, therefore, is that economies of scale make freeholders more efficient at conducting maintenance than individual leaseholders. These possible efficiency gains would bias our tests toward finding no bubble, because they would reduce the total cost of maintaining a leasehold, increasing its value relative to a freehold. However, if these efficiency gains were large, even individual freeholders could contract out maintenance work to large companies, thus sharing in the efficiency gains.

To test whether these possible biases affect the relative pricing of extremely-long leaseholds and freeholds, we next conduct several cross-sectional tests that show that our results are robust to considering only market segments with large or small service charges. A first set of regressions compares the relative pricing of extremely-long leaseholds across houses and flats.[28] As discussed above, since apartment buildings have more common areas than houses, these properties are more likely to have repair work carried out by the freeholder, in return for the leaseholders paying a service charge. If the presence of these service charges had important pricing implications for extremely-long leaseholds, then we would expect our results to be sensitive to the split between houses and flats. Columns 1 to 3 of Table V show results from regression (3) for flats, with variations in the choice of time fixed effects. As we observed for houses, extremely-long leaseholds on flats trade at the same price as otherwise similar

[27]Data from the 2011–2012 English Housing Survey (EHS) show that, on average, only 7.4% of leaseholders living in houses pay a service charge, with the others conducting maintenance and repairs themselves. For leaseholders living in flats, 68% pay a service charge.

[28]Flats are not the main focus of our analysis, since most of them trade on leaseholds between 99 and 250 years of initial maturity; only 3% of flats trade as freeholds. In addition, as discussed in Giglio, Maggiori, and Stroebel (2015), since lease registration was more spotty prior to the Land Registration Act 2002, for flats we need to focus on transactions since 2003.

TABLE V

CROSS-SECTIONAL ANALYSIS BY IMPORTANCE OF MAINTENANCE FEES[a]

	(1)	(2)	(3)	(4)	(5)	(6)
700+ Year Leasehold	−0.005	−0.005	−0.005	0.001	0.001	−0.007
	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.016)
Fixed Effects	PC × Y	PC × Q	PC × M	PC × Y × Prop	PC × Y × Prop	PC × Y × Prop
Controls	✓	✓	✓	✓	✓	✓
Sample	U.K. Flats 2004–2013	U.K. Flats 2004–2013	U.K. Flats 2004–2013	U.K. Houses 2003–2013	U.K. Houses Northern Region 2011, 2012	U.K. Houses "Rest of England" 2011, 2012
R-squared	0.712	0.712	0.712	0.841	0.774	0.817
N	1,344,558	1,344,558	1,344,558	4,367,179	140,102	235,049

[a]Table shows results from regression (3). The dependent variable in columns 1 to 3 is the log price paid for flats in England and Wales between 2004 and 2013. In columns 4 to 6, it is the log price paid for houses. In column 4, we focus on all transactions since the Commonhold and Leasehold Reform Act 2002. In column 5, we focus on transactions in 2011 and 2012 from the "Northern Region" identified by the English Housing Survey 2011–2012, which includes the NUTS1 regions North West, North East, and Yorkshire and the Humber. In this region, only 2.5% of owners of leasehold houses reported paying a service charge. In column 6, we focus on transactions in 2011 and 2012 from the "Rest of England" region identified by the English Housing Survey 2011–2012, which includes the NUTS1 regions Eastern, East Midlands, West Midlands, and South West. In this region, 30.9% of owners of leasehold houses reported paying a service charge. In columns 1 to 3, we include fixed effects at the 3-digit postcode by transaction date level (year, quarter, and month, respectively). In columns 4 to 6, we include fixed effects at the 3-digit postcode by transaction year by property type (detached, semi-detached, terraced) level. We control for property size, the number of bedrooms and bathrooms, property age, property condition, whether there is parking, and the type of heating. Standard errors are clustered at the 3-digit postcode level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

freeholds. As a second test, column 4 focuses on transactions of houses following the Commonhold and Leasehold Reform Act 2002, which, as described above, further strengthened the legal protection of leaseholders against the imposition of unreasonable service charges by freeholders. The price difference between extremely-long leaseholds and freeholds continues to be zero in this sample.

While the English Housing Survey shows that, on average, only 7.4% of leaseholders in houses pay a service charge, there is significant regional heterogeneity within this subgroup. Among the three regions identified by the EHS, the share of leasehold houses with service charges varies between 2.5% for the Northern Region, in which most extremely-long leaseholds are located, and 30.9% for the "Rest of England" region. Columns 5 and 6 of Table V show results from regression (3) separately for these two regions. Again, there is no price difference between extremely-long leaseholds and freeholds, independently of how common service charges are.

3.3.2. *Assignment of Redevelopment Rights*

Since the extremely-long leasehold contracts considered in our tests span several hundred years, it is important to understand the provisions that regulate the ability of leaseholders to make adjustments and improvements to the property, or to redevelop it.

The treatment of redevelopment rights could affect leasehold values through two channels. First, if one were to lose the value of improvements after the expiry of the lease, this might reduce the leaseholders' incentives to engage in profitable redevelopment, and thus reduce the value of the leasehold (Capozza and Sick (1991)). However, the quantitative importance of having to forfeit improvements to the property at lease expiry depends on the remaining lease length, because the effect on the leasehold price today is the *present value* of giving up these improvements. Since this present value is essentially zero for extremely-long (700+ year) leaseholds, even when evaluated at low discount rates, redevelopment rights do not affect the value of extremely-long leaseholds through this channel.

A second concern, and one which is potentially more important in our setting, is that leaseholders might be prevented from making adjustments to the property during the lifetime of the lease, which could immediately reduce the value of extremely-long leaseholds. While this would bias our tests in favor of finding a classic rational bubble, we show below that such restrictions are rarely binding for extremely-long leases, and therefore have no quantitatively important effect on our results.

First, as we discuss in Appendix A.3 of the Supplemental Material, formal restrictions on leaseholders' ability to make changes to the property are limited. Leaseholders are generally allowed to conduct renovations and non-structural improvements without the consent of the freeholder. For structural changes, the leaseholder usually has to obtain the consent of the freeholder. However, a covenant against making improvements without consent is subject to the provision that consent shall not be unreasonably withheld (Section 19(2) of the Landlord and Tenant Act 1927). A freeholder would have to show that the redevelopment significantly and adversely affects her rights to ownership. In the case of extremely-long leases, most redevelopments would not infringe on the rights of the residual freeholder. This restriction is therefore unlikely to significantly affect the value of those leaseholds. Consistent with this, as shown in Section 3.1, rents for leasehold and freehold properties are very similar, demonstrating that any restrictions on a leaseholder's ability to perform improvements are limited.

To provide further evidence that redevelopment options do not significantly affect the prices of extremely-long leaseholds, we next test for cross-sectional variation in the price difference between such leaseholds and freeholds across areas with differential redevelopment potential. If there were meaningful restrictions on a leaseholder's ability to redevelop a property, then this would depress the relative price of leaseholds particularly in areas with high redevelopment potential. We construct different measures of the redevelopment potential of regions in the U.K.; we then classify areas into quintiles according to the distribution of each measure, and compute the 700+ year leasehold discount across quintiles.

Columns 1 and 2 of Table VI show the results of regression (3), when we measure an area's redevelopment potential by the change in the housing stock between the 2001 and 2011 censuses. Areas with significant growth in the housing stock are areas that attract major residential real estate investment, and houses in those areas are likely to have larger redevelopment potential. The most disaggregated U.K. geography at which the housing stock is reported is the Lower Layer Super Output Area (LSOA); there are 34,378 unique LSOAs, populated by between 400 and 1,200 households. Column 1 uses the full sample, while column 2 focuses on transactions between our two census observations, 2001 and 2011. We find no statistically significant price difference between extremely-long leaseholds and freeholds either in high-redevelopment-potential areas (top quintile) or in low-redevelopment-potential areas (bottom quintile). Figure 3 graphs these leasehold discounts for each quintile, with each panel corresponding to one column in Table VI.

In columns 3 and 4 of Table VI, we exploit a different measure of redevelopment potential: the transaction share of new properties in each LSOA (column 3), or in each LSOA-year (column 4). Again, regions with many sales of newly built properties promise more redevelopment potential for existing units. While a few of the estimates—in some of the quintiles—are statistically significant, they are all economically small, and only one has a sign consistent with the presence of a bubble (with a magnitude of 1.2%). There are no patterns for the price differences between extremely-long leaseholds and freeholds across the various quintiles of redevelopment potential.

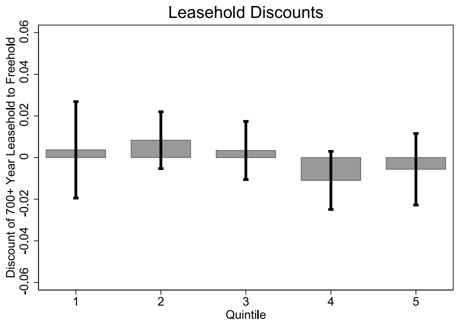
In columns 5 and 6 of Table VI, we present a third measure of redevelopment potential: the transaction share of flats in each LSOA (column 5), or

TABLE VI

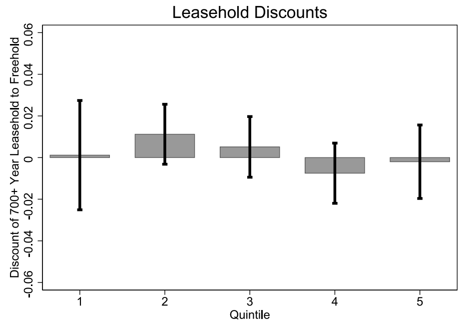
CROSS-SECTIONAL ANALYSIS BY VALUE OF REDEVELOPMENT RIGHTS[a]

	(1)	(2)	(3)	(4)	(5)	(6)
700+ Year Leasehold						
Top quintile	−0.006	−0.002	0.014*	0.024***	0.001	−0.006
	(0.009)	(0.009)	(0.007)	(0.007)	(0.008)	(0.007)
4th quintile	−0.011	−0.007	−0.004	0.013**	−0.001	0.008
	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)
3rd quintile	0.003	0.005	−0.001	0.008	0.009	0.015**
	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)
2nd quintile	0.008	0.011	−0.001	−0.012**	0.006	−0.002
	(0.007)	(0.007)	(0.006)	(0.006)	(0.008)	(0.006)
Bottom quintile	0.004	0.001	−0.003	−0.004	−0.011	−0.003
	(0.011)	(0.013)	(0.011)	(0.006)	(0.011)	(0.006)
Fixed Effects & Controls	✓	✓	✓	✓	✓	✓
Dev. Potential Definition	Δ Housing Stock in LSOA	Δ Housing Stock in LSOA	Share New in LSOA	Share New in LSOA and year	Share Flats in LSOA	Share Flats in LSOA and year
Sample	Full Sample	2001–2011	Full Sample	Full Sample	Full Sample	Full Sample
N	7,601,627	4,959,583	7,602,276	7,602,276	7,602,276	7,602,276

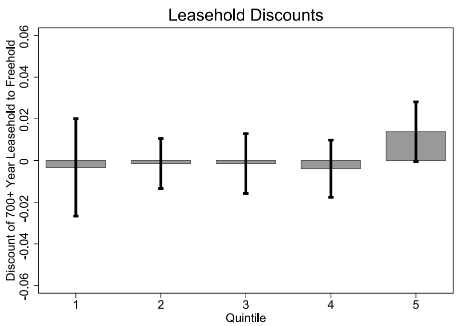
[a]Table shows results from regression (3). The dependent variable is the log price paid in arms-length sales of houses in the U.K. between 1995 and 2013. In each column, we report the coefficients on the 700+ leasehold indicator, by quintiles of measured development potential. The different columns correspond to different definitions of redevelopment potential, used to divide the sample into quintiles, and different time periods. The top quintile corresponds to areas with the highest redevelopment potential. In columns 1 and 2, we measure the redevelopment potential by the increase in the housing stock at the LSOA-level between the 2001 and 2011 censuses. Column 1 runs the regression on the full sample, column 2 on the period between the two census observations (2001–2011). In columns 3 and 4, we measure redevelopment potential as the fraction of sales of new properties in each area, while in columns 5 and 6, we measure redevelopment potential as the fraction of sales that are of flats in each area. In columns 3 and 5, we construct the quintiles of redevelopment potential by sorting transactions at the LSOA level; in columns 4 and 6, we instead construct them by sorting at the LSOA × year level. All specifications include fixed effects at the 3-digit postcode by transaction quarter by property type (detached, semi-detached, terraced) level, as well as controls for property size, the number of bedrooms and bathrooms, property age, property condition, whether there is parking, and the type of heating. Standard errors are clustered at the 3-digit postcode level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



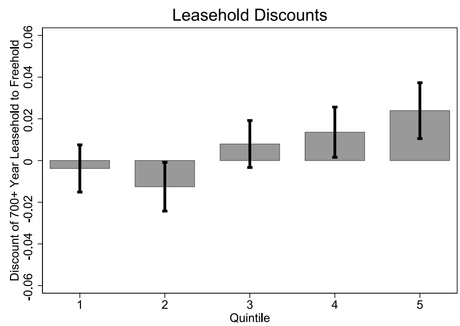
(A) Change in Housing Stock



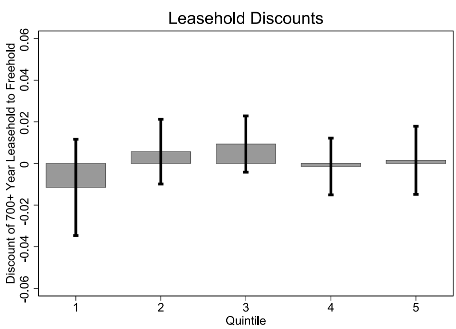
(B) Change in Housing Stock (2001-2011)



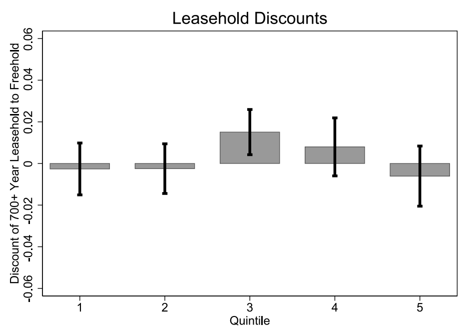
(C) Share of New Properties, by LSOA



(D) Share of New Properties, by LSOA and year



(E) Share of Flats, by LSOA



(F) Share of Flats, by LSOA and year

FIGURE 3.—Cross-section of bubble claim by redevelopment potential (U.K.). *Note*: Figures show estimates of the discount between 700+ year leaseholds and freeholds from regression (3), by quintile of measured redevelopment potential across areas. Higher quintiles indicate higher redevelopment potential. Panels A and B measure redevelopment potential using the growth in the total housing stock between 2001 and 2011. Panel A uses the full sample, while Panel B focuses on transactions in the years 2001–2011. Panels C and D measure redevelopment potential by the transaction-share of new properties in each LSOA (Panel C) and each LSOA-year (Panel D). Panels E and F measure redevelopment potential by the transaction-share of flats in each LSOA (Panel E) and each LSOA-year (Panel F). The variables used to measure redevelopment potential—and the quintile subdivision—correspond to the ones described in Table VI. The bars indicate the 95% confidence interval of the estimate using standard errors clustered at the 3-digit postcode level.

in each LSOA-year (column 6). Areas with many flats offer opportunities for land-use intensification for the houses in our regression sample, whereby one house might be profitably redeveloped into a larger number of flats. Again, we find no evidence for a classic rational bubble across any of the quintiles of the distribution of this measure of redevelopment potential.

Finally, while the houses that we consider in our main analysis might have significant redevelopment option value, units within structures should have much less. If leasehold contracts meaningfully constrained the redevelopment option, then we would expect the price difference between leaseholds and freeholds to vary across houses and flats. However, we showed in Section 3.3.1 that the absence of a price difference between extremely-long leaseholds and freeholds is consistent across both houses and flats.

Overall, there is no difference between the prices of extremely-long leaseholds and freeholds, even for properties with very high redevelopment potential. This confirms the prior, based on our analysis of the institutional framework, that restrictions to a leaseholder's ability to redevelop a property do not significantly affect the relative value of extremely-long leaseholds and freeholds.

3.3.3. *Leasehold Extensions and Expiration*

Leaseholders and freeholders can, in principle, agree on a price to extend the contract and increase the maturity of the lease ("lease extension"), or, alternatively, transfer the whole freehold interest to the leaseholder ("enfranchisement"). Such transactions are more common in the U.K. than they are in Singapore, where the largest freeholder, the Singapore Land Authority, has made it clear that their "policy is to allow leases to expire without extension."[29] These transactions, and in particular the extensions, are generally undertaken on short duration leases to avoid expiration and reversion of the property to the freeholder, and are not common for the extremely-long leases that are the focus of this paper.[30] Since these are private market transactions among willing participants, we would expect them to happen at market values: the freeholder receives a mutually-agreed compensation for the sale of part or all of the residual value of the freehold.[31]

The possibility that a contract of finite maturity, like the leasehold, could be extended at market prices by purchasing a new contract, in this case an ex-

[29]Extensions in Singapore are possible in exceptional circumstances, and are considered on a case by case basis, where the main criteria are whether "they are in line with planning intention and help to further specific economic and social objectives." Direct quotes in this footnote and the text above are from an SLA press release available at http://www.sla.gov.sg/News/tabid/142/articleid/171/category/Press.

[30]More than 80% of extensions in Giglio, Maggiori, and Stroebel's (2015) sample occur on leases with fewer than 80 years left.

[31]Lease extensions are not always registered with the Land Registry, and are not included in our data.

tended lease or the freehold, does not per se affect our test for classic rational bubbles. In an equilibrium with classic rational bubbles, the freehold would be more expensive than the extremely-long leasehold precisely by the amount of the bubble. Lease extensions that add a finite number of years to a leasehold contract with more than 700 years of remaining maturity would be priced at essentially zero, because the new lease would still not include the bubble and would have the same fundamental value as the old lease. Instead, enfranchisements would occur at a price that equals the value of the bubble, because by acquiring the infinite-maturity freehold interest, the leaseholder would obtain the bubble that is attached to it.

In the U.K., but not in Singapore, leaseholders of houses in some cases have the statutory right to enfranchise or extend the lease by 50 years (Leasehold Reform Act 1967). The law stipulates that, while such rights are granted, the freeholder needs to be compensated for any loss of value at market prices.[32] These statutory rights do not materially affect our empirical results. In fact, our results are consistent across the U.K. and Singapore, but no statutory rights are present in Singapore. For the U.K., the statutory rights can only adversely bias our test if enfranchisements and extensions are underpriced; but even in this case, the statutory rights could not explain our finding of a zero price difference between contracts estimated with very tight standard errors. For this to happen in the presence of a bubble, leaseholders must expect to be able to get the freehold *for free* and *with certainty*, in effect expropriating the freeholder. This runs contrary to the evidence in Giglio, Maggiori, and Stroebel (2015), who showed that the private market and the courts impose compensation that is negatively related to remaining maturity for shorter-maturity leaseholds (between 80 and 300 years). Finally, lease extensions and enfranchisement involve significant transaction costs for the leaseholder, thus reducing the possible price impact of the statutory rights.[33]

[32]In fact, the price "shall be the amount which at the relevant time the house and premises, if sold in the open market by a willing seller, might be expected to realise." In practice, this means that if the two parties, the leaseholder and the freeholder, cannot reach an agreement in the private market, they can resort to a court (a Leasehold Valuation Tribunal for most of our sample) tasked with determining the "market price" of the extension or enfranchisement. Court decisions, even if clearly special in many dimensions, different from market transactions, and related to a selected sample of market participants, have at least been consistent with our empirical result that there is no difference in value between extremely-long leaseholds and freeholds. If compensation was awarded to the freeholder, it was unrelated to the future value of the contract, that is, the possible bubble, and instead connected to the loss of immediately payable ground rents. In none of the many court decisions we investigated did any participant raise the possibility of a classic rational bubble and the related resale option value of the freehold contract.

[33]The leaseholder has to pay the cost of valuation services, legal counseling, and legal expenses, and has to bear the uncertainty associated with possible court proceedings.

3.4. *Heterogeneous Buyers and Frictions*

A further possible concern is that the market for leaseholds and freeholds might be segmented in the presence of heterogeneous buyers.[34] In the U.K., there are no legal restrictions that prevent agents from transacting either of the two contracts.

However, even in the absence of legal restrictions, one could be concerned about implicit restrictions. To address this concern, we study whether buyers of freeholds and leaseholds differ in ways indicative of market segmentation. Since our data set does not report buyer characteristics, we instead analyze data on owners of houses from the Survey of English Housing, an annual household-level survey conducted between 1993 and 2007. The survey contains information about 187,335 households, and it reports several household characteristics, as well as whether the household owns a freehold or a leasehold. Table A.II of the Supplemental Material shows that observable differences between house owners who are freeholders or leaseholders are minimal. In particular, the first two columns of the table report the sample means and standard deviations of the main variables. Column 3 shows the average difference between leaseholders and freeholders. Column 4 shows the difference conditional on geographic fixed effects, while column 5 shows the difference conditional on geographic fixed effects as well as our hedonic controls (to the extent they are available in this data set). As is clearly visible from columns 4 and 5, once we control for geographic fixed effects, there is no economically or statistically significant difference between freeholders and leaseholders in our sample. For example, the difference in weekly income between owners of leasehold and freehold houses is less than £11, and there is no difference in the number of family members. These results minimize the concern that the two markets are segmented in a meaningful way.

Financing frictions are also not a concern for our tests. While it is possible that leaseholds with particularly short maturities of less than 60 years are less valuable as collateral for mortgages, extremely-long leaseholds are treated identically to freeholds for financing purposes.[35] Intuitively, while a 999-year lease will eventually run down to 60 years of maturity left and then might incur a loss in its collateral value, this loss is 939 years into the future and

[34]From a theoretical perspective, the main concern is segmentation. Our tests are robust to the presence of heterogeneous agents as long as they can all trade the freehold and leasehold contracts. In equilibrium, these agents would have to agree on the price of all traded assets, including the bubble asset. Our tests would then correctly detect the equilibrium pricing of the bubble asset. Segmentation is a possible concern precisely because it could prevent this equalization of valuations in market prices.

[35]Banks in the U.K. typically require 30 years of remaining lease length after the expiry of the mortgage. Since the most common length of U.K. mortgages is 25 years, this restriction starts to bind for leases shorter than 60 years. Similarly, in Singapore, households are not allowed to use their pension contributions for a property down payment if the lease has fewer than 60 years of maturity.

has no impact, even at low discount rates, on the current value of the lease. Consistent with this, Appendix Table A.II of the Supplemental Material shows that freeholders and leaseholders have the same probability of having financed their house with a mortgage. Overall, we can exclude the possibility that any of our results are driven by differential financing frictions between extremely-long leaseholds and freeholds.

3.5. *Stability of Property Rights*

One natural concern when analyzing contracts with horizons that span several hundred years is whether agents today expect such property rights to be enforced in the future. Ex ante, such a concern might be more relevant for Singapore, a relatively young country. Yet, despite Singapore's relatively recent independence in 1965, it consistently tops the ranks of countries with the strongest property rights around the world.[36] The enforcement of property rights is of paramount importance for a small open economy and world financial center. Similarly, the U.K. has one of the longest track records of any country in upholding and promoting property rights. Concerns over the stability of its laws and institutions are as close to non-existent as can be found in real world applications of economic theory.

From a theoretical perspective, we can distinguish two effects of property rights on our tests: a general concern with the enforceability of all contracts, in our case of both leaseholds and freeholds, and a concern with selective enforcement of one type of contract above the other. Classic rational bubbles rely on the existence of property rights that are strong enough that agents can expect today to sell the bubble asset at increasing prices to future generations of agents. If agents were expecting that governments would expropriate the bubble asset for sure at some future finite time, then no classic rational bubble could exist. While we cannot rule out this unobservable expectation of expropriation, by testing the theory in countries with very strong property rights and on an asset, real estate, that is comparatively harder to expropriate than fiat or intangible assets, we provide a testing ground favorable to finding a classic rational bubble.[37]

A more nuanced possibility is that agents might expect leasehold and freehold contracts to be selectively enforced. If agents were to expect leasehold contracts to be reneged upon but freehold contracts to be upheld, then this

[36]For example, it is ranked number 7 worldwide in the 2013 International Property Rights Index compiled by The Property Rights Alliance, and ranked number 2 worldwide in the 2014 Index of Economic Freedom compiled by the Heritage Foundation.

[37]Importantly, if agents expected expropriation of the kind described above, our test would be perhaps less informative, since such complete expropriation is ruled out in the theory of the classic rational bubble, but still be unbiased since it would find no bubble in a setup where the bubble indeed cannot exist.

could generate a price discrepancy between the two that resembled, mistakenly, a classic rational bubble. This effect makes our result of no classic rational bubble in the data a conservative one. On the contrary, if agents were to expect that leasehold contracts be converted into freehold ones for free, thus expropriating the rights of the underlying freeholder, our test would be biased against finding a classic rational bubble.

However, a longstanding tradition of strong institutional enforcement of both leasehold and freehold contracts minimizes the concern that our test could be biased by selective enforcement. Both types of contracts have been originated, traded, and enforced repeatedly in the past (see Appendix A.3.5 of the Supplemental Material for statistics on the number of lease determinations). We also stress that since both types of contracts are commonly used in private residential real estate transactions, selective enforcement would result in large-scale transfers that would be politically daunting for most governments. The stability of the contracting environment is highlighted by the fact that in our sample of U.K. transactions, we observe 271,661 secondary market transactions of leases that were originated before 1914, more than 100 years ago. We observe 25,497 transactions of leaseholds created more than 150 years ago. The oldest lease to transact in our U.K. sample was originally created in 1555. Similarly, in our sample of Singapore transactions, we observe 25,297 transactions of leases that were originated even before independence in 1965, and enforced despite the separation from the United Kingdom. The oldest lease to transact in our sample had originally been created in 1827, only three years after the Anglo-Dutch Treaty of 1824 confirmed the status of Singapore as a British possession, and only one year after the British East India Company included Singapore in the newly-formed Straits Settlements. Since past experience is likely to strongly influence expectations, concerns over current expectations of future ability to freely transact each type of contract are minimal.

3.6. *Timing the Market: Maturity Choice*

While the predictions of classic rational bubble models for trading volume are very sensitive to the specific modeling assumptions and are often indeterminate, it is still interesting to investigate whether the origination of new leases, or the trading of existing ones, responds meaningfully to the possible presence of a bubble. For example, one could conjecture that if there were a bubble in some years, developers would opt to sell the new properties as freeholds to capture the bubble, rather than as long-dated leaseholds.

Appendix Figure A.23 of the Supplemental Material shows that in both the U.K. and in Singapore, there is little annual variation in the fraction of resale transactions under each type of contract, extremely-long leasehold or freehold. In particular, the share of freeholds over the total of freeholds and extremely-long leaseholds does not respond to variations in the housing market, such as movements in the overall house price level or the price-rent ratio. Similarly, the trading activity does not respond to variation in the (small and statistically insignificant) point estimates of our test for the bubble. In the U.K., there is also no time-series variation in the share of extremely-long leaseholds or freeholds on newly-built properties, where the seller could, in principle, choose whether to sell the property as a leasehold or a freehold. For Singapore, the share of extremely-long leaseholds among newly-built properties is somewhat more volatile, though the baseline number is less than 4,000 per year. In addition, newly-built properties in Singapore are normally in large apartment buildings with hundreds of units, and so the change of one development project's status from extremely-long leasehold to freehold can induce significant variation in the share of extremely-long leaseholds. Overall, we find that, consistent with the idea that there is no classic rational bubble in the data, there is no systematic variation in the type of contract being traded or originated.

4. IMPLICATIONS OF DIFFERENT TYPES OF BUBBLES

In this paper, we rule out the presence of bubbles that feature a failure of the transversality condition in the U.K. and Singapore housing markets. We remain silent on the presence of other types of bubbles that might occur in finite-horizon economies or on finite-maturity assets. In this final section, we argue that distinguishing among different types of bubbles is important because they have markedly different positive and normative implications.

While it is beyond the scope of this paper to provide a general treatment of these differences, we use a tractable, stylized framework to analyze one feature of bubbles that has attracted significant attention: the response of bubbles to interest rate changes. Both policy and academia have discussed whether monetary policy should "lean against the wind" by increasing interest rates to burst an asset price bubble. Similarly, there has been a lively debate on whether the Federal Reserve kept interest rates too low during the 2001–2005 period, thus contributing to asset bubbles, including in U.S. real estate (Allen and Gale (2004), Bernanke (2010), Galí (2014)). We show that the response of bubbles to interest rate changes depends crucially on the type of bubble considered.

We consider a stylized economy that builds on the work of Harrison and Kreps (1978), Scheinkman and Xiong (2003), and follows Simsek (2010, 2013). The economy has infinite horizon and is populated by overlapping generations. Each generation is alive for 1 period: agents born at time t die at time $t+1$. There are two assets: the risk-free rate in perfectly elastic supply at $1+r$, and an asset in positive supply of 1 unit that represents a claim to future dividends. Agents can borrow unlimited amounts, but cannot short the asset.[38] Dividends are denoted by D_t, and follow a stochastic process, such that

[38] In making this assumption, we follow Harrison and Kreps (1978) and Scheinkman and Xiong (2003) who showed the assumption to be equivalent to assuming that agents have infinite wealth

$D_{t+1} = D_t s_{t+1}$, where s_{t+1} is an i.i.d. random variable with compact and strictly positive support, and whose expected value is 1. All traders are risk neutral, but they form expectations in different ways. Each period, two groups of traders are born: one group is optimistic about next period's dividend and thinks that $E_t^o[D_{t+1}] = D_t(1+\varepsilon)$ with $r > \varepsilon > 0$, where E_t^o denotes the expectation taken under the optimistic belief by traders born in period t. The other traders are neutral in the sense that they compute expectations using the true distribution of s_{t+1}. Both groups of traders agree on the distribution of further changes in dividends after the next period (i.e., $E_t^o[s_{t+j}] = E_t[s_{t+j}]$, $\forall j \geq 2$). The optimist's "buy-and-hold" present discounted value of the dividends is defined to be

$$P_{o,t} \equiv \sum_{j=1}^{\infty} \frac{E_t^o[D_{t+j}]}{(1+r)^j} = \frac{D_t(1+\varepsilon)}{r}.$$

We follow Harrison and Kreps (1978) and the subsequent literature, and take $P_{o,t}$ to be the measure of the fundamental value of the asset.[39] The market value of the asset exceeds this fundamental value, because it potentially contains two bubbles: a resale-option bubble and a purely speculative bubble. Since agents can borrow and lend freely, in the equilibrium we construct the asset will be held in each period by the most optimistic agents in that period, thus satisfying the recursion

$$P_t = \frac{D_t(1+\varepsilon) + E_t^o[P_{t+1}]}{1+r}.$$

By forward iteration, this recursion yields

$$P_t = \frac{D_t(1+\varepsilon)}{r-\varepsilon} + (1+r)^t M_t,$$

where M_t is a positive martingale for both optimists and neutral agents, with initial value at $t = 0$ assumed to be $M_0 \geq 0$. It is convenient to rewrite this as

$$P_t - P_{o,t} = P_{o,t}\frac{\varepsilon}{r-\varepsilon} + (1+r)^t M_t = B_{HK,t} + B_t. \tag{4}$$

$P_{o,t}$ is a measure of the fundamental value, $B_{HK,t} \equiv P_{o,t}\frac{\varepsilon}{r-\varepsilon}$ is the Harrison and Kreps resale-option bubble with $\frac{\varepsilon}{r-\varepsilon} > 0$, and $B_t \equiv (1+r)^t M_t$ is the purely speculative bubble.

and discount assets at rate r. We could have alternatively specified the set-up with infinite wealth and discount rate r. Unlimited borrowing allows us to not specify the mass of agents or their endowments. Upon birth, agents borrow what they need to invest in the assets.

[39]Intuitively, the literature thinks of bubbles as being related to speculation in sequential trading, and thus chooses a buy-and-hold valuation that, by definition, does not reflect the value of future re-trading of the asset, to be the fundamental value. Other definitions of the fundamental value are certainly possible, but we choose the one that dominates the literature for consistency.

The term $\frac{\varepsilon}{r-\varepsilon}$ in equation (4) reflects a (proportional) increase in market price compared to the fundamental value that is due to the fact that optimists in each period anticipate being able to sell to future optimists. This is the resale-option bubble that is the focus of Harrison and Kreps (1978), Scheinkman and Xiong (2003), and Simsek (2010). While these papers and this section consider an infinite-horizon economy and infinite-maturity asset for convenience, this type of bubble would also be present in a finite-maturity asset or a finite-horizon economy, with the property that the bubble converges to zero as the economy approaches the last period. Our tests are silent on this kind of bubble.

Harrison and Kreps (1978) and Scheinkman and Xiong (2003) noted, however, that their economies feature equilibria with an additional type of bubble, the purely speculative bubble $B_t = (1+r)^t M_t$. This bubble grows in expectation at rate r, and originates from the infinite maturity of the asset. It is purely speculative, because it only requires the infinite circular argument of an expectation to potentially sell the asset to other agents in the future. Our tests rule out this purely speculative bubble by showing that $M_t = 0$.

One consequence of our results is to support empirically the theoretical decision in Harrison and Kreps (1978), Scheinkman and Xiong (2003), and Simsek (2010) to select and focus on the minimal bubble equilibrium where $M_t = 0\ \forall t$. Selecting among the two types of bubbles is not a purely quantitative decision. For example, the two types of bubbles have opposite qualitative comparative statics with respect to the interest rate r. Let lower case b denote the logarithm of each type of bubble and $b_{ToT,t} \equiv b_{HK,t} + b_t$ be the total (log) size of the bubble; then the semi-elasticity of the total bubble to the interest rate is[40]

$$\frac{\partial b_{ToT,t}}{\partial r} = \underbrace{-\frac{1}{r}}_{<0} \underbrace{-\frac{1}{r-\varepsilon}}_{<0} \underbrace{+t\frac{1}{1+r}}_{>0}. \tag{5}$$

The first term, $(-\frac{1}{r} < 0)$, captures that the fundamental value of the asset falls with higher interest rates, and since the resale-option bubble is proportional to the fundamental value, the resale-option bubble falls. The second term, $(-\frac{1}{r-\varepsilon} < 0)$, highlights the dynamic dimension of the resale-option bubble: its value comes from the fact that today's optimists can sell the asset at a high price to tomorrow's optimists, and so on. A higher interest rate reduces the

[40]In the interest of simplicity, we consider here comparative statics rather than a model with interest rates that change endogenously. We suspect that much of the intuition and the overall result carry over to a more extensive model, but do not formally develop such analysis here. Comparative statics in the presence of multiple equilibria need to be interpreted with care; in particular, the value of M_t is a pure sunspot that could, in principle, arbitrarily depend on r. In equation (5), we found it most natural to keep M_0 constant and to consider the evolution of two economies that are identical, even in the realizations of the stochastic process M_t, other than in the level of the interest rate r. We abuse the notation and refer to $b_{ToT,t}$ as the total log-size of the bubble, but we note here that, more precisely, it is the sum of the log-size of each bubble.

present value of these future resale cash flows, and therefore the resale-option bubble coming from the dynamic component. Since both of the terms (the first two in equation (5)) associated with the semi-elasticity of resale-option bubbles to interest rates are negative, the resale-option bubbles decrease in value with increases in interest rates.

The last term, $(t\frac{1}{1+r}) > 0$, shows that the purely speculative bubble, like the classic rational bubble in Galí (2014), increases with higher interest rates. This occurs because the bubble, being based on pure speculation, has to grow in expectation at the rate of interest.

This section shows that the choice of the class of bubbles considered in theoretical models has important implications for the normative and positive implications of those models. In this light, the fact that our tests rule out the presence of classic rational bubbles in housing markets might shift the literature towards focusing on other, more empirically-plausible models of bubbles. The implications of including such possibly-irrational bubbles in macroeconomic models remain an interesting avenue for further research.

5. CONCLUSION

We provide a direct test for bubbles associated with failures of the transversality condition, the most prominent type of which is the classic rational bubble, and find no evidence of such bubbles in housing markets even during periods and submarkets where the most advanced existing econometric time-series tests detect a classic rational bubble. Our findings inform the ongoing effort to understand real estate prices and the theoretical effort to distinguish between different models of bubbles. Future research should investigate which conclusions of the theoretical macroeconomics literature, which has mostly relied on classic rational bubbles, are robust to the introduction of (possibly behavioral) bubbles that can occur in finite time.

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