# Why do REIT Returns Poorly Reflect Property Returns? Unrealizable Appreciation Gains due to Trading Constraints as the Solution to the Short-Term Disparity

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**Abstract**

This study addresses the short-term disparity between REIT returns and direct property returns, and argues that this phenomenon is due to the trading constraints in the direct property market imposed on REITs (the *dealer rule*), which render them unable to time markets in order to realize short-term property appreciation profits. This makes REITs primarily a *property income* investment rather than a full *property* investment, and explains the observed disparity. Specifically, a REIT investor is consistently exposed to rental cash flows, but is not exposed to property appreciation returns that are not contained in income, that is, cyclical price changes caused by a change in the capitalization rate. I test this hypothesis over a 1978-2009 data sample and find strongly that REIT returns consistently reflect property income returns, but not property appreciation returns. This result makes this study, to my knowledge, the first in the literature to find a consistent link between REIT returns and any portion of direct property returns, even at short time horizons, in the context of a linear factor model. I then set up a natural laboratory to test the trading-constraints explanation by examining the appreciation dependence of different types of REITs, which should be differently affected by the trading constraints. I find that returns to UPREITs, which are less affected by the constraints, have a stronger appreciation dependence than returns to regular REITs. As a robustness check, I also perform a size test and find that large REITs, which are less affected by the constraints, have a stronger appreciation dependence than small REITs. When testing the effects of UPREIT and size characteristics simultaneously, I find a consistent UPREIT effect. I further find that Real Estate Operating Companies (REOCs), which are not subject to trading constraints, show short-term property appreciation dependence. These findings offer strong support for the trading-restrictions explanation.

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# 1 Introduction

Equity Real Estate Investment Trusts (REITs) are widely considered by analysts and institutional investors as additions to a diversified multi-asset portfolio, for a more liquid and more easily accessible alternative to holdings of direct real estate. However, as is widely documented, equity REIT returns do not strictly follow the movements and returns of the underlying direct property market (see, for example, Giliberto (1990, 1993), Liu and Mei (1992), Myer and Webb (1994), Lizieri and Satchell (1997), Ghosh, Guttery and Sirmans (1998), Ling and Naranjo (1999), Glascock, Lu and So (2000), Clayton and Mackinnon (2003), Riddiough, Moriarty and Yeatman (2005), Pagliari, Scherer and Monopoli (2005) ).[1] While in the long run a fundamental relationship between securitized and unsecuritized real estate seems to exist, in the short run REIT returns do not follow those of the underlying property market.

This study presents a novel view on the relation (or apparent lack thereof) between the two markets, by examining the dependence of REIT returns on direct property returns decomposed into income- and appreciation components. Specifically, I argue that, due to the selling constraints which REITs face in the direct property market in order to fulfill the *dealer rule*, which prohibits holding property primarily for resale, REITs are *property income vehicles*, rather than complete *property investment vehicles*. This means that, by investing in a REIT, an investor only receives exposure to the rental returns from the REIT's portfolio, and virtually no exposure to the portfolio's short-term property value growth. In particular, property prices contain forecastable short-term growth opportunities not yet present in rents (often referred to as price changes brought about purely by a change in the capitalization rate); it is these changes that are missing from REIT returns. There is evidence in the literature that, due to the inefficiency of the direct property market, systematic profits can be made by *timing* changes in this largely cyclical price component and, correspondingly, reallocating funds from overvalued to undervalued market segments.[2] Because, in order to retain tax-free status, a REIT is required to hold each property in its portfolio for four years, and after that is only allowed to sell 10% of its asset base at a time, a REIT manager's ability to realize such market timing profits is very limited;[3] this virtually eliminates an essential component of property returns from REIT cash flows.[4] The more efficient stock market seems to be aware that REIT managers are often unable to exploit

[1]While Pagliari et al. (2005) also detect a gap in the performance of the two asset classes, they find that this has narrowed somewhat in recent years.

[2]For example Liu and Mei (1994), Geltner and Mei (1995), Mühlhofer (2009).

[3]The 10% rule applies to REITs which engage in more than seven sales transactions in a given tax year. Until 1999, a REIT also faced the limitation that only 30% of its income could come from capital gains on the sale of property. Each sale which does not meet these criteria is considered a *prohibited transaction* and is subject to a 100% gains tax. In 2008, the four-year minimum holding period was reduced to two years.

[4]There is evidence in the literature that REIT managers create value through active strategies. For example, Campbell, Petrova and Sirmans (2006) find that REIT property sell-offs generate value for REIT share holders; Hartzell, Sun and Titman

a peak in a price cycle as an opportunity to sell and realize appreciation profits, and thus such appreciation gains largely disappear from REIT prices because they cannot be realized. Anecdotally, for example, when Blackstone privatized Equity Office Properties, it immediately sold about 60% of the REIT's portfolio, which the REIT itself could not have done. This implies that there was a substantial efficiency loss through the inability to sell, imposed on EOP's portfolio by its REIT structure. Further to this anecdotal evidence, Mühlhofer (2009) systematically tests the bindingness of the *dealer rule* directly and finds that REITs are significantly bound by this.

My analysis shows that about 90% of short-term property-price variation is due to cyclical changes in cap rates, the component of property returns which, I argue, is not realized in REIT returns in the short run. The absence of this expectation component of property returns from REIT returns explains the apparent lack of short-term correlation in the performance of the two asset classes, found in previous literature. According to my rationale, REIT returns should depend on property returns, but only on the income component. However, since fluctuations in that component of appreciation which is not contained in income constitute a large part of overall property returns, no correlation would be found between REIT returns and total property returns. This is in line with findings in past literature and would explain the puzzle of what drives a wedge between the returns of the two asset classes.[5]

I test this hypothesis with a linear factor model which measures incremental information content of property income variation, as well as variation in property appreciation not contained in income, for explaining REIT returns. While in this paper I argue in favor of a dependence of REIT returns on property returns, the latter do not constitute the only return driver for REITs; the asset class has more than one single source of variation driving returns.[6] The approach I use allows me to test whether the two components of property returns provide useful incremental information content for explaining REIT returns, after other potential drivers of systematic returns have been accounted for. I estimate this model over a 1978 to 2009 period, accounting for structural breaks to allow for heterogeneity in the characteristics of the REIT industry before, during, and after the REIT boom of the 1990s.[7] I find that REIT returns have a strong positive dependence on the marginal information content of property income returns over the entire sample period, with virtually no significant dependence on the marginal information content of appreciation, over income.

(2010) also find some evidence in favor of market timing in well-governed REITs; finally Hochberg and Mühlhofer (2011) find a large degree of ability by REIT managers to create value though actively trading across sub-markets.

[5]This result is also consistent with the findings in Mühlhofer and Ukhov (2011) who show that direct-property income provides useful information content in predicting REIT dividend yields in a Campbell and Shiller (1988) VAR setting.

[6]A statistical factor analysis in Hartzell, Mühlhofer and Titman (2010), for example, reveals that 13 statistical factors are necessary to explain REIT returns.

[7]See, for example, Ott, Riddiough and Yi (2005) for evidence of such structural breaks.

These findings thus show that REITs, systematically, are *property income* investment vehicles, rather than full *property investment* vehicles: this should contribute to the investment community's perception of the role of REITs in a multi-asset portfolio. More importantly, however, this presents a solution to the puzzle of the short-term relationship between the direct property market and the REIT market. All results in this study include the 2008-2009 financial crisis, which should yield even stronger credibility to the economic relationships described, as they seem to hold even in market turmoil.

I then proceed to give evidence that the trading restrictions imposed by the *dealer rule* constitute the cause for the absence of property appreciation returns from REIT returns. I test this hypothesis by making use of the distinction between Umbrella Partnership REITs (UPREITs) and regular REITs as a natural laboratory, by testing whether appreciation dependence differs between the two types of structure. In an UPREIT, the REIT holds shares in a limited partnership, known as the operating partnership, which then in turn holds the portfolio of properties.[8] The partnership structure enables an UPREIT to efficiently acquire properties through Section-1031 like-kind exchanges, by allowing previous owners (known as contributing partners) to sell the property to the UPREIT by exchanging it for shares in the operating partnership's overall property portfolio. The advantage of undertaking such a transaction, is that (like all 1031 exchanges) this does not trigger a taxable event for the contributing partner, who can subsequently exchange his or her portfolio shares for cash or REIT shares, when this is most advantageous for tax purposes. This has proven to be such an efficient way of assembling a property portfolio, that a majority of REITs have adopted the UPREIT structure nowadays.

In the context of this study, an UPREIT's primary modus operandi of acquiring properties through 1031 exchanges enables a natural laboratory for testing the effect of the *dealer rule*. The reason for this is that because the IRS does not view a 1031 exchange as a sale, the property's basis flows from the seller (the contributing partner) to the UPREIT. This means, among other things, that the UPREIT inherits the contributing partner's holding period, from the time the property was purchased by the contributing partner. Since the private entities that ordinarily constitute contributing partners tend to hold their properties for a long time, in part due to the high transactions costs they face, this means that on most of its properties the UPREIT will have likely met the minimum holding period for the *dealer rule* within a very short time of its purchase of the property. Thus, through this mechanism, an UPREIT is virtually freed from the minimum

[8] In spot checks of 10K forms by UPREITs I find that all the firms checked hold their entire property portfolio through their operating partnership. Thus, the Operating Partnership should be thought of as an UPREIT's primary means of property acquisition.

holding periods and can easily transact virtually unconstrained.[9] [10] Besides this important distinction, UPREITs and non-UPREITs are operationally very similar to each other in their activities. In a parallel study to this one, Mühlhofer (2009) shows explicitly that UPREITs trade significantly more frequently than non-UPREITs, and that non-UPREITs are significantly bound by the minimum holding periods, while UPREITs are indifferent to this. These results should help validate the effectiveness of my natural laboratory.

The account above leads to a hypothesis that returns to UPREITs should show appreciation dependence, while those to regular REITs should not, if the dealer rule is in fact the cause for this disparity. I test this hypothesis over a 1994 to 2009 sample (retaining the same structural breaks) and find that the returns to UPREITs do show a significantly positive appreciation dependence, while the returns to regular REITs do not. These findings from my natural laboratory present strong evidence in favor of the hypothesis that the trading constraints constitute the cause for the short-term disparity. Once again, these results hold through the 2008-2009 financial crisis, which emphasizes the strength of the economic relationships described here.

I then perform a robustness check based on firm size to rule out the possibility that the appreciation effects obtained in the 1990s are simply due to the improved quality of the direct-property appreciation series in this decade. Size should also, at least in part, help a REIT overcome the 10% portion of the selling constraint, as a large REIT will be less affected by this constraint than a small one, because the former can dispose of more properties without infringing on the 10% boundary than the latter. I perform this test over a 1978 to 2009 sample (again with structural breaks) and find that returns to large REITs show a weak but significantly positive appreciation dependence in the early part of the sample, before 1992. These findings should alleviate the concern that the appreciation dependence found in the 1990s is simply due to the appreciation data being fundamentally different in that time period, and should further reinforce the support for the trading constraints hypothesis. The results from these two firm-characteristic tests also contribute to this study's implications to portfolio management. While REITs, as an asset class, constitute a property income investment, a portfolio manager can invest in UPREITs, and, to a lesser extent, in large

[9] It is also possible for a non-UPREIT to conduct a 1031 exchange. This would happen when the REIT disposes of a property. The exchange is done by selling the property and putting the proceeds into a QI (a Qualified Intermediary). Within 14 days of the sale of the old property, a new property to be acquired must be identified, and within six months, the new property must be purchased. If all these conditions are met, such a transaction is also viewed as a 1031 exchange, and thus will not be considered a prohibited transaction, regardless of previous holding period, because 1031 exchanges by definition are made on investment property, rather than dealer property. However, it seems apparent that despite this ability by non-UPREITs, an UPREIT still has a competitive advantage in this respect, as these rules are quite binding, in comparison to those governing the situation for an UPREIT, which can effectively sell the property outright whenever it deems necessary, even without an immediate replacement. This point is emphasized by Ling and Petrova (2008), who demonstrate that property buyers in this type of 1031 exchange systematically overpay. The ability to sell outright is especially useful when downsizing to cash out before a falling commercial property market, which a regular REIT cannot do through a 1031 exchange.

[10] This information is derived from an interview with Kevin Habicht, Chief Financial Officer of National Retail Properties, Inc. (NNN), a major US REIT, conducted on 9/2/2009. This information is also used in Mühlhofer (2009) and explained in similar language as here.

REITs, in order to obtain both the income and the appreciation component of property returns.

Given that size and UPREIT status are strongly correlated, I then perform a further robustness check in which I distinguish between both types of firm characteristics. This should alleviate a concern that UPREIT status really only proxies for size, which in turn proxies for other unobserved variables. I find that property appreciation dependence exists for both small and large UPREITs, while small non-UPREITs do not show this.[11] This supports the hypothesis that UPREIT status is the stronger determinant for appreciation dependence, which supports the trading-restrictions hypothesis.

I finally perform the same test on a sample of Real Estate Operating Companies (REOCs). These are property companies that follow a business strategy which does not allow them to qualify for REIT status. Given that these companies are not REITs they also do not underlie the *dealer rule*. If the *dealer rule* constitutes the cause for a property firm's lack of appreciation dependence, we should see REOCs show such dependence. A test of the returns to REOCs over a 1995 to 2005 sample (with the same structural break) reveals that this is the case: REOC returns show positive appreciation dependence in all time periods. This gives further support to my trading restrictions hypothesis.

In past literature, one study also performs a comparison of REIT returns to property returns decomposed into income and appreciation, namely Pagliari and Webb (1995). In that study, the authors try to equate property and REIT market return components, comparing direct-property income to REIT dividends and direct-property appreciation to REIT share prices. While this comparison seems very elegant and appealing, the results are generally inconclusive, at least on the income question. The problem here seems to be that dividends are managed, and that, therefore, despite the high dividend payout requirements that REITs face, property-level net operating income that enters the REIT is not necessarily carried through to the end investor purely as a dividend: any required return not paid out as a dividend will simply cause a share price adjustment. For this reason, in this study, I take a more relaxed view than Pagliari and Webb, analyzing REIT total returns (consisting of both share price changes and dividend payouts combined), and compare these to a decomposition of property returns into income and appreciation returns.

Overall, the relation between the returns to private real estate and publicly traded real estate remains of prominent interest in the literature. Aside from the studies cited earlier, several other studies examine the nature and correlations of REIT returns. Lizieri, Satchell and Zhang (2007) employ a statistical factor analysis technique to model REIT returns. Liow (2011) examines time-varying cross correlations of Asian securitized real estate markets, finding that significant co-movement exists among these. This is consistent

[11] The results for large non-UPREITs are inconclusive, given that too few such firms exist.

with the idea of a common underlying real estate factor for all these markets. Yunus (2009) also examines cross-market dependence among international securitized real estate markets and finds evidence of cointegration among many of them. Finally Yunus, Hansz and Kennedy (2011) further explore the issue of cointegration and find long-run time-series relationships among public and private markets in the US and UK. A VECM technique in the same study also reveals some short term dependence between public and private markets.

Notwithstanding these results, this study remains the first, to my knowledge, to document a consistent short-term relationship between REIT returns and any component of private real estate returns, in the context of a simple linear factor model, the setting in which the puzzle about this disparity first arose. It is also the first to formally show that this dependence is limited to direct property income in this setting. Further, the *dealer rule* and its effects seems to have been largely overlooked by literature so far.[12]

While the reason for my consideration of UPREIT status and size is that REITs which differ in these characteristics should be differently affected by the selling constraints and therefore should differ in their appreciation dependence, previous studies examine these firm characteristics for other reasons. For example, Han (2006) uses UPREIT status to assess the degree to which firm management is monitored. There are several studies illustrating size effects for REITs, for example, Colwell and Park (1990) and McInstosh, Liang and Tompkins (1991), who find that small REITs give greater returns without greater risk, Clayton and Mackinnon (2003), who find that a significant real estate risk factor, distinct from general stock- and bond-market risk factors, emerges for REITs in the 1990s, especially for small REITs, and Ambrose, Ehrlich, Hughes and Wachter (2000) and Ambrose, Highfield and Linneman (2005) who investigate REIT economies of scale. Thus, while previous studies exist which examine size and UPREIT status, the findings presented here should not find an immediate parallel in previous REIT literature.

The rest of this study proceeds as follows. Section 2 expands the intuition on why a selling-constrained REIT will be largely unable to realize short-term appreciation profits and why UPREITs and large REITs have an advantage in this respect. Section 3 tests whether REITs as a whole have appreciation dependence. Section 4 presents the UPREIT test to offer support for the trading-restrictions explanation. Section 5 presents the robustness check based on firm size. Section 6 presents the simultaneous test of size and UPREIT status, while Section 7 presents the REOC test. Section 8 concludes.

[12] Except in the concurrent study of Mühlhofer (2009).

## 2 How Trading Constraints Affect a REIT's Appreciation Returns

There are two return components to owning property for rent. Primarily, by owning a property, one has a right to the rental cash flows generated by that property. Traditionally (and most simplistically) this is the only investment value of a property, and property prices are determined by discounting the rental cash flows in perpetuity.

There is a vital component missing here, though, as prices must reflect forecastable growth opportunities, or, generally, information concerning future events that is known today. In a liquid rental market, however, rent levels will only contain information concerning today, and information concerning any future events would only appear in rents once the events have actually occurred. Thus, property prices will contain an additional expectations-based return component which is not mirrored in rents. This component, which I argue is missing from REIT returns, is often expressed as fluctuations in the capitalization (cap) rate and exists only in the short run: in the long run, the information content of prices and rents is identical.

As has been shown in past literature (Liu and Mei (1994), Geltner and Mei (1995), Mühlhofer (2009), among others) the majority of short-term property price fluctuations comes from this latter price component, and, due to the inefficiency of the property market, changes in this price component are forecastable in such a way as to make it possible to time turnarounds in property-market and property-submarket cycles, and so to generate returns which systematically outperform those of a simple buy-and-hold strategy devoid of timing considerations. It is this abnormal return generated by the timing of market and submarket cycles, which REITs are unable to realize due to their selling constraints: the more efficient stock market realizes this, and therefore property returns due to fluctuations in cap rates are absent from REIT returns.

To illustrate this situation, I consider a cyclical property submarket, as illustrated in Figure 1, page 48. I further assume that a REIT manager can time the turnarounds of this submarket with some degree of accuracy, and therefore buys into the market at time $t_0$, paying a price of $P_0$ for a particular portfolio of properties. The optimal selling point in this example would be $t_1$, which, once again, the REIT manager can pinpoint with some degree of accuracy, at which point the portfolio can be sold at a price $P_1$, yielding a profit of $P_1 - P_0 > 0$ from market timing.[13] Ignoring discounting and intermediate rental cash flows (which can be done here without loss of generality), the more efficient stock market would be able to predict the selling price of the portfolio, just like the manager, and value this portfolio at $P_1$ as soon as this selling time and price has been forecast, as this will be the value of the portfolio at the optimal selling point.

[13] The funds resulting from this transaction will then most likely be reinvested into a different market sector which is undervalued at time $t_1$.

Now assume that the REIT in question faces a selling restriction, which does not allow the manager to sell before time $t_2$, at which point the price of the portfolio has declined to $P_2 < P_1$. The efficient stock market is once again able to forecast the selling price of the portfolio and thus values the portfolio at $P_2$, since any higher value cannot be achieved for this portfolio in this cycle. Thus, while without selling constraints the value of this part of the REIT's portfolio would have changed from $P_0$ to $P_1$, in a constrained setting the portfolio value only changes from $P_0$ to $P_2$, eliminating a substantial part of the property portfolio's appreciation-based price fluctuation. Through this mechanism, the inability to sell reduces or even eliminates a REIT's ability to realize profits derived from the timing of cyclical property-price fluctuations across submarkets, and thus these price fluctuations do not appear in REIT values.[14] On the other hand, because realizing income returns does not require the ability to sell, any fluctuation in rent levels will clearly be reflected in REIT returns. The picture here is analogous to stock-market momentum and reversals (Jegadeesh and Titman (1993)). Stock portfolios will only exhibit momentum profits (i.e. what is commonly known as the *momentum factor*) if the manager performs active momentum trading. There is no dependence on the *momentum factor* by passive portfolios.

UPREITs are at a distinct advantage over regular REITs in this situation, as described in the previous section, in that their business model which centers around 1031 exchanges into their operating partnership enables them to largely avoid the minimum holding periods of the *dealer rule*. Note that the UPREIT manager need not choose to sell at $t_1$. The fact that, unlike in a regular REIT, the opportunity exists to sell the portfolio for $P_1$, will cause the stock market to value the portfolio at that price once this price forecast is available.[15] Large REITs also have a certain advantage over small REITs in this respect. While UPREIT status is more effective in helping maintain an actively traded portfolio without penalty, large REITs will be able to at least partly overcome the second half of the constraint, which only allows a REIT to sell 10% of its asset base at a time. To illustrate this, consider the following situation.

A small REIT owns nine approximately equally valued properties in the same market sector and has owned these for more than four years. The REIT now receives a reliable (and, ex-post, correct) *sell* signal on eight of these properties. Of course, the firm cannot sell all the properties within a year and move these funds into a different market sector, while still retaining both its REIT status, and the profits from these transactions. Thus, the firm is unable to satisfactorily time the market, which is necessary for realizing the short-term property appreciation profits that would stem from this signal.

Now suppose, on the other hand, that the REIT that owns these nine properties is a larger firm, which

[14] It can be shown that even in a less stylized setup, perhaps with myopic or noisy price forecasts, this intuition still holds.
[15] Once again, one can ignore discounting and intermediate rental cash flows for this example, without loss of generality.

owns them as part of a portfolio of 100 properties, and receives that same sell signal on the same eight properties. Assuming the holding period has been met, this REIT will be able to sell all eight of these properties, exploiting the full value of the sell signal, and capitalizing on the positive short-term price shock. Thus, this REIT has managed to time the market satisfactorily and realizes short-term appreciation gains. It should be noted, of course, that in this example both REITs have owned the properties for over four years. Size, unlike UPREIT status, only helps overcome the 10% hurdle, and not the 4-year constraint. I take firm size as a proxy for the number of properties contained in a REIT's portfolio, which, as I have just illustrated, should at least partly affect a REIT's ability to time the market while still fulfilling the trading constraints.[16]

# 3 Tests for Income and Appreciation Dependence

## 3.1 Data and Methodology

Under the assumption of a relatively informationally efficient stock market, in order to assess whether REITs can realize short-term property appreciation returns, I test whether returns in property appreciation are found in REIT returns, once income is accounted for. In other words, I test whether property appreciation provides useful incremental information content, beyond what can be attributed to interest-rate and stock-market related sources of variation, as well as direct-property income information.[17] Empirically, I estimate a set of linear factor models, with returns to a value-weighted portfolio of equity REITs as the dependent variable, and as independent variables a set of interest-rate and stock-market control variables, as well as income and appreciation returns to a large diversified portfolio of directly held properties.

As data for the REIT price series in this section, I use total returns for the National Association of Real Estate Investment Trusts' (NAREIT's) FTSE-NAREIT US Equity REITs Index. As mentioned previously, this series includes both share price changes and dividend returns of the index constituents. In using a REIT index as the dependent variable I proxy for a well-diversified REIT portfolio held by an investor.

The REIT boom of the 1990s substantially altered the REIT industry, increasing the level of institutional investor participation in the market (and therefore increasing investors' general understanding thereof), as

[16]The general notion that there should be efficiency gains from larger-size REITs is also consistent with the economies of scale discussion in studies such as Ambrose et al. (2005).

[17]The fact that REIT performance is strongly correlated with general stock market variables is an idea that is quite common in the REIT literature and has often been observed (Mengden and Hartzell (1986), Peterson and Hsieh (1997), Oppenheimer and Grissom (1998), Clayton and Mackinnon (2003), Chiang, Lee and Wisen (2005), to name a few), and REITs' interest rate dependence is also widely documented, as changes in interest rate proxy for changes in the discount factor of property cash flows. Furthermore, the relatively constant and high dividends streams which REITs produce, give these stocks a fixed-income-like behavior, further heightening interest rate dependence.

well as generally causing enormous growth in the size of the industry, through numerous IPOs. These changes suggest that we may see a difference in results before, during, and after the REIT boom. Ott, Riddiough and Yi (2005), for example, give strong evidence of structural breaks in the development of the REIT industry, finding significant differences in the nature of the performance of REITs before and after about 1992 or 1993. The boom of the 1990s is generally thought to have ended around 1999, from which point on, the industry exhibited more consolidation, as well as internal appreciation of already existing companies.[18] Therefore I define the following time-subperiod dummy variables:

$$boom = \begin{cases} 1 & \text{if } t \text{ lies between the third quarter of 1992 and the end of 1998} \\ 0 & \text{otherwise.} \end{cases} \tag{1}$$

$$new = \begin{cases} 1 & \text{if } t \text{ occurs after January 1, 1999} \\ 0 & \text{otherwise.} \end{cases} \tag{2}$$

I interact these dummy variables individually with all explanatory variables.[19] [20]

For direct market data I use the National Property Index (NPI) from the National Council of Real Estate Fiduciaries (NCREIF). This quarterly index is based on a database of institutionally-held non-agricultural investment-grade real estate comprising over 6100 properties nationwide, with a combined value of around $247 billion, as of the end of 2010. The index returns data is split into two components, income, and appreciation, both of which I use as data for the respective direct-market variable. Both NCREIF series are of quarterly frequency, and that is the frequency used throughout this study, over a 1978 to 2009 time window, unless otherwise stated.

The income return component is computed purely on the basis of the net operating income each property generates. The appreciation component is computed as the scaled difference between each property's appraisal-based market value at the beginning of a quarter and at the end. As is widely documented, this latter type of data series suffers from temporal lag bias, induced by *stale appraisals*, and appraisal *smoothing* or *anchoring*.[21]

To correct for these effects, I use the de-smoothing methodology of Cho, Kawaguchi and Shilling (2003). I choose this methodology, because it generates an autocorrelation structure for the series of appreciation

[18] The analysis of Ott et al. (2005) ends in 1999.

[19] A sensitivity analysis to the cutoffs used to define the *boom* and the *new* period reveals that the results are not dependent upon selection of these cutoff points within about two years.

[20] The general notion of time-varying exposure to various risk factors is also consistent with Chiang et al. (2005).

[21] See Clayton, Geltner and Hamilton (2001), for example.

returns that seems economically plausible. Earlier studies (such as, for example, Blundell and Ward (1987), or Fisher, Geltner and Webb (1994)), assume a true underlying price process that is weak-form efficient (i.e. has zero autocorrelation).[22] The procedure of Cho et al. (2003), in contrast allows for non-zero autocorrelation in returns. Figure 2, page 49 shows the autocorrelation structure of the de-smoothed appreciation series I obtain through this procedure. It exhibits positive autocorrelation (momentum) over short time horizons and some degree of negative autocorrelation (reversals) over a long horizon. Economically, one would expect an inefficient real estate market with slow information incorporation followed by overreaction to exhibit this type of autocorrelation structure. Given that this study tests an economic hypothesis primarily about time-series interrelationships of markets, it is economically important that the return series themselves have plausible time-series properties. The time-series properties which this de-smoothing methodology generates, make this an appealing procedure for adapting the appreciation series to proxy better for the variation in property appreciation returns, and for their true information content.

The Cho et al. (2003) model can be summarized in the following five equations (in their notation):

$$
\begin{aligned}
P_t^* &= b_1 P_{t-1}^* + b_2 P_{t-4}^* + w_0 P_t && (3)\\
r_t^* &= b_1 r_{t-1}^* + b_2 r_{t-4}^* + w_0 r_t && (4)\\
r_t &= \alpha + \rho r_{t-1} + \epsilon_t, &&\\
&\quad \text{with } E(\epsilon_t) = 0,\ \sigma^2(\epsilon) = \sigma^2,\ |\rho| < 1, \text{ and } \alpha \neq 0 && (5)\\
r_t^* - \rho r_{t-1}^* &= \alpha w_0 + b_1 \left( r_{t-1}^* - \rho r_{t-2}^* \right) + b_2 \left( r_{t-4}^* - \rho r_{t-5}^* \right) + \epsilon_t', &&\\
&\quad \text{where } \epsilon_t' = w_0 \epsilon_t && (6)\\
w_0 &= 1 - b_1 - b_2 && (7)
\end{aligned}
$$

where $P_t$ is the logarithm of the underlying (and unobservable) true property price index, $P_t^*$ is the logarithm of the smoothed property price index, $r_t = P_t - P_{t-1}$, and $r_t^* = P_t^* - P_{t-1}^*$. Equations 3 and 4 describe appraised values (returns) as a function of true values (returns) and 1- and 4-quarter lagged values (returns). Equation 5 describes the true property price process as long-term mean reverting, while equation 6 is a derivation of the previous two equations. The unsmoothed index return becomes

$$
r_t = \frac{r_t^* + b_1 r_{t-1}^* + b_2 r_{t-4}^*}{w_0} \qquad (8)
$$

[22]The transactions-based return series generated by the methodology of Fisher, Geltner and Pollakowski (2007), also exhibits an autocorrelation structure that seems economically unwarranted ex-ante.

Like Cho et al. (2003), I estimate equation 6 as follows: I insert a starting value of 0.5 for $\rho$ to estimate $b_1$ and $b_2$. These estimates are then inserted into a version of Equation 6, rearranged to yield an estimate for $\rho$, and this model is estimated. The new value for $\rho$ is reinserted into Equation 6 and so forth.[23] After fifty iterations of this procedure, consecutive estimates differ by less than the machine zero. Table 1, page 37, shows the results from the final iteration. The results qualitatively resemble those of Cho et al. (2003); besides this, no further inferences need to be drawn from these parameter values for the purposes of this study.

Conceptually, this series has now been brought from an appraisal-based level to a transactions level. In order to provide a valid comparison between the information content of the direct and the securitized property market, I further correct for the effects of *price discovery* between the two markets. This phenomenon, which, once again, is well-documented in the literature (Giliberto (1990), Myer and Webb (1993), Barkham and Geltner (1995), among others), consists of a time lag between securitized and unsecuritized real estate, with prices of the former being found to lead those of the latter by six months to two years, depending on the study.[24]

This means that, once a transaction is complete (and has appeared in the public record), the pricing information contained therein is already three to twelve months old, as price tends to be more or less locked in toward the beginning of a transaction.[25] Therefore, de-smoothed data still shows old pricing information. Since the objective of this study is to compare information content between the securitized and the direct market, and because money generally only changes hands once the transaction is official, I remedy this situation by treating a transaction in the real estate market as a forward contract, for which one only observes the forward price and not the spot price of the underlying. Assuming rational parties in the transaction and a no-arbitrage outcome, it is clear from general finance theory how a forward contract is priced in relation to the underlying spot price. It is thus possible to deduce the implied spot price from observing the forward price (and thus to deduce the pricing information at the time the price was set), by using normal forward-pricing relationships. Only this way will we have pricing information that is comparable to that we receive from the REIT market.

[23] A coarse grid search testing starting values of $\rho$ between 0.05 and 0.95 by increments of 0.05 always yields conversion to the same final parameter estimates, with differences of less than $10^{-8}$ after at most 100 iterations.

[24] These lags between the securitized market and the direct market are due to the transaction time of the direct market and therefore no arbitrage can be made here.

[25] See, for example Geltner and Miller (2001), or Crosby and McAllister (2005) for detailed outlines of the sales process of a property.

The well-known forward pricing relationship for a security without dividends is

$$F_t = S_0(1+i)^t \tag{9}$$

where $F_t$ is the forward price for a contract expiring at time $t$, $S_0$ is the spot price today of the underlying asset, and $i$ is the risk-free interest rate per period. Solving for $S_0$ we get:

$$S_0 = \frac{F_t}{(1+i)^t} \tag{10}$$

In this case I want to obtain quarterly returns data, rather than price data, so I let $R_0 = \ln(S_0) - \ln(S_{-1})$. Using quarterly annualized 6-month Treasury-note rate data, and a transaction time of three quarters yields:

$$\begin{aligned} R_0 \quad = \quad & \ln\left[\frac{F_3}{(1+i_1)^{1/4}(1+i_2)^{1/4}(1+i_3)^{1/4}}\right] \\ & - \ \ln\left[\frac{F_2}{(1+i_0)^{1/4}(1+i_1)^{1/4}(1+i_2)^{1/4}}\right] \end{aligned} \tag{11}$$

Reducing, gives

$$R_0 = \ln F_3 - \ln F_2 + \frac{\ln(1+i_0) - \ln(1+i_3)}{4} \tag{12}$$

or

$$R_0 = r_3 + \frac{\ln(1+i_0) - \ln(1+i_3)}{4} \tag{13}$$

where $r_t$ is the return on the reverse-engineered appraisal-based index, $t$ quarters from the quarter being observed. The amount of lead time to be used (three quarters) is found by maximizing the time-displaced cross correlation of this series with property income. Since the income series does not suffer from appraisal-related error (as there is no appraisal used in constructing it), or from transaction-time bias, this can be used to determine the optimal lead: by maximizing the time-displaced cross correlation with income, I search empirically at which lead value the income-related component of appreciation best reflects current market income and find the above lead time. Furthermore, three quarters is also well within the range of realistic transaction lengths of three to twelve months quoted in the literature and by property professionals.[26] I now have a direct-market appreciation series whose information content can be compared to that of REIT returns.

I now isolate the expectations component of appreciation, as this is the information which, I argue,

[26] For example, Geltner and Miller (2001), as well as various interviews conducted by the author.

is not reflected in REIT prices. As mentioned earlier, property prices consist of a component derived by perpetualizing income in addition to this expectations component, and the time-lead for this study's appreciation series has been chosen in order to maximize the income component of prices. Therefore, I construct an appreciation series which is orthogonal to income information – the latter being, by construction, featured rather prominently in any property price series – thus consisting of only this isolated expectations component, which, I argue, is missing from REIT returns. This represents the marginal information content of direct-property price appreciation.

Because I also want to assess the marginal information content of property income, after other return drivers have been accounted for, and collinearities exist between property income and those return drivers, I perform an additional orthogonalization, similar to Clayton and Mackinnon (2003). I estimate the following two models.

$$income_t = \gamma + \vec{\psi}' \vec{con}_t + \iota_t \tag{14}$$

$$apprec_t = \delta + \vec{\zeta}' \vec{con}_t + \zeta_2 \widehat{\iota_t} + \xi_t \tag{15}$$

In this notation, $income_t$ is the time series of direct-property income returns, $apprec_t$ is the time series of adjusted direct-property appreciation returns, and $\vec{con}_t$ is the vector of market and interest-rate control variables. I then define $inc_t = \widehat{\iota_t}$ and $app_t = \widehat{\xi_t}$. Thus $inc$ contains only information that cannot also be found in general trend, stock market factors, or interest rate factors, and $app$ only contains information that cannot also be found in trend, the stock market, interest rates, or property income. This isolates the pure expectation component of property price.[27]

For the set of stock market factors, I take quarterly total returns to the Center for Research into Securities Prices (CRSP) value-weighted market index, as well as quarterly returns to the Fama and French (1992) size and book-to-market factors. For the interest rate factor, I use changes in 3-year US Treasury note rates ($d3yrtr$), as well as changes in the shape of the term structure ($long/short$). This latter factor is computed as the yield on long-term Treasury bonds (30-year bonds where this data is available, and otherwise 20-year bonds) divided by the yield of short-term Treasury bills (3-months), minus one.

Table 2, page 38, shows summary statistics for the variables described above, as well as for the five control variables. The variable $tret$ (the unmodified total return to the NCREIF NPI) is not used in this study, but is shown only for informational purposes. The control variables are $vwretd$, the total return to

[27] These orthogonalizations also mirror an investor's information gathering process with costly information, from the cheapest, most public sources, to the most expensive private sources.

the value-weighted stock market index return from the Center for Research into Securities Prices (CRSP), the Fama and French (1993) size and book-to-market factors, *smb* and *hml*, the quarterly change in the medium-term (3-year) Treasury note rate, *d3yrtr*, and the variable *long/short* which indicates the shape of the term structure of interest rates.

The top panel of Table 2, shows means and standard deviations for all variables. Note that *apprec* has almost the same standard deviation as *tret*, while *income* has only one tenth that standard deviation; this, combined with the correlation between *tret* and *apprec* of greater than .99 (shown in the second panel of the table) explains why previous studies, which have largely compared REIT returns to total property returns have struggled to find a link between the two. The variable which is not reflected in REIT returns (property appreciation) constitutes nearly the entire variation of total property returns, and therefore trumps the small variation in the income component.

The increase in standard deviation between *apprec* and *ds.apprec* is the result of the de-smoothing procedure. Note also the increase in correlation between *income* and *apprec* versus *income* and *ds.apprec*: this demonstrates the price-discovery correction that was undertaken. In this way, the information content of the income and appreciation series should be matched. Finally of note is the extremely high standard deviation of the term-structure variable *long/short*. This is partly due to the fact that the data sample contains the 2008-2009 financial crisis, during which short-term interest rates went to almost zero, while long-term rates remained reasonably positive.

I estimate all models with base effects of dummy variables (in this section *boom* and *new*), as well as interaction effects of these two dummy variables with each of the six explanatory variables. In subsequent sections, I then also include dummy variables for different REIT portfolios with all single– and double-interaction effects. In many cases this would lead to estimating a model which is poorly identified, given that I use quarterly data, and in the case of UPREIT models I only have a time interval from 1994 to 2009 at my disposal.

I therefore undertake one more modification with respect to the five control variables. Instead of using these variables as they are, I take principal components of the space of these five controls and use the first two of these. Empirically, this should be justifiable, as the focus of this study is not the dependence of REIT returns on any of these variables, but rather the dependence of REIT returns on the marginal information content of direct-property factors. In order to achieve this, I need to include the systematic variation of this space of five controls, but I have no need to distinguish which component of this variation comes from which variable. The principal components approach allows me to do this with fewer right-hand variables in each model than the raw variables by themselves would give.

Table 3, page 39, shows rotations and variance statistics for the principal components of the control space. The bottom line of the table shows, for principal component $N$ the cumulative proportion of the vector space's variance that is captured by principal components 1 through $N$ combined. This reveals that with the first two principal components alone, I already capture 99.99% of the variance of the control space. I therefore decide to include the first two principal components of this space as controls in each model. Examining the rotations or factor loadings for these two components reveals that the first component is made up almost exclusively of term-structure variation.[28] The second component then contains in large fraction the value-weighted stock market portfolio, and also prominently features the two Fama-French factors.[29] While the choice to include two components seems to largely leave out that portion of variation that stems from changes in the Treasury rate, it should be noted that only the fifth principal component (which accounts for a truly negligible fraction of variation of the system) loads on this factor. Therefore, the unique variation that can be ascribed to this variable does not seem to be important for the overall variation contributed to the model by the control space. It is thus apparent that including the first two principal components of the control space is extremely helpful to me in this context, in that doing so still captures all important variation of this space (99.99% of it), while saving as many as fifteen explanatory variables when counting base effects and interaction effects in certain models. The procedures for orthogonalization outlined above still apply, of course, with $\vec{con}_t$ being constituted of the first two principal components of the control space.

The final model I estimate in this section, thus has as a dependent variable the FTSE-NAREIT US Equity REIT Index, and as independent variables two principal components of the control space, that component of direct-property income which is orthogonal to the controls, and that component of de-smoothed and price-discovery corrected property appreciation which is orthogonal to the control space and the income variable. In this way, I test the dependence of REIT returns on the marginal information content offered by both property-return components. All regressors are interacted with the time-period structural breaks *boom* and *new*.

## 3.2 Results

Table 4, page 40, reports the results from estimating the model described above. The first panel of Table 4 shows a positive coefficient of 8.1697, significant at the 5% level for *inc* (i.e. property income). This indicates

[28] Note that the loadings are standardized so that each set sums to 1.

[29] The signs of the loadings yield very little economic interpretation, as these principal components are only defined up to a rotation in space.

that a one-percentage-point change in property income, causes approximately an eight-percentage-point change in REIT prices. The size of this effect is mandated by the comparatively low standard deviation of property income. The same panel shows an insignificant coefficient for *app* (i.e. property appreciation). This result, therefore, supports my hypothesis, in that, according to this data, at least in the base period before 1992, REIT returns had a strong property income dependency and no property appreciation dependency. $PC1$ and $PC2$ have significant coefficients, the former positive and the latter negative. The base effect for the dummy *boom* is positively significant, while that for *new* is negatively significant, indicating higher-than-average returns during the 1992–1998 period and lower-than-average returns after 1998. This should be congruous with prior economic intuition.

The second panel of Table 4 shows a large positive significant coefficient for $inc \cdot boom$ and an insignificant coefficient for $inc \cdot new$. These are marginal effects of income during the two time periods and indicate an increased positive income effect during the 1992–1998 time period relative to the base effect in the first panel, and an effect that is unchanged from the base after 1998. Economically, this means that throughout all sub periods REITs show a strong association with property income. The F-statistics in the bottom panel of the table confirm this; these are statistics testing the hypothesis that the sums of base effects plus interaction effects are equal to zero, against the two-sided alternative. The advantage of these statistics is that they take into consideration any possible covariance effects at work between the coefficients. Simply adding base and marginal effects does not do this. These statistics show that both $inc + inc \cdot boom$ as well as $inc + inc \cdot new$ are statistically different from zero (one at the 0.1% level and one at the 5% level), confirming the hypothesis that REITs have a strong positive relation to property-income returns, throughout the sample. Given that the data has quarterly frequency this strongly supports my hypothesis that REIT returns have a strong positive relationship with property income, even in the short term.

The second panel of Table 4 shows a coefficient that is positive and significant at the 5% level for $app \cdot boom$ with an insignificant coefficient for $app \cdot new$. These indicate marginal effects over the insignificant base effect for *app* that are positively significant and insignificant, respectively. The F statistics for $app + app \cdot boom$ and $app + app \cdot new$ confirm the result of a weakly positive (10% significant) association of REIT returns with property appreciation during the 1992 to 1998 boom, with no statistical relationship thereafter. Recall further from the first panel that there was also no statistical relationship between these two variables before 1992. This result could be explained by the fact that during the property boom of the 1990s both securitized and unsecuritized property prices were monotonically increasing, and that this causes this positive association. It does seem to be the case overall that fundamentally such a relationship does not seem to exist, as it is not visible anywhere but in the strong up-market of the mid 1990s.

In general, these results are, to my knowledge, first in the literature to show a strong persistent positive dependence of REIT returns on any component of direct property returns, even in the short run, within the context of a linear factor model. It is simply the case that REITs only reflect property income returns, rather than overall property investment returns. These results, in conjunction with the standard deviations shown in Table 2 then present a possible explanation for the apparent lack of a short-term relationship between the securitized and the unsecuritized property market found in previous literature: there actually exists such a relationship, but only between REIT- and property income returns, and not between REIT- and property appreciation returns. Since the latter constitute over 90% of the variation of total property returns, previous literature, which did not examine the decomposition into income and appreciation, has not found any strong relationships in this respect. This would be because variation in the appreciation component overwhelms that in the income component when the two are added together, leaving only noise in the overall relationship with REITs when total returns are used as a factor. Based on these results, one should thus classify REITs as *property income* investment vehicles, rather than full *property* investment vehicles when assessing their role in a multi-asset portfolio.

# 4 The UPREIT Natural Laboratory

In the previous section, I show for the first time in the literature a short-term dependence between REIT returns and direct-market property returns, by subdividing direct-market returns into income and appreciation, and showing that REIT returns have a strong positive relationship with income. I also show that REIT returns do not reflect gains in short-term property appreciation in their underlying portfolio. I now argue that this phenomenon is due to the trading constraints which REITs face when trading in the direct property market (the *dealer rule*), and which they must fulfill in order to retain their tax-free status. To test this explanation, I use the UPREIT vehicle as a natural laboratory. Given that UPREITs, which acquire properties primarily through 1031 transactions can to a large extent avoid this restriction, the distinction between regular REITs and UPREITs presents an ideal setup for testing the effects of the trading restriction.[30] As is discussed in Han (2006), an UPREIT's structure could also destroy value in this respect as shareholders in the Operating Partnership have tax-timing incentives which may differ from the market-timing incentives of shareholders in the REIT. Generally it is the case that the REIT owns the majority of the Operating Part-

[30]See Sections 1 and 2 for further explanations about why trading constraints would cause this, and how the UPREIT structure helps a firm in this respect.

nership, which helps align the Partnership's actions with REIT shareholder incentives. However, implicitly, this test also weighs these two issues against each other and should help disentangle them. Empirically, I test whether returns to UPREITs show a different appreciation dependency on the marginal information content of property appreciation, than the returns to regular REITs.

## 4.1 Data and Methodology

The empirical setup I use to test the differing appreciation dependence between the returns of UPREITs and those of regular REITs is the following: I simultaneously estimate the effect of the same explanatory variables as in the previous section, on the returns to a value-weighted portfolio of UPREITs and a value-weighted portfolio of regular REITs. I then test whether the appreciation effects differ from each other. If UPREITs show an appreciation dependence while regular REITs do not, this supports my hypothesis. Conducting this estimation simultaneously on both portfolios allows for covariance effects between them.

The REIT returns data I use to form my portfolios which act as dependent variables consists of returns data for all firms classified as Equity REITs in the CRSP universe; all right-hand side data is the same as in the previous section. To identify whether firms are UPREITs or not, I use SNL Datasource, which publishes a firm-by-firm database, listing specifically, at the end of each year, whether a firm classifies itself as an UPREIT for that year or not, in its 10k form submissions. This database starts with the end of the year 1993, so, due to the nature of the portfolio sort, I start this analysis at the beginning of the year 1994, and end at the end of 2009.

For the lefthand-side variable I use returns to a portfolio consisting of all firms that are classified as UPREITs in SNL's dataset and another portfolio of all firms that are not classified as UPREITs. Each portfolio return series consists of value-weighted returns. I use a quarterly frequency, reconstituting portfolios annually according to firms' UPREIT status and value-weighting them quarterly. Specifically, at the beginning of quarter 1 of year $t$, I assign firms to an UPREIT or a regular-REIT portfolio, according to their declarations made at the end of year $t-1$. At the same time, I compute value weights based on the closing prices and shares outstanding of the previous trading day and weight firms accordingly. At the end of quarter 1, I record the value-weighted portfolio returns to the two portfolios, reweight the portfolios according to the closing prices the last trading day of quarter 1, and hold firms in this weighting until the end of quarter 2, and so forth. It is apparent how, by doing this, I simulate a strategy that is tradeable. Table 5, page 41, presents summary statistics for the numbers of firms per portfolio. Figure 3, page 50 presents relative market capitalizations of the two portfolios. These two illustrations make it clear that the UPREIT soon became the dominant form of REIT in this sample. However, it is also apparent that both the number of firms and the market capitalization of regular REITs is sufficient throughout this period to make this a meaningful natural laboratory setup.

I stack the value-weighted return series for the two portfolios ($vwret_{k,t}$) to make a panel. I define a dummy variable, $UPREIT$, as follows:

$$UPREIT = \begin{cases} 1 & \text{if } k \text{ is the portfolio of UPREITs} \\ 0 & \text{if } k \text{ is the portfolio of regular REITs} \end{cases} \tag{16}$$

This variable serves as the panel indicator. All other right-hand side variables vary only over time and not through the cross section (i.e. they are recycled). As before, I examine all base effects and all interaction effects with the dummy $UPREIT$. As before, I use the first two principal components of the five-variable control space. I also include the time-period structural break *new*, defined as before, as well as all of the interaction effects and multiple interaction effects associated with it. Note that the data for this test begins in 1994. Therefore the base period is analogous to the *boom* period in the previous section, while the *new* periods in both sections are analogous to each other.

## 4.2 Results and Implications

The results from the estimation of the model described above are presented in Table 6, page 42. The base effects in the first panel of Table 6 show a positive coefficient of 12.33 for *inc*, which is significant at the 0.1% level. The base effect for *app* is once again not distinguishable from zero. These results indicate that for the returns to regular REITs (i.e. non-UPREITs) over this time period there is a strong association with property-level income, but not with property-level appreciation, as was shown in the previous section.

The *upreit* dummy has a positive coefficient of approximately .1, which is significant at the 5% level. This indicates that the conditional mean return to UPREITs exceeds that of regular REITs. This means that the market perceives the UPREIT vehicle to be value-enhancing overall, which supports a hypothesis that the efficiency gains from the UPREIT structure outweigh possibly diverging incentives between Operating-Partnership shareholders and REIT shareholders and yields further justification for this natural laboratory.

The next panel of Table 6 shows single interaction effects. Of primary importance for this section is the coefficient for $app \cdot upreit$, which is positive, with a value of 1.74 and statistically significant at the 0.1% level. This indicates that UPREITs show a significantly greater dependence on direct-property appreciation returns than regular REITs, which show none. I once again report the F statistic testing whether the combined effect of $app + app \cdot upreit$ is equal to zero against the two-sided alternative, at the bottom of the table. The F value of 13.437 significant at the 0.1% level shows that this hypothesis can be strongly rejected. This means that UPREIT returns have a strong statistical dependence on the marginal information content of direct-property appreciation, while returns to regular REITs do not (as shown by the insignificant coefficient for *app* in the top panel). Given the fundamental difference between regular REITs and UPREITs in terms of the constraint which the *dealer rule* poses, with little other fundamental differences between the two types of entity, this result lends strong support to my hypothesis that the constraints imposed by the rule prevent REITs from realizing property appreciation returns. The marginal effect for $inc \cdot upreit$ is indistinguishable from zero, which with a significantly positive base effect indicates that UPREITs also retain the income dependence already shown by regular REITs. The F statistic for $inc + inc \cdot upreit$ of 7.014, significant at the 1% level confirms this picture. Therefore, the returns to UPREITs, which are in a better position to overcome the constraints of the *dealer rule*, show a dependency on both components of direct property return, while those to regular REITs only show income dependence.

The interaction effects and double-interaction effects with *new* reported in the second and third panels illustrate whether there are any changes to the relationships described here, between the REIT boom of the 1990s and the post-boom period. Of note in the second panel is the negative marginal effect shown by $inc \cdot new$. This indicates that regular REITs lose at least part of their income dependence after the boom of the 1990s. Given that regular REITs never show appreciation dependence (the coefficient for $app \cdot new$ is indistinguishable from zero), it can be said that in the *new* era, regular REITs only have a weak dependence on property fundamentals. The continued significance of $PC2$ implies that these firms still do follow stock market effects.

In the third panel of Table 6, one can observe a negatively significant marginal effect for $app \cdot upreit \cdot new$, significant at the 0.1% level. This implies that after the REIT boom of the 1990s, UPREITs lose at least part of their appreciation dependence. The question of whether they only lose part or all of this, can be answered by examining the F statistic for the total effect of $app + app \cdot upreit + app \cdot upreit \cdot new$. This has a value of 3.3721, which means that the null hypothesis of zero appreciation effect for UPREITs in the new period is still rejected, at least at the 10% level. The exact p-value (not reported in the table) is actually 6%, so the 5%-threshold is only missed by a slim margin. Recall that the *new* period includes the financial crisis of 2008-2009. The fact that despite this, there is this level of statistical significance in this effect leads to the conclusion that the economic relationship described here is strong. Given that regular REITs never show appreciation dependence, while UPREITs show this, even during the period which includes the 2008-2009 financial crisis, my hypothesis regarding the dealer rule is strongly supported. The insignificant marginal effect for $inc \cdot upreit \cdot new$, together with the high F statistic for the hypothesis regarding $inc + inc \cdot upreit + inc \cdot upreit \cdot new$ (16.167, significant at the 0.1% level) leads to the conclusion that income dependence is also retained by UPREITs after 1999. Therefore these vehicles offer returns related to both components of property investment, while regular REITs only offer income returns.

The picture presented throughout is thus that the returns to regular REITs are only income driven, while UPREIT returns also reflect the marginal effects of property appreciation returns caused purely by a change in cap rate. Thus, UPREITs much better reflect the values of their underlying property portfolios than regular REITs and one can consider UPREITs true *property* investment vehicles and not just *property income* vehicles, as I have classified the REIT asset class in the previous section. Since UPREITs are not as hindered by the trading constraints on their properties, they have a much better ability to time the market than regular REITs, and correspondingly their returns reflect appreciation cash flows not contained in income. Regular REITs are restricted in this ability, and correspondingly their returns do not reflect these cash flows. The trading restrictions hypothesis seems to be strongly supported by these results, outweighing any possible delay-of-taxes effects that would cause worse market timing in an UPREIT. The results of Mühlhofer (2009) lend further support to this hypothesis, in that this study shows that UPREITs trade properties significantly more actively than regular REITs and that, even in a rising market, when a firm would make gains on a property which then must be surrendered if a *prohibited transaction* is undertaken, UPREITs seem indifferent to the holding constraint, while regular REITs seem bound by it. These results further help in qualifying the role of REITs in a multi-asset portfolio. While REITs, in principle, only offer an investor exposure to property income returns, one need only invest in a portfolio of UPREITs in order to additionally gain exposure to the expectation component of property returns.

# 5 Robustness Check: Size Test

While the previous section presents evidence that the trading restrictions explanation is indeed the reason for the lack of dependency of REIT returns on property-market appreciation, the previous setup only allows a test for this phenomenon after 1994, since that is the start of the data set (and besides this, the UPREIT vehicle only started in 1992). Because of this, there may be a concern that, since any appreciation effects found so far are in the late part of the time window, these appreciation effects are due mainly to the improvement in the quality of NCREIF's data set over time, as a more accurate appreciation series might be more closely related to REIT returns than a noisier one. To rule out this possibility, I conduct another firm-characteristic test, based on size. The advantage with size is that observing this characteristic only requires CRSP data, which is available all the way back to 1978 where the NCREIF's direct property market data series starts. The rationale behind why large REITs should have a better ability to overcome at least the 10% part of the selling constraint is outlined in Section 2, page 8.

### 5.1 Data and Methodology

To test whether large REITs better realize appreciation returns than small REITs, I use the an analogous setup to that found in Section 4. However, instead of using returns to two portfolios sorted by UPREIT status as a dependent variable, I use returns to two size-sorted portfolios, one consisting of small firms and one consisting of large firms. Figure 4, page 51 shows the size distribution for firms in 1998. While this is only a snapshot, this type of distribution with a very large number of small firms and a small number of large firms is typical of the size distributions at any time during the 1978 to 2009 sample period. If one were to conduct a portfolio sort which allocates firms with below-median market capitalization to the *small* portfolio and firms with above-median market capitalization to the *large* portfolio, this would yield a *small* portfolio consisting of firms which are, by characteristic, small-capitalization firms and a *large* portfolio also made up primarily of relatively small firms with only a few large firms mixed in. Such portfolios would not allow me to truly isolate size characteristics. Thus, instead I conduct a portfolio sort which gives the *small* portfolio the lowest one third of total industry market capitalization and the *large* portfolio the upper two thirds of total industry market capitalization.[31] This sort ensures that the *small* portfolio contains firms that are truly small-capitalization firms and the *large* portfolio contains firms that are truly large-capitalization firms.

Specifically, I construct portfolios as follows. At the beginning of quarter $t$, I compute values for firm market capitalization ($cap$) and total industry market capitalization ($icap$) for the quarter, based on the closing prices and shares outstanding for each firm at the close of the last trading day of quarter $t-1$ as follows:

$$cap_{i,t} = prc_{i,t-1} \cdot shrout_{i,t-1} \tag{17}$$

$$icap_t = \sum_{i=1}^{N} cap_{i,t} \tag{18}$$

[31] A sort which gives each portfolio one half of industry capitalization would have yielded too few firms in the large portfolio in the early part of the sample, making it impossible to distinguish systematic risk factors from idiosyncratic ones for that portfolio.

where $prc_{i,t-1}$ is the share price of firm $i$ at the end of quarter $t-1$ and $shrout_{i,t-1}$ is the amount of shares outstanding for this firm at this time; $i \in [1, N]$ represents each Equity REIT that exists in the CRSP database on the last trading day of quarter $t-1$.

Based on these market capitalization figures, I form two size portfolios, such that the total capitalization of the *small* portfolio is one third of $icap_t$ and the capitalization of the *large* portfolio is the remaining two thirds. More precisely, at the end of each quarter, I select the firm in the market that has the smallest market capitalization and assign it to the *small* portfolio. Then I select the next larger firm and also assign it to this portfolio, and so forth, until the total market capitalization of the *small* portfolio just exceeds $\frac{icap_t}{3}$. The last marginal firm is then removed from the *small* portfolio and assigned to the *large* portfolio, together with all other remaining firms, so that the *large* portfolio has a capitalization just exceeding $\frac{2icap_t}{3}$. Table 7, page 43, shows the maximum, mean, and minimum number of firms in each portfolio. I then compute value-weighted returns for each portfolio, based on firm capitalizations at the end of the final trading day of the previous quarter,[32] and stack the return series $vwret_{k,t}$ with $k \in small \cup large$ and define a *large* dummy such that

$$large = \begin{cases} 1 & \text{if } k = large \\ 0 & \text{if } k = small \end{cases} \tag{19}$$

As before, I recycle all explanatory variables throughout the panel. I include, once again, the first two principal components of the control space, as well as the previously defined dummies *boom* and *new*. Once again, I include all interaction effects and double interaction effects.

## 5.2 Results and Implications

Table 8, page 44, shows results for the regression estimated in this section. The first two principal components of the control space, as well as all of their interaction and double-interaction effects were included in the model, but omitted from the table to save space. In the first panel of the table, the results fairly closely resemble those from the previous sections: *inc* has a positive coefficient of 24.06, significant at the 0.1% level, while *app* is insignificant. The results imply that small REITs before 1994 did not react to direct-property appreciation shocks that are not contained in income, implying that an investor misses out on short-term property appreciation returns from small firms, consistent with the hypotheses of the paper.

[32]Notice, once again that this portfolio sort represents a tradeable strategy, as market capitalizations and weights from the end of the previous quarter are used.

In the second panel, the coefficient for $app \cdot large$ is 0.95, with a t-statistic of 1.49. While this makes the marginal effect for appreciation dependence of large firms over that of small firms insignificant, the hypothesis test for the overall appreciation dependence of large firms ($H_0 : app + app \cdot large = 0$), reported at the bottom of the table does reject the hypothesis of no appreciation dependence for these firms with an F value of 5.44, at a 5% significance level.[33] This means that the returns to large REITs, in the period before 1994, did exhibit positive appreciation dependence. The somewhat weak statistical significance of this result is congruous with economic intuition, in that size only helps a REIT overcome the 10% portion of the trading constraint and does nothing to help with the minimum-holding period portion.

The third panel of the Table reveals negative and insignificant coefficients for $app \cdot large \cdot boom$ and $app \cdot large \cdot new$. The F statistics for the hypothesis tests for $app + app \cdot large + app \cdot large \cdot boom$ and $app + app \cdot large + app \cdot large \cdot new$ indeed reveal that the hypothesis of zero overall appreciation dependence by large firms in both time segments after 1994 cannot be rejected. This means that during the REIT boom of the 1990s and thereafter, large REITs as an asset class lost their original appreciation dependence. All marginal effects of income throughout the table are either insignificant or too small in magnitude to eliminate the base effect of a positive income dependence for any type of firm, at any time period. The hypothesis tests at the bottom of the table confirm this for large firms throughout the sample. Therefore, all firms do maintain a positive income dependence, even when split by size.

The lack of appreciation dependence of large firms after 1994 is actually very much supported by economic intuition, in that the UPREIT structure became available during this time. Therefore, firms that wanted to effectively pursue strategies involving active portfolio turnover would have resorted to this entity type, and would not have been forced to grow very large large in order to support such a business model, if they retained a small size to be more efficient for their particular purpose. In the next section, I present results from double-sorted portfolios by size and UPREIT status. These results will support this explanation, by showing that UPREIT status is the statistically distinguishing characteristic for appreciation dependence. Before the early 1990s, however, the UPREIT structure was not available, and these results show that during this time size helped REITs pursue strategies that allowed them to create appreciation returns for investors at least to some extent, consistent with the idea of size helping overcome part of the trading constraints. In any case, the purpose for this robustness test of showing that any appreciation dependence found in the latter part of the sample is not simply due to data improvements in NCREIF's appreciation series has been

[33] In this case, there are differing statistical inferences between a casual examination of the insignificant base effect plus the marginally insignificant marginal effect, versus the significant rejection in the formal test of the overall effect. This is due to the formal test's incorporating the covariance structure.

fulfilled, in that these results show appreciation dependence even in the early part of the sample, exactly among those firms where the trading-constraint hypothesis says we should find them.

# 6 Size and UPREIT Status

Given the results of the previous section, I now present a further robustness check to disentangle the effects of size and UPREIT status on the property-appreciation dependence of REIT returns. Given that size and UPREIT status are somewhat collinear, in that in the later part of the sample most large REITs also tend to be UPREITs, there could be a concern that UPREIT status simply proxies for a size effect, which in turn proxies for other attributes commonly associated with larger REITs. It therefore makes sense to disentangle these two characteristics and isolate the effect of UPREIT status when controlling for size. If this test determines that UPREIT status is truly the important characteristic even when controlling for size, this should yield strong credibility to the *dealer rule* hypothesis.

## 6.1 Data and Methodology

The empirical setup in this section is analogous to that employed for the UPREIT test and the size test. I construct value-weighted portfolios by firm characteristic and use these as a dependent variable to simultaneously estimate the usual factor model on the stacked returns of all portfolios. I then assess differences among portfolios through dummy variables and their interaction effects.

Specifically, I simply combine the portfolio assignments from the UPREIT section with the portfolio assignments from the size section. This means firms are sorted *unconditionally* on both size and UPREIT status. An unconditional sort is the correct methodology in this case, as I am interested in sorting according to absolute firm characteristics, rather than relative to a more narrowly defined peer group. Combining the two portfolio assignments I get four portfolios: one of *small* Regular REITs (non-UPREITs that are part of the bottom third of total industry capitalization); one of *large* Regular REITs (non-UPREITs that are part of the upper two thirds of total industry capitalization); one of *small* UPREITs (UPREITs that are part of the bottom third of total industry capitalization); and one of *large* UPREITs (UPREITs that are part of the upper two thirds of total industry capitalization). I use the two dummy variables *large* and *upreit* and their combinations, in a four-portfolio panel regression, in which the dependent variable consists of the stacked returns to the four double-sorted portfolios and the independent variables are recycled across portfolios. Since UPREIT status comes from SNL Financial, I am forced to use a 1994-2009 time window here again.

Given the large number of explanatory variables required for this test (two separate portfolio indicators and all interactions and double interactions of these), I omit the time-period structural break *new* in this setting, and refrain from estimating interaction effects of the two principal components of the control space. I do this, in order to retain a reasonably identified model matrix: it is the case that the only degrees of freedom in this setting come from the time series, since the cross-section is controlled for by dummy variables.

Table 9, page 45, shows the maximum, mean, and minimum number of firms in each portfolio, resulting from this sort. Of primary concern in this table is the portfolio *Regular REITs, large.* This portfolio contains a maximum number of two firms, and in many periods contains no firms at all. At such times, therefore, this produces no observations for the four-portfolio panel, making the panel unbalanced. From two or only one firms and very few time series observations, it is not be possible to test systematic effects in a meaningful way. I therefore refrain from discussing any economic inferences that would present themselves about large regular REITs; these results should simply be ignored. However, if I can attribute an UPREIT effect for both small and large UPREITs, the robustness check has served its purpose, in that this allows us to observe an UPREIT effect after controlling for size. The table shows that both these portfolios should have enough firms to do this.

## 6.2 Results and Implications

Table 10, page 46, shows the results from the simultaneous test of size and UPREIT status on the direct-property appreciation dependency of REIT returns. The first panel of the table shows an insignificant effect for *app*, implying that small non-UPREITs do not reflect short-term property appreciation dependence, consistent with the trading restrictions hypothesis. The second panel of the table shows single-level marginal effects of size and UPREIT status. As mentioned above, there are very few large firms that are not UPREITs (even none at many dates), and so the marginal effects with *large* only, should be ignored due to low power. The marginal effect of $app \cdot upreit$ is not significantly different from zero. This would imply that small UPREITs do not differ significantly from small non-UPREITs in their appreciation dependence. However, the hypothesis test for $app + app \cdot upreit$ at the bottom of the table does reject the hypothesis of zero overall appreciation effect for the portfolio of small UPREITs at the 5% level (with an F statistic of 5.22). This means that once the covariance structure between the coefficient estimates is taken into account, the overall effect of direct-property appreciation on small UPREITs is significantly positive. This is the most important result for this robustness test, as it shows that UPREIT status does not proxy for size or size-related effects: even for the returns of small firms that are UPREITs, I find a significantly positive dependence on property appreciation returns. For small firms that are not UPREITs, there is no such dependence. This strongly supports my trading-restrictions hypothesis.

The third panel of Table 10 shows a positive marginal effect for $app \cdot large \cdot upreit$, significant at the 10% level.[34] The hypothesis test for $app + app \cdot upreit + app \cdot large \cdot upreit$ rejects the null of zero cumulative effect at a 1% significance level (with an F statistic of 7.21), implying that the returns to large UPREITs also show a positive dependence on property appreciation returns, just like those to small UPREITs, or more so. We therefore have a picture in which returns to small regular REITs do not show appreciation dependence, while those to both small and large UPREITs do.[35] These results thus show that UPREIT status, rather than size, is the determining factor for whether or not REIT returns show dependence on property appreciation returns, even when controlling for both types of characteristics. Given that UPREIT status is by far more effective in allowing an active portfolio trading strategy with respect to the *dealer rule*, this supports my trading-restrictions hypothesis.

## 7 REOCs

I present one final robustness check using Real Estate Operating Companies (REOCs). REOCs are companies that operate commercial properties in a similar way to REITs, but whose business model does not allow them to qualify for tax-free REIT status. This means that these firms do not need to follow any of the criteria necessary to qualify as a REIT, including the *dealer rule*. For the purposes of this study, I therefore have another type of property firm to examine, which is publicly traded, and which is not constrained in trading its underlying property portfolio. A finding that the returns to this type of firm are driven by property appreciation returns would lend even stronger credibility to the trading-restrictions hypothesis.

In order to test the appreciation dependence of REOCs, I use the same empirical model as the base specification in Section 3, except with a value-weighted portfolio of REOCs as the dependent variable. This return series is taken from total returns to SNL's REOC indices. SNL has two REOC index series, one for firms that are hotels and one for all other firms. I take an equal-weighted average of the two series, and thereby generate the same index as the REOC factor used in Hartzell, Mühlhofer and Titman (2010). The time period I use for this test is from the beginning of 1995 until the end of 2005. I therefore use only the time-period structural break *new*; the base period in the model corresponds to the *boom* period in Section 3. As before, I use the first two principal components of the five-variable control space.

[34] The t-statistic is 1.97, so this is actually very close to the 5% significance level.
[35] The results for large regular REITs should be treated as inconclusive.

Table 11, page 47, shows the results from this test. The first panel of the table reveals that in terms of base effects REOC returns show a strong positive dependence on property income, with a coefficient of 53.70, significant at the 5% level. More importantly however, REOC returns show a strong positive dependence on property appreciation returns, with a coefficient of 4.82, significant at the 0.1% level.

The second panel of the table shows a negative significant marginal effect for $inc \cdot new$, and the hypothesis test at the bottom of the table for $inc + inc \cdot new$ fails to reject the hypothesis of zero cumulative income effect for REOCs after 1999. This means that after the boom of the 1990s, REOCs lost their ability to produce property income returns for investors. With appreciation, however, the table shows an insignificant marginal effect for $app \cdot new$ over a positive base effect. The hypothesis test at the bottom of the table confirms this result, rejecting the hypothesis for $app + app \cdot new$ of zero effect at the 5% level. This means that during the entire sample, REOC returns show a strong positive dependence on property appreciation returns. This positive dependence of REOC returns on property appreciation lends strong support to my trading-restrictions hypothesis. The results show that a similar vehicle as REITs, but without trading constraints, yields property-appreciation returns.

## 8 Conclusion

This study addresses the short-term disparity between REIT returns and direct property returns, and argues that, due to the selling constraints which REITs face in the direct property market because of the *dealer rule*, a REIT is primarily a *property income* vehicle rather than a full *property investment* vehicle. The evidence presented here supports this hypothesis, showing that REIT returns do not show a dependence on the marginal information content of property appreciation returns that are not contained in income, from 1978 until 2009. I am thus the first to show that, even in the short run, in a linear factor model, REIT returns have a strong property dependence, albeit only a dependence on the income component. This is a solution to the long-standing puzzle in the literature concerning this disparity.

I then use a natural-laboratory approach to present evidence for the trading constraints explanation, by examining whether returns to UPREITs better reflect direct property appreciation returns than returns to regular REITs and strongly find that this is so. Since UPREITs differ systematically from regular REITs mainly in that their organizational structure makes them able to overcome the minimum holding period, I find my hypothesis strongly supported. The results of Mühlhofer (2009) showing that UPREITs tend to trade their portfolio more actively and tend to be less affected by the four-year constraint are in line with these findings and lend additional support to the trading-restrictions explanation.

I perform a robustness check to verify that, since any appreciation effects previously identified occur starting in the 1990s, these are genuine, and not due only to the fact that the quality of the NCREIF appreciation series improved at that time. I find that the returns to large REITs better reflect property appreciation returns than those to small REITs in the early part of my sample, all the way back to 1978. I argue that large REITs are less affected by the second half of the trading constraint (the 10% constraint) than small REITs and that therefore this result supports the trading explanation as opposed to the data-quality idea. I perform a further robustness test in which I simultaneously test for the effects of size and UPREIT status, which reveals that UPREIT status is the determining factor for appreciation dependence, when controlling for size. A further robustness test of the appreciation dependence of Real Estate Operating Companies (REOCs) which are not REITs and therefore not subject to the *dealer rule* shows that these companies also show property appreciation dependence. This lends further strong support to my trading-restrictions hypothesis.

This study shows, contrary to previous literature, that there does exist a strong short-run dependence between the direct- and the securitized property market, in that REIT returns are strongly dependent on direct-property income. As an implication, however, these results suggest that it is fallacious to treat REITs as a direct substitute for property in a multiple asset portfolio, as is often done, as, generally, REITs will only expose an investor to property income cash flows and not appreciation cash flows not contained in income. The results suggest further that UPREITs are investment vehicles that are superior to regular REITs in providing a liquid total-property investment vehicle, as opposed to just an income vehicle, as these entities manage to carry both property return components through to the investor, due to the fact that they can dispose of properties to time markets.

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Table 1: Regression results: Final iteration, iterative estimation of $r_t^* - \rho r_{t-1}^* = \alpha w_0 + b_1 \left(r_{t-1}^* - \rho r_{t-2}^*\right) + b_2 \left(r_{t-4}^* - \rho r_{t-5}^*\right) + \epsilon_t'$

This table presents parameter estimates used to construct a de-smoothed direct-property appreciation series, according to the methodology of Cho, Kawaguchi and Shilling (2003). I estimate these parameters through an iterative estimation procedure which yields, in each iteration, first an estimate for $b_1$ and $b_2$ and then for $\rho$. This table presents the final iteration. The regressions statistics with subscripts $b1, b2$ are for the final equation that estimates $b1$ and $b2$, while those with subscript $\rho$ are for the equation that estimates $\rho$.

| | Estimate | t-statistic |
|---|---|---|
| $\alpha w_0$ | 0.0004 | 0.31 |
| $b_1$ | $-0.1230$ | $-1.46$ |
| $b_2$ | 0.3781 | $4.53^{***}$ |
| $\rho$ | 0.8090 | $14.8^{***}$ |
| $N_{b1,b2} = 125$ | $\overline{R^2}_{b1,b2} = 0.1597$ | $F_{b1,b2} = 12.787$ |
| $N_\rho = 125$ | $\overline{R^2}_\rho = 0.6376$ | $F_\rho = 219.169$ |

$^*$: significant at the 5% level. $^{**}$: significant at the 1% level. $^{***}$: significant at the 0.1% level.

Table 2: Summary Statistics

This table shows means, standard deviations, and correlations for the variables used in this study. *NAREIT* is the FTSE-NAREIT US Equity REIT Index; *tret*, *income*, and *apprec* are total return, income return, and apprecitaion return to the NCREIF national property index (NPI); *ds.apprec* is the de-smoothed and price-discovery adjusted *apprec*; *vwretd* is total returns to the CRSP value-weighted stock market index; *smb* and *hml* are returns to the Fama and French (1993) size- and book-to-market factors; *d3yrtr* is changes in the three-year Treasury rate; *long/short* is a variable describing the shape of the term structure of interest rates, defined as the ratio of the long-term Treasury bond rate over the short-term Treasury bill rate minus one. All variables are of quarterly frequency, with observations from 1978 to the end of 2009.

| | Mean | Standard Deviation |
|---|---|---|
| NAREIT | 0.03402 | 0.091665 |
| tret | 0.02149 | 0.022841 |
| income | 0.01872 | 0.002524 |
| apprec | 0.00277 | 0.022124 |
| ds.apprec | 0.00224 | 0.027486 |
| vwretd | 0.03098 | 0.086325 |
| smb | 0.00846 | 0.053146 |
| hml | 0.00753 | 0.071718 |
| d3yrtr | −0.00002 | 0.006689 |
| long/short | 2.96993 | 7.714955 |

**Correlations**

| | NAREIT | tret | income | apprec | ds.apprec | vwretd | smb | hml | d3yrtr | long/short |
|---|---|---|---|---|---|---|---|---|---|---|
| NAREIT | 1.0000 | | | | | | | | | |
| tret | 0.1715 | 1.0000 | | | | | | | | |
| income | 0.1688 | 0.3241 | 1.0000 | | | | | | | |
| apprec | 0.1575 | 0.9941 | 0.2198 | 1.0000 | | | | | | |
| ds.apprec | 0.2933 | 0.3835 | 0.4366 | 0.3455 | 1.0000 | | | | | |
| vwretd | 0.6218 | 0.0889 | 0.1479 | 0.0748 | 0.2856 | 1.0000 | | | | |
| smb | 0.4661 | −0.1155 | 0.0922 | −0.1297 | 0.0607 | 0.4838 | 1.0000 | | | |
| hml | 0.3092 | −0.0165 | 0.0607 | −0.0240 | 0.1431 | −0.2228 | −0.0329 | 1.0000 | | |
| d3yrtr | 0.0243 | 0.0269 | 0.0397 | 0.0232 | −0.0784 | −0.0874 | −0.0394 | 0.1485 | 1.0000 | |
| long/short | −0.0102 | −0.6579 | −0.3179 | −0.6420 | −0.0234 | −0.0430 | 0.1466 | 0.1457 | −0.0954 | 1.0000 |

Table 3: Rotations and Variance Statistics for Principal Components of the Control Space

The table lists statistics on the principal components of the control space. The control variables are the total return to the CRSP value-weighted stock market index (*vwretd*), changes in the three-year Treasury rate (*d3yrtr*), the Fama and French (1993) book-to-market and size factor returns (*hml* and *smb*), and a variable describing the shape of the term structure of interest rates (*long*/*short*), defined as the ratio of the long-term Treasury bond rate over the short-term Treasury bond rate. The first panel lists factor loadings (rotations), and the second panel lists statistics on the variance explained by each principal component.

| | PC1 | PC2 | PC3 | PC4 | PC5 |
|---|---|---|---|---|---|
| vwretd | 0.000022 | −0.881103 | 0.269181 | 0.388828 | 0.003437 |
| d3yrtr | −0.000042 | 0.008295 | 0.010449 | 0.002725 | 0.999907 |
| hml | 0.000410 | 0.346533 | 0.926873 | 0.143711 | −0.012952 |
| smb | 0.000219 | −0.321720 | 0.261416 | −0.910029 | 0.002417 |
| long/short | 1.000000 | −0.000052 | −0.000443 | 0.000132 | 0.000047 |
| Standard deviation | 7.7150 | 0.0937 | 0.0686 | 0.0438 | 0.0066 |
| Proportion of Variance | 0.9997 | 0.0001 | 0.0001 | 0.0000 | 0.0000 |
| Cumulative Proportion | 0.9997 | 0.9999 | 1.0000 | 1.0000 | 1.0000 |

Table 4: Regression Results: REIT Income/Appreciation Dependency, with Time-Period Structural Breaks. Dependent variable: Total returns to the FTSE-NAREIT US Equity REIT index. This table presents results, testing the dependency of REIT returns on income- and appreciation-returns from a direct property portfolio. The direct-property appreciation series ($app$) has been adjusted to correct for temporal lag bias and for price discovery effects. The income series ($inc$) is the de-trended component of property income orthogonal to the control variables, and the appreciation series is de-trended and orthogonalized with the control variables and with respect to ($inc$), to isolate the expectations-based component of property returns. $PC1$ and $PC2$ are the first and second principal components, respectively, of the control space; $boom$ is a dummy variable equal to one for observations starting the second quarter of 1992 and ending the final quarter of 1998 and zero otherwise; $new$ is a dummy variable equal to one for observations starting the first quarter of 1999, and zero otherwise. At the bottom of the table, I present F-tests for various hypotheses concerning interaction effects, against the respective two-sided alternative. All parameter estimates are computed using two-step feasible GLS, to correct for Heteroskedasticity.

| | Estimate | t-statistic |
|---|---|---|
| $(Intercept)$ | 0.0998 | $6.23^{***}$ |
| $PC1$ | 0.0367 | $4.23^{***}$ |
| $PC2$ | $-0.5823$ | $-10.47^{***}$ |
| $inc$ | 8.1697 | $2.43^{*}$ |
| $app$ | 0.1988 | 1.51 |
| $boom$ | 0.0513 | $2.83^{**}$ |
| $new$ | $-0.0559$ | $-3.28^{**}$ |
| $PC1 \cdot boom$ | 0.1288 | $4.43^{***}$ |
| $PC2 \cdot boom$ | 0.3041 | $3.37^{**}$ |
| $PC1 \cdot new$ | $-0.0420$ | $-3.84^{***}$ |
| $PC2 \cdot new$ | 0.3607 | $2.89^{**}$ |
| $inc \cdot boom$ | 49.7680 | $4.19^{***}$ |
| $inc \cdot new$ | 0.1036 | 0.02 |
| $app \cdot boom$ | 1.2341 | $2.03^{*}$ |
| $app \cdot new$ | 0.4728 | 1.49 |
| $N = 126$ | $\overline{R^2} = 0.6877$ | $F = 20.662$ |
| $H_0 : inc + inc \cdot boom = 0$ | $F = 11.952^{***}$ | |
| $H_0 : inc + inc \cdot new = 0$ | $F = 5.3509^{*}$ | |
| $H_0 : app + app \cdot boom = 0$ | $F = 3.4434^{\circ}$ | |
| $H_0 : app + app \cdot new = 0$ | $F = 2.485$ | |

$^{\circ}$ : $p < 10\%$. $^{*}$: $p < 5\%$ $^{**}$: $p < 1\%$. $^{***}$ : $p < 0.1\%$

Table 5: Number of Firms per UPREIT Portfolio

This table shows the maximum, mean, and minimum number of firms assigned to the Regular REIT and the UPREIT portfolio. Firms are reassigned annually, according to their declarations on 10k form submissions, compiled by SNL. Value-weighted returns to these portfolios are then computed.

| Portfolio | Maximum | Mean | Minimum |
|---|---|---|---|
| Regular REITs | 76 | 43 | 26 |
| UPREITs | 124 | 102 | 52 |

Table 6: Regression Results for UPREIT Test.

Dependent Variable: Value-Weighted Portfolio Return ($vwret_{k,t}$). This table presents regression results giving evidence for the trading-restrictions hypothesis by testing whether the returns to regular REITs and UPREITs, which are differently affected by the constraints, differ in their appreciation dependency. The direct-property appreciation series ($app$) has been adjusted to correct for temporal lag bias and for price discovery effects. The income series ($inc$) is the de-trended component of property income orthogonal to the control variables, and the appreciation series is de-trended and orthogonalized with the control variables and with respect to ($inc$), to isolate the expectations-based component of property returns. $PC1$ and $PC2$ are the first and second principal components, respectively, of the control space; $upreit$ is a dummy variable equal to one for the portfolio of UPREITs and zero for the portfolio of regular REITs; $new$ is a dummy variable equal to one for observations starting the first quarter of 1999, and zero otherwise. At the bottom of the table, I present F-tests for various hypotheses concerning interaction effects, against the respective two-sided alternative. All parameter estimates are computed using two-step feasible GLS, to correct for Heteroskedasticity and to account for the parallel heteroskedasticity structure between the two portfolios.

| | Estimate | t-statistic |
|---|---|---|
| $(Intercept)$ | 0.0220 | 0.44 |
| $PC1$ | $-0.0137$ | $-1.22$ |
| $PC2$ | 0.0885 | $4.17^{***}$ |
| $inc$ | 12.3275 | $3.85^{***}$ |
| $app$ | 0.0944 | 0.75 |
| $upreit$ | 0.0969 | $2.22^{*}$ |
| $new$ | $-0.0133$ | $-0.35$ |
| $upreit \cdot new$ | $-0.0578$ | $-1.3$ |
| $PC1 \cdot upreit$ | $-0.0312$ | $-1.61$ |
| $PC2 \cdot upreit$ | 0.4568 | $6.75^{***}$ |
| $inc \cdot upreit$ | 8.8185 | 1.25 |
| $app \cdot upreit$ | 1.7432 | $4.28^{***}$ |
| $PC1 \cdot new$ | 0.0122 | 1.09 |
| $PC2 \cdot new$ | $-0.0618$ | $-2.1^{*}$ |
| $inc \cdot new$ | $-11.7262$ | $-3.58^{***}$ |
| $app \cdot new$ | $-0.0488$ | $-0.38$ |
| $PC1 \cdot upreit \cdot new$ | 0.0292 | 1.5 |
| $PC2 \cdot upreit \cdot new$ | $-0.2062$ | $-1.58$ |
| $inc \cdot upreit \cdot new$ | $-3.9320$ | $-0.51$ |
| $app \cdot upreit \cdot new$ | $-1.4761$ | $-3.39^{***}$ |
| $N = 134$ | $\overline{R^2} = 0.3309$ | $F = 4.462$ |
| $H_0 : inc + inc \cdot upreit = 0$ | $F = 7.014^{**}$ | |
| $H_0 : inc + inc \cdot upreit + inc \cdot upreit \cdot new = 0$ | $F = 16.167^{***}$ | |
| $H_0 : app + app \cdot upreit = 0$ | $F = 13.437^{***}$ | |
| $H_0 : app + app \cdot upreit + app \cdot upreit \cdot new = 0$ | $F = 3.3721^{\circ}$ | |

$^{\circ}$ : $p < 10\%$. $^{*}$: $p < 5\%$ $^{**}$: $p < 1\%$. $^{***}$ : $p < 0.1\%$

Table 7: Number of Firms per Size Portfolio

This table shows the maximum, mean, and minimum numbers of firms assigned to the *small* and the *large* size portfolio. Portfolios are reconstituted and value-weighted at the beginning of each quarter, based on the closing prices and shares outstanding of the previous trading day. Firms are assigned from smallest to largest, so that the *small* portfolio has a total market capitalization of 1/3 of the industry's total capitalization and the *large* portfolio 2/3. The marginal firm which crosses the break is assigned to the *large* portfolio.

| Portfolio | Maximum | Mean | Minimum |
|---|---|---|---|
| *small* | 233 | 166.8 | 93 |
| *large* | 19 | 9.5 | 3 |

Table 8: Regression Results for Size Test.

Dependent Variable: Value-Weighted Portfolio Return ($vwret_{t,k}$). This table shows regression results for a size test, as a robustness test to the results of the previous sections, as large REITs can overcome at least parts of the selling constraint. The direct-property appreciation series ($app$) has been adjusted to correct for temporal lag bias and for price discovery effects. The income series ($inc$) is the de-trended component of property income orthogonal to the control variables, and the appreciation series is de-trended and orthogonalized with the control variables and with respect to ($inc$), to isolate the expectations-based component of property returns. $large$ is a dummy variable equal to one for the portfolio of large firms (those which make up the top 2/3 of market capitalization) and zero for the portfolio of small firms; $new$ is a dummy variable equal to one for observations starting the first quarter of 1999, and zero otherwise. At the bottom of the table, I present F-tests for various hypotheses concerning interaction effects, against the respective two-sided alternative. As in previous models, two principal components of the control space, as well as interactions of these are included, but these results are omitted from the table to improve readability. The parameter estimates are computed using two-step feasible GLS, to correct for Heteroskedasticity and to account for the parallel heteroskedasticity structure between the two portfolios.

| | Estimates | t-statistic |
|---|---|---|
| $(Intercept)$ | 0.0442 | 1.95° |
| $inc$ | 24.0633 | 6.02*** |
| $app$ | 0.1940 | 0.45 |
| $large$ | −0.0075 | −0.76 |
| $boom$ | 0.0507 | 1.64 |
| $new$ | −0.0014 | −0.05 |
| $inc \cdot large$ | −5.1651 | −0.83 |
| $app \cdot large$ | 0.9484 | 1.49 |
| $inc \cdot boom$ | 23.5970 | 1.47 |
| $inc \cdot new$ | −12.2537 | −2.11* |
| $app \cdot boom$ | 1.0606 | 1.05 |
| $app \cdot new$ | 0.1565 | 0.28 |
| $large \cdot boom$ | −0.0099 | −0.56 |
| $large \cdot new$ | 0.0037 | 0.18 |
| $inc \cdot large \cdot boom$ | 28.1773 | 1.18 |
| $inc \cdot large \cdot new$ | 0.0762 | 0.01 |
| $app \cdot large \cdot boom$ | −0.6251 | −0.37 |
| $app \cdot large \cdot new$ | −0.5244 | −0.61 |
| $N = 236$ | $\overline{R^2} = 0.5934$ | $F = 15.913$ |
| $H_0 : inc + inc \cdot large = 0$ | $F = 15.458^{***}$ | |
| $H_0 : inc + inc \cdot large + inc \cdot large \cdot boom = 0$ | $F = 4.0175^{*}$ | |
| $H_0 : inc + inc \cdot large + inc \cdot large \cdot new = 0$ | $F = 5.796^{*}$ | |
| $H_0 : app + app \cdot large = 0$ | $F = 5.4365^{*}$ | |
| $H_0 : app + app \cdot large + app \cdot large \cdot boom = 0$ | $F = 0.1025$ | |
| $H_0 : app + app \cdot large + app \cdot large \cdot new = 0$ | $F = 0.7326$ | |

$^{\circ}$ : $p < 10\%$. *: $p < 5\%$ **: $p < 1\%$. *** : $p < 0.1\%$

Table 9: Number of Firms per Double-Sorted Portfolio (UPREIT and Size)

This table shows the maximum, mean, and minimum numbers of firms assigned to portfolios after an unconditional double-sort by size and UPREIT status. Portfolios are reconstituted and value-weighted at the beginning of each quarter, based on the closing prices and shares outstanding of the previous trading day. Firms are assigned based on UPREIT status, as well as by size from smallest to largest, so that the *small* portfolio has a total market capitalization of 1/3 of the industry's total capitalization and the *large* portfolio 2/3. The marginal firm which crosses the break is assigned to the *large* portfolio.

| Portfolio | Maximum | Mean | Minimum |
|---|---|---|---|
| Regular REITs, *small* | 75 | 42.75 | 26 |
| Regular REITs, *large* | 2 | 0.75 | 0 |
| UPREITs, *small* | 113 | 91.02 | 35 |
| UPREITs, *large* | 14 | 9.33 | 3 |

Table 10: Regression Results for Combined Size-UPREIT Test.

Dependent Variable: Value-Weighted Portfolio Returns ($vwret_{t,k}$) to unconditionally double-sorted portfolios by size and UPREIT status. This table shows regression results for a model presenting a combined size-UPREIT test, to disentangle these two potentially collinear effects. The direct-property appreciation series ($app$) has been adjusted to correct for temporal lag bias and for price discovery effects. The income series ($inc$) is the de-trended component of property income orthogonal to the control variables, and the appreciation series is de-trended and orthogonalized with the control variables and with respect to ($inc$), to isolate the expectations-based component of property returns. $PC1$ and $PC2$ are the first and second principal components, respectively, of the control space; $large$ is a dummy variable equal to one for the portfolio of large firms (those which make up the top 2/3 of market capitalization) and zero for the portfolio of small firms; $upreit$ is a dummy variable equal to one for the portfolio of UPREITs and zero for the portfolio of regular REITs. At the bottom of the table, I present F-tests for various hypotheses concerning interaction effects, against the respective two-sided alternative. The principal components are not interacted with any dummy variables in this model, to avoid a poorly identified model matrix. The parameter estimates are computed using two-step feasible GLS, to correct for Heteroskedasticity and to account for the parallel heteroskedasticity structure between the four portfolios.

| | Estimates | t-statistic |
|---|---|---|
| $(Intercept)$ | 0.0402 | $4.22^{***}$ |
| $PC1$ | 0.0050 | 1.6 |
| $PC2$ | $-0.3450$ | $-6.68^{***}$ |
| $inc$ | 3.2090 | 0.97 |
| $app$ | 0.3424 | 0.94 |
| $large$ | $-0.0991$ | $-1.81^{\circ}$ |
| $upreit$ | $-0.0094$ | $-0.88$ |
| $inc \cdot large$ | 38.8482 | $1.72^{\circ}$ |
| $app \cdot large$ | $-2.6415$ | $-2.18^{*}$ |
| $inc \cdot upreit$ | 5.2098 | 1.39 |
| $app \cdot upreit$ | 0.4969 | 0.97 |
| $large \cdot upreit$ | 0.1089 | $1.93^{\circ}$ |
| $inc \cdot large \cdot upreit$ | $-42.5978$ | $-1.84^{\circ}$ |
| $app \cdot large \cdot upreit$ | 2.7060 | $1.97^{\circ}$ |
| $N = 201$ | $\overline{R^2} = 0.3557$ | $F = 9.495$ |
| $H_0 : inc + inc \cdot upreit = 0$ | $F = 21.486^{***}$ | |
| $H_0 : inc + inc \cdot upreit + inc \cdot large \cdot upreit = 0$ | $F = 2.1927$ | |
| $H_0 : app + app \cdot upreit = 0$ | $F = 5.2166^{*}$ | |
| $H_0 : app + app \cdot upreit + app \cdot large \cdot upreit = 0$ | $F = 7.2096^{**}$ | |

$^{\circ}$ : $p < 10\%$. $^{*}$: $p < 5\%$ $^{**}$: $p < 1\%$. $^{***}$ : $p < 0.1\%$

Table 11: Regression Results for REOC Test.

Dependent variable: $vwret_{reoc,t}$ the value-weighted return to a portfolio of REOCs. This table presents results, testing the dependency of REOCs returns on income- and appreciation-returns from a direct property portfolio, given that they are not affected by any holding constraints. The direct-property appreciation series ($app$) has been adjusted to correct for temporal lag bias and for price discovery effects. The income series ($inc$) is the de-trended component of property income orthogonal to the control variables, and the appreciation series is de-trended and orthogonalized with the control variables and with respect to ($inc$), to isolate the expectations-based component of property returns. $PC1$ and $PC2$ are the first and second principal components, respectively, of the control space; $new$ is a dummy variable equal to one for observations starting the first quarter of 1999, and zero otherwise. At the bottom of the table, I present F-tests for various hypotheses concerning interaction effects, against the respective two-sided alternative. All parameter estimates are computed using two-step feasible GLS, to correct for Heteroskedasticity.

| | Estimates | t-statistic |
|---|---|---|
| $(Intercept)$ | 0.0366 | $2.69^{*}$ |
| $PC1$ | $-0.1889$ | $-0.74$ |
| $PC2$ | 0.5443 | $4.48^{***}$ |
| $inc$ | 53.7018 | $2.41^{*}$ |
| $app$ | 4.8222 | $3.77^{***}$ |
| $new$ | 0.0221 | 1.35 |
| $PC1 \cdot new$ | 0.1115 | 0.43 |
| $PC2 \cdot new$ | $-0.4130$ | $-2.93^{**}$ |
| $inc \cdot new$ | $-62.3459$ | $-2.6^{*}$ |
| $app \cdot new$ | $-1.2380$ | $-0.58$ |
| $N = 41$ | $\overline{R^2} = 0.512$ | $F = 5.663$ |
| $H_0 : inc + inc \cdot new = 0$ | $F = 2.2481$ | |
| $H_0 : app + app \cdot new = 0$ | $F = 4.2949^{*}$ | |

$^{\circ}$ : $p < 10\%$. $^{*}$: $p < 5\%$ $^{**}$: $p < 1\%$. $^{***}$ : $p < 0.1\%$

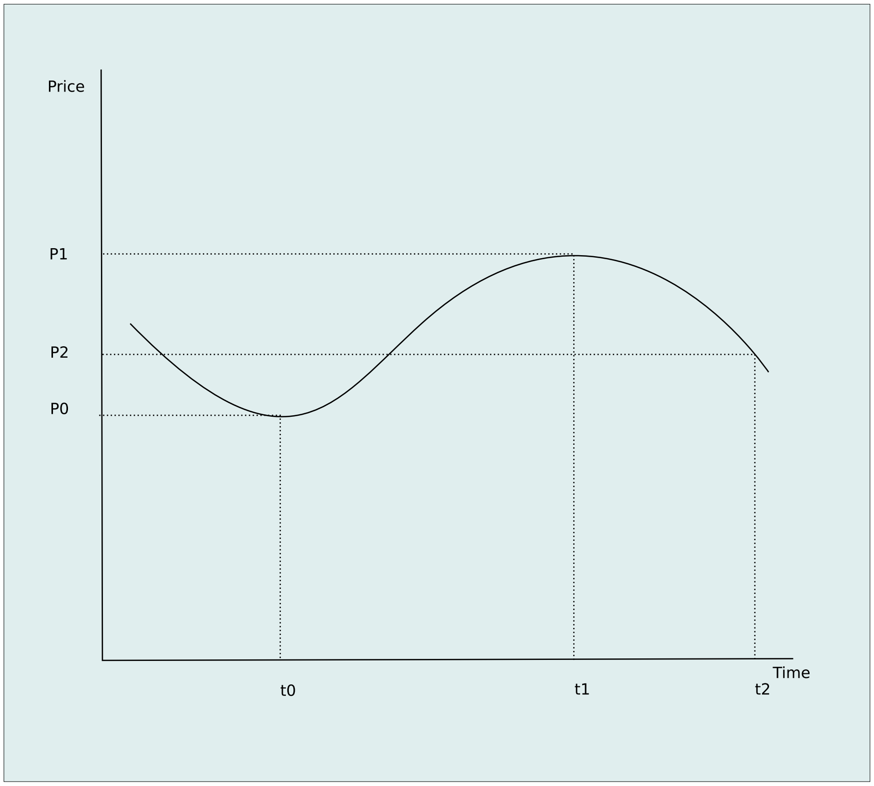


Figure 1: Illustration of the Mechanism by which Trading Constraints in the Direct Property Market hinder REITs from generating Timing-Based Appreciation Profits.

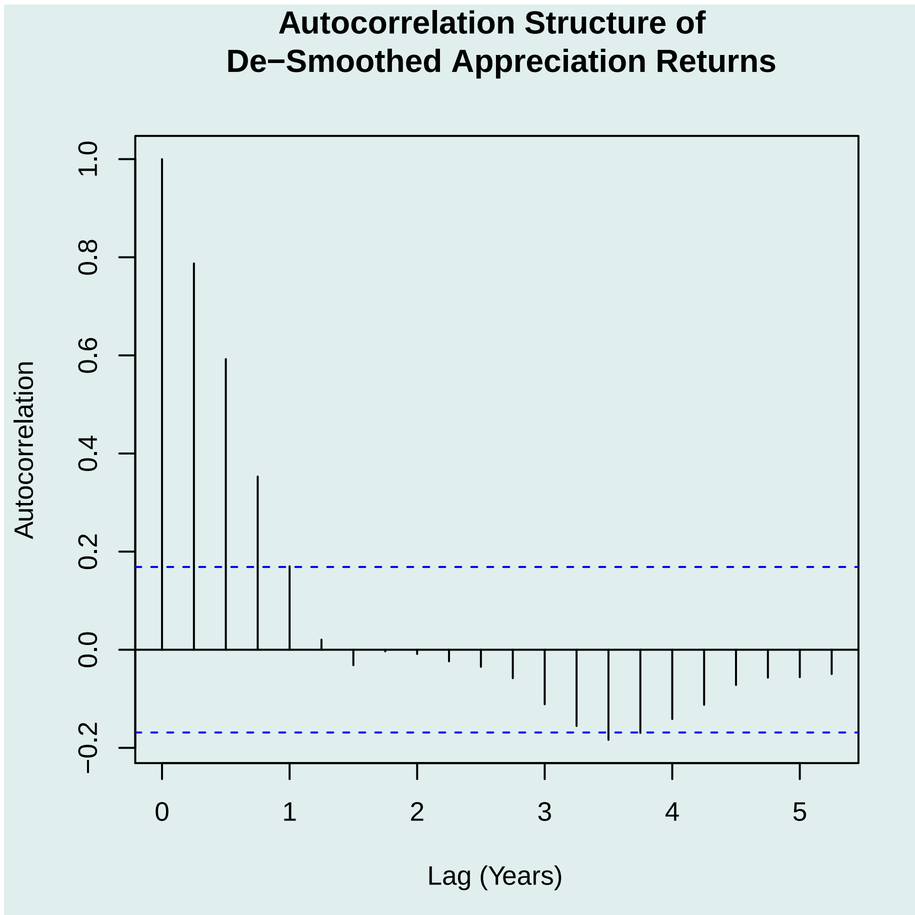


Figure 2: Illustration of the autocorrelation structure of NCREIF appreciation returns, de-smoothed according to the procedure of Cho, Kawaguchi and Shilling (2003). The dashed lines show a 95% confidence interval, for the hypothesis test that the true autocorrelation is zero.

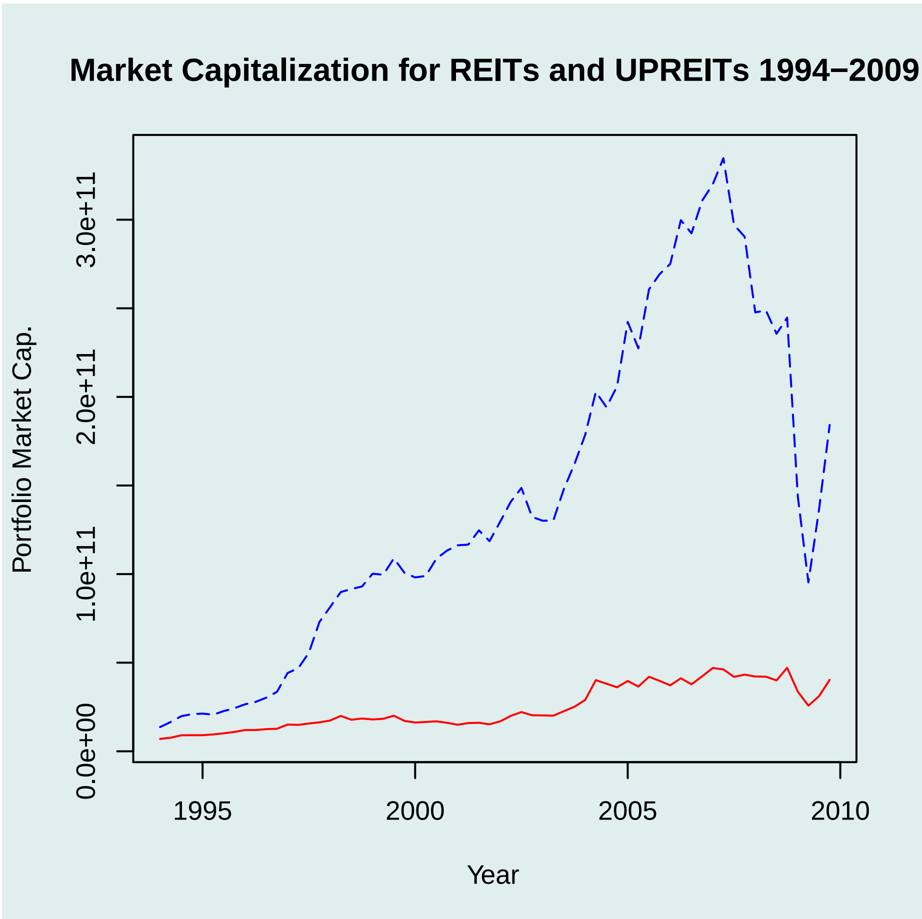


Figure 3: Illustration of the market capitalization of UPREITs (the blue dashed line) versus that of regular REITs (the red solid line) between 1994 and 2009. Only firms covered by SNL are shown.

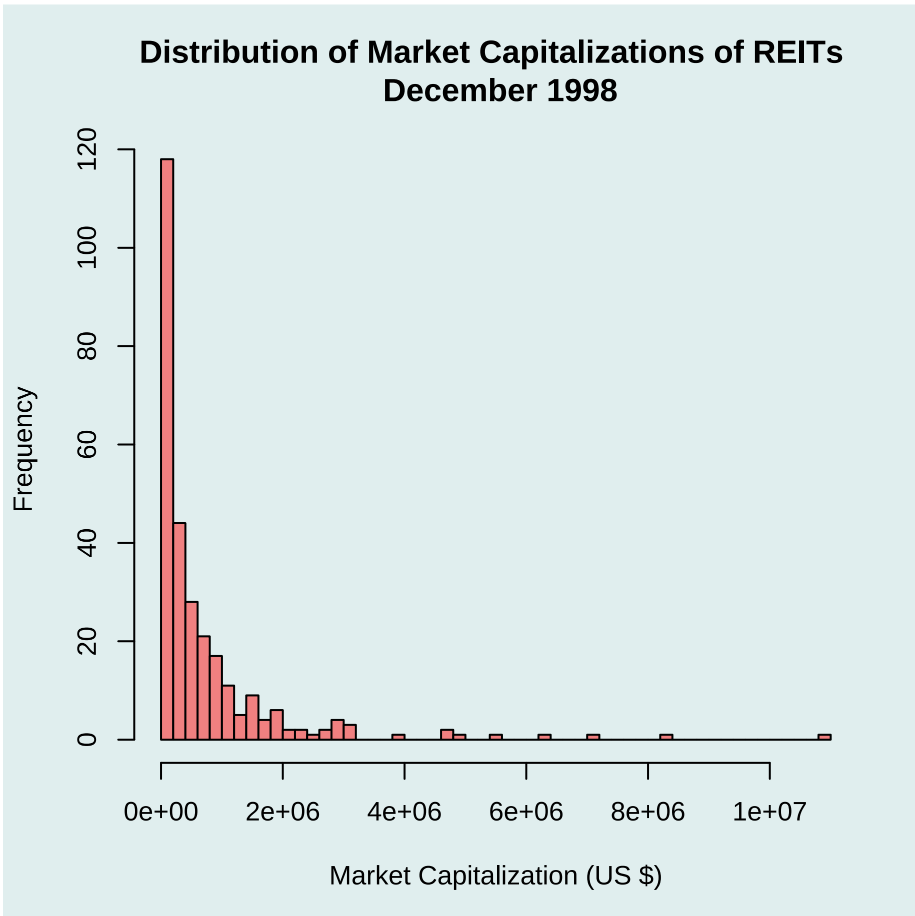


Figure 4: Illustration of the distribution of individual firm market capitalizations. While this cross-sectional distribution is at a single date, the illustration is certainly representative of the entire sample.