Benchmarking Intensity

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Benchmarking incentivizes fund managers to invest a fraction of their funds' assets in their benchmark indexes, and such demand is inelastic. We construct a measure of inelastic demand a stock attracts, *benchmarking intensity (BMI)*, computed as its cumulative weight in all benchmarks, weighted by assets following each benchmark. Exploiting the Russell 1000/2000 cutoff, we show that changes in stocks' BMIs instrument for changes in ownership of benchmarked investors. The resultant demand elasticities are low. We document that both active and passive fund managers buy additions to their benchmarks and sell deletions. Finally, an increase in BMI lowers future stock returns. (*JEL* G11, G12, G23)

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The asset management industry has been growing in size and importance over time. To date, it has amassed more than $100 trillion in assets under management (AUM) worldwide.[1] A large fraction of these funds are managed against benchmarks (e.g., the S&P 500, FTSE-Russell indexes, etc.). Benchmarks convey to fund investors information about

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the types of stocks the fund invests in and act as a useful tool for performance evaluation of fund managers. With a growing investor appetite for different investment styles, benchmarks are becoming increasingly heterogeneous. As of June 2021, the AUM share of U.S. domestic equity funds benchmarked to the S&P 500 was 35%, the next 27% was split between the Russell indexes, followed by 15% benchmarked to the CRSP indexes.[2] Our objective is to link membership in multiple benchmarks to stock prices and expected returns, as well as the demand by fund managers.

In this paper, we argue that stocks included in a benchmark form a preferred habitat for fund managers evaluated against that benchmark. In our model, benchmarked fund managers have an incentive to hold stocks in their benchmarks, which makes a fraction of their demand for these stocks inelastic. We derive a measure, which we term *benchmarking intensity* (BMI), that captures the aggregate inelastic demand of all benchmarked managers. We define the benchmarking intensity of a stock as the cumulative weight of the stock in all benchmarks, weighted by assets under management following each benchmark, relative to the stock's market capitalization. For the former, we use the historical composition of 34 U.S. equity indexes.[3] For the assets, we use the AUM of U.S. equity mutual and exchange-traded funds. We extract the history of fund benchmarks directly from their prospectuses.[4]

We exploit the variation in the benchmarking intensity of stocks that transition across the Russell 1000/2000 index cutoff to establish the effects of BMI on stock prices, expected returns, fund ownership, and demand elasticities. First, we show that the change in BMI resulting from an index reconstitution is positively related to the size of the index effect.[5] Second, we argue that a change in a stock's BMI predicts the change in ownership of benchmarked investors in this stock. Specifically, it accounts for both active and passive managers' demand and for all relevant benchmarks that include this stock, which allows us to establish a lower bound for the price impact of benchmarked managers' trades. We then use changes in BMI as an instrumental variable to estimate the price impact of institutional investors' trades (or the price elasticity of demand). Third, we highlight that active managers contribute

[2] Figure B1 in the Internet Appendix plots assets under management of U.S. domestic equity mutual funds and exchange-traded funds in our sample, by benchmark. The heterogeneity of benchmarks is apparent from the figure, especially for mid-cap and small stocks.

[3] Taken together, these indexes represent close to 90% of the U.S. domestic equity mutual and exchange-traded fund assets in our sample.

[4] Details of the procedure and methods used to validate our benchmark data are described in the text. Previous research has used a snapshot of fund benchmarks or assumed S&P 500 as a universal benchmark.

[5] The index effect is a boost to a company's share price when it is added to an index.

substantially to the benchmarking intensity and document that they buy additions to their benchmarks and sell deletions. Finally, we show that, consistent with our theory, an increase in a stock's benchmarking intensity leads to underperformance relative to comparable stocks for a period of 1 to 5 years. This result shows that it may take much longer for arbitrageurs to absorb a demand shock from benchmarked fund managers than previously documented or that the effect of index inclusion on stock prices is permanent.

We start with a simple model that highlights the channel through which a stock's benchmarking intensity affects its price and expected return. The model features fund managers alongside standard direct investors. All investors are risk averse. A fund manager's compensation depends on performance relative to her benchmark. The model predicts that such performance evaluation makes benchmark stocks the preferred habitat of managers evaluated against that benchmark. The fund manager's higher demand for her benchmark stocks makes prices of these stocks higher in equilibrium and their expected returns lower. This effect is permanent, persisting for as long as the stocks remain in the benchmark. In an equilibrium with heterogeneous benchmarks, the variable that captures the additional (inelastic) demand of benchmarked managers – beyond what the standard risk-return trade-off would predict – is exactly the benchmarking intensity.

In our empirical analysis, we explore how a shock to a stock's BMI affects its price and ownership. Isolating the effects of this variation is challenging because, through index membership, BMI may be related to other stock characteristics, most importantly size and liquidity. Our solution is to exploit the cutoff between the Russell 1000 and 2000 indexes, which separates stocks that are very similar in size and other characteristics but differ significantly in terms of their benchmarking intensities. Mechanical index reconstitution rules lead to the close-to-random index assignment into the Russell 1000 and 2000 indexes, which serves as a source of (conditionally) exogenous variation in benchmarking intensity. So our tests compare stocks close to the cutoff that experience different changes in BMI.

We empirically link the size of the price pressure experienced by a stock to the change in its benchmarking intensity. Corroborating the results of Chang, Hong, and Liskovich (2015), we document price pressure upon index reconstitution (the index effect). As in the rest of the index effect literature, Chang, Hong, and Liskovich (2015) look only at the average effect.[6] Our contribution is to show, in the cross-section of stocks around the Russell cutoff, that stocks whose BMI

[6] The exceptions are Greenwood (2005) and Wurgler and Zhuravskaya (2002), both of whom link the size of the index effect to arbitrage risk.

changed the most experience the largest index effect. We then use our regression estimates from this analysis to establish a lower bound on the price impact of benchmarked fund managers' trades and find that a 1% change in BMI leads to a 27-bps higher return in the month of index reconstitution. It is a lower bound because, in practice, fund managers incur transaction costs, which often prevents them from trading as BMI would predict, especially if the funds are active.

We show that BMI predicts changes in institutional ownership and we can therefore estimate the actual price impact of institutional investors' trades. Ownership changes are, of course, endogenous, and we argue that changes in BMI act as a valid instrument for them. The literature has used the Russell 1000/2000 index membership (dummy) as an instrument for institutional ownership, but this instrument is rather coarse. The advantage of BMI is that it is a continuous measure, which makes it a stronger instrument, and we argue that it remains (conditionally) exogenous. The instrumental variable approach yields an estimate of 1.5 for the price impact of institutional investors' trades. Similarly to Koijen and Yogo (2019), we highlight that the resultant price elasticities of demand for stocks are quite low.

BMI allows us to measure the price elasticity of demand for stocks more precisely than in the related literature, not only because it is continuous but also because it takes into account the inelastic demand of active managers stemming from different benchmarks that include these stocks. To measure the price elasticity of demand, most papers have exploited index reconstitutions and have used the resultant change in passive assets as a shock to net supply. If active managers' demand features an inelastic component, measures of elasticity based on a passive demand change upon index reconstitution will be inaccurate. We also argue that accounting for heterogeneous benchmarks (e.g., that each Russell 1000 stock also belongs to the Russell 1000 Value and/or Growth, and often to the Russell Midcap) is important when estimating the elasticity of demand for stocks.

We show that both active and passive investors have a considerable fraction of holdings concentrated in their benchmarks and that their rebalancing around the Russell cutoffs is consistent with changes to their benchmarks. The majority of recent studies attributed the discontinuities in ownership around the cutoff to passive investors, that is, index and exchange-traded funds. In line with the literature, we find highly significant rebalancing of index additions and deletions for passive funds in the direction imposed by their benchmarks. For example, passive funds benchmarked to the Russell 2000 purchase 77 bps of shares of stocks added to the Russell 2000. These funds also sell deleted stocks in similar proportions. Using the data on funds' benchmarks, we are able to demonstrate the same pattern in active funds. We find that active funds benchmarked to the Russell 2000 also sell deletions, decreasing their ownership share by 55 bps. Active funds benchmarked to the Russell 1000 and Russell Midcap increase their ownership shares in stocks added to the Russell 1000 and Midcap by 12 and 39 bps, respectively. We do not have an identification strategy of comparable quality for other benchmarks but we show that aggregate active fund portfolios indeed resemble their benchmarks. So in line with our theory, stocks inside the benchmarks serve as both active and passive funds' preferred habitats.

To validate the prediction of our model that the more active funds have a smaller inelastic component in their demand functions, we sort funds based on their Active Share, a measure of activeness proposed by Cremers and Petajisto (2009). We show that more active funds both invest less in their benchmarks and rebalance index additions and deletions less than their less active peers. We also explore implications of differences in fund activeness in the construction of BMI.

Finally, we find that, consistent with our theory, stocks whose BMIs have gone up significantly underperform in the long run. Exploiting again the Russell cutoff, we show that increased inelastic demand of benchmarked fund managers leads to lower expected returns of these stocks for horizons of up to 5 years relative to their peers close to the cutoff. The economic magnitudes are sizeable, averaging 2.8% lower return in the first year for additions to the Russell 2000 index.

This paper is related to several strands of literature, including equilibrium asset pricing with benchmarked fund managers, index effect, and empirical research on the effects of institutional ownership.

Among theoretical contributions, the first paper to study benchmarking was Brennan (1993). Brennan derives a two-factor asset pricing model in a two-period economy with a benchmarked fund manager. Cuoco and Kaniel (2011), Basak and Pavlova (2013), and Buffa, Vayanos, and Woolley (Forthcoming) investigate equilibrium asset pricing effects of delegated portfolio management in dynamic economies. The closest paper to ours in this strand of literature is Kashyap et al. (2021). None of these works, however, considers heterogeneous benchmarks. The only paper that does is Buffa and Hodor (2018), but they focus primarily on asset return comovement. In our model, heterogeneous habitats of fund managers arise because of the heterogeneity in benchmarks. Such habitats could also be driven by optimal narrow investment mandates in delegated asset management (e.g., van Binsbergen, Brandt, and Koijen 2008; He and Xiong 2013) or different investor styles (Barberis and Shleifer 2003). A related idea of studying how investor habitats affect asset prices is exlored in preferred habitat models of the term structure of interest rates (e.g., Vayanos and Vila 2021).

Both our theoretical and empirical results are related to the index effect literature. The index effect was first documented by Shleifer (1986) and Harris and Gurel (1986) for additions to the S&P 500 index and subsequently found in many other markets and asset classes.[7] This literature typically measures the average size of index effect, while we show how it varies in the cross-section with the change in BMI.

The existence of the index effect challenges the standard theories, which predict that demand curves for each stock are very elastic and therefore index inclusion should have no effect on asset prices and expected returns. The index effect literature has converged to the view that stocks are not perfect substitutes, which suggests that the demand curves for stocks are downward-sloping. Our preferred habitat model provides a microfoundation for why stocks are imperfect substitutes.[8] In the model, fund managers' demand features an inelastic component due to benchmarking. This affects stock prices and expected returns for as long as the stocks remain in the benchmark.

Our analysis delivers an alternative estimate of stock price elasticity of demand based on an index inclusion event. Most of the known estimates are based on a single index membership, while the BMI measure accounts for the demand related to all large benchmarks in a comprehensive way. Furthermore, the change in a stock's BMI helps measure the price elasticity of demand more accurately in a world where active managers' demand has both elastic and inelastic components. Recent literature stresses the importance of incorporating downward-sloping demand curves for stocks in the asset pricing and macrofinance models (e.g., Gabaix and Koijen 2020), and our results may inform such models.

Our instrumental variable approach to computing demand elasticities is related to that in Koijen and Yogo (2019), who propose a characteristics-based demand-system approach which can be used to estimate price impact of a given institutional investor. We focus on aggregate demand of benchmarked institutions and perform estimation in changes. Our estimate of the aggregate price impact is slightly lower than theirs, most likely because we consider stocks around the Russell 1000/2000 cutoff, which are closer substitutes.

The closest empirical work to ours is Chang, Hong, and Liskovich (2015). Theirs was the first paper to build a regression discontinuity

[7] Most of the studies focus on S&P 500 and Russell composition changes, though others also cover such index families as MSCI, DJIA, Nikkei, FTSE, CAC, Toronto Stock Exchange Index, etc. For example, Chen, Noronha, and Singal (2005) document a long-lasting price increase of the S&P 500 additions, which increases in magnitude through time. Hacibedel and van Bommel (2007) also find permanent price increase for emerging markets indexes within the MSCI family. Greenwood (2005) documents an index effect for a redefinition of the Nikkei 225 index in Japan.

[8] Petajisto (2009) offers a complemetary view, also based on asset manager demand.

design (RDD) on the cutoff between the Russell 1000 and 2000 indexes in order to quantify the price pressure stemming from institutional demand. The paper finds a 5% index effect in the month of addition to the Russell 2000. It also documents a decreasing trend in this index effect and attributes it to the alleviation of limits to arbitrage. Even though we use the same cutoff for identification, we are the first to document the resultant difference in the long-run returns (12 months to 5 years) of stocks that moved indexes and those that did not. We view the duration of this effect as evidence that index membership affects the risk premium of a stock. Furthermore, we discuss the advantages of using BMI over the index membership dummy to measure demand elasticities and show how the estimates of Chang, Hong, and Liskovich change in a setting with heterogeneous benchmarks.

A growing body of literature studies implications of passive ownership for corporate governance using the Russell cutoff.[9] This literature documents predictable rebalancing of passive funds around the cutoff, but not active. In line with the findings of this literature, we find that the *total* active ownership in stocks that switched indexes does not change. However, our granular data allows us to show that the identities of active funds change as benchmarks would predict. For example, a stock that is deleted from the Russell 2000 is sold by the active funds benchmarked to the Russell 2000 and bought by active funds benchmarked to the Russell 1000 and Midcap. As a result, monitoring incentives of active managers may change and this may affect corporate governance.

1. Model of Delegated Asset Management with Heterogeneous Benchmarks

To illustrate the main mechanism, we first develop a simple model of asset prices in the presence of benchmarking. It builds on Brennan (1993) and Kashyap et al. (2021) and introduces heterogeneous fund managers whose performance is evaluated relative to a variety of benchmarks. The goal of the model is to characterize a relationship between benchmarking intensity, our measure of capital that is inelastically supplied by fund managers, and stock returns.

1.1 Model

Except for the presence of fund managers, our environment is standard. There are two periods, $t=0,1$. The financial market consists of a riskless asset with an exogenous interest rate normalized to zero (e.g., a storage technology) and N risky assets paying cash flows D_i, $i=1,\dots,N$ in

[9] The list of papers includes but is not limited to: Heath et al. (2021), Appel, Gormley, and Keim (2016, 2019), Glossner (Forthcoming), Schmidt and Fahlenbrach (2017).

period 1. The cash flows of the risky assets are given by

$$D_i = \overline{D}_i + \beta_i Z + \epsilon_i, \quad \beta_i > 0, i = 1,\ldots,N,$$

where $Z \sim N(0,\sigma_z^2)$ is a common shock and $\epsilon_i \sim N(0,\sigma_\epsilon^2)$ is an idiosyncratic one. The vectors $D \equiv (D_1,\ldots,D_N)'$ and $S \equiv (S_1,\ldots,S_N)'$ denote vectors of period-1 cash flows and period-0 risky asset prices, respectively. Period-1 risky asset prices equal D. The risky assets are in fixed supply of $\overline{\theta} \equiv (\overline{\theta}_1,\ldots,\overline{\theta}_N)'$ shares. It is convenient to introduce the notation $\Sigma \equiv \Sigma_z + I_N \sigma_\epsilon^2$ for the variance-covariance matrix of cash flows D, where Σ_z is a $N \times N$ matrix with a typical element $\beta_i \beta_j \sigma_z^2$ and I_N is an $N \times N$ identity matrix. We also set $\overline{D} \equiv (\overline{D}_1,\ldots,\overline{D}_N)'$ and $\beta \equiv (\beta_1,\ldots,\beta_N)'$.

J benchmark portfolios are used to evaluate performance. Each benchmark j is a portfolio of $\omega_j \equiv (\omega_{1j},\ldots,\omega_{Nj})'$ shares of the assets described above. Some components of ω_j can be zero.

There are two types of investors: direct investors and fund managers. Direct investors, whose mass in the population is λ_D, manage their own portfolios. Fund managers manage portfolios on behalf of fund investors. Fund investors can buy the riskless asset directly, but cannot trade stocks; they delegate the selection of their portfolios to portfolio managers. The managers receive compensation from fund investors. Each manager is evaluated relative to a benchmark. We denote the mass of managers evaluated relative to benchmark j by λ_j.[10] All investors have a constant absolute risk aversion (CARA) utility function over terminal wealth (or compensation), $U(W) = -\exp^{-\gamma W}$, where γ is the coefficient of absolute risk aversion.

The terminal wealth of a direct investor is given by $W = W_0 + \theta_D'(D - S)$, where the $N \times 1$ vector θ_D denotes the number of shares held by the direct investor, and W_0 is the investor's initial wealth. The direct investor chooses a portfolio θ_D to maximize his utility $U(W)$. A fund manager's j compensation w_j consists of three parts: one is a linear payout based on absolute performance of the fund, the second piece depends on the performance of the fund relative to the benchmark portfolio j, and the third is independent of performance (c). Specifically,

$$w_j = aR_j + b(R_j - B_j) + c, \quad a \geq 0, b > 0$$

where $R_j \equiv \theta_j'(D - S)$ is the performance of the fund's portfolio and $B_j \equiv \omega_j'(D - S)$ is the performance of benchmark j.[11] The parameters a and b are the contract's sensitivities to absolute and relative performance, respectively. The fund manager chooses a portfolio of θ_j shares to maximize his utility $U(w_j)$.

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[10] For simplicity, we assume that each fund investor employs one fund manager, but this assumption can be easily relaxed.

[11] Ma, Tang, and Gómez (2019) and Evans et al. (2020) analyze compensation of fund managers in the U.S. mutual fund industry and provide evidence supporting our

1.2 Portfolio choice and asset prices

The portfolio demand of the direct investors is the standard mean-variance portfolio:[12]

$$\theta_D = \frac{1}{\gamma}\Sigma^{-1}\left(\overline{D} - S\right). \tag{1}$$

In contrast, the fund managers do not have the same risk-return trade-off as direct investors, because of their compensation contracts. The portoflio demand of manager j is given by

$$\theta_j = \frac{1}{\gamma(a+b)}\Sigma^{-1}\left(\overline{D} - S\right) + \frac{b}{a+b}\omega_j. \tag{2}$$

The fund manager splits his risky asset holdings across two portfolios: the mean-variance portoflio (the first term in (2)) and the benchmark portfolio (the second term). The latter portfolio arises because the manager hedges against underperforming the benchmark. Consistent with the preferred habitat view, the manager thus has a higher demand for stocks in her benchmark. Notice that the demand for the benchmark portfolio ω_j is inelastic. It does not depend on the riskiness of the assets and depends only on the parameters of the compensation contract. It follows that, *ceteris paribus,* stocks with a higher benchmark weight have a higher weight in the fund manager's portfolio.

By clearing markets for the risky assets, $\lambda_D\theta_D + \sum_{j=1}^{J}\lambda_j\theta_j = \overline{\theta}$, we compute equilibrium asset prices.

$$S = \overline{D} - \gamma A\Sigma\left(\overline{\theta} - \frac{b}{a+b}\sum_{j=1}^{J}\lambda_j\omega_j\right), \tag{3}$$

where $A \equiv \left[\lambda_D + \frac{\sum_j \lambda_j}{a+b}\right]^{-1}$ modifies the market's effective risk aversion.[13]

Equation (3) elucidates the determinants of the index effect in our model. The index effect manifests itself through the benchmarking-induced price pressure term $\frac{b}{a+b}\sum_{j=1}^{J}\lambda_j\omega_j$. This term reflects the

specification here. Endogenizing this compensation structure is beyond the scope of this paper; see Kashyap et al. (2020) who derive it as part of an optimal contract. Finally, see Kashyap et al. (2021) (Internet Appendix B) for an alternative specificaton of a benchmark, in which constituents are value-weighted. Such specification is not as analytically tractable as ours, but it delivers similar insights.

[12] All proofs are in the appendix.

[13] Our model can be extended to incorporate passive managers, who simply hold the benchmark portfolio. Suppose the total mass of fund managers benchmarked to index j, λ_j, consists of a mass λ_j^P of passive managers and a mass λ_j^A of active. Then the expression for stock prices is

$$S = \overline{D} - \gamma A\Sigma\left(\overline{\theta} - \sum_{j=1}^{J}\left[\frac{b}{a+b}\lambda_j^A\omega_j + \lambda_j^P\omega_j\right]\right), \text{ where } A \equiv \left[\lambda_D + \frac{\sum_j \lambda_j^A}{a+b}\right]^{-1}.$$

cumulative inelastic demand of fund managers and motivates our benchmarking intensity measure used in the empirical part of the paper. Equation (3) implies that if a stock gets added to a benchmark or if its weight in a benchmark increases, its price goes up. Another implication is that the larger the mass of fund managers (λ_j's) following a benchmark, the higher the benchmarking-induced price pressure and hence the bigger the index inclusion effect. The more benchmarks a stock belongs to and the bigger its weight in the benchmarks, the more demand from fund managers it attracts and therefore the higher the stock's price.

Our next set of predictions is about the expected stock returns (or the cost of equity). The expected return of stock i, expressed as a per-share return $\Delta S_i \equiv \overline{D}_i - S_i$, is given by[14]

$$E[\Delta S_i] = \gamma A \beta_i \sigma_z^2 \beta' \left(\overline{\theta} - \frac{b}{a+b} \sum_{j=1}^{J} \lambda_j \omega_j \right) + \gamma A \sigma_\epsilon^2 \left(\overline{\theta}_i - \frac{b}{a+b} \sum_{j=1}^{J} \lambda_j \omega_{ij} \right). \quad (4)$$

Equation (4) implies that the price pressure we discussed above is permanent, and it lasts for as long as a stock remains in the fund managers' benchmarks. Therefore, *ceteris paribus*, stocks with higher benchmarking intensities, defined in our model as $\sum_{j=1}^{J} \lambda_j \omega_{ij}$, have lower expected returns. Furthermore, if a stock's benchmarking intensity goes up (e.g., because of an index inclusion), its price should rise upon announcement and the expected return after the announcement should be lower.

In summary, our model produces the following predictions:

Prediction 1: Stocks with higher benchmarking intensities have lower expected returns.

Prediction 2: If a stock's benchmarking intensity goes up (e.g., because of an index inclusion), its price should rise.

Prediction 3: If a stock's benchmarking intensity goes up, the funds' ownership of the stock ($\sum_j \theta_{ij}$) should rise.

Prediction 4: If a stock enters benchmark j and exits benchmark k, funds benchmarked to index j increase their demand for the stock (θ_{ij}) while those benchmarked to index k decrease their demand (θ_{ik}).

Before turning to testing the above predictions empirically, we should acknowledge that in the model benchmark weights are known with

[14] In models with CARA preferences and normally distributed cash flows, the return is usually expressed in per-share terms. In our empirical analysis, however, we use per-dollar returns, $r_{it+1} \equiv (S_{it+1} - S_{it})/S_{it}$, as in the empirical literature. We acknowledge this inconsistency, but we still prefer to keep our theoretical results in terms of per-share returns, for expositional clarity.

certainty and hence it cannot speak to differences between anticipated and unanticipated changes in weights. The appendix analyzes an extension of our model in which benchmark weights are uncertain. Specifically, we add an extra period, $t=-1$, in which benchmark weights are unknown and investors form expectations about them. In that extension, all expressions from this section remain intact and hence Predictions 1–4 continue to hold. Additionally, we show that realized stock returns at $t=0$, when benchmark weights become known, depend on $\sum_{j=1}^{J}\lambda_j\omega_j - E^*[\sum_{j=1}^{J}\lambda_j\omega_j]$, where the expectation $E^*[\cdot]$ is under the risk-neutral measure. In words, what matters for stock returns is an *unanticipated* change in BMI, rather than BMI. The measure of an unanticipated change in BMI is more challenging to construct empirically than that of BMI itself, and hence in our empirical tests we focus on a change in BMI following an index reconstitution, leaving the construction of a measure of an unanticipated change in BMI to future research.

2. Benchmarking Intensity in the Data

In this section, we use data on U.S. domestic equity mutual funds and their prospectus benchmarks to build a measure of benchmarking intensity. We document its basic properties and apply this measure to the computation of the price elasticity of demand for stocks.

2.1 Data set

The main sample is an annual panel of stocks which were the Russell 3000 constituents in 1998-2018.[15] The main three pillars of data are historical benchmark weights, fund and institutional holdings, and stock characteristics. The second and third are standard, we report details on them in Section B.2 of the Internet Appendix.

In contrast to the previous studies, the data set is granular with respect to benchmark information. It includes primary prospectus benchmarks scraped directly from historical fund prospectuses available on the website of the U.S. Securities and Exchange Commission[16] and augmented with a Morningstar snapshot. The scraping procedure and its validation are described in detail in Section B.3.1 of the Internet Appendix. We obtain benchmark constituent data from the following sources. All the constituent weights for 22 Russell benchmark indexes

[15] Our main sample starts in 1998, before which we do not have benchmark data of sufficient quality. Even though the SEC’s electronic archives date back to 1994, many funds do not report their benchmarks in files available prior to 1998. Please find the details in Section B.3. Our sample ends in December 2018 because the holdings data used for the analysis of fund ownership are available with a lag.

[16] Follow https://www.sec.gov/edgar/searchedgar/mutualsearch.html.

are from FTSE Russell (London Stock Exchange Group). The Russell indexes include (all total return in USD): Russell 1000, 2000, 2500, 3000, 3000E, Top 200, Midcap, and Small Cap Completeness (blend), as well as their Growth and Value counterparts. Constituent weights for the S&P 500 TR USD and S&P MidCap 400 TR USD are from Morningstar and available from September 1989 and September 2001, respectively, to October 2015. We construct constituent weights for S&P 500 after October 2015 manually from constituent lists and prices available through CRSP. We generate the S&P 400 weights from holdings of index funds (Dreyfus and iShares).[17] The constituent weights for the CRSP U.S. indexes are from Morningstar and available from 2012. These indexes include (all total return in USD): Total Market, Large Cap, Mid Cap, and Small Cap (blend), as well as their Growth and Value counterparts.

Our benchmark data offer two advantages over those used by prior research. First, the benchmark information is a dynamic panel encompassing benchmark changes.[18] Therefore, it accurately reflects the benchmark used by funds at any point in time.[19] Second, we obtain Russell index data from FTSE Russell directly: our data set includes proprietary total market values (capitalization) as of the rank day in May and provisional constituent lists available before the reconstitution day in June.

We report the descriptive statistics of the main calculated variables used in analysis in Tables B2 and B3 in the Internet Appendix.

2.2 Empirical measure of benchmarking intensity

Guided by the model, we calculate the *benchmarking intensity (BMI)* for stock i in month t as

$$BMI_{it} = \frac{\sum_{j=1}^{J} \lambda_{jt} \omega_{ijt}}{MV_{it}}, \tag{5}$$

where λ_{jt} is the assets under management (AUM) of mutual funds and ETFs benchmarked to index j in month t, ω_{ijt} is the weight of stock i in index j in month t and MV_{it} is the market capitalization of stock i in month t. In our baseline BMI in (5), we treat active and passive

[17] Since the S&P 400 index is relatively small, these weights do not contribute much to the analysis. We do not include the S&P 600 index because its share is even smaller and the holdings-based weights are not of sufficient quality.

[18] See Appendix, in which we show that our scraping procedure picked up such important benchmark changes as Vanguard’s move from the MSCI to CRSP indexes in 2013.

[19] We attribute funds with benchmarks with non-value-weighted constituents and SRI screened funds to their “parent” benchmarks, for example, the S&P 500 equal-weighted index to the S&P 500 index. These funds are small in our sample and removing them does not change the results.

assets equally; Section 2.3.5 explores alternative definitions that adjust BMI for fund activeness. In our theory, the price impact of additional inelastic demand ($\Delta S_i/\Delta\sum_{j=1}^{J}\lambda_j\omega_{ij}$) is constant and does not depend on the stock's supply (Equation (4)), which is unrealistic. This feature of CARA models makes them tractable, but in our empirical analysis, to be consistent with the empirical literature on price impact, the natural object to work with is the total inelastic demand the stock attracts, *as a fraction of the stock's market capitalization*. Furthermore, stock weight in any value-weighted index j is

$$\omega_{ijt}=\frac{MV_{it}\mathbf{1}_{ijt}}{\sum_{k=1}^{N}MV_{kt}\mathbf{1}_{kjt}}=\frac{MV_{it}\mathbf{1}_{ijt}}{\text{IndexMV}_{jt}},$$

where the index membership dummy $\mathbf{1}_{ijt}$ is equal to one if stock i belongs to index j at time t and IndexMV_{jt} is the total market cap of all stocks in index j at time t. Hence, an additional advantage of this scaling of our theoretical measure is that the MV_{it} terms cancel out from (5) and we can rewrite BMI as

$$BMI_{it}=\sum_{j=1}^{J}\frac{\lambda_{jt}\mathbf{1}_{ijt}}{\sum_{k=1}^{N}MV_{kt}\mathbf{1}_{kjt}}=\sum_{j=1}^{J}\frac{\lambda_{jt}\mathbf{1}_{ijt}}{\text{IndexMV}_{jt}}, \tag{6}$$

Related literature has established that a stock's transitions between the Russell 1000 and 2000 indexes, captured by $\mathbf{1}_{ijt}$, can be used as an instrument for changes in the stock's ownership. Since our BMI additionally depends only on aggregated variables, such as the total AUM of each index the stock belongs to and the total market capitalization of each index, ΔBMI could plausibly be a valid instrument for changes in stock ownership.[20] We will examine this conjecture in detail in Section 2.3.4.

[20] There are two potential caveats. First, some index providers use the float-adjusted market cap for the purposes of index construction. That is, strictly speaking, (6) should be $BMI_{it}=\sum_{j=1}^{J}\frac{\lambda_{jt}FF_{ijt}\mathbf{1}_{ijt}}{\sum_{k=1}^{N}MV_{kt}FF_{kjt}\mathbf{1}_{kjt}}$, where FF_{ijt} denotes the float factor of stock i in index j at time t (the float factor may be index specific). Because this float factor reflects stock liquidity, it could be a potential source of endogeneity. Russell primarily uses companies' SEC filings to compute their free float. In our regression analysis, we use the official Russell free float in May, provided to us by Russell, as one of our control variables and supplement it with bid-ask spread to account for any stale information in the float factor. We could also scale BMI by float-adjusted market value provided by Russell instead of the total market value from CRSP to completely exclude FF from the numerator. Our results are robust to this alternative scaling and we choose the total market value scaling as our baseline because it makes our measure easy to replicate. Second, value and growth indexes typically include only a fraction of the market value of the stock that they deem related to value or growth style. We see that, on average, this split of shares between Russell value and growth indexes does not strongly affect changes in BMI around the Russell cutoff (the necessary assumptions are discussed in Internet Appendix B.22). Furthermore, all our results are robust to controlling for the stock's Russell proprietary value ratio in May, M/B, and sales growth. To further alleviate possible concerns about endogeneity of ΔBMI, in Section 2.3.4 we perform overidentifying restrictions tests.

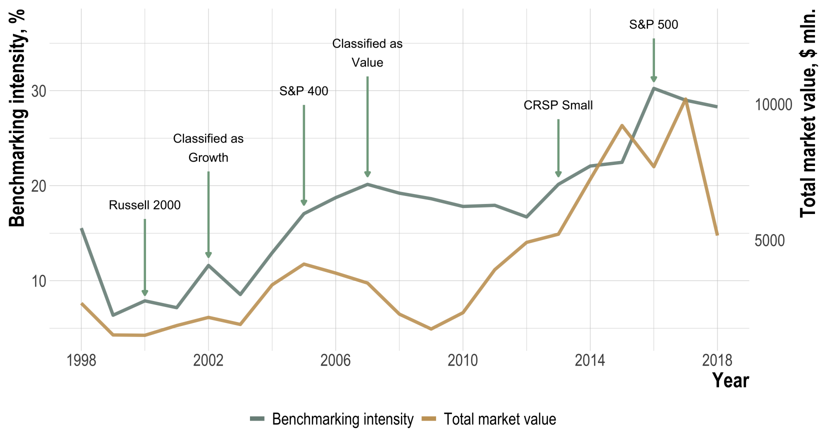

Figure 1
Benchmarking intensity of Foot Locker Inc.
This figure plots the benchmarking intensity (left axis) and the total market value (right axis) of Foot Locker Inc. stock over time. Arrows point to the years when the stock was added to the benchmarks.

Notice that the computation of BMI does not rely on holdings data. Historical holdings data are available at best quarterly and can be noisy while index composition and funds' AUM are observed monthly.

Even though benchmarking intensity is typically slow moving, considerable variation comes from index membership. A useful illustration is a retailer Foot Locker Inc. (ticker FL). Figure 1 depicts a year-on-year evolution of its benchmarking intensity. Despite the evident comovement between size and benchmarking intensity, the latter has more variation due to the changing index membership and index asset flows: in 2000, FL joins the Russell 2000; in 2005, the S&P 400; in 2012, FL gets into the CRSP Small; in 2016, it gets added to the S&P 500.

Figure 2 illustrates the contribution of membership in each index to the benchmarking intensity of FL. Even though the stock's addition to S&P 500 clearly increases its BMI, the size and variation of other components are significant. Panel A of Figure B3 in the Internet Appendix shows how much different benchmark styles (i.e., value, growth, and blend) contribute to FL's BMI. In our data, we only have style indexes for the Russell and CRSP families, so the rest is attributed to blend. Even with this limitation, it is apparent that style benchmarks occupy a considerable fraction of BMI. These two illustrations highlight one of the key contributions of our measure – it takes into account the heterogeneity of benchmarks and the overlaps between them.

Since the benchmarking intensity measure is built using the AUM of both active and passive funds, variation emerges from the relative

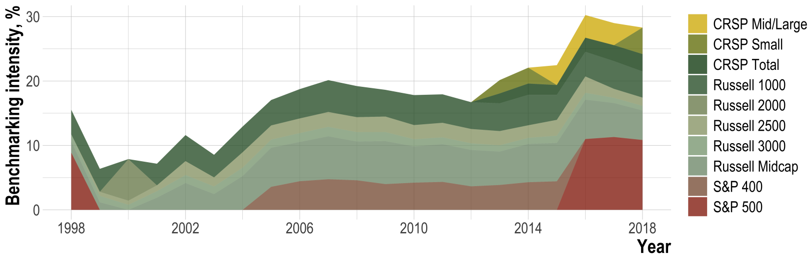

Figure 2
Decomposition of the benchmarking intensity of Foot Locker Inc.
This figure plots the evolution of each index group within the benchmarking intensity of Foot Locker Inc. stock over time. Index groups include blend, value, and growth indexes.

importance of these two fund types as depicted in panel B of Figure B3 in the Internet Appendix. The BMI of FL is dominated by the inelastic demand from active funds, even though the contribution of passive funds has grown. This illustrates another important advantage of BMI – unlike passive ownership, a measure of institutional demand used in the extant literature – the BMI accounts for the inelastic demand of active funds as well.

Table 1 documents descriptive statistics for BMI in our sample. S&P 500 stocks have the highest average BMI, while membership in the Russell 2000 contributes the most to the BMI of an average stock. The reported statistics also highlight the increasing heterogeneity of benchmarks for U.S. equities: the average number of benchmarks increased from 7 to 10 and the concentration of benchmark shares in BMI went down (as shown in panel B). Together, value and growth indexes are at least as important as blend indexes, contributing on average over 50% to the BMI. Furthermore, active funds contribute 83% to the BMI over the full sample period, even though their share declined to an average of 65% in the recent 5 years.[21]

BMI is not free of limitations. Empirically, we only observe benchmarks of the U.S. funds, while U.S. firms have seen an increasing share of foreign owners. This implies that the BMI we compute is a proxy of the true BMI which should include foreign funds benchmarked to U.S. stock indexes. We focus on mutual funds and ETFs but other investors, such as pension funds and insurance companies, may also invest through benchmarked managers. Because BMI is additive and only the numerator depends on AUM, the omission of foreign funds and other benchmarked institutions scales BMI down. While we do

[21] We exclude two stocks from our sample whose BMI exceeded 100% because of incorrect market values reported in CRSP. There are no stocks with BMI above 100% in our analysis sample in Section 2.3.2, that is, next to the Russell cutoff.

not have data for assets under management across all benchmarked institutions, we have checked data for separate accounts available in Morningstar. The distribution of assets across benchmarks is remarkably similar to that for mutual funds, with the exception of CRSP benchmark indexes. We are comforted by the fact that adding such benchmarked institutions will maintain the cross-sectional ranking in our sample. On the theory side, we assume that there are no transaction costs and that fund mandates only differ in the benchmark used. In practice, however, trading is costly and funds may have other constraints, such as bounds on sector exposure. This is expected to skew the weights used to compute BMI. We will discuss the consequences of considering trading costs at the end of Section 4.1.

2.3 Benchmarking intensity and the price elasticity of demand

In this section, we explore the relationship between the benchmarking intensity, the size of the index effect, and demand elasticities. We exploit the cutoff between the Russell 1000 and 2000 indexes, which separates stocks that are very similar in size and other characteristics but differ significantly in terms of their benchmarking intensities.

Table 1
Properties of benchmarking intensity

	By time period					By benchmark					
	Full sample	1998-2000	2001-2006	2007-2012	2013-2018	S&P 500	Russell 1000	Russell 2000	Russell Midcap	Russell Value indexes	Russell Growth indexes
A. Descriptive statistics											
Average BMI, %	15.4	10.2	15.2	17.1	15.5	19.6	16.3	17.5	16.6	16.9	17.2
SD of BMI, %	8.9	5.2	5.8	9.3	10.7	6.4	7.1	8.0	7.6	7.9	7.6
Minimum BMI, %	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Maximum BMI, %	98.7	86.8	57.4	98.7	70.1	56.6	56.6	98.7	56.6	98.7	86.8
Average no. of benchmarks	9.0	7.5	9.0	8.3	10.0	9.7	11.0	9.5	11.4	10.5	10.6
B. Average contribution of indexes, %											
S&P 500	8.5	9.7	9.6	9.7	6.3	53.5	26.1	0.2	17.8	9.5	8.3
S&P 400	2.0	0.0	2.0	2.8	1.8	0.0	4.7	0.7	5.9	2.2	1.9
Russell 1000	8.5	12.1	9.2	7.5	7.8	21.7	26.8	0.0	26.4	9.6	8.2
Russell Midcap	7.4	6.7	8.1	9.2	5.8	12.6	23.3	0.0	29.1	7.4	8.2
Russell 2000	50.6	49.4	53.0	56.3	44.6	0.7	0.0	79.5	0.0	53.1	52.6
Russell 2500	8.5	11.5	10.6	8.8	5.7	1.3	6.2	10.2	7.8	7.7	10.3
Russell 3000	6.1	10.5	7.5	5.8	3.8	5.9	7.4	5.9	7.2	6.3	6.4
CRSP U.S. Large and Mid	0.4	0.0	0.0	0.0	1.2	1.6	1.3	0.0	1.5	0.4	0.4
CRSP U.S. Small	1.5	0.0	0.0	0.0	4.3	0.1	1.6	1.6	2.0	1.6	1.5
CRSP U.S. Total	6.5	0.0	0.0	0.0	18.7	2.4	2.5	2.0	2.4	2.3	2.1
C. Average contribution of styles, %											
Blend	48.6	37.3	42.3	49.0	56.5	63.2	45.5	46.6	40.2	47.8	44.2
Value	25.3	25.1	25.1	27.3	23.5	20.6	28.0	25.6	30.9	38.9	11.7
Growth	26.1	37.6	32.5	23.7	20.0	16.1	26.5	27.8	28.9	13.2	44.1
D. Average contribution of fund types, %											
Active	83.0	96.5	93.4	89.9	65.2	80.4	83.4	88.6	84.4	86.5	87.5
Passive (index and ETFs)	17.0	3.5	6.6	10.1	33.8	19.6	16.6	11.4	15.6	13.5	12.7

This table reports the descriptive statistics for benchmarking intensity. Columns “By time period” show statistics for the respective period. Columns “By benchmark” show statistics for stocks that belong to the respective benchmark. BMI statistics (average, standard deviation, minimum, and maximum) are in percentage points. Contribution is in percentage points. Contribution of indexes is the average of the ratios of BMI coming from the AUM benchmarked to an index to the total BMI of the stock. Contribution of indexes is across index styles, for example, line for the Russell 1000 includes blend, value, and growth. Average number of benchmarks is for a stock. Averages are simple arithmetic means across stock-years.

2.3.1 The Russell index cutoff. The Russell indexes undergo an annual reconstitution every June. All eligible stocks are ranked based on their market cap value, and the top-1,000 stocks get assigned to Russell 1000. The ranking is based on a fixed date in May so any shock to a stock next to the cutoff can send it to one or the other side.[22] Figure B2, panel A, in the Internet Appendix plots index weights of stocks on the rank day (May 31^{st}) in 2006. All stocks to the right of 1,000th rank cutoff in May are assigned to the Russell 2000 in June. To the left of the cutoff, one can see that stocks will have smaller index weights because they are the smallest constituents of the value-weighted Russell 1000 index. Similarly, to the right of the cutoff are the largest stocks of the Russell 2000 index, so their weight is high.

It is important to note that it is not the discontinuity in index weights at the Russell cutoff that drives the variation in our benchmarking intensity measure.[23] The averaged benchmarking intensity plotted in panels B and D of Figure B2 in the Internet Appendix also has a discontinuity around the Russell cutoff and it is larger for larger stocks. This pattern is, however, driven by stock membership in different indexes as well as the variation in the ratio of AUM to Index MV. The latter is significantly larger to the right of the cutoff. Furthermore, larger stocks are more likely to be in the S&P 500 and S&P 400 indexes, which makes the curves downward sloping.[24]

In contrast to the literature, which typically accounts only for the Russell 1000 (blend) and Russell 2000 (blend), we consider all nine Russell indexes that contribute to the discontinuity at the cutoff. These indexes include the Russell 1000 (blend, value, and growth) and Russell Midcap (blend, value, and growth) to the left of the cutoff and the Russell 2000 (blend, value, and growth) to the right of it.[25] Style funds (i.e., value and growth) have historically had a larger market share on the Russell 1000 side of the cutoff, while blend funds have been more important on the Russell 2000 side. Moreover, we include funds benchmarked to the Russell Midcap – an index that spans stocks smaller than rank 200 within the Russell 1000. It assigns a higher weight to the

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[22] Extensive details about the Russell reconstitution are reported in Section B.9 of the Internet Appendix. The introduction of “banding” policy is discussed therein.

[23] If BMI of a stock were scaled differently, for example, using total benchmarked AUM instead of the stock’s market value, it would pick up the variation in index weights too.

[24] Even though S&P 500 is designed to represent 500 largest companies, we see that it includes some of the Russell 2000 stocks in our sample because of the differences in the S&P and Russell index construction methodologies. All our results are robust to excluding changes in S&P and CRSP indexes.

[25] This set does not include Russell indexes that do not contribute to the discontinuity near the 1000/2000 cutoff. These are, for example, Russell 3000, Russell 2500, and Russell Small Cap Completeness. However, all these indexes are still accounted for in the BMI; they just do not contribute to the discontinuity.

stocks near the cutoff than the Russell 1000 index because it excludes its 200 largest constituents. The AUM of funds benchmarked to the Russell Midcap in our sample is almost as high as that of all Russell 2000 funds (Figure B1 and Table B4 in the Internet Appendix).

Because of the updated reconstitution methodology, since 2007 there has been a market value region in which both Russell 1000 and Russell 2000 stocks are present. Figure B2, panel C, in the Internet Appendix plots the index weights around the cutoffs on the rank day (May 31st) in 2012. In that year, the band is between ranks 823 and 1,243. The discontinuity is still apparent: Russell 2000 stocks (in gray) have higher index weights. BMI mirrors the new pattern due to higher AUM/IndexMV ratio of the Russell 2000 indexes: the curve for Russell 2000 stocks lies above that for the Russell 1000 (Figure B2, panel D, in the Internet Appendix).

What we exploit in most of our analysis is the increase in BMI for stocks added to the Russell 2000 or the decrease in BMI for stocks just deleted from it. We argue that this variation is exogenous in Section 2.3.4.

We use a local linear regression approach, that is, our samples are restricted to the neighborhood of the cutoff (rectangular kernel). Our default bandwidth is 300 stocks around the cutoff and we report the robustness with respect to this choice for all our tests. For the period up to 2006, the cutoff rank around which we center the analysis is 1000. For each year starting from 2007, we compute the left and right cutoffs based on the Russell methodology.[26]

We also exclude stocks that move more than 500 ranks in one year. Our results are not sensitive to this filter but we prefer to keep it in place to ensure the comparability of stocks.

2.3.2 BMI and index effect. In this section, we show that a higher benchmarking intensity change leads to a larger price pressure (short-term return) upon an index inclusion event. This corresponds to Prediction 2 of our model. We first confirm the result in the literature that, on average, stocks added to the Russell 2000 index experience a positive return in June. Second, we present novel results suggesting that the size of the index effect is linked to the change of a stock’s BMI in the cross-section.

Similar to Chang, Hong, and Liskovich (2015), we see a positive return upon addition to the Russell 2000 and a negative return following

[26] Market value levels for the cutoffs we compute are reported in Table B1 in the Internet Appendix, where we almost fully match historical values reported by Russell on the website: https://www.ftserussell.com/research-insights/russell-reconstitution/market-capitalization-ranges.

deletion from it in our data.[27] Identification details and estimation results are presented in the Internet Appendix B.15.

Next, we show stocks with larger changes in BMI experience higher returns in June. We estimate the following specification:

$$\begin{aligned} Ret_{it}^{June} = & \alpha \Delta BMI_{it} + \zeta logMV_{it} + \phi' BandingControls_{it} \\ & + \xi Float_{it} + \delta' \bar{X}_{it} + \mu_t + \varepsilon_{it}. \end{aligned} \tag{7}$$

In this specification, Ret_{it}^{June} is the return of stock i in June of year t,[28] winsorized at 1%. ΔBMI_{it} is the difference between the BMI of stock i in May of year t and its BMI in June of the same year.[29] As we discuss later in Section 2.3.4, conditional on $logMV$, $BandingControls_{it}$ and $Float_{it}$ in May, the change in BMI due to the Russell reconstitution is exogenous. $logMV_{it}$ is the logarithm of total market value, the ranking variable as of May provided by Russell.[30] BandingControls$_{it}$ include dummies for being in the band, being in the Russell 2000, and their interaction in May of year $t-1$. $Float_{it}$ is the Russell float factor, a proprietary liquidity measure affecting index weight. $\bar{X}$ is a vector consisting of: 5-year monthly rolling β^{CAPM} computed using CRSP total market value-weighted index and 1-year monthly rolling average bid-ask percentage spread. We include β^{CAPM} because, as implied by our model, it affects expected returns. We supplement the controls with bid-ask spread to account for any stale information in the float factor. μ_t are year fixed effects. In the baseline analysis, we perform this estimation for all stocks within 300 ranks around the cutoff.

Table 2 presents the estimation results. Consistent with our model's Prediction 2, price pressure is the highest for stocks experiencing the largest increase in BMI, all else equal. Specifically, a 1% increase in BMI leads to a 27-bps higher return in June. To better understand the magnitudes, we report the estimates of price pressure in quartiles of BMI change. A stock in the top quartile has an 80-bps higher return in June

[27] We obtain lower magnitudes due to using proprietary ranking variable and a different methodology.

[28] Consistent with Chang, Hong, and Liskovich (2015), we find that June is the month when we expect the price pressure due to the Russell reconstitution. In Section 2.3.4, we also consider quarterly return, for April to June.

[29] Here, we consider the total change in BMI. Table B10 in the Internet Appendix uses change in BMI only from the Russell indexes that have the same cutoff. The correlation between the baseline change in BMI and the change in BMI from the Russell indexes is 93.2%. Furthermore, Internet Appendix Table B11 uses the baseline change in BMI but excludes stocks that contemporaneously moved S&P and CRSP indexes. The magnitudes are very similar. In future research, it is useful to be aware that there might be significant changes in BMI due to switches in other indexes, especially if one studies other index cutoffs.

[30] One useful alternative to the proprietary Russell market value is a ranking variable constructed by Ben-David, Franzoni, and Moussawi (2019) for Ben-David, Franzoni, and Moussawi (2018) using standard stock databases.

Table 2
BMI change and return in June

	Return in June					ΔBMI, %
	(1)	(2)	(3)	(4)	(5)	(6)
ΔBMI	0.26**	0.27**	0.28**			
	(2.55)	(2.66)	(2.74)			
$1(\Delta BMI$ quartile 1)				-0.010***	-0.010***	-3.02
				(-3.41)	(-3.39)	
$1(\Delta BMI$ quartile 2)				-0.004**	-0.005***	-0.39
				(-2.16)	(-2.67)	
$1(\Delta BMI$ quartile 3)				0.006***	0.005***	0.49
				(3.62)	(3.50)	
$1(\Delta BMI$ quartile 4)				0.008**	0.009***	3.24
				(2.26)	(2.64)	
Fixed effect	Year	Year	Stock & year	N	N	
$\bar{X}$ controls	N	Y	Y	N	Y	
Observations	14,549	14,549	14,549	14,549	14,549	
Adj. R^2, %	17.1	17.5	19.2	1.3	1.8	

This table reports the results of estimating Equation (7) for stocks in the full sample (1998-2018). The dependent variable is the winsorized return of stock i in June in year t (in columns 1–3 and demeaned by year in columns 4 and 5). The independent variable is ΔBMI_{it}, the change in the BMI of stock i between June and May of year t, or the dummies for its quartiles. All regressions include *logMV* (the logarithm of proprietary total market value), *Float* (proprietary float factor), *BandingControls* (being in the band, being in the Russell 2000 and their interaction in May). Columns 2, 3, and 5 include controls in $\bar{X}$ (β^{CAPM} and bid-ask spread). All controls are demeaned by year in columns 4 and 5. The constant is excluded. The bandwidth is 300 around both cutoffs. The last column reports the mean percentage ΔBMI_{it} in each quartile. t-statistics based on standard errors double-clustered by stock and year are in parentheses. $^*p<.1$; $^{**}p<.05$; $^{***}p<.01$.

relative to an average stock in that year, while a stock in the bottom quartile has a 110-bps lower return. These magnitudes are consistent with the average index effect size we get with a dummy approach in Table B6 in the Internet Appendix. The results are robust to alternative specifications and band widths[31] as well as using a deflated version of the change in BMI.[32]

Therefore, in contrast with the existing literature which looks at the average index effect for stocks added to the index or deleted from it, our analysis suggests that the size of index effect is proportional to the

[31] Column 1 in Table 2 only includes controls specific to the Russell index membership (*logMV* and banding controls). Column 3 adds stock fixed effects. Estimates for narrower bands are presented in Table B8 in the Internet Appendix. In unreported analysis, we ran the regression with terciles and quintiles of BMI change instead of quartiles and the results are similar.

[32] As discussed above, prices do not enter BMI. However, to alleviate any concern about the mechanical relationship between returns in June and change in BMI, we report the estimates of (7) using deflated change in BMI in Table B9 in the Internet Appendix. Specifically, deflated BMI is computed using index composition in June but with May prices; that is, it accounts for the new index membership of stock i, but not its return in June. Estimates are not significantly different from those in Table 2.

stock's BMI change.[33] It is a natural result because, as we will show in the following section, the change in BMI, in fact, allows us to compute the price elasticity of demand.

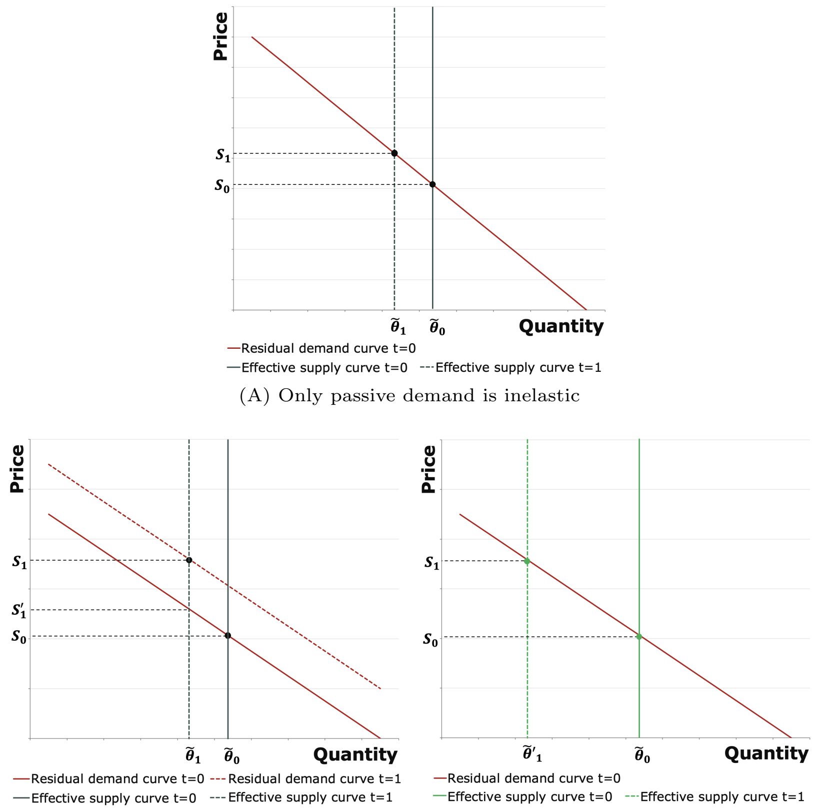
2.3.3 Implications for the price elasticity of demand. Our heterogeneous benchmarks model has nontrivial implications for the stock price elasticity of demand. Even though this parameter enters many macroeconomic models, the literature offers a rather wide range of its estimates (e.g., Wurgler and Zhuravskaya 2002) and sometimes focuses on the demand curves of different groups of investors. Importantly, previous research has studied single stock demand curves using only one benchmark (starting from Shleifer 1986) and, in most cases, assumed that only passive managers (index funds and ETFs) have inelastic demand.

For the experiment below, consider a one-stock version of our model ($N=1$). Additionally, to fix ideas, we separate fund managers into active and passive ones, as in footnote 13.

Most of the existing literature implicitly assumes that active investor demand (corresponding to benchmarked active managers and direct investors in our model) is fully elastic. If it is the case, the change in passive investor demand due to index reconstitution can be used as a shock to the supply of shares available to the rest of the market (effective supply). This is illustrated in Figure 3, panel A. When the passive investor demand increases, the effective supply reduces from $\tilde{\theta}_0$ to $\tilde{\theta}_1$, and the new equilibrium price is higher, $S_1 > S_0$. Using the change in passive benchmarked assets that corresponds to $\tilde{\theta}_1 - \tilde{\theta}_0$ and the size of the index effect, that is, $(S_1 - S_0)/S_0$, allows us to measure the price elasticity of demand of the rest of the market, typically computed as $(\tilde{\theta}_1 - \tilde{\theta}_0)/(S_1 - S_0) \times S_0/\tilde{\theta}_0$. We refer to the demand of the rest of the market as residual demand.

In our model, however, the standard approach will not recover the price elasticity of demand. The demand of passive managers benchmarked to index j for any particular stock is fully inelastic: $\theta_j^P = \omega_j$. Then, the effective supply of shares available to benchmarked active managers and direct investors is $\tilde{\theta} = \bar{\theta} - \sum_j \lambda_j^P \omega_j$. Because of benchmarking, the aggregate demand function of benchmarked active managers and direct investors features an inelastic component, the last

[33] Greenwood (2005) and Wurgler and Zhuravskaya (2002) perform a cross-sectional analysis for one benchmark and show that arbitrage risk is positively associated with the index effect for Nikkei 225 and S&P 500 stocks, respectively. Motivated by their work, we explore implications of arbitrage risk, as proxied by stock idiosyncratic volatility or short interest, for our results. We also find that the larger the arbitrage risk, the higher the index effect. Controlling for either of these proxies does not change the economic or statistical importance of BMI.



(B) Inelastic component in active demand (C) Using BMI to shift effective supply

Figure 3
Demand curves and index effect
This figure illustrates index reconstitution implications when only passive investors' demand reacts inelastically (panel A), active investors also have inelastic component in demand function (panel B), and when BMI change is used to shift effective supply (panel C). Effective supply in panels A and B is the total supply of shares, $\bar{\theta}$, minus the holdings of passive managers. In panel C, it additionally excludes the inelastic component of holdings of active managers. Residual demand is the total demand of the rest of the market, that is, (elastic) active managers and direct investors.

term in the equation below:

$$\Theta^{Active+Direct} = \frac{1}{\gamma} A^{-1}\Sigma^{-1}(\bar{D} - S) + \frac{b}{a+b}\sum_j \lambda_j^A \omega_j.$$

This equation as a function of S represents the demand curve in Figure 3, panel B. With benchmarking, an index inclusion event will trigger not only a parallel shift in effective supply to the right but also an upward parallel shift in residual demand. As illustrated in Figure 3, panel B, the observed price pressure will be $(S_1 - S_0)/S_0$, not $(S_1' - S_0)/S_0$. If we use the former price pressure with the change in passive demand to compute elasticities, we will conclude that the residual demand curve is steeper than it actually is. Therefore, if the world is close to our model economy, using the benchmarked passive asset change and the observed price pressure does not deliver the correct estimate of the price elasticity of demand. As shown in Section 3, active managers indeed have inelastic demand for stocks in their benchmarks and constitute, on average, 80% of asset managers in our sample.

What is the appropriate way to compute elasticity? One could separate elastic and inelastic components of active managers' demand and subtract the latter from the effective supply: $\tilde{\theta}' = \bar{\theta} - \left[\sum_j \lambda_j^P \omega_j + \frac{b}{a+b} \sum_j \lambda_j^A \omega_j\right]$. But in the data, we normally do not observe these components individually. In our model, however, BMI is exactly $\sum_{j=1}^J \left[\lambda_j^P \omega_j + \frac{b}{a+b} \lambda_j^A \omega_j\right]$. In other words, the change in BMI due to an index reconstitution event directly measures the shift in effective supply resulting from the inelastic response of both passive and active managers.[34] Figure 3, panel C illustrates this. The difference between the solid green and dashed green lines is the total change of effective supply due to the inelastic demand of both active and passive managers. Since this change in BMI is observable, it allows us to trace the correct slope of the residual (elastic) demand function.

The BMI-based estimate of elasticity can be derived from Table 2. Since $Ret^{June}/\Delta BMI = 0.27$, the corresponding price elasticity of demand is $-1/0.27 = -3.7$. This estimate is an upper bound for elasticity because our calculation of BMI is based on $\frac{b}{a+b} = 1$.[35] Our estimates are regression-based, we also compare them with those computed in Chang, Hong, and Liskovich (2015) in Internet Appendix B.22, which are based on averages.

[34] Data on manager compensation are generally not available. The only estimate of $\frac{b}{a+b}$ in the literature is provided in Ibert et al. (2018) on Swedish data, which exhibit structural differences to U.S. data. We assume that $\frac{b}{a+b} = 1$ in our main results but also provide a sensitivity analysis to this ratio below.

[35] This implies that active managers are strongly concerned about relative performance and the sensitivity of their compensation to absolute performance, a, is small. If a is higher, the inelastic component constitutes a smaller fraction of their demand for risky stocks. Therefore, they contribute less to the overall inelastic demand in the economy. In the language used in this section, it means that the shift in effective supply of a stock due to an index inclusion is smaller. In our calculation, the corresponding change in the stock's price is fixed, as estimated in the data. Hence, the same change in price is associated with a smaller change in demand, resulting in lower elasticity of residual (elastic) demand. For example, for $\frac{b}{a+b} = 0.6$ and $\frac{b}{a+b} = 0.8$, the price elasticity of the residual demand would be -2.50 and -3.13, respectively. If the shift in the dashed green line in Figure 3, panel C, is smaller, the residual demand curve (red line) must be steeper to result in the same (observed) price change. We discuss sensitivity of our results with respect to $\frac{b}{a+b}$ in more detail in Section 2.3.5.

Importantly, the heterogeneity of benchmarks has significant quantitative implications for the measures of elasticity relative to a single-benchmark case. Internet Appendix B.22 shows that the BMI change is the same as the change in total benchmarked assets used by Chang, Hong, and Liskovich (2015) only if a stock does not enter any benchmark other than the Russell 1000 and 2000 and if all its shares are floated. The literature has not considered the demand that stems from such large indexes as the Russell 1000 Growth and Russell Midcap,[36] and hence the change in demand is typically mismeasured. As shown in Table B16 in the Internet Appendix, accounting for all benchmarks in the same sample and with the same price pressure estimate as in Chang, Hong, and Liskovich, we obtain elasticity of -1.02 (30% less elastic than -1.46 in their paper).

Our estimates of the price elasticity of demand in this section should be viewed as an upper bound for several reasons. First, as explained above, our baseline calculation of the change in BMI assumes the strongest benchmarking incentives for active funds, that is, we use $\frac{b}{a+b}=1$. For any other $\frac{b}{a+b}\in(0,1)$, the change in BMI is lower and, therefore, elasticity is lower as well. We discuss sensitivity of our results to and provide a range of emprical estimates for $\frac{b}{a+b}$ in Section 2.3.5. Second, our estimates are based on the total change in BMI, which is larger than the *unanticipated* change in BMI, as explained in Section 1 and the appendix. If we could measure and use this unanticipated change in BMI, we would get even lower elasticity estimates. We demonstrate that in Section 2.3.5 and provide some evidence and further discussion in Internet Appendix B.19. Finally, not all of the changes in stocks' BMI, a theoretical measure, translate into changes in actual fund ownership. In practice, fund managers incur transaction costs, which often prevents them from trading as our frictionless model would predict. In the section that follows, we address this by providing estimates of the actual price impact, using ΔBMI as an instrument for stock ownership.

2.3.4 BMI as an IV. In this section, we estimate price impact of benchmarked investors' trades by examining directly how changes in their ownership of a stock affect the stock's price. Of course, as our theory illustrates, stock ownership and prices are jointly determined in equilibrium. In this section, we address this identification challenge with an instrumental variable approach. We propose using changes in BMI—a

[36] Benchmarked assets of the Russell indexes are shown in Table B4 in the Internet Appendix. Russell Value and Growth indexes are even larger than blend indexes in terms of the assets benchmarked to them. Moreover, since the Russell Midcap represents the smallest 800 stocks in the Russell 1000, the stock would exit it too. The size of the investor base of the Russell Midcap is just as large as that for the Russell 2000. It is therefore surprising that most of the literature studying the Russell cutoff has not taken all these indexes into account.

measure of inelastic demand that a stock attracts—as an *instrument* for changes in institutional ownership.[37] Changes in BMI should therefore predict how benchmarked investors rebalance their portfolios in response to a Russell index reconstitution (relevance condition). Intuitively, a change in BMI acts as a shock to the effective supply of a stock.

Our best proxy for the total ownership of a stock i at time t by benchmarked investors is institutional ownership, available from the Thomson Reuters Institutional Holdings (13F) Database, which reports total institutional holdings. Institutional ownership is defined as

$$IO_{it} = \frac{\sum_{j=1}^{\bar{J}} \lambda_{jt}\theta_{ijt}}{MV_{it}}, \tag{8}$$

where θ_{ijt} denotes the actual weight of stock i held by institutional investor j and $\bar{J}$ is the total number of institutional owners. The definition in (8) mirrors that of our BMI (Equation (5)), except that it has actual portfolio weights θ_{ijt} as opposed to benchmark index portfolio weights ω_{ijt}. We acknowledge that IO_{it} also contains holdings of non-benchmarked institutional investors, but as long as our instrument is sufficiently strong, this should not pose a problem for our estimation.

We would like to estimate the following structural equation:

$$Ret_{it}^{June} = \alpha \Delta IO_{it} + \epsilon_{it}, \tag{9}$$

where Ret_{it}^{June} is stock i's return in June of year t, winsorized at 1%, and ΔIO_{it} is the change in institutional ownership measured from March until June of year t.

The problem with estimating Equation (9) by ordinary least squares (OLS) is that the change in institutional ownership ΔIO is an equilibrium object and hence is endogenous. We therefore expect the OLS estimate of α to be biased.[38] To overcome this problem, we use an instrumental variable approach. Specifically, we use ΔBMI as an instrument for the change in effective supply of the stock. The main threat to this identification strategy is the presence of the index membership dummy in the expression for BMI (6), because index membership is potentially endogenous. However, a large literature uses membership in the Russell 2000 index as an instrument for institutional ownership in a similar setting (e.g., Crane, Michenaud, and Weston 2016; Glossner Forthcoming).[39] This literature argues that, after controlling

[37] We thank Moto Yogo for this insight, which has inspired this section.

[38] If in our data supply shocks dominate, we expect the estimate of β in a regression of prices on quantities, $P = \beta Q + \varepsilon$, to be positive, and negative if demand shocks dominate. If Q included only supply shocks, we could use OLS regression to measure the true price elasticity of demand. However, since Q in our setup is likely to include demand shocks, the regression will not produce a true coefficient β and the estimate will be biased towards zero.

[39] The consensus in this literature is that the Russell 2000 membership dummy is a weak instrument for institutional ownership, a point that we confirm below.

for factors that determine index inclusion, most importantly for the ranking variable ($logMV$) that Russell uses for index assignment at the end of May, the index membership dummy is exogenous. In Section 2.2, we have also acknowledged our concern that a change in stocks' liquidity could be a potential source of endogeneity of ΔBMI (because of stocks' float factors entering the expression for BMI), and to address that concern we control for the Russell proprietary stock-level float factor as of May. Finally, Appel, Gormley, and Keim (2019) advocate including banding controls, and we do so in our specification.[40]

Armed with the instrument and a set of controls, we perform the following two-stage least squares estimation. The first-stage regression is

$$\begin{aligned} \Delta IO_{it} =& \alpha_1 \Delta BMI_{it} + \zeta_1 logMV_{it} + \phi_1' BandingControls_{it} \\ &+ \xi_1 Float_{it} + \delta_1' \bar{X}_{it} + \mu_{1t} + \varepsilon_{it}. \end{aligned} \tag{10}$$

The second stage is

$$\begin{aligned} Ret_{it}^{June} =& \alpha \widehat{\Delta IO}_{it} + \zeta logMV_{it} + \phi' BandingControls_{it} \\ &+ \xi Float_{it} + \delta' \bar{X}_{it} + \mu_{2t} + \eta_{it}. \end{aligned} \tag{11}$$

$logMV_{it}$ is the logarithm of total market value, the ranking variable as of May provided by Russell, $Float_{it}$ is the Russell float factor, μ_{1t} and μ_{2t} are year fixed effects, and $\bar{X}_{it}$ and $BandingControls_{it}$ are the vectors of controls, as specified before. We perform the estimation in the neighborhood of 300 ranks around the cutoffs. By estimating this model, we aim to uncover the price impact of the actual change in institutional ownership, which is typically different from what is predicted based on ΔBMI_{it}. In reality, institutional investors do not hold all stocks in their benchmarks because of, for example, trading costs, from which our model abstracts.

The reason we are reluctant to use mutual fund and ETF ownership instead of institutional ownership in (10) is that a change in BMI due to index reconstitution should affect all benchmarked institutional investors (e.g., pension funds), not only mutual funds and ETFs, and therefore the exclusion restriction that ΔBMI affects the outcome variable only through changes in fund ownership is potentially violated.

To further alleviate concerns about the possible endogeneity of ΔBMI, we conduct overidentifying restrictions tests. Specifically, we use two instruments in the first-stage regression (10): ΔBMI and D^{R2000}, with the latter being the index membership dummy used as an

[40] There is one cutoff, at rank 1,000, before 2007, and two cutoffs afterwards. We explained this in detail in Section 2.3.

instrument for institutional ownership changes in the related literature cited above. Since with two instruments our model is overidentified, we can implement the Hansen J test. If the model with two instruments passes the J test, we can view this as statistical evidence that ΔBMI is (conditionally) exogenous.

Table 3 reports our results. First, the OLS estimate of the effect of the change in institutional ownership on stock returns is clearly biased. We therefore focus on the 2SLS estimates. The reported F-statistics indicate that the first stage specifications with one (ΔBMI) and two instruments (ΔBMI and D^{R2000}) are both strong. The reason for the higher t-statistic on ΔBMI relative to that on the dummy is that the former offers continuous treatment, while the dummy is a coarse binary variable. Consistent with this observation, the F-statistic of the first-stage regression, in which we include only the index membership dummy D^{R2000} and not ΔBMI, is lower than the conventional value of 10.[41] Although it is a coarse instrument, the index membership is conditionally exogenous and hence we are able to run the test of overidentifying restrictions to determine whether ΔBMI is a valid instrument. With a p-value of 19%, the Hansen J test cannot reject the null that that both of instruments are exogenous (conditional on $logMV$ and other controls).[42]

The estimates of the price impact in the specifications with one and two instruments are essentially the same, given by 1.47.[43] It is instructive to compare our estimates to those in the related literature. Recent papers using the demand system approach to asset pricing, proposed in Koijen and Yogo (2019), estimate price impact at an investor group level. Koijen and Yogo document that the aggregate price impact varies over the business cycle and ranges from 2 to 4. Our estimates in Section 2.3.3, obtained via a different method, are within their confidence intervals. The estimate in this section corresponds to the price elasticity of demand of households (owners residual to institutions whose market share is about 30%) of $1/1.47*1/30\%=2.26$. It is slightly higher than the elasticity in Koijen and Yogo (2019), which is closer to one. A potential argument for the difference is that we are performing our estimation in a small neighborhood around the Russell 1000/2000 cutoff, and stocks close to this cutoff are more substitutable in investor portfolios than large stocks like Apple and Microsoft.[44] In

41 See Stock and Yogo (2002) for details.

42 This is a conservative p-value as the J test is run under the assumption of heteroscedastic and autocorrelated (HAC) disturbances.

43 Our estimates are similar for a narrower bandwidth. We report them in Table B13 in the Internet Appendix.

44 If we estimate specifications (10) and (11) using changes in mutual fund and ETF ownership as opposed to changes in institutional ownership, we get a higher estimate of

Table 3
Change in BMI as an instrument for change in institutional ownership

	Return in June, %				Return in April-June, %
	OLS			2SLS	
	(1)	(2)	(3)	(4)	(5)
A. Second-stage estimates					
ΔIO, %	0.09***	2.27	1.46**	1.47**	2.26**
	(3.75)	(1.44)	(2.55)	(2.57)	(2.80)
B. First-stage estimates					
ΔBMI, %			0.20***	0.19***	0.19***
			(5.90)	(6.34)	(6.43)
D^{R2000}		0.85***	-0.15		
		(2.78)	(-0.54)		
F-stat (excl. instruments)		7.73	20.07	40.20	41.41
Hansen J test, p-value			.19		
Controls	Y	Y	Y	Y	N
Observations	12,862	12,862	12,862	12,862	12,862

This table reports α_1 and α from estimating (10) and (11), respectively, in the full sample period (1998-2018). Bandwidth is 300 stocks around the cutoffs. The dependent variable is return in June. ΔIO the change in total institutional ownership of stock i from March to June in year t. Specifications in (1)-(4) include $logMV$ (the logarithm of proprietary total market value), $Float$ (proprietary float factor), $BandingControls$ (being in the band, being in the Russell 2000 and their interaction in May), $\bar{X}$ (β^{CAPM} and bid-ask spread), and year fixed effects. Specification in (5) includes year fixed effects only. Hansen J test is performed under the assumption of HAC disturbances. In parentheses are t-statistics based on standard errors double-clustered by stock and year. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$.

general, demand of the remaining investors in the market, primarily households and some hedge funds, which in our model are represented by direct investors, is quite elastic because they do not face institutional constraints or compensation contracts that introduce inelastic elements in their demand functions.

One may question our analysis by saying that the price impact should be driven by the *unanticipated* change in BMI, that is, $\Delta BMI - E^{*}[\Delta BMI]$, as opposed to ΔBMI, as explained in Section 1 and the appendix. Our approach to estimating demand elasticities in this section is robust to this criticism. As long as ΔBMI is a valid instrument, our estimates of the demand elasticity of households should not change.

One drawback of the above approach to estimating price impact is that 13F institutional ownership is not observed at a monthly frequency, and so the periods over which we measure returns and changes in ownership are not perfectly aligned. An advantage is that this variable

price impact, around 2.6. However, this estimate should be treated with caution because it attributes all of the price impact to mutual funds and ETFs, while some of it may come from other benchmarked institutional investors, such as pension funds.

accounts for any rebalancing in anticipation of changes in BMI, but for the purposes of measuring price impact, we would have liked to use the change in ownership in June. For robustness, we also run a specification, in which as a dependent variable we use stock return from April to June, that is, for the same period as the change in ownership. In this specification, however, we cannot use our proprietary controls as they already reflect returns in April and May, and so we drop them. We report the estimated price impact in Table 3, column 5, and it is not statistically different from our main estimate in column 4.[45]

Some of the discrepancy between the ownership predicted by BMI and the actual ownership is driven by so-called ”optimized sampling”. Optimized sampling is a portfolio construction technique in which ex ante tracking error is balanced with expected transaction costs.[46] It directly interferes with the incentives to hold the benchmark portfolio. In the presence of transaction costs, funds no longer hold benchmark securities proportionally to benchmark weights. Rather, they typically hold the largest stocks with benchmark weights, completely omit the smallest and some mid-range stocks, and overweigh most of the mid-range stocks (see the illustration in Figure B4 in the Internet Appendix). Optimized sampling done by active funds is reflected in lower $\frac{b}{a+b}$. If some passive funds were to engage in optimized sampling (Appendix Figure B5 illustrates such a case), that would make the coefficient for passive AUM lower than one. Therefore, optimized sampling is another reason our estimates of price impact represent a lower bound.

2.3.5 BMI adjusted for fund activeness. In our baseline BMI in (5), we treat passive and active funds symmetrically. Our model, however, implies that passive and active funds should contribute to BMI differently. The weighted BMI is

$$BMI^w = BMI^{Passive} + \frac{b}{a+b} BMI^{Active}. \tag{12}$$

As earlier, $BMI^{Passive} = \sum_j \lambda_j^P \omega_j$ and $BMI^{Active} = \sum_j \lambda_j^A \omega_j$ (see footnote 13 and Section 2.3.3). The parameter $\frac{b}{a+b}$ measures fund activeness. It is equal to zero for a fund that does not have any benchmarking ($b=0$) and equals to one in the limit of $b \to \infty$ (the fund

[45] In Table B14 in the Internet Appendix, we report estimation results for specifications in columns 1–4 of Table 3 but without controls. The coefficients are lower but not statistically different. Furthermore, the banding controls and $logMV$ are important for the magnitude of the coefficient. These are Russell’s proprietary variables that we use to ensure conditional exogeneity. Accordingly, the Hansen J test rejects the model when they are removed.

[46] In practice, transaction costs are an important consideration. Not buying an asset in the benchmark saves on transaction costs but increases the manager’s tracking error relative to the benchmark. Optimized sampling addresses this trade-off.

is a ‘closet indexer’). Parameters a and b come from the fund manager’s compensation contract. Unfortunately, detailed data on compensation contracts of U.S. mutual fund managers is not available, and contract details documented in Ma, Tang, and Gómez (2019) are not sufficient to pin down $\frac{b}{a+b}$. We therefore attempt to determine $\frac{b}{a+b}$ from theory and based on a revealed preference argument (i.e., fund managers’ observed portfolio choice). In the main text, we do so for the weighted BMI implied by our model (Equation (12)), and in Internet Appendix C, we generalize the model and consider active fund manager heterogeneity.

We first explore how our aggregate elasticity estimates change as we vary $\frac{b}{a+b}$. Table 4 presents the sensitivity of price impact α in equation (7) to the values of $\frac{b}{a+b} \in [0,1]$. As we decrease $\frac{b}{a+b}$, the overall shift in effective supply is smaller, so for the same fixed change in stock price, the estimate of price impact has to go up. This is exactly what we see in the table. The point estimate for $\frac{b}{a+b}=0$ is slightly lower than that for $\frac{b}{a+b}=0.2$, but not statistically different. In unreported tests, we confirm that this nonmonotonicity in Table 4 is most likely due to the small relative size of passive AUM tracking the Russell indexes at the beginning of our sample (only 2% of all AUM) and disappears if we restrict the sample to after 2001.

At the end of Section 2.3.3, we have highlighted that using the total as compared to *unanticipated* change in BMI will have implications for the price impact estimates. In Table 4, we therefore report the price impact estimates for both the total change in BMI and unanticipated change in BMI. It is challenging to accurately measure $\Delta BMI - E^*[\Delta BMI]$, that is, how much of the BMI change is not anticipated, and in Table 4 we assume that it is around 50% of the total change, ΔBMI.[47] This effectively increases price impact by a factor of two. If the future literature is able to pin down this unanticipated component, our estimates can be scaled accordingly. Our goal in Table 4 is simply to give a plausible set of estimates that account for anticipated changes in BMI.

Table 4 gives a useful insight into the effect of the parameter $\frac{b}{a+b}$ on the estimate of price impact. However, it does not pin it down. One way to pin down the level of $\frac{b}{a+b}$ is to rely on a theoretical argument, coupled with a calibration. Kashyap et al. (2021) use data on institutional ownership of the S&P 500 and the total market index in 2017 as well as standard asset pricing moments to calibrate an economy similar to ours, but without passive managers. They find that $\frac{b}{a+b}$ is 0.82 in their economy. This number should be translated into a weighted average of

[47] In Internet Appendix B.19, we provide suggestive evidence for anticipatory price pressures in months before the reconstitution, demonstrating that at least some part of the total change in BMI is expected by the markets.

Table 4
BMI change and return in June, varying $\frac{b}{a+b}$

	α estimate				Implied elasticity	
$\frac{b}{a+b}$	ΔBMI^w (1)	$0.5\times\Delta BMI^w$ (2)	t-statistic (3)	Adj. R^2, % (4)	ΔBMI^w (5)	$0.5\times\Delta BMI^w$ (6)
1.0	0.27**	0.54**	(2.66)	17.53	-3.69	-1.85
0.8	0.32**	0.65**	(2.64)	17.51	-3.09	-1.54
0.6	0.40**	0.81**	(2.62)	17.49	-2.48	-1.23
0.4	0.53**	1.06**	(2.58)	17.44	-1.89	-0.94
0.2	0.74**	1.47**	(2.50)	17.34	-1.36	-0.68
0.0	0.72**	1.45**	(2.29)	17.04	-1.38	-0.69

This table reports the results of estimating Equation (7) for stocks in the full sample (1998-2018) using different weights $\frac{b}{a+b}$ when computing ΔBMI^w. The specification is the same as that in column 2 of Table 2. The dependent variable is the winsorized return of stock i in June in year t. The independent variable is ΔBMI^w_{it} in column 1 and $0.5\times\Delta BMI^w_{it}$ in column 2. The values in columns 3 and 4 are the same for estimates in columns 1 and 2. Columns 5 and 6 report the aggregate elasticities implied by the estimates of α in columns 1 and 2, respectively, computed as $-1/\hat{\alpha}$. t-statistics are based on standard errors double-clustered by stock and year. $^*p<.1$; $^{**}p<.05$; $^{***}p<.01$.

1 for passive assets and $\frac{b}{a+b}$ for active, implying that $\frac{b}{a+b}=0.72$ in our economy. Furthermore, when endogenizing contract parameters a and b, Kashyap et al. (2020) show that, with imperfect risk sharing, $\frac{b}{a+b}$ lies between 0.5 and 1.

In Appendix Table C7, we report our IV-based price impact estimates from Section 2.3.4 for different values of $\frac{b}{a+b}\in[0,1]$. We expect them to be the same if ΔBMI is a good instrument. We find that the estimates are very similar for $\frac{b}{a+b}>0.4$. Some discrepancy below that level comes from the low fraction of passive AUM in the early part of the sample. Furthermore, the explanatory power of our instrument for institutional ownership, as measured by the first-stage R^2, is maximized for the values of $\frac{b}{a+b}$ between 0.4 and 0.6.

In Internet Appendix C, we additionally allow for active fund manager heterogeneity. In our model, a complicated nonlinear relationship exists between any conventional measure of activeness, such as Active Share (Cremers and Petajisto 2009), and $\frac{b}{a+b}$. Because of this, we instead use a measure from the following regression approach. Specifically, we estimate a coefficient in the regression of fund portfolio weights on its benchmark portfolio weights, to parallel Equation (2). We find that the AUM-weighted average coefficient for all active funds is 0.57.[48] At the same time, the coefficients monotonically increase from 0.23 to 0.72 for similarly sized groups of funds ranked by their Active Share. This is consistent with the variation in Active Share itself that Cremers and Petajisto document. As is, such a regression suffers from endogeneity,

[48] AUM weighting is implied by our model. Estimates from an OLS regression are similar. See Internet Appendix C for details.

which may be caused by excluding the mean-variance portfolio (the first term in Equation (2)) or other potential stock-level time-varying characteristics that are not present in our model, such as liquidity, that may simultaneously affect benchmark and portfolio weights. That is why the reported estimates should be interpreted as correlations. Several further disadvantages to using these estimates as inputs to the weighted BMI are discussed in detail in Internet Appendix C. With the currently available compensation data and caveats to our empirical approach to evaluating $\frac{b}{a+b}$, we cannot do justice to active funds heterogeneity in BMI^w in this paper. One alternative avenue to pursue would be to employ the demand-system approach of Koijen and Yogo (2019) to estimate heterogeneous parameters $\frac{b}{a+b}$, fund by fund. This estimation approach is structural and it requires an investor-level specification of demand curves. The additional feature of the demand curves in our model, relative to those in Koijen and Yogo, is the presence of benchmark-tracking concerns in Equation (2). One could generalize the Koijen and Yogo's empirical specification to capture these additional elements along the lines of Koijen, Richmond, and Yogo (2021). We will leave this analysis for future research.

Our preferred weighted BMI specification is the one without heterogeneity across active funds, for several reasons. First, the only heterogeneity in our model is between passive and active funds. The second consideration is parsimony. Finally, we need to rely on considerably fewer assumptions to compute it, making our measure more transparent and easier to replicate (e.g., we do not need to assume a specific distribution of $\frac{b}{a+b}$ across active funds or estimate this parameter, which adds noise and limits our sample because holdings data are not available for all funds). While our investigation in Internet Appendix C suggests considerable heterogeneity across active funds, if one were to commit to an estimate of $\frac{b}{a+b}$ of a representative active fund, it would be around 0.57.

Finally, in Internet Appendix C, we fully explore how our results change under the assumption of heterogeneity between active and passive funds, as in Equation (12). We reestimate all our empirical specifications (in Sections 2.3.1 and 2.3.4) using two values of $\frac{b}{a+b}$: (1) a theory-implied calibrated value of $\frac{b}{a+b}=0.72$, based on Kashyap et al. (2021), and (2) an empirical one, based on our estimated average sensitivity of fund weights to benchmark weights ($\frac{b}{a+b}=0.57$). In brief, the results of the reestimation are as follows. The price impact estimates from the reestimated Table 2 are higher (meaning that demand elasticities are smaller), due to a lower quantity change ($\Delta BMI^w < \Delta BMI$) for the same price change. We would not expect the IV-based elasticity estimates to change, and indeed they are virtually the same as in the main text (Table 3).

3. Benchmarking Intensity and Fund Ownership

Starting with Gompers and Metrick (2001), empirical literature has documented a range of effects of institutional trading and ownership on stock prices. A recent strand of literature looks into the effects of ownership on corporate outcomes. There has been little research, however, on benchmarking-induced ownership.

Benchmarking intensity reflects the incentives elicited by the contracts of asset managers, both active and passive. In this section, we show that both investor types have a considerable fraction of holdings concentrated in their benchmarks and that they rebalance stocks relevant for *their* benchmarks around the Russell cutoffs. That is, we document a heterogeneity of investor habitats dictated by their benchmarks, reflecting their inelastic demand for stocks in these benchmarks.

3.1 Net purchases of index additions and deletions

Earlier studies documented that index funds and ETFs buy additions to and sell deletions from their benchmarks. We argue that this list is incomplete and that active managers engage in the same behavior but detecting it requires granular data on their benchmarks.

To see which funds rebalance additions and deletions, we estimate the following equations at a stock level, which in changes is:

$$\begin{aligned}\Delta Own_{ijt} = {} & \alpha_{1j} D_{it}^{R1000\rightarrow R2000} + \alpha_{2j} D_{it}^{R2000\rightarrow R1000} + \zeta_j logMV_{it} \\ & + \xi_j Float_{it} + \delta_j' \bar{X}_{it} + \mu_{jt} + \epsilon_{ijt}, \qquad (13)\end{aligned}$$

and in levels is:

$$\begin{aligned}Own_{ijt} = {} & \alpha_j D_{it}^{R2000} + \psi_j Own_{ijt-1} + \zeta_j logMV_{it} \\ & + \phi_j' BandingControls_{it} + \xi_j Float_{it} + \delta_j' \bar{X}_{it} + \mu_{jt} + \epsilon_{ijt}. \qquad (14)\end{aligned}$$

In the above equations, $D_{it}^{R1000\rightarrow R2000}$ equals one when stock i is moved from the Russell 1000 to Russell 2000 on the reconstitution day in June of year t. Likewise, $D_{it}^{R2000\rightarrow R1000}$ is 1 when the stock is moved from the Russell 2000 to Russell 1000. D_{it}^{R2000} is 1 when stock i belongs to the Russell 2000 on the reconstitution day in June of year t. ΔOwn_{ijt} is the change in the fraction of shares outstanding of stock i owned by all funds with benchmark j aggregated into a single portfolio from March to September of year t. We further split them by type (active/passive), for example, active funds benchmarked to the Russell 1000 index. Own_{ijt} and Own_{ijt-1} are measured in September and March of year

t, respectively. We perform our analysis on the changes in ownership from March to September because it is based on quarterly filings and it is in line with most of the previous studies (e.g., Appel, Gormley, and Keim 2016). Because changes in the ownership share are more difficult to detect for fund groups with smaller AUM, we also report the results for extensive margin, with the trade dummy used as a dependent variable. Own_{ijt} is the same in levels: fraction of shares outstanding owned or a dummy for whether the aggregate fund portfolio benchmarked to index j owns it or not. All other variables are defined as earlier.

Conditional on $logMV$, dummies $D^{R2000 \to R1000}$ and $D^{R1000 \to R2000}$ represent an exogenous change in index membership.[49] We confirmed that the results are equivalent to using a 2SLS estimator, with index membership instrumented with a prediction as of the rank date in May.[50] Hence, our results identify the effect of addition to or deletion from an index without a concern that an omitted variable might be driving both membership in the index and the change in ownership of funds benchmarked to that index.

We estimate Equations (13) and (14) at a stock level for each aggregate portfolio of funds with the same benchmark and distinguish between active and passive funds. For example, we run a separate regression for the change in the ownership share of the active Russell 1000 funds. In this example, the interpretation of α_1 is the change in their ownership share due to the stock's addition to the Russell 2000 index (and its deletion from the Russell 1000 index group, i.e., the Russell 1000 blend, Russell Midcap blend, and their value and growth counterparts).[51]

Table 5 documents that both passive and active funds rebalance additions and deletions. We report the most conservative results with double-clustered standard errors. Consistent with the literature, we find highly significant stock ownership changes for passive funds in line with their benchmarks. For example, passive funds benchmarked to the Russell 2000 purchase 77 bps of shares of stocks added to the Russell 2000. These funds also sell deleted stocks in similar proportions (84 bps). At the same time, we see that active funds benchmarked to the Russell 2000 also sell deletions, decreasing their ownership share by 55 bps. Another example is that active funds benchmarked to the Russell Midcap sell, on average, 26 bps of deleted shares (from Russell 1000 and Midcap) and buy 39 bps of the added ones. These magnitudes are large

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49 This is argued, for example, in Schmidt and Fahlenbrach (2017). Similarly, conditional on $logMV$ and $BandingControls_{it}$, index membership dummy D^{R2000} is exogenous.

50 We report the results of the prediction step in Table B5 in the Internet Appendix.

51 We explore even more granular rebalancing by style in Section B.29 in the Internet Appendix.

given the average ownership levels of aggregate portfolios of funds with the same benchmark.

Table 5
Rebalancing of additions and deletions, by benchmark and fund type

	Change in the aggregate ownership of funds with the same benchmark					
	Stocks ranked <1000				Stocks ranked >1000	
Benchmark	Russell 1000		Russell Midcap		Russell 2000	
Fund type	Active	Passive	Active	Passive	Active	Passive
A. Change in ownership share						
$D^{R2000 \to R1000}$	0.122***	0.105***	0.394***	0.113***	-0.546***	-0.840***
	(2.97)	(3.60)	(4.41)	(3.16)	(-4.95)	(-4.18)
$D^{R1000 \to R2000}$	-0.101**	-0.100***	-0.264***	-0.103***	0.123	0.771***
	(-2.22)	(-3.29)	(-3.69)	(-2.90)	(1.47)	(3.61)
B. Change in holding status						
$D^{R2000 \to R1000}$	0.356***	0.459***	0.288***	0.437***	-0.319***	-0.921***
	(7.05)	(7.93)	(5.02)	(5.20)	(-7.13)	(-11.47)
$D^{R1000 \to R2000}$	-0.298***	-0.828***	-0.237***	-0.694***	0.113**	0.829***
	(-4.68)	(-5.84)	(-5.62)	(-4.27)	(2.39)	(6.87)
C. Ownership share						
D^{R2000}	-0.032	-0.067**	-0.136**	-0.065*	0.267**	0.653***
	(-1.05)	(-2.42)	(-2.24)	(-1.90)	(2.50)	(3.01)
D. Holding status						
D^{R2000}	-0.177***	-0.351***	-0.057***	-0.651***	0.002	0.613***
	(-8.91)	(-6.72)	(-4.92)	(-4.72)	(0.45)	(13.06)

This table reports α_{1j} and α_{2j} from estimating (13) (panels A and B) and α_j from estimating (14) in the full sample period (1998-2018). Estimation is performed at a stock level for an aggregate portfolio of funds benchmarked to index j (active or passive). Bandwidth is 300 stocks around the cutoffs. The dependent variable in panel A is the change in fraction of shares owned by the respective aggregate portfolio in stock i from March to September in year t. In panel B, it is the direction of the trade of the group (1 for buy, 0 for no trade, and -1 – for sell). In panel C, it is the ownership share in September. In panel D, it is a dummy that equals 1 if the stock is held by the aggregate portfolio in September and 0 if it is not. Regressions in both panels C and D additionally control for the value of the dependent variable in March and include *BandingControls* (being in the band, being in the Russell 2000 and their interaction in May). All regressions include *logMV* (the logarithm of proprietary total market value), *Float* (proprietary float factor), $\bar{X}$ (β^{CAPM} and bid-ask spread), and year fixed effects. In parentheses are t-statistics based on standard errors double-clustered by stock and year. $^{*}p<.10$; $^{**}p<.05$; $^{***}p<.01$.

On the extensive margin, the benchmark-driven rebalancing is even easier to detect. As Panel B of Table 5 reveals, active funds are likely to sell deletions from their benchmarks and buy additions. Panel D shows that all aggregate fund portfolios in our sample are more likely to hold stocks added to their benchmarks and less likely to hold the deleted stocks.

Our results are robust to alternative specifications, varying band widths and controlling for the polynomials of the ranking variable, $logMV$.[52]

Because the composition of active funds holding the added stock changes significantly, the incentives active managers to monitor this stock may change too. The new literature on passive ownership and corporate governance relies on continuity of active ownership around the Russell cutoff.[53] In Table B22 in the Internet Appendix, we use the approach of Appel, Gormley, and Keim (2019) on our data. One cannot detect a discontinuity in the *total* ownership of active mutual funds. However, the discontinuities are apparent when active funds are split by benchmark.[54] This may affect corporate governance, casting doubt on the plausibility of the exclusion restriction in the growing number of studies on the effects of passive ownership. Our results highlight the importance of considering active funds' benchmarks when studying the implications of ownership changes around the Russell cutoff.

In Table B20 in the Internet Appendix, we further split active funds into less and more active within each benchmark group by median active share or tracking error and compare the changes in holding status of index additions and deletions across groups. We see that for all indexes, the point estimates are monotonic: more active funds are less likely to rebalance.

Overall, results in this section suggest that, in line with our theory, Russell benchmarks serve as both active and passive funds' preferred habitats. In the next section, we will argue that the same holds true for all important equity indexes in the United States.

3.2 External validity

As Robert Stambaugh (2014) pointed out in his 2014 AFA Presidential Address, U.S. mutual funds' tracking errors have been going down. In our data set, this trend is drastic. A simple average tracking error of active funds went down from 7% per annum in the early 2000s to below

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52 We report the results for a narrower band in Table B17 in the Internet Appendix. We add stock fixed effects in Appendix Table B18. Furthermore, Appendix Table B19 reports how the estimates change between the 1998-2006 subsample and the 2007-2018 subsample.

53 The list of papers includes but is not limited to: Appel, Gormley, and Keim (2016, 2019) and Schmidt and Fahlenbrach (2017).

54 Our findings do not contradict those of Appel, Gormley, and Keim (2016, 2019): because of the sheer size of the Russell 2000 passive funds, the total passive ownership is higher for stocks to the right of the cutoff.

4% in the 2010s.[55] For passive funds, these numbers have been below 2% and closer to 0.5%, respectively. Given that the share of passive funds grew significantly over the past two decades,[56] the overall industry tracking error is at its historical low.[57]

Exploiting the granularity of our data set, we characterize how close fund portfolios and returns are to their benchmarks. We aggregate assets of all funds with the same benchmark and of the same type (active or passive) into one portfolio. Table 6 reports characteristics of the most important aggregate fund portfolios in our sample. We compute the percentage of portfolio AUM invested in its benchmark stocks and the number of benchmark stocks held. In 2018, the average is high at 75% and 77%, respectively, for active funds. Both figures are close to 100% for passive funds.[58]

While there is some heterogeneity in portfolios of active funds benchmarked to the same index, Table 6 shows that, on aggregate, they resemble their benchmarks. From 1998 to 2018, the active shares and tracking errors went down across indexes,[59] on average decreasing from 65 to 51% and from 4.8 to 2.3%, respectively.[60] It is also reassuring to see that value-weighted individual funds' tracking errors also decreased from 8.4 to 4%. In line with our discussion of optimized sampling in Section 2.3.4, we see that the aggregate portfolio of funds with the same benchmark is more likely to include the largest 25 stocks in the index compared to the smallest stocks. It is particularly pronounced for active funds that hold all top-25 stocks and only 17 of 25 smallest stocks on average.

[55] This trend is shared by active funds across most important benchmarks, as illustrated by Appendix Figure B6.

[56] The assets of stock index mutual funds and ETFs now match that of active funds, according to Gittelsohn (2019).

[57] Active share is also decreasing over time in our sample.

[58] With the exception of the Russell 3000 Value portfolio, which is represented by one fund and smallest in size.

[59] The only exception is the active share of the S&P 400 portfolio, for which we only have derived index weights after 2002.

[60] Related literature often uses S&P 500 as a benchmark for all U.S. mutual funds to compute tracking errors instead of the actual fund benchmark. In unreported analysis, we see that the resultant tracking errors are several times larger than those using prospectus benchmarks.

Table 6
Characteristics of the aggregate portfolios of funds with the same benchmark

Benchmark index	Fraction of index stocks held, %	% of portfolio in index stocks	AUM, $ billion		Number of funds		Active share, %		Tracking error (fund average), %		Aggregate TE, %		No. top-25/ bottom-25 index stocks held
			1998	2018	1998	2018	1998	2018	1998	2018	1998	2018	
A. Active funds													
Russell 1000	95.1	97.6	12.5	82.9	14	31	58.8	47.7	9.3	4.0	7.9	2.9	25/24
Russell 1000 Growth	91.5	89.8	224.5	352.9	97	121	40.1	34.2	7.4	4.0	3.9	2.3	25/24
Russell 1000 Value	94.0	84.2	179.2	416.6	87	131	44.5	36.0	5.5	2.9	2.6	1.3	25/24
Russell 2000	96.4	66.2	29.0	135.4	86	126	61.9	51.9	9.7	4.6	4.4	2.2	25/25
Russell 2000 Growth	86.7	47.7	27.4	93.7	63	86	62.6	61.0	9.3	5.4	4.1	3.6	25/21
Russell 2000 Value	98.9	58.9	13.9	92.3	40	88	70.3	52.9	7.3	3.6	2.9	1.8	25/24
Russell 2500	86.1	78.7	9.5	30.7	10	37	81.7	68.1	7.2	4.4	3.4	2.9	25/14
Russell 2500 Growth	65.6	73.7	15.0	51.5	15	22	82.1	53.6	9.8	4.7	4.6	2.6	25/11
Russell 2500 Value	60.3	70.5		16.4		19		68.7		3.5		2.3	25/14
Russell 3000	57.2	95.7	9.8	63.0	15	40	75.9	38.8	8.0	2.6	5.7	1.2	25/0
Russell 3000 Growth	29.7	86.5	60.2	101.9	22	29	65.7	46.0	9.0	4.8	7.0	3.6	25/2
Russell 3000 Value	26.1	84.0	55.6	57.6	11	31	77.3	49.1	5.1	3.2	3.5	1.7	25/0
Russell Midcap	73.2	80.4	8.3	64.1	25	36	70.6	56.6	9.6	4.3	5.7	2.1	25/13
Russell Midcap Growth	92.8	67.5	50.7	159.3	60	63	68.8	52.8	10.3	4.0	5.1	2.1	25/24
Russell Midcap Value	91.4	69.6	17.9	140.7	22	54	77.0	48.9	8.5	3.3	5.2	1.7	25/23
S&P 400	67.4	30.4	7.9	32.1	16	15	69.0	77.4	10.6	4.6	7.0	3.1	21/16
S&P 500	99.4	87.0	651.9	1,574.5	340	362	34.7	30.1	7.5	4.7	4.0	1.7	25/25
Mean/total	77.2	74.6	1,373.4	3,465.5	923.0	1,291.0	65.1	51.4	8.4	4.0	4.8	2.3	25/17
B. Passive funds													
CRSP U.S. Large	98.9	99.9		20.1		1		1.2		0.1		0.1	25/25
CRSP U.S. Large Growth	99.9	100.0		80.6		1		0.2		0.1		0.0	25/25
CRSP U.S. Large Value	98.1	99.9		67.3		1		2.1		0.1		0.1	25/25
CRSP U.S. Mid	98.8	99.7		97.9		1		1.3		0.1		0.1	25/25
CRSP U.S. Mid Growth	100.0	100.0		12.4		1		0.0		0.1		0.1	25/25
CRSP U.S. Mid Value	97.9	99.5		18.0		1		2.5		0.2		0.2	25/25
CRSP U.S. Small	99.3	100.0		90.7		1		0.7		0.1		0.1	25/25
CRSP U.S. Small Growth	99.4	100.0		23.5		1		0.5		0.1		0.1	25/25
CRSP U.S. Small Value	99.2	99.9		30.9		1		0.9		0.1		0.1	25/25
CRSP U.S. Total	98.7	100.0		744.5		2		2.0		0.1		0.0	25/21
Russell 1000	99.0	99.7	1.2	37.1	1	14	36.6	6.7	4.6	0.4	4.5	0.2	25/24
Russell 1000 Growth	99.8	97.9		62.0		11		4.9		0.9		0.7	25/25
Russell 1000 Value	98.9	99.3		50.1		13		3.4		0.3		0.2	25/24
Russell 2000	99.1	99.4	1.0	59.5	5	18	11.7	2.7	2.3	0.4	1.7	0.2	25/25
Russell 2000 Growth	98.9	99.8		11.1		4		1.1		0.1		0.1	25/25
Russell 2000 Value	99.2	99.6		11.1		6		1.1		0.1		0.1	25/25
Russell 3000	98.9	99.9		13.2		9		4.4		0.7		0.5	25/25
Russell 3000 Value	27.1	88.3		3.9		1		31.4		1.1		1.1	23/0
Russell Midcap	98.9	99.5		19.7		5		3.0		0.2		0.1	25/24
Russell Midcap Growth	99.8	100.0		8.9		1		0.2		0.1		0.1	25/25
Russell Midcap Value	98.5	99.6		10.8		1		1.6		0.1		0.1	25/24
S&P 400	99.7	97.2		65.1		16		2.9		0.2		0.1	25/25
S&P 500	99.6	99.6	146.3	1,019.1	46	83	1.1	4.1	0.4	0.2	0.2	0.1	25/25
Mean/total	94.9	98.8	148.6	2,261.3	52.0	187.0	16.5	4.2	2.4	0.3	2.1	0.2	25/24

This table shows characteristics of the aggregate portfolios of active mutual funds (panel A) and passive mutual funds and ETFs (panel B). Each portfolio is a value-weighted sum of funds benchmarked to the respective index. Active share is for the aggregate portfolio. Tracking error is value-weighted across constituent funds, annualized. Aggregtate TE is for the aggregate portfolio. AUM and number of funds are as of June 1998 or June 2018. The last column is as of June 2018. The rest of the values are averages for the respective year. Only aggregate portfolios larger than $1 billion in assets are shown. Active share for S&P 400 in 1998 column is for 2002, from when we have the index weights.

Results in this section suggest that benchmarks define funds' preferred habitats.[61] Importantly, active funds look like preferred habitat investors as well. In line with our model, they hold a significant fraction of assets in benchmark stocks and rebalance additions to and deletions from their benchmarks.

4. Benchmarking Intensity and Stock Risk Premium

In this section, we explore the prediction of our theory that stocks with higher benchmarking intensities have lower expected returns. In particular, we find that a stock that experiences a conditionally exogenous increase (decrease) in its BMI due to the Russell reconstitution has a lower (higher) return for 1 to 5 years. We argue that this is not driven by a negative return momentum and future changes in cash flows or liquidity.

4.1 BMI and long-run returns

In this section, we show that a higher benchmarking intensity leads to lower returns in the long run. Specifically, stocks with a larger increase in BMI in year t significantly underperform up to year $t+5$.

Our goal is to test the negative relationship between benchmarking intensity and stock returns predicted by our theory. As explained in Section 2.3, the Russell cutoff provides a convenient setup because the change in BMI is conditionally exogenous.

As earlier, we employ a stock-level specification to estimate α:

$$\begin{aligned} Y_{it+h} = {} & \alpha \Delta BMI_{it} + \zeta logMV_{it} + \phi' BandingControls_{it} \\ & + \xi Float_{it} + \delta' \bar{X}_{it} + \mu_i + \mu_t + \varepsilon_{it}. \end{aligned} \tag{15}$$

In the above specification, the dependent variable, Y_{it+h}, is an average long-run return of stock i from September of year t over the investment horizon h.[62] Specifically, we consider the 12-, 24-, 36-, 48-, and 60-month excess returns, which are not risk-adjusted. ΔBMI_{it} is the change in BMI from May to June in year t.[63] We use our baseline BMI defined in Equation (5) here, and present the analysis for the weighted BMI, adjusted for fund activeness in Appendix C. μ_i are stock fixed effects

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[61] All our analysis is conditional on the benchmark in the manager's contract. Our model does not take a stand on how end investors pick the benchmark or fund to invest in. Possible rational explanations include the need to hedge endowment shocks of a particular type or to hedge displacement risk. Behavioral explanations include psychological foundations for why investors prefer growth over value, overreaction, and extrapolation of past returns.

[62] We measure long-run returns from September to avoid price pressure from potentially delayed rebalancing of index funds in July and August. Results for returns computed from July are reported in Appendix Table B23.

[63] As was shown earlier, it does not pick up the change in price in June.

to remove any unobserved constant heterogeneity.[64] All other variables are defined as earlier. Our samples are again restricted to stocks around the cutoff, we report results with band widths of 300 and 150.

Our dependent variable spans horizons from 12 months to 5 years. Some ambiguity surrounds what constitutes "the long run" in the literature. The IPO performance literature (following Ritter (1991)) typically defines it as 3 years. The long-run reversal literature (started by De Bondt and Thaler (1985)) uses horizons from 18 months to 5 years. In our case, an additional problem is posed by flippers, that is, stocks that switch from one benchmark to the other several times during the horizon that we are considering. Our model requires the stock's BMI to remain largely unchanged for the expected return result to play out as predicted.[65]

Table 7 documents the results of estimating Equation (15). As the coefficient on ΔBMI is significantly negative, stocks with an increase in benchmarking intensities have lower returns in the future. The effect persists for up to 5 years after the reconstitution.[66]

The magnitude of this effect is economically significant. To interpret the magnitude for an average added or deleted stock in our sample, we need to take into account the average size of ΔBMI for added and deleted stocks, 5.22% and -4.40%, respectively. Our baseline estimates imply that addition to the Russell 2000 results in around 2.8% lower return in the following year, while deletion from it leads to a 2.4% higher return. Magnitudes are roughly the same across specifications with different controls and for a narrower bandwidth. Panel E of Table 7 shows that after 2007, the magnitudes are not significantly lower.

Consistent with the model, this analysis shows that an increase in the size of the preferred habitat has a long-lasting effect on stock returns.[67] Interpreted through the lens of our model, this means that

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[64] They are expected to be more important for the long-run returns compared to the short-run tests in the first part of the paper. We report results with and without stock fixed effects.

[65] Our theoretical predictions concern stocks that joined a set of indexes and stayed in them until the end of the investment horizon. In unreported analysis, we see that our results are considerably stronger, both statistically and in magnitude, if we drop stocks that moved between the Russell 1000 and 2000 indexes more than once in the relevant horizon. However, excluding flippers introduces a selection bias. A stock that was added to the Russell 2000 index has to appreciate to come back to the Russell 1000 the next year. Therefore, by excluding flippers, we naturally exclude stocks with the most positive return realizations, which biases the estimate of α in (15) downward.

[66] Even though it might seem from the table that most of the effect is concentrated in the first 12 months after index reconstitution, the negative relationship is long term. To confirm this, we report Table B24 in the Appendix, which uses average returns over a future period as the dependent variable. It shows that the returns are lowest in the 1-12 months period, and they are significantly lower for the period between 13 and 24, and weakly lower between 25 and 36 as well as 37 and 48 months.

[67] Permanent, as long as the stock stays in the benchmark.

Table 7
BMI change and long-run returns

	Excess returns, average over horizon				
Horizon (months)	12	24	36	48	60
A: All baseline controls					
ΔBMI	-0.045**	-0.037***	-0.020***	-0.016**	-0.009**
	(-2.81)	(-3.63)	(-3.87)	(-2.75)	(-2.16)
Observations	13,813	12,318	10,928	9,731	8,633
B: Baseline controls without stock fixed effects					
ΔBMI	-0.039*	-0.034**	-0.016**	-0.015**	-0.010
	(-1.86)	(-2.50)	(-2.31)	(-2.18)	(-1.58)
Observations	14,351	12,800	11,388	10,091	8,988
C: $LogMV$, $Float$ and $BandingControls$ only					
ΔBMI	-0.039**	-0.034***	-0.020***	-0.016***	-0.011***
	(-2.69)	(-3.63)	(-4.52)	(-3.23)	(-3.15)
Observations	14,700	13,124	11,605	10,279	9,082
D: All baseline controls and a narrower band					
ΔBMI	-0.033**	-0.029***	-0.016***	-0.014**	-0.010**
	(-2.38)	(-3.18)	(-3.54)	(-2.81)	(-2.91)
Observations	7,640	6,830	6,078	5,378	4,743
E: All baseline controls and interaction with post-banding dummy					
ΔBMI	-0.044*	-0.046***	-0.020***	-0.015*	-0.009
	(-1.94)	(-3.35)	(-3.02)	(-1.84)	(-1.47)
$\Delta BMI \times D^{>2006}$	-0.001	0.017	0.001	-0.003	0.000
	(-.07)	(1.46)	(.09)	(-.35)	(.01)
Observations	13,813	12,318	10,928	9,731	8,633

This table reports the results of the regression of the long-run returns on change in BMI, ΔBMI, in the full sample (1998-2018). The dependent variable is an average monthly excess return from September in year t over the respective horizon. Panels A and B include all baseline controls, while panel C is the log total market value, the proprietary ranking variable, and the banding controls only. Panel E adds an interaction between ΔBMI and $D^{>2006}$, which equals one in years 2007-2018 and zero otherwise. In panels A, B, C, and E, we limit the sample to 300 stocks around the cutoffs. Panel D limits the sample to 150 stocks around the cutoffs. The baseline controls include $logMV$ (the logarithm of proprietary total market value), $Float$ (proprietary float factor), $BandingControls$ (being in the band, being in the Russell 2000 and their interaction in May), $\bar{X}$ (β^{CAPM} and bid-ask spread), and stock and year fixed effects. t-statistics based on standard errors double-clustered by stock and year are in parentheses. $^{*}p<.10$; $^{**}p<.05$; $^{***}p<.01$.

inelastic demand from the benchmarked institutions lowers the stock risk premium.

4.2 Robustness

4.2.1 Alternative explanations. Our model is static and it cannot speak to transitory effects of index inclusion on stock prices. In an extention of the model that incorporates additional periods, one could generate a partial or full reversion of the index effect. Suppose that there is not enough arbitrage capital in June to prevent the index effect. However, in the longer run, as arbitrage capital flows in, the index effect reverses and prices get closer to their fundamental values. This slow-moving capital theory was proposed by Grossman and Miller (1988) and Duffie (2010).[68] A dynamic version of our model in which direct investors do not rebalance immediately or end investors are slow to relocate their capital across benchmarks could capture this effect. Our long-run underperformance result is consistent with transitory effects, especially because the bulk of the long-run effect that we find comes from the first 2 years (see Table B24 in the Internet Appendix). One could also attribute it to a time-varying risk premiums due to the stocks' inclusion in other benchmarks over a 5-year horizon (and other changes to their BMIs).

It has been argued that the transition to the Russell 2000 increases passive ownership of a stock, which may have implications for corporate governance. The positive return in June could be a signal of an improvement in corporate governance that would take place in the future. The documented effects on corporate governance, however, seem to be mixed, with some metrics improving and some deteriorating.[69] Therefore, the expected cash flow or performance impact is not clear. Moreover, the majority of documented effects of Russell reconstitution on firm fundamentals are short-term: they are measured in the year following the reconstitution, while our main focus is on long-term returns.

Our model assumes that firms' cash flows are fixed and a change in BMI affects firm value through the discount rate, so we need to rule out the cash flow channel. We investigate whether the firms' cash flows change in response to the change in BMI. In particular, we regress the 3-year change in fundamental characteristics associated with cash flows on ΔBMI and our standard controls. Table B25 in the Internet Appendix describes the variables, and Table B26 reports the estimates. We see that

[68] The limits-to-arbitrage literature (see, e.g., Gromb and Vayanos (2010) for a survey) attributes the inability of arbitrageurs to fully absorb demand shocks (e.g., resulting from index inclusion) to capital constraints and other frictions that they face.

[69] See Heath et al. (2021), Appel, Gormley, and Keim (Forthcoming), Schmidt and Fahlenbrach (2017), and Appel, Gormley, and Keim (2016).

firms with a larger increase in BMI seem to have a weakly lower M/B ratio and weakly higher profitability. The latter is consistent with the literature (Appel, Gormley, and Keim (2016)), but both go against our finding that these firms underperform. In general, we find little evidence the change in BMI is related to future changes in accounting variables driving cash flows.

One may have a concern that stocks added to the Russell 2000 benefit from improved liquidity. Intuitively, if a stock has a higher BMI, it might be more subject to liquidity-based trading and, potentially, more available for lending. In Appendix Table B26, we also report whether turnover, short interest ratio, percentage bid-ask spread, and ILLIQ of Amihud (2002) change with BMI. We find that a change in short interest is positively related to the change in BMI. It is, in fact, consistent with our model: as stock price increases with BMI, direct investors are more likely to sell it (short). In practice, stocks with higher BMIs might have lower short-selling costs because of a larger securities lending supply by long-only funds. At the same time, turnover and liquidity measures are not related to BMI, and the loading on Pastor and Stambaugh (2003) liquidity factor does not change either.[70] Therefore, a decrease in the liquidity premium is unlikely to drive our findings.

Another alternative explanation for our long-run results could be that returns of firms that have transitioned to the Russell 2000 are lower because these firms have fallen on hard times and their cash flows are deteriorating. If this momentum continues, it is not surprising to see that the firms added to the Russell 2000 have lower future returns relative to firms that stayed in the Russell 1000. In unreported tests, we see that controlling for past returns only slightly lowers the estimates. Nonetheless, we took further steps to ensure this explanation is ruled out. We have checked explicitly whether our results are driven by future financial distress. First, in our data set, Altman z-scores do not change upon index reconstitution (Appendix Table B26). Moreover, excluding firms classified by Altman z-score as being ”in distress” or ”in the grey zone” does not change either the significance or magnitude of our results. Second, excluding firms that ever filed for bankruptcy or experienced credit rating downgrades does not affect the estimates.[71]

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[70] Although our model does not suggest any changes in risk factor loadings, in unreported tests we check if they are affected by the change in BMI. We find no robust changes in Fama-French-Carhart, Fama-French five-factor, or Pastor and Stambaugh (2003) loadings. This analysis involves estimating a regression of the five-year change in loadings on change in BMI with our standard controls. All loadings are 5-year computed from monthly rolling regressions of stock excess returns on factor returns available from Ken French’s website or WRDS, with a minimum of 2 years of data required for estimation.

[71] We have also experimented with excluding firms that had a rapid deterioration in their market value rank prior to reconstitution. While our baseline analysis excludes jumps of 500 ranks, we have tried excluding firms that lost even as little as 100 ranks. Our results remained qualitatively unchanged, albeit the magnitude of the effect was larger.

4.2.2 Further remarks on identification approach. Our identification approach avoids several problems that have been highlighted in the literature (e.g., Wei and Young (Forthcoming)). Specifically, we do not use June weights for assignment or sample selection. Moreover, our proprietary ranking variable alleviates questions regarding the conditional exogeneity assumption.[72]

Furthermore, the variation in BMI does not conflict with the known discontinuities around the Russell cutoff. That is, the local variation in total institutional ownership (IO), passive IO, benchmarked IO, and ETF ownership are implicit in the construction of our measure. They are also assumed to be time varying since the amount of capital linked to indexes changes (shown in Table B4 in the Internet Appendix) and new indexes emerge. Therefore, BMI is a unifying measure that implies some variation in all aforementioned variables; whether it is more pronounced in a particular sample depends on the distribution of assets between benchmarks.

5. Conclusion

In this paper, we propose a measure that captures inelastic demand for a stock – benchmarking intensity. Exploiting a variation in the benchmarking intensity of stocks moving between the Russell 1000 and Russell 2000 indexes, we document the effects of a change in BMI on stock prices, expected returns, ownership, and demand elasticities.

Our measure reflects the inelastic demand of both active and passive funds for stocks in their benchmarks. According to our preferred habitat view, active funds are not genuinely active investors. Rather, they simply deviate from their benchmarks to a larger extent than passive funds. In our sample, active funds own large fractions of shares outstanding, higher than passive funds, and that is why they contribute significantly to the aggregate inelastic demand for benchmark stocks. On average, a large part of active funds’ holdings is in benchmark stocks, both in terms of the number of stocks and AUM share. We find evidence of the inelastic demand of active managers in the ownership data. Studying the rebalancing around the Russell cutoff, we document that both active and passive managers buy additions to their benchmarks and sell deletions. Because of this, our framework has important implications for measuring the price elasticity of demand for stocks. The demand elasticities differ from those in the previous research based on index inclusions because the literature has not accounted for the inelastic component in active managers’ demand and for the heterogeneity of benchmarks.

[72] As we discussed above, the assignment prediction quality is very high.

Our model abstracts from transaction costs but, in practice, they are important. To save on transaction costs, fund managers often engage in so-called ”optimized sampling”, which leads to exclusion of some of the smallest stocks in the benchmark from the funds’ portfolios. However, changes in BMI still represent a strong instrument for changes in institutional ownership and can be used for estimating demand elasticities. Our measure of BMI can be further refined by accounting for assets of benchmarked investors other than mutual funds and ETFs. This is likely to make BMI stronger as an instrument.

Appendix: Uncertain Benchmark Weights

In this appendix, we derive Equations (1) and (2) and, more importantly, add one period to the model, $t=-1$, in which investors are uncertain as to whether a stock is going to be included in a benchmark or not. Our goal is to show how expectations about the stock’s potential entry in the benchmark influence its returns.

Specifically, our model now features period $t=-1$, in which there is no cash flow news and in which stock 1’s weight in the benchmarks of fund managers is uncertain. The vector of benchmark j weights is therefore $\omega_j^s=(k_j\omega_{j1}^s,\omega_{j2},\ldots,\omega_{jN})$, where k_j is a benchmark j specific constant, which may be zero, and ω_j^s is a random variable realized at $t=0$. $\mathcal{S}$ possible realizations of ω_j^s occur with probabilities π^s, $s=\{1,\ldots,\mathcal{S}\}$. In light of the fact that we have added one extra period to the model, we now use the notation S_t and W_t for the stock’s value and investor wealth, respectively, at time $t\in\{-1,0,1\}$.

Investors choose portfolios at times $t=-1$ and $t=0$ to maximize their expected utility

$$E[U(W)],$$

where $U(W)=-e^{-\gamma W}$. The argument W in the utility function is equal to W_1 for direct investors and w_j for fund managers benchmarked to benchmark j.

The main result of this appendix is summarized in the following proposition.

Proposition A1. The stock’s per share return depends on $BMI-E^*[BMI]$, where the expectation $E^*[\cdot]$ is taken under the risk-neutral measure. Specifically,

$$S_0-S_{-1}=\gamma A\Sigma\frac{b}{a+b}\left(\sum_{j=1}^{J}\lambda_j\omega_j-E^*\left[\sum_{j=1}^{J}\lambda_j\omega_j^s\right]\right), \qquad \text{(A1)}$$

where $\frac{\pi_s MU^s}{\sum_{s=1,\ldots,\mathcal{S}}\pi_s MU^s}$, $s=\{1,\ldots,\mathcal{S}\}$, are the risk-neutral probabilities, with MU^s denotes the direct investor’s marginal utility in state s.

Proof of Proposition A1. We solve the model backwards, first deriving equilibrium at $t=0$, when the only uncertainty is about the realization of the cash flow, and then moving to $t=-1$. The direct investor's problem at time $t=0$ is

$$\max_{\theta_D} E_0[-\exp\{-\gamma W_1\}]. \tag{A2}$$

To evaluate the expectation in (A2), we need the following property. Suppose $Y \sim N(E[Y], Var[Y])$ is an $N\times 1$ random vector, α is a (constant) scalar and x is a constant vector. Then

$$Ee^{\alpha x' Y} = e^{\alpha x' E[Y] + \frac{\alpha^2}{2} x' Var[Y] x}. \tag{A3}$$

Substituting in the budget constraint $W_1 = W_0 + \theta_D'(D - S_0)$, using property (A3), we can equivalently represent the direct investor's problem as follows:

$$\max_{\theta_D} \left[-\exp\{-\gamma[W_0 + \theta_D'(\overline{D} - S_0) - \frac{1}{2}\gamma \theta_D' \Sigma \theta_D]\} \right].$$

The solution to this optimization problem is straightforward and yields the demand function (1). The demand function (2) is derived analogously.[73]

We now turn to the direct investor's problem at time $t=-1$. Substituting (1) and the budget constraint $W_0 = W_{-1} + \theta_{-1}(S_0 - S_{-1})$ and using the law of iterated expectations, we arrive at the following optimization problem:

$$\max_{\theta_{-1}} E \left[-\exp\{-\gamma[W_{-1} + \theta_{-1}(S_0 - S_{-1}) + \frac{1}{\gamma}(\overline{D} - S_0)' \Sigma (\overline{D} - S_0) \right.$$
$$\left. - \frac{1}{2}\frac{1}{\gamma}(\overline{D} - S_0)' \Sigma (\overline{D} - S_0)]\} \right],$$

where the expectation is over possible realizations of benchmark weights ω_j. Substituting S_0 from Equation (3), we arrive at

$$\max_{\theta_{-1}} \sum_{s=1,\ldots,\mathcal{S}} \pi_s \left[-\exp\{-\gamma[W_{-1} + \theta_{-1} \left(\overline{D} - \gamma A \Sigma \left(\overline{\theta} - \frac{b}{a+b} \sum_{j=1}^{J} \lambda_j \omega_j^s \right) - S_{-1} \right) \right.$$
$$\left. + \frac{1}{2} A \left(\overline{\theta} - \frac{b}{a+b} \sum_{j=1}^{J} \lambda_j \omega_j^s \right)' \left(\overline{\theta} - \frac{b}{a+b} \sum_{j=1}^{J} \lambda_j \omega_j^s \right)]\} \right].$$

[73] The optimization problem of a fund manager benchmarked to portfolio j is $\max_{\theta_j} E_0[-\exp\{-\gamma(aR_j + b(R_j - B_j) + c)\}]$ or equivalently, $\max_{\theta_j} E_0[-\exp\{-\gamma((a\theta_j + b\omega_j)'(D - S_0) - b\omega_j'(D - S_0))\}]$. Using the change of variables $z \equiv a\theta_j + b\omega_j$, we can reduce this problem to that considered in (A2).

The first-order condition with respect to θ_{-1} is

$$\sum_{s=1,\ldots,\mathcal{S}} \pi_s MU^s \left(\overline{D} - \gamma A \Sigma \left(\overline{\theta} - \frac{b}{a+b} \sum_{j=1}^{J} \lambda_j \omega_j^s \right) - S_{-1} \right) = 0. \tag{A4}$$

Equation (A4) is a familiar no-arbitrage condition at time $t=-1$ stating that $E[M^s(S_0 - S_{-1})] = 0$, where the stochastic discount factor M is given by $M^s = MU^s / \sum_{s=1,\ldots,\mathcal{S}} \pi_s MU^s$, $s = \{1,\ldots,\mathcal{S}\}$. In equilibrium, S_{-1} satisfies (A4). We can therefore use (A4) to express S_{-1} as an expectation under the risk-neutral measure, $E^*[\cdot]$,

$$S_{-1} = \overline{D} - \gamma A \Sigma \left(\overline{\theta} - \frac{b}{a+b} \underbrace{E^* \left[\sum_{j=1}^{J} \lambda_j \omega_j^s \right]}_{E^*[BMI]} \right), \tag{A5}$$

with the risk-neutral probabilities given by

$$\pi_s^* = \frac{\pi_s MU^s}{\sum_{s=1,\ldots,\mathcal{S}} MU^s}. \tag{A6}$$

It is easy to see that the risk-neutral measure defined above is a valid probability measure. Note that markets are complete in our model and hence the risk-neutral probabilities are the same if we used the marginal utility of either fund manager in (A6). Equation (A1) then follows from (3) and (A5). □

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