Same Dollar, Different Impact: Investor Flows Are Not Equally Price-Moving*

Shiyang Huang Ruiqing Hu Yang Song Hong Xiang

June 1, 2026

Abstract

We demonstrate that how investor demand moves prices depends on the institution through which it is executed. Using novel holdings data on separate accounts, the dominant institutional investment vehicle, we show that separate account flows create demand comparable to mutual fund flows but generate essentially no price impact, fragility, or fire-sale dynamics. The contrast persists within mutual fund–separate account twins run by the same managers under identical strategies. Trade-level data from a major transition manager show that specialized institutional execution can substantially lower trading costs and price impact. The evidence reveals large heterogeneity in demand transmission: not all flows are equally price-moving.

*We thank Travis Bagley (Russell Investments), Steve Dimmock, Darrell Duffie, Richard Evans, and Jiacui Li for helpful comments. In addition, we benefit from comments from seminar and conference participants at Arizona State University, Southern Methodist University, Korea University-KAIST finance seminar, National University of Singapore, Shanghai Advanced Institute of Finance (SAIF), Peking University HSBC Business School, Peking University School of Economics, and the 7th HKU Summer Finance Conference. Huang and Hu are with The University of Hong Kong, Song is with The University of Washington, and Xiang is with The Hong Kong Polytechnic University. Send correspondence to Yang Song at songy18@uw.edu.

1 Introduction

One central theme in asset pricing research is whether, and by how much, investor demand moves asset prices (e.g., Duffie, 2010). Using the most readily available flows and holdings data of mutual funds, a large literature shows that mutual fund flows generate sizable price impact (e.g., Lou, 2012; Li, 2022; Ben-David et al., 2022a; Peng and Wang, 2023), amplify price fragility (e.g., Greenwood and Thesmar, 2011; Huang et al., 2025b), and trigger fire-sale dynamics (e.g., Coval and Stafford, 2007). These findings have shaped views on how demand pressure transmits into prices and have motivated a growing literature that estimates price multipliers from institutional holdings data (e.g., Koijen and Yogo, 2019; Gabaix and Koijen, 2021).

Yet mutual funds represent only one way in which delegated capital is organized and traded. They predominantly serve retail investors and provide daily liquidity at net asset value.[1] A comparable share of delegated assets is managed through institutional investment vehicles, such as separate accounts, whose investors, liquidity arrangements, and execution processes differ sharply from those of open-end mutual funds. Whether the price effects documented for mutual fund flows are pervasive across investment vehicles, or instead reflect the particular structure through which mutual fund demand is implemented, remains an important unanswered question.

In this paper, we address this question by studying the price effects of flows to separate accounts, the dominant investment vehicle for institutional investors such as pensions, insurance companies, endowments, and sovereign wealth funds. Our analysis uses a novel quarterly holdings dataset of U.S. equity separate accounts from 2001 to 2024. Although

[1] Based on the ICI Fact Book (2024), more than 90% of the assets of mutual funds are held by household and retail investors.

the separate account sector rivals the mutual fund industry in scale, as shown in Figure 1, its flow dynamics and pricing implications remain far less understood. The setting therefore allows us to study whether comparable demand has comparable price effects when it is executed through institutional investment vehicles.

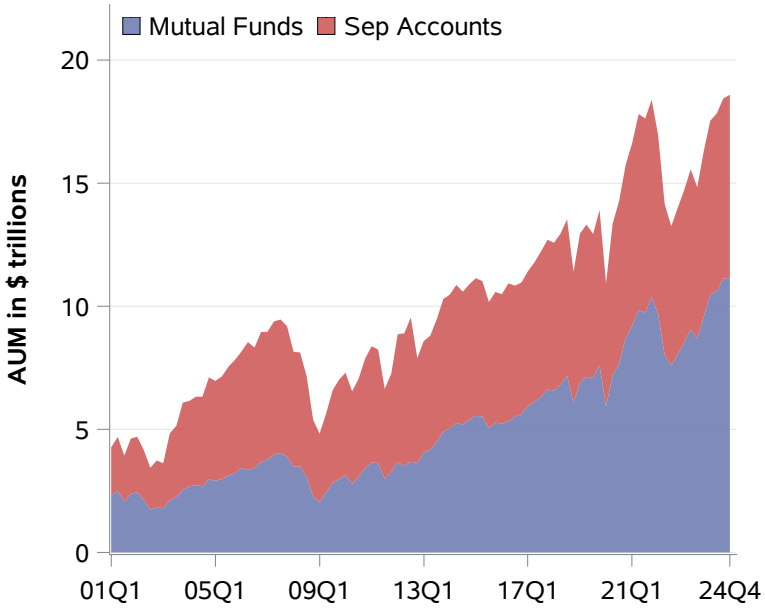


Figure 1: **Total assets of U.S. equity mutual funds and separate accounts.** This figure plots total assets of U.S. equity mutual funds and separate accounts at each quarter-end from 2001Q1 to 2024Q4.

Our findings demonstrate that how investor demand moves prices depends not only on the size of the demand shock, but also on the institution through which that demand is executed. Specifically, we find that separate account flows create portfolio demand comparable to mutual fund flows, but generate essentially no price impact, do not amplify price fragility, and do not trigger fire-sale dynamics. To sharpen the comparison, we examine separate account–mutual fund "twins," pairs that are managed under the same investment strategies by the same managers (Huang et al., 2025a). Even within these twins, separate account flows barely move prices, whereas flows of their mutual fund counterparts create substantial price impact, indicating that the differences are not driven

by investment strategies employed by mutual funds and separate accounts. Moreover, the contrast persists after controlling for market-wide liquidity at the time flows occur, suggesting it is not primarily driven by institutional investors' ability to time favorable liquidity conditions when allocating capital among separate accounts.

We provide evidence that this contrast is related to differences in institutional execution design. Mutual funds are bound by daily NAV liquidity provision and often learn investor flows late in the trading process, limiting their ability to source liquidity patiently. In addition, mutual fund trades are typically routed through broker-dealers, which may introduce agency and routing frictions into the execution process (Evans et al., 2023). Separate account reallocations, by contrast, are typically sponsor-initiated allocation decisions, often communicated in advance and implemented with greater execution discretion. For large reallocations, institutional investors often rely on transition managers—specialized intermediaries designed to minimize transaction costs and information leakage.[2] Using trade-level data from 1,787 transition programs executed by a leading transition manager, we show that such institutional transitions can be implemented with little measurable price impact.[3] These results indicate that transition management is an important observable channel through which institutional execution design dampens price pressure, although it is unlikely to be the only channel.

Taken together, the evidence shows that investor demand is not equally price-moving across investment vehicles. Strikingly, even flows of investment vehicles following the same investment strategies can influence prices very differently. This heterogeneity has important implications for demand-based asset pricing, for the interpretation of price

[2]Some of the leading transition managers are Russell Investments, State Street Corporation, Citi, Bank of New York Mellon, and Northern Trust Corporation.

[3]A transition program moves a legacy portfolio to a target portfolio or moves capital from or into cash (also known as "one-sided events"). See Section 6 for more institutional details of transition management.

multipliers estimated from pooled institutional holdings, and for the design of liquidity provision in open-end mutual funds.

Our analysis is built on a novel dataset of quarterly holdings of U.S. equity separate accounts spanning 2001 to 2024. To set the stage, we first examine how separate account holdings change in response to institutional capital flows. We find that, similar to mutual funds, capital flows of separate accounts create flow-induced demand: existing holdings of separate accounts increase by 0.78% with 1% inflow and decrease by 0.93% with 1% outflow. Thus, flows of separate accounts create demand on existing portfolio holdings similar to mutual fund flows.

We then assess the price impact of separate account flow-induced trading, using the measure of Lou (2012). We find that the flow-induced price impact of separate accounts is economically negligible and statistically insignificant across different specifications. In sharp contrast, when the aggregate mutual fund sector buys 1% of a stock's shares outstanding in response to flows, the stock experiences a 1.64% contemporaneous price run-up (t-statistic = 5.80), followed by a significant reversal.

We next examine flow-induced fire-sale dynamics following Coval and Stafford (2007), who document that mutual funds experiencing extreme outflows tend to fire sell existing holdings, creating significant downward price pressure on stocks held in common by distressed funds. We again find stark differences between separate accounts and mutual funds. For stocks most exposed to mutual fund fire sales, we observe a significant price decline of -2.27% (t-statistic = -5.82) through the end of the fire-sale quarter. For separate account fire sales, even though the most affected stocks experience larger demand shocks, the corresponding price decline is much more modest: the cumulative abnormal return through the quarter-end is -0.57%, which is statistically indistinguishable from zero with

a t-statistic of -1.24. This sharp contrast indicates that separate account flow-induced fire sales exert far smaller price effects than mutual fund fire sales.

Having established the differential price impact of mutual funds and separate accounts, we next examine the implications for flow-induced price fragility following Greenwood and Thesmar (2011). The intuition is straightforward: if flow-induced trading generates non-fundamental price pressures, then variation in such trading should create non-fundamental return volatility for the affected stocks. We find that mutual fund flow-induced fragility imposes substantial non-fundamental risks on stocks, whereas the effect of separate account flow-induced fragility is economically small and statistically insignificant after controlling for the mutual fund effects.

A potential explanation for our findings is that the investment strategies employed by separate accounts differ substantially from those of mutual funds, yielding different capacities to accommodate flows. To test this hypothesis, we exploit mutual fund–separate account "twins," which are managed under the same strategy by the same portfolio managers.[4] We observe the same patterns among these twins, indicating that the differential price impacts of mutual fund and separate account flows are not driven by differences in investment strategies.

Another possible explanation is that institutional investors, like pension funds, endowments, and insurance companies, are better at "timing" favorable market-wide liquidity conditions when allocating capital among separate accounts. That is, these institutions move capital in and out of separate accounts when market-wide liquidity is higher. To rule out this hypothesis, we use the aggregate market liquidity measure of Pástor and Stambaugh (2003) and divide the sample into two equal halves based on liquidity levels.

[4]Huang et al. (2025a) show that the majority of mutual funds have twin IVs that serve institutional investors.

Within each subsample, we re-estimate the flow-induced price impact. We find that the price impact of separate account flows remains insignificant and negligible in both low- and high-liquidity periods, whereas mutual fund flow-induced price impacts are significant in both.

After showing that neither differences in investment strategies nor market-wide liquidity timing accounts for the contrast, we turn to the institutional process through which flow-induced demand is implemented. Mutual fund flows are often uncertain and may be confirmed only late in the day or even the next morning (Wahal and Wang, 2022). Bound by daily NAV pricing, mutual funds therefore face pressure to trade promptly, limiting their ability to source liquidity patiently. Consistent with this execution constraint interpretation, we find that mutual fund flow-induced price impact is stronger when flows are more volatile, stronger for unexpected than expected flows, and larger for index funds, where cash drag and tracking error concerns make prompt execution especially important.

Separate account capital reallocations are implemented in a different institutional environment. These reallocations are typically sponsor-initiated allocation decisions, often communicated in advance and implemented with greater discretion over timing and execution. For large reallocations, institutional investors often rely on transition managers—specialized intermediaries designed to minimize transaction costs, information leakage, and implementation shortfall.

To examine one observable component of this channel, we obtain trade-level data on 1,787 U.S. equity transition programs executed by a leading transition manager over 2013–2021. On average, these programs involve $248 million in U.S. equity trades and are typically completed within five days. The trade-level data show that large institutional reallocations can be implemented with low implicit costs and little measurable

price impact. We also find suggestive evidence that transition management activity is concentrated around large separate account allocation episodes. Together, these findings support the broader interpretation that institutional execution design helps shape how investor demand is transformed into trades and, ultimately, into prices.

We interpret the transition management evidence as one observable mechanism rather than a complete attribution of the separate account result to a single intermediary. We also do not observe the execution channel for every separate account flow. Some institutional capital reallocations may be handled directly by asset managers, and it is possible that asset managers prioritize institutional mandates and execute institutional flows more carefully. To the extent that such choices attenuate price pressure of separate account flows, part of the contrast that we document may reflect differences in asset managers' trading behavior and execution quality, in addition to formal transition management. Rather than weakening our interpretation, these possibilities reinforce our conclusion that investor demand can transmit to prices in highly heterogeneous ways: not all flows are equally "price-moving."

Our results have important implications for the large literature examining investor demand and asset prices. They highlight substantial heterogeneity in how different investment vehicles transmit demand shocks into prices, implying that the price impact of investor demand depends critically on institutional structure. This point is particularly relevant for recent work that uses 13F filings to estimate price multipliers, as 13F-based estimations pool retail-oriented mutual funds with institutional investment vehicles, potentially obscuring substantial differences in how demand shocks translate into price impact. Relatedly, our findings caution against extrapolating the demand effects of mutual funds to the broader institutional landscape.

Our findings also have implications for the design of liquidity provision in open-end funds. Daily redeemability at NAV provides a valuable form of liquidity transformation, but it also compresses the horizon over which managers must accommodate investor flows. When subscriptions and redemptions are converted into trades under tight timing constraints, the costs of liquidity provision can spill over to the underlying securities through non-fundamental price pressure, fire-sale dynamics, and excess volatility. The comparison with separate accounts shows that these costs are not an unavoidable consequence of investor demand itself. Comparable portfolio demand generates little price disruption when capital is allocated through institutional arrangements that allow longer planning horizons, greater execution discretion, and specialized trading infrastructure.

Our evidence points to a liquidity-transformation tradeoff: daily NAV redeemability expands investor access, but it can also make flow-driven trades more price-moving when execution is constrained. Policies that better align the price and timing of investor liquidity with the trading costs of the underlying portfolio and that improve the execution process through which flows are implemented may reduce the spillovers from fund flows to asset prices. More broadly, our results suggest that open-end fund fragility is partly an institutional-design problem: the same dollar of investor demand can be benign or destabilizing depending on how the liquidity promise is implemented.

The remainder of the paper proceeds as follows. Section 2 discusses the relation of our work to the existing literature. Section 3 describes the holdings data of separate accounts and other datasets used in the paper. Section 4 compares the price effects of mutual fund and separate account flows through various empirical settings. Section 5 examines possible explanations for the divergent price impacts. Section 6 studies institutional execution design and uses transition manager data to provide mechanism evidence on

the low-cost implementation of large institutional reallocations. Section 7 concludes. Additional results are supplemented in the appendices.

2 Relation to Prior Literature

Our paper contributes to the literature in several ways. First, it contributes to the literature on investor demand and asset prices. While benchmark asset-pricing frameworks often decompose price movements into cash flow news and discount rate variation (Cochrane, 2011), this literature documents that investor demand can have large price effects (e.g., Duffie, 2010). Evidence comes from index additions and deletions (e.g., Harris and Gurel, 1986; Shleifer, 1986; Wurgler and Zhuravskaya, 2002; Chang et al., 2015; Pavlova and Sikorskaya, 2023),[5] as well as mutual fund flows (e.g., Teo and Woo, 2004; Coval and Stafford, 2007; Froot and Teo, 2008; Lou, 2012; Ben-David et al., 2022a; Li and Lin, 2025; Huang et al., 2025b). A recent demand-based asset pricing literature estimates price multipliers using fund-family-level 13F filings that aggregate holdings of mutual funds and various institutional investment vehicles (e.g., Koijen and Yogo, 2019; Gabaix and Koijen, 2021; Koijen et al., 2024). We contribute to this literature by demonstrating substantial heterogeneity in the extent to which investor demand moves prices across different investment vehicles. Our results suggest that demand-based asset pricing should distinguish not only who demands an asset and how much they demand, but also through which investment vehicle that demand is implemented and how the resulting trades are executed. Strikingly, even flows of different investment vehicles managed under the same investment strategies by the same portfolio managers can have stark differences in price impact.

[5] Greenwood and Sammon (2025) show that the index effect has mostly disappeared in recent years despite the significant increase in index investing.

Our paper is also related to the small but growing literature on institutional investment products. Early notable studies include Busse et al. (2010), Evans and Fahlenbrach (2012), Elton et al. (2014), and Jenkinson et al. (2016). More recently, Gerakos et al. (2021) provides a comprehensive analysis of institutional product performance, while Jones et al. (2022) compares the performance of institutional products with mutual funds. Evans et al. (2022) examine diseconomies of scale in institutional separate accounts employing quantitative or fundamental approaches. Huang et al. (2025a) argue that researchers should include twin institutional investment vehicles in order to reach an accurate measure of scale, and the assets of institutional vehicles meaningfully influence managers' portfolio decisions. We diverge from these studies by emphasizing the stark differences in the price impacts of institutional versus retail investor flows.

Our paper is also among the very few academic studies on transition managers—an important class of intermediaries in institutional investing yet largely overlooked by academic research. An exception is Ang and Madhavan (2024), who show that transition managers can execute large-scale transitions with costs closely aligned to pre-trade estimates and even large and complex trades can be executed at modest cost.[6] We extend this literature by linking transition management to the transmission of institutional demand into prices. Our trade-level evidence shows that large institutional reallocations can be executed at low cost and with limited measurable price impact, providing direct evidence on an execution technology through which institutional demand shocks can be absorbed with less price disruption.

[6]Kyle and Obizhaeva (2016) use transition trades from a major transition manager during 2001 to 2005 to test various market microstructure theories.

3 Data

Our fund sample consists of U.S. domestic equity mutual funds and separate accounts from Morningstar, covering both actively and passively managed strategies. The Morningstar mutual fund database reports returns and characteristics at the share-class level. We aggregate multiple share classes of the same mutual fund into a single fund by taking asset-weighted averages of returns and characteristics, with total fund AUM defined as the sum of AUM across its share classes. Following Chen et al. (2004), Pástor et al. (2015), and others, we include a mutual fund in the sample once its inflation-adjusted AUM exceeds $15 million.[7]

The separate account data in Morningstar is reported by asset management companies at the composite level, where multiple separate accounts following the same investment strategy are aggregated into a single composite. The data are high-quality and reliable.[8] According to Morningstar, a substantial majority—approximately 90%—of the institutional products in the database are managed by firms that comply with Association for Investment Management and Research (AIMR) reporting standards.

Because disclosure is voluntary, coverage of separate accounts is limited prior to 2000. To ensure sufficient representation of institutional products throughout the sample, we begin our analysis in 2001Q4 and end in 2024Q4. Figure 1 plots total asset under management of mutual funds and separate accounts in our sample. One can see that the total size of separate accounts is comparable to the mutual fund sector.

We also obtain mutual fund and separate account holdings from Morningstar. Mutual fund holdings are reported at the portfolio level, while separate account holdings are re-

[7] We follow Pástor et al. (2015) and use U.S. stock market capitalization as the scaling factor to adjust AUM to December 2024 levels.

[8] See Huang et al. (2025a) for a detailed discussion of Morningstar's separate account data.

ported at the composite level. To recover composite-level positions for separate accounts, we combine the portfolio weights from the holdings data with each composite's AUM at the corresponding quarter-end.

We use stock price data from the CRSP database. Our sample stocks include common stocks (CRSP share code 10 or 11) listed on the NYSE, AMEX, and NASDAQ. To be included in the sample, the stock must be held by at least one mutual fund or separate account during the quarter.

[Table 1 About Here]

Table 1 reports summary statistics for the fund-quarter observations (Panel A) and stock-quarter observations (Panel B) in our sample. One can see that separate accounts (at the composite level) on average have a smaller AUM, a younger age, and a lower portfolio turnover ratio than mutual funds. Panel B shows that the sample stocks exhibit a large variation in flow-induced trades from both mutual funds and separate accounts, and the detailed construction of these measures is explained in the following section.

4 Comparing Flow-Induced Price Effects of Separate Accounts and Mutual Funds

In this section, we compare the price impact of flow-induced trading between mutual funds and separate accounts. We begin by documenting that, similar to mutual funds, holdings of separate accounts proportionally increase with capital inflows and decrease with capital outflows. We then assess the price impact of separate account flows across three empirical settings: (1) the price impact of flow-induced trading (Lou, 2012); (2) the price dynamics associated with fire sales (Coval and Stafford, 2007); and (3) flow-induced

fragility and the resulting non-fundamental return volatility (Greenwood and Thesmar, 2011). Across all three settings, we find that separate account demand barely moves prices and does not trigger fire-sale dynamics or price fragility.

4.1 Separate account flow-induced demand

To set the stage for measuring flow-induced demand shocks, we first examine how separate account holdings change with respect to institutional capital flows. Prior studies show that mutual fund holdings tend to proportionally change in response to inflows (outflows) (e.g., Lou, 2012), a pattern that allows researchers to construct measures of mutual fund flow-induced demand shocks. In this subsection, we extend this analysis to separate accounts.

We measure quarterly percentage fund flow as the change in total net assets (TNA) after adjusting for portfolio returns:

$$Flow_{i,t} = \frac{TNA_{i,t} - TNA_{i,t-1} \times (1 + Ret_{i,t})}{TNA_{i,t-1}}, \tag{1}$$

where $TNA_{i,t}$ is the total net assets of fund i at quarter t end, and $Ret_{i,t}$ is the fund gross return in quarter t. Fund flows are computed at the mutual fund level or separate account composite level, and quarterly fund flows are winsorized at the 500% level.

We examine flow-induced trading using Fama-MacBeth regressions on the sample of fund-quarter-stock observations, restricting the stock universe to each fund's existing holdings at the beginning of the quarter. The regression specification is:

$$Pct_Trade_{i,j,t} = \beta_0 + \beta_1 \times Flow_{i,t} + Controls + \varepsilon_{i,j,t}, \tag{2}$$

where $Pct_Trade_{i,j,t} = Shares_{i,j,t}/Shares_{i.j.t-1} - 1$ is the split-adjusted percentage change

in the number of shares of stock j held by fund i in quarter t. $Flow_{i,t}$ is the percentage flow of fund i in the same quarter. The control variables include the lagged ownership of the stock j by fund i as a fraction of shares outstanding, the stock's Amihud illiquidity measure, and the portfolio weight of stock j in fund i. We also interact $Flow_{i,t}$ with each of these stock characteristics. Finally, we split the sample into inflow and outflow subsamples based on the sign of $Flow_{i,t}$, and estimate the regressions separately for each subsample.

Table 2 presents the regression results. Columns (1)-(4) report the results for mutual funds. The coefficient estimates on β_1 suggest mutual funds scale up existing holdings by 0.70% in response to a 1% inflow and scale down existing holdings by 0.75% in response to a 1% outflow, consistent with the findings of Lou (2012). Columns (5)-(8) report the results for separate accounts. We find that existing holdings of separate accounts increase by 0.78% with 1% inflow and decrease by 0.93% with 1% outflow. Thus, flows of separate accounts create demand shocks on existing portfolio holdings similar to mutual fund flows. Unlike retail mutual fund investors, however, institutional investors often use transition managers to implement trades associated with large capital allocation, which we explain in detail in Section 6.

[Table 2 About Here]

4.2 Price impact of separate account flow-induced trading

In this subsection, we analyze the price impact of separate account flow-induced trading (FIT). Motivated by the results in the previous subsection and following Lou (2012), we construct a measure of flow-induced trading that captures the aggregate demand shock exerted on each stock:

$$\text{FIT}_{j,t} = \frac{\sum_i \text{Shares}_{i,j,t-1} \times \text{Flow}_{i,t} \times \text{PSF}}{\text{Shrout}_{\text{j,t}-1}}, \tag{3}$$

where $\text{Shares}_{i,j,t-1}$ is the number of shares of stock j held by fund i at the end of quarter $t-1$, PSF is a partial scaling factor which is empirically close to one,[9] and $\text{Shrout}_{\text{j,t}-1}$ is the number of shares outstanding of stock j at the end of quarter $t-1$. The numerator of FIT aggregates the flow-induced share demand for stock j across all funds, and it is further scaled by the stock's total shares outstanding.[10]

To compare the price impact between mutual funds and separate accounts, we construct FIT measures separately based on mutual fund data and separate account data. Table 1 shows that both mutual fund- and separate account-based FIT measures exhibit substantial variations among stock-quarter observations. In particular, the standard deviations of the separate account FIT measures are larger than those of mutual funds, indicating larger variations in demand shocks from separate account flows.

We estimate the price impact of mutual fund and separate account flow-induced trading using Fama–MacBeth regressions at the stock–quarter level over the sample period of 2001Q4–2024Q4. The regression specification is:

$$Ret_{j,t} = \beta_0 + \beta_1 \times FIT_{j,t}^{MF} + \beta_2 \times FIT_{j,t}^{SA} + Controls_{j,t-1} + \varepsilon_{j,t}, \tag{4}$$

where $Ret_{j,t}$ is the return of stock j in quarter t, and the main independent variables are the mutual fund–based and separate account–based FIT measures for the same stock-quarter. The control variables include stock characteristics measured at the previous

[9]The partial scaling factor captures funds' differential trading behavior in response to inflows and outflows and across stocks with different characteristics (see Table 2). For simplicity, we set PSF to one in our analysis and confirm the results are not sensitive to the choice.

[10]As a result, a FIT value of 1% indicates that funds collectively purchase 1% of the stock's shares outstanding in response to flows.

quarter-end, such as size, book-to-market ratio, past twelve-month return, asset growth, gross profitability, and Amihud illiquidity. We exclude stocks with share prices below $1 at the previous quarter-end to mitigate the influence of penny stocks.

The coefficient estimates of β_1 and β_2 in equation (4) capture the price impact of the flow-induced trading by mutual funds and separate accounts, respectively. Importantly, this regression is estimated only for stock–quarter observations held by both the mutual fund and separate account sectors, ensuring that we are comparing price impacts arising from different investor demands within a common universe of stocks. Panel A of Table 3 reports the results.

[Table 3 About Here]

Columns (1)-(2) show that the price impact of mutual fund flow-induced trading is both statistically and economically significant. For example, column (2) shows that when the aggregate mutual fund sector buys 1% of a stock's shares outstanding in response to flows, the stock experiences a 1.60% contemporaneous price run-up (t-statistic = 5.24). In sharp contrast, columns (3)-(4) show that FIT^{SA} has virtually no price impact. In columns (5)-(6), when we include both FIT^{MF} and FIT^{SA} as explanatory variables while controlling for other stock characteristics, the estimated price impacts remain essentially unchanged.[11]

We further examine heterogeneity in flow-induced trading and its associated price impact; the results are reported in Figure 2. First, we compare the price impact estimated in the full sample with that in a subsample of extreme FIT, defined as the top and bottom

[11] As a robustness check, we construct an alternative measure of FIT that scales the flow-induced shares demand by the total stock shares held by funds, and we re-estimate the price impact estimation. Appendix Table A.1 confirms the differential price impact between FIT^{MF} and FIT^{SA} under the alternative FIT measure.

10% of stocks in each quarter. The estimated price impact of extreme separate-account FIT remains statistically indistinguishable from zero. Second, we compare the price impact of FIT generated by larger versus smaller fund families. Specifically, in each quarter, we split mutual funds (and, separately, separate accounts) into two groups based on family-level AUM and reconstruct FIT within each subsample. For mutual funds, flow-induced trades from larger families exhibit smaller price impact than smaller families, but the difference is not statistically significant. This is consistent with the idea that larger mutual fund families are marginally better at handling flow-induced price impact. For separate accounts, the estimated price impact for both groups remains statistically indistinguishable from zero.

[Figure 2 About Here]

We next examine the return reversal patterns following flow-induced demand shocks. As mutual fund FIT generates non-fundamental price pressures, we expect these contemporaneous price impacts to reverse in subsequent quarters. In contrast, because separate account FIT produces negligible contemporaneous price movements, no meaningful reversals should follow. In Panel B of Table 3, we estimate Fama-MacBeth regressions of quarterly stock returns on the twelve-quarter average of past FIT^{MF} and FIT^{SA}. We indeed find a strong reversal associated with past FIT^{MF}, while past FIT^{SA} displays no reversal pattern. Overall, these results indicate that separate account flow-induced demand is far less price-moving.[12]

[12]The price-impact tests in this subsection are designed to measure flow-induced price pressure at the stock-quarter horizon, as in the flow-based asset-pricing literature. They are not designed to rule out transitory execution costs associated with separate-account trades. If a flow-induced trade temporarily moves prices but the effect is absorbed and reversed within the quarter, it would not generate the persistent price distortion, reversal, fire-sale dynamics, or fragility that are the focus of our main tests. We examine short-horizon implementation costs directly in Section 6 using trade-level transition management data.

4.3 Price dynamics of separate account fire sales

In this subsection, we further examine flow-induced price pressures when funds are experiencing extreme outflows, following the research design of Coval and Stafford (2007). The key idea is that funds facing extreme outflows tend to fire-sell existing holdings, generating significant downward—and largely non-fundamental—price pressures on stocks held in common by distressed funds. We compare the stock price dynamics associated with mutual fund fire sales to those associated with separate account fire sales to assess the differential price impact of the two types of investment vehicles.

We closely follow Coval and Stafford (2007) in implementing the "fire-sale" test. In each quarter t, we classify the bottom 10% of funds by percentage flow as experiencing extreme outflows and the top 10% as experiencing extreme inflows.[13] The actual sales (purchases) of a stock by funds in the extreme-outflow (extreme-inflow) group are defined as fire sales (fire purchases). We then construct a $PRESSURE$ variable for each stock-quarter observation as:

$$PRESSURE_{j,t} = \frac{\sum_i(\max(0, \Delta Shares_{i,j,t})|Flow_{i,t} > Pctl(90th)) - \sum_i(\max(0, -\Delta Shares_{i,j,t})|Flow_{i,t} < Pctl(10th))}{AvgVolume_{j,t-4:t-3}}. \quad (5)$$

Here, the numerator is the difference between aggregate fire purchases and aggregate fire sales for each stock, and we scale this difference by the stock's average quarterly trading volume over quarters $t-4$ to $t-3$. Thus, $PRESSURE_{j,t}$ measures the net fire-sale volume for stock j in quarter t.

Following Coval and Stafford (2007), we define fire-sale stocks in each quarter as those

[13] Flow rankings of mutual funds and separate accounts are generated separately within their own pools.

in the bottom decile of the $PRESSURE_{j,t}$ distribution. In particular, fire-sale stocks are identified separately for distressed mutual funds and distressed separate accounts. We find that the sample mean $PRESSURE$ among fire-sale stocks is -0.44% for mutual fund-based fire sales and -0.68% for separate account-based fire sales. Hence, on average, separate account–based fire-sale stocks experience slightly larger flow-induced demand shocks than those associated with mutual fund fire sales in our sample.

We conduct an event study to examine the cumulative abnormal returns (CARs) associated with fire sales.[14] For each fire-sale stock in each calendar quarter from 2001Q4 to 2024Q4, we track its CAR from one quarter before the fire-sale event quarter to five quarters afterward. We then compute the equal-weighted average CAR across all fire-sale stocks in each event quarter, followed by time-series averaging across calendar quarters to obtain the event-time CARs and their associated t-statistics.

[Figure 3 About Here]

Panel (a) of Figure 3 plots the event-time CAR patterns for mutual fund–based and separate account–based fire sales, and Table 4 reports the corresponding CARs and t-values. For mutual fund fire sales, we observe a pronounced price decline of -2.27% (t-statistic $= -5.82$) through the end of the event quarter.[15] Over the subsequent five quarters, prices gradually recover, and CARs revert toward 0%. This pattern indicates that mutual fund fire sales impose substantial non-fundamental downward pressure on stock prices.

[Table 4 About Here]

[14] Following Coval and Stafford (2007), abnormal returns are computed relative to the equal-weighted average return of stocks held by mutual funds.

[15] Consistent with Coval and Stafford (2007), we also find that the stock prices of fire-sale stocks begin to decline in the quarter preceding the event quarter.

For separate account-based fire sales, however, the price decline is much more modest. The CAR through the end of the fire-sale quarter is only -0.57%, and it is statistically indistinguishable from zero with a t-statistic of -1.24. This stark contrast indicates that fire sales by separate accounts exert far smaller price effects than those prompted by mutual fund distress, despite comparable or even larger underlying demand shocks. These results reinforce the finding that separate account flows exert far weaker price pressures than mutual fund flows.

4.4 Flow-induced stock price fragility

Having documented the differential price impacts of mutual fund versus separate account flow-induced trading and fire sales, we now examine their implications for flow-induced price fragility following Greenwood and Thesmar (2011). The underlying intuition is that if flow-induced trading generates non-fundamental price pressures, then variation in such trading should induce non-fundamental volatility in the prices and returns of those stocks. In this sense, an ex-ante measure of expected variation in flow-induced trading—referred to as fragility—should predict a stock's future return volatility.

We follow Greenwood and Thesmar (2011) in constructing mutual fund- and separate account-based measures of fragility as

$$G_{j,t} = W_{j,t}{}' E_t(\Omega_{t+1}) W_{j,t}, \tag{6}$$

where $W'_{j,t} = [w_{1,j,t}, w_{2,j,t}, ..., w_{K,j,t}]$ is the vector of funds' ownership shares in stock j, with $w_{i,j,t} = Shares_{i,j,t}/Shrout_{j,t}$, and $E_t(\Omega_{t+1})$ is the conditional variance-covariance matrix of fund flows in quarter $t+1$. To estimate $E_t(\Omega_{t+1})$, we compute the variance-covariance matrix of fund flows using an eight-quarter rolling window. Following Greenwood and

Thesmar (2011), we restrict the computation of fragility to large-cap stocks—those above the NYSE median market capitalization—to keep the matrix operations computationally manageable.[16] As equation (6) makes clear, fragility is jointly determined by a stock's ownership structure and the expected variance–covariance matrix of fund flows: greater ownership concentration and more highly correlated fund flows lead to higher stock price fragility.

We estimate the predictive power of fragility using Fama–MacBeth regressions of one-quarter-ahead stock return volatility on mutual fund– and separate account–based fragility:

$$\sigma_{j,t+1} = \beta_0 + \beta_1 \times \sqrt{G_{j,t}}^{MF} + \beta_2 \times \sqrt{G_{j,t}}^{SA} + \sigma_{j,t} + \varepsilon_{j,t+1}, \tag{7}$$

where $\sigma_{j,t+1}$ is the standard deviation of daily stock returns for stock j in quarter $t+1$. $\sqrt{G_{j,t}}^{MF}$ and $\sqrt{G_{j,t}}^{SA}$ are the square roots of mutual fund- and separate account-based fragility, respectively. We control for the lagged volatility to absorb persistence in stock-level volatility.

Table 5 reports the regression results. For example, column (5) reports volatility forecasts by mutual fund-based and separate account-based fragility without controlling for lagged volatility. A 0.67% increase in $\sqrt{G_{j,t}}^{MF}$ (from the 10^{th} to the 90^{th} percentile) predicts a 0.34% increase in daily return volatility (=0.511×0.67%), which is economically large relative to the mean daily return volatility of 2.13% (see Table 1). The coefficient is also highly significant, with a t-statistic of 6.6. By contrast, a 1.39% increase in $\sqrt{G_{j,t}}^{SA}$ (from the 10^{th} to the 90^{th} percentile) predicts only a 0.03% increase in stock return volatility (=0.025×1.39%), and it is not statistically significant. Column (6) further shows that $\sqrt{G_{j,t}}^{MF}$ remains a strong predictor of future volatility after controlling for

[16] Appendix B provides additional details on the derivation and estimation of the fragility measure.

lagged volatility, whereas $\sqrt{G_{j,t}}^{SA}$ continues to exhibit insignificant predictive power.

[Table 5 About Here]

Overall, these results show that mutual fund flow-induced fragility imposes substantial non-fundamental risks on stocks, whereas the influence of separate account–based fragility is limited. This pattern is fully consistent with our earlier findings on differential flow-induced price impacts, reinforcing the conclusion that separate account flows transmit far weaker demand-driven distortions to asset prices than mutual fund flows.

5 Exploring Some Possible Explanations

In this section, we explore why mutual fund flows move prices while separate account flows do not. We evaluate two possibilities: (*i*) differences in investment strategies between mutual funds and separate accounts, and (*ii*) "liquidity timing" by institutional investors when allocating capital among separate accounts. We find neither of these explanations is supported in the data.

5.1 Analysis based on separate account-mutual fund twins

A potential explanation for our earlier findings is that investment strategies employed by separate accounts differ substantially from those used by mutual funds, giving rise to different capacities to accommodate flows. To test this possibility, we exploit the "twin" setting of mutual fund–separate account pairs.

Specifically, the twins refer to pairs of mutual funds and separate accounts that are managed under the same investment strategy by the same portfolio managers. As demonstrated by Huang et al. (2025a), the majority of mutual funds have twin institutional investment vehicles, serving institutional investors. These mutual fund-separate account

twins allow us to identify differences in price impact across investor types while holding investment strategy and portfolio management constant.

To implement this analysis, we use the StrategyID provided by Morningstar to link separate accounts and mutual funds that operate under the same investment strategy. We retain only those separate accounts and mutual funds that can be matched to at least one counterpart, and we refer to this subsample as the "twin sample." We then reconstruct the FIT measures, as described in Section 4.2, using only the funds in the twin sample. Intuitively, FIT^{MF} and FIT^{SA} within this sample represent demand shocks originating from twin vehicles that follow the same strategy and are managed by the same portfolio managers.

Panel A of Table 6 reports the estimated price impacts of FIT^{MF} and FIT^{SA} based on the regression specification of equation (4). Within the twin sample, the price impact of FIT^{MF} remains substantial, whereas the price impact of FIT^{SA} continues to be statistically indistinguishable from zero. For example, column (4) shows that a mutual fund FIT equivalent to 1% of a stock's shares outstanding is associated with a 1.80% contemporaneous price increase (t-statistic = 4.08), while an equivalent separate account FIT moves prices by only 0.02% (t-statistic = 0.08).

[Table 6 About Here]

Additionally, we re-estimate the fire-sale and fragility tests from Sections 4.3 and 4.4 using the twin sample. In Panel (b) of Figure 3, we observe that fire sales by separate accounts with twins generate only mild price declines, whereas fire sales by mutual funds with twins produce sizable and persistent downward price pressures. Panel B of Table 6 further shows that fragility induced by separate accounts within the twin sample does

not significantly predict future stock return volatility, while mutual fund–induced fragility remains both statistically and economically meaningful.

Taken together, the results in this subsection indicate that differences in investment strategies do not explain the contrasting price impacts of mutual fund and separate account flows.

5.2 Controlling for aggregate market liquidity

Another potential explanation for the differential price impacts is that institutional investors, such as pensions, endowments, and insurance companies, might be better at timing market-wide liquidity when allocating capital among separate accounts. As a result, flows move in and out of separate accounts when the aggregate market liquidity is more favorable.

To test this hypothesis, we use the aggregate market liquidity measure of Pástor and Stambaugh (2003).[17] We split the sample into two equal halves based on the level of market liquidity and re-estimate the price-impact regressions separately within each subperiod. Table 7 reports the estimated price impacts of FIT^{MF} and FIT^{SA} during high- and low-liquidity periods.

[Table 7 About Here]

One can see that FIT^{MF} exerts highly significant price impacts in both high- and low-liquidity periods, with stronger effects when liquidity is low. For example, the price-impact estimate of FIT^{MF} is 1.91 in the low-liquidity quarters, compared with 1.38 in

[17] We thank Robert Stambaugh for making this dataset publicly available: `https://finance.wharton.upenn.edu/~stambaug/`. We use the "level of aggregate liquidity" series shown in Figure 1 of Pástor and Stambaugh (2003).

high-liquidity quarters. In contrast, the price-impact estimates of FIT^{SA} are insignificant in both low- and high-liquidity periods.

Additionally, Appendix Figure A.1 illustrates the fire-sale CAR patterns across high- and low-liquidity periods. In both environments, mutual fund fire sales generate substantially larger downward price pressures than separate account fire sales. Appendix Table A.3 further reports volatility forecasts based on fragility in the two subperiods. Mutual fund flow-induced fragility significantly predicts future volatility under both high- and low-liquidity conditions, whereas the effect of separate account–based fragility remains muted in each case.

In summary, these findings indicate that the differential price impacts of mutual fund and separate account flows hold under different market-wide liquidity conditions.

6 Institutional Execution Design and Transition Management

Having ruled out differences in investment strategies and market-wide liquidity timing, we next examine how flow-induced demand is converted into trades. We do not seek to attribute the entire separate account result to a single channel. Instead, we ask whether institutional capital reallocations are executed in ways that can dampen price impact.

Our mechanism analysis proceeds in several steps. We first show that mutual fund price impact is stronger when execution constraints are likely to bind: when flows are more volatile, less predictable, and for index funds, where cash drag and tracking error concerns make prompt execution especially important. We then describe how institutional capital transitions differ from mutual fund flow accommodation. Using trade-level data from a leading transition manager, we show that large institutional capital reallocations can be

implemented at low cost and with little measurable price impact. Finally, we provide suggestive evidence that transition management activity is more concentrated around large separate-account allocation episodes. Together, these tests support the broader interpretation that institutional execution design shapes how investor demand transmits into prices.

6.1 Mutual fund execution constraints and institutional transitions

Mutual funds are structurally obligated to prioritize daily liquidity, because daily-NAV pricing requires managers to accommodate investor subscriptions and redemptions promptly. Since mutual fund flows are uncertain and often confirmed late in the day or even the next morning, managers face pressure to trade quickly, risking either cash drag or tracking error and limiting their ability to source liquidity patiently.[18] In addition, mutual fund trades are typically routed through broker-dealers that rely heavily on displayed liquidity and execution algorithms that prioritize timely completion over information-leakage control (e.g., Evans et al., 2023). This reliance on visible liquidity can expose mutual fund trades to information leakage, leading to higher implicit trading costs.

The execution constraints of mutual funds yield testable implications. Price impact should be larger when mutual fund flows are more volatile, when flows are harder to anticipate, and when the fund mandate leaves less scope for temporary deviations from target holdings. Consistent with these predictions, Appendix Figure A.2 shows that mutual fund flow-induced price impact is higher in periods of high flow variation, stronger for unexpected than expected flows, and larger for index funds, for which cash drag and

[18] A mutual fund manager's knowledge of client flows varies depending on the platform used by investors. For some platforms, mutual fund managers may learn about client purchases or sales as late as the next morning (Wahal and Wang, 2022).

tracking error are particularly salient.

The execution problem differs for institutional capital transitions. Separate account reallocations are typically sponsor-initiated, planned in advance, and not constrained by the daily-liquidity structure governing open-end mutual funds. In addition, large reallocations are often delegated to transition managers, specialized intermediaries charged with minimizing transaction costs, information leakage, and implementation shortfall (Jones et al., 2025). We provide more institutional details on transition managers in the next subsection.

6.2 Background on transition management

The transition management industry emerged in the late 1980s and early 1990s. Russell Investments is widely recognized as a global leader, and major providers such as State Street Corporation, Bank of New York Mellon, Citi, and Northern Trust Corporation have also operated in this space for more than 30 years. These firms provide dedicated infrastructure—multi-venue trading platforms, execution desks, derivative overlay tools, and comprehensive reporting—that enables institutions to carry out sizable capital allocations efficiently.

[Figure 4 About Here]

A typical portfolio transition follows a structured sequence, as illustrated in Figure 4. First, the incoming manager is notified several days in advance of the sponsor's intention to allocate assets. Next, the outgoing manager is informed—typically one to two days before the transition—of the upcoming withdrawal. To reduce information leakage and prevent front-running, the outgoing manager is instructed to cease trading and to submit a certified holdings file. Immediately prior to the transition, the transition manager receives

the verified legacy portfolio along with the target holdings specified by the incoming manager. Using these inputs, the transition manager designs and executes the reallocation from the old to the new portfolio. Upon completion, the transition manager delivers a detailed post-trade report summarizing execution costs, trading strategy, and realized outcomes. Similar procedures apply when institutional investors move capital into or out of cash (also known as "one-sided events").

Market participants describe several features through which transition managers are designed to mitigate price impact (e.g., Bagley and Kothare, 2014; Meketa Investment Group, 2019). First, transition managers trade across a wide range of execution venues to access liquidity while minimizing signaling. Rather than routing large directional orders to public order books, they strategically allocate flow to dark pools, conditional block venues, crossing networks, and closing auctions. They complement these with principal or risk-transfer trades when appropriate. The ability to fragment orders across venues, each with different liquidity and transparency properties, allows transition managers to reduce the market footprint of large trades and avoid the price slippage associated with concentrated execution.

Second, transition managers benefit from scale and network externalities that expand the effective supply of liquidity. Because they intermediate a continuous flow of transitions across clients, they frequently observe natural offsets—buyers in one mandate matched with sellers in another—which allows them to internalize liquidity and reduce the amount of risk sent to the open market. Their long-standing relationships with broker-dealers, electronic liquidity providers, and crossing platforms further widen the set of counterparties willing to supply liquidity. These institutional connections and flow-aggregation advantages enable transition managers to absorb large trades with far less reliance on

public depth, thereby reducing the market footprint of large trades.

Third, transition managers are cautious about information leakage. Large reallocations are often predictable and easily identified by liquidity suppliers, leading to anticipatory quoting and adverse price drift. Transition managers reduce this informational footprint by controlling the dissemination of trade lists and using derivative overlays to neutralize market exposures before trading cash securities. These practices reduce the extent to which counterparties can infer the direction or magnitude of the underlying transition, lowering the market's incentive to adjust prices against the order.

6.3 Estimating price impact of transition trades

We next use trade-level transition-management data to examine whether large institutional reallocations can be implemented with limited price pressure. Whereas Section 4 studies whether flow-induced demand generates stock-quarter price pressure, return reversals, fire-sale dynamics, and fragility, this subsection studies a complementary object: the implementation costs incurred when institutional reallocations are executed by transition managers. The goal is not to map every separate account flow to an observed transition program, but to assess whether the execution technology available to institutional reallocations is capable of absorbing large demand shocks at low cost.

The dataset comprises 1,787 distinct transition programs executed between 2013 and 2021. Each transition program moves a legacy portfolio to a target portfolio or moves capital from or into cash. Table 8 reports summary statistics for these programs. The average transition program involves $248 million in traded value of U.S. public equities, spans 578 individual trades, and covers 237 distinct stocks. Transitions are typically executed over a short horizon: 31.45% of programs are completed within a single day, and

60.16% are completed within five days.

[Table 8 About Here]

We proceed by estimating the implicit costs of transition trades. The estimates indicate that trades executed during these transition programs involve low implicit costs and little measurable relation between trade size and price impact. Specifically, we measure trading costs as the difference between the trade execution price and a benchmark price:

$$TradeCost = D \times \frac{Price - BenchmarkPrice}{BenchmarkPrice}, \tag{8}$$

where $Price$ is the execution price of the trade and D denotes the trade direction, equal to 1 for buys and -1 for sells. We use two benchmark prices: (i) the previous day's closing price, and (ii) the same-day opening price. This transaction-cost measure captures implicit trading costs—including price impact and bid–ask spread components—but excludes explicit costs such as commissions.[19]

With this cost measure, we estimate a cross-sectional regression of trade-level transaction costs on trade- and transition-program-level characteristics:

$$TradeCost_{j,p,t} = \alpha + \beta_1 TradeSize_{j,p,t} + Controls + \epsilon_{j,p,t}, \tag{9}$$

where $TradeCost_{j,p,t}$ is the implicit cost per trade dollar for stock j executed in transition program p at time t, and $TradeSize_{j,p,t}$ is the trade's dollar volume expressed as a fraction of the stock's same-day dollar trading volume from CRSP. The control variables include stock- and program-level characteristics: the log of market capitalization, idiosyncratic volatility, Amihud illiquidity, daily turnover, the inverse of stock price, a NASDAQ indicator, and the total dollar trading volume of the transition program.

[19] Based on our conversations with the transition manager, commissions are negligible.

To compare directly with the evidence of mutual fund trading in Busse et al. (2021), we estimate the regression coefficients using a Fama–MacBeth procedure. Specifically, we pool all trades within each calendar month and run a cross-sectional regression, then compute the time-series averages of the monthly coefficient estimates and their associated t-statistics. Table 9 reports the results.

[Table 9 About Here]

Across all specifications, the relation between trade size and implicit trading costs is economically small and statistically weak. For example, column (1) indicates an estimated β_1 of 0.01 (t-statistic = 1.28) when the previous day's close is used as the benchmark. Similar patterns emerge when using the same-day opening price as the benchmark or when estimating costs separately for buy and sell trades. For comparison, Busse et al. (2021) estimate a similar regression for mutual fund trades and obtains a β_1 of 0.811 with a t-statistic of 7.84. These results suggest that the marginal cost of executing transition trades is far below the marginal cost typically estimated for mutual-fund trades. The evidence is therefore consistent with specialized institutional execution dampening the price pressure associated with large reallocations.

We further compare the total transaction costs of each transition program with those of mutual funds. For each transition program p, we compute the volume-weighted or equal-weighted average trading cost, as defined in equation (8), across all trades in the program. We then sort all transition programs in our sample into quintiles based on total trading volume and report the average program-level trading cost within each quintile.

Table 10 reaches the same conclusion at the transition-program level. Averaging across all trades within a program, implicit trading costs are modest. For example, when we use the previous close price as the benchmark for trade costs, the program-level average

trade cost ranges from 4 to 11 bps, with the smallest programs averaging 11 bps and the largest programs averaging 6 bps. When we use the current-day open price as the benchmark, the program-level average trade costs range between 7 and 15 bps. These estimates are similar whether we weight trades by volume or equally, and they do not increase monotonically with either program size or program duration. The low average costs therefore do not appear to be driven only by small or unusually simple transitions.

[Table 10 About Here]

Our estimates are close to those reported by Ang and Madhavan (2024), the only other study of transition programs, to the best of our knowledge. Ang and Madhavan (2024) analyze transaction costs of portfolio transitions executed between 2016 and 2023 from a major transition manager. For the 1,058 transition trades of U.S. equity portfolios, Ang and Madhavan (2024) estimate per dollar implicit transition costs due to price impact to be about 12 bps, similar to our estimated per dollar cost of 4 to 11 bps (7 to 15 bps relative to the current-day open price).[20]

These magnitudes of transaction costs are substantially lower than those observed for mutual funds. For example, Edelen et al. (2013) estimate that an average trading volume of 177% of mutual fund positions generates total costs of 144 bps, with 94 bps attributed to price impact. Busse et al. (2021) estimate that with an average turnover rate of 98%, mutual fund trades incur total transaction costs of 75.1 to 101.4 bps, with 47 to 73.5 bps arising from price impact.

In summary, these estimates show that large institutional reallocations can be implemented at costs far below those typically estimated for mutual-fund trading. They

[20] Ang and Madhavan (2024) report the round-trip implicit costs to be 24 bps, mapping to a per dollar cost of 12 bps.

therefore provide direct evidence that specialized institutional execution technology is capable of absorbing large demand shocks with limited price disruption.

6.4 Transition management and large institutional reallocations

In this subsection, we provide suggestive evidence that transition management services are more likely to be used during large separate account allocation episodes. Because our data cover transition programs executed by a single transition manager and the data do not identify the underlying clients, we interpret the results in this subsection as suggestive.

We begin with a time-series test. Following the fire-sale analysis in Section 4.3, we use a similar definition of fire-sale events to identify episodes of large-scale capital allocation. Specifically, a fund-quarter observation is classified as a fire sale if its quarterly flow falls below the 10th percentile of quarterly flows in the full sample.[21] Each quarter, we compute the number of separate account fire-sale events as a fraction of the number of existing separate accounts and denote this measure *SA_FireSale%*. We also compute the number of sales-oriented transition programs executed by our transition manager in that quarter, scaled by the number of existing separate accounts, and denote this measure *Transit_Program%*.[22]

We then estimate a time-series regression of *Transit_Program%* on contemporaneous *SA_FireSale%*, controlling for contemporaneous market liquidity and market returns, as well as one-quarter-lagged *Transit_Program%*. Panel A of Table 11 reports the regression results. We find that separate-account fire sales are positively and statistically signifi-

[21] We define fire-sale events for separate accounts and mutual funds separately using their respective full-sample flow breakpoints.

[22] Sales-oriented transition programs are defined as transition programs in which more than 50% of the dollar trading volume is from sales of stocks. Among the 1,787 transition programs in our data, 64.4% are sales-oriented programs. The results are robust to alternative cutoff values for defining sales-oriented transition programs.

cantly associated with the incidence of transition programs. By contrast, the association is indistinguishable from zero for mutual fund fire-sale events.

[Table 11 About Here]

To provide additional evidence on transition manager use at the individual separate account level, we match transition programs to separate account–quarter observations in our sample. Because we do not observe the identities of transition program clients, the matching is necessarily "fuzzy" and relies on overlap in stock holdings and the magnitude of capital flows associated with the transition. We also note that Morningstar reports separate account data at the composite level—aggregating multiple accounts that follow the same investment strategy into a single composite—which further complicates matching transition programs to separate accounts.

Using the conservative matching procedure described in Appendix Section C, we are able to match 114 transition programs to 183 separate account-quarter observations.[23] We then examine flows in the matched separate account–quarter observations to assess how separate-account flows relate to the use of transition management. To be consistent with the previous analysis, we focus on sales-oriented transition programs here.

Panel B of Table 11 shows that, when we define extreme flow events using the quarter-specific 10th-percentile (5th-percentile) flow cutoff, the incidence of such events among matched separate-account quarters is 34.34% (22.22%), significantly higher than the full-sample benchmark of 10% (5%). In addition, among matched separate-account quarters, the median quarterly percentage flow is −5.73%, significantly lower than the full-sample median of −0.39%. While these results are suggestive because we observe programs from

[23] Among the 114 matched transition programs, 69 are two-sided programs, which involve an outflow from one separate account and a corresponding inflow to another separate account as part of the transition process.

only one transition manager, they are collectively consistent with transition management being more likely to be used for large capital allocation episodes, in line with industry reports (Bagley and Kothare, 2014; Meketa Investment Group, 2019).

We interpret the transition management evidence as identifying an important component of a broader institutional execution channel. Transition managers are unlikely to account for every separate account flow in our sample. Some reallocations may be executed directly by asset managers, and institutional mandates may receive more careful execution even without a formal transition manager. These possibilities do not weaken the main interpretation. Instead, they point to the same economic force: separate account flows are implemented in an institutional environment with more planning, discretion, and execution technology than the daily-liquidity environment of open-end mutual funds. The central implication is therefore not that one specific intermediary eliminates price impact, but that the transmission from investor demand to prices depends on the institutional process through which trades are executed.

7 Concluding Discussion

We demonstrate that how investor demand moves prices depends not only on the size of the demand shock, but also on the institution through which that demand is executed. Using a novel quarterly holdings dataset of separate accounts, the dominant investment vehicle for institutional investors and a sector comparable in size to the mutual fund industry, we document a striking contrast between mutual funds and separate accounts. While mutual fund flows generate substantial price impact, amplify price fragility, and trigger fire-sale dynamics, separate account flows, despite generating comparable portfolio demand, exhibit negligible price effects. The contrast persists within mutual fund–separate

account twins managed under identical strategies by the same portfolio managers, and it remains after controlling for market-wide liquidity conditions at the time flows occur.

The evidence points to institutional execution design as an important determinant of demand transmission. While mutual funds must accommodate flows within a daily-liquidity structure, separate account reallocations are implemented in an institutional environment that allows longer notification horizons, greater execution discretion, and, for many large reallocations, specialized transition management. Trade-level evidence from a leading transition manager shows that large institutional transitions can be executed at low cost and with little measurable price impact. These findings support the view that institutional execution design plays an important role in dampening the price effects of separate account flows.

Our results have several implications for the literature on investor demand and asset prices. They show that demand shocks need not have uniform price effects across investment vehicles. Even flows associated with the same investment strategies and the same portfolio managers can influence prices very differently depending on the vehicle through which they are implemented. This finding cautions against generalizing the demand effects of mutual funds to the broader institutional landscape. It also suggests that price multipliers estimated from pooled 13F holdings may obscure economically important heterogeneity in how different types of intermediated demand transmit into prices. The broader lesson is that demand-based asset pricing should distinguish not only who demands an asset and how much they demand, but also how that demand is transformed into trades.

Our results also speak to the design of liquidity provision in open-end funds. Daily NAV redeemability offers valuable liquidity transformation, but the costs of this liquidity

can extend beyond fund investors when flow-driven trades create price pressure, fire-sale dynamics, and excess volatility in the underlying securities. The comparison with separate accounts shows that these spillovers are not an unavoidable consequence of investor demand. Rather, they depend on the institutional process through which demand is implemented. Policies or fund-design choices that better align the timing, pricing, and execution of investor liquidity with the trading costs of the underlying portfolio may therefore reduce the extent to which fund flows destabilize asset prices. More broadly, our evidence suggests that flow-driven market fragility depends not only on who demands liquidity, but also on how intermediaries transform that demand into trades.

References

Ang, Andrew, and Ananth Madhavan, 2024, Filled and killed: Forecast and realized trading costs across horizons from global equity and fixed income portfolio trades, *Available at SSRN 4782032* .

Bagley, Travis, and Shashank Kothare, 2014, Transition management explained, Technical report, Russell Investments.

Barberis, Nicholas, Andrei Shleifer, and Jeffrey Wurgler, 2005, Comovement, *Journal of Financial Economics* 75, 283–317.

Ben-David, Itzhak, Francesco Franzoni, and Rabih Moussawi, 2018, Do ETFs increase volatility?, *Journal of Finance* 73, 2471–2535.

Ben-David, Itzhak, Francesco Franzoni, Rabih Moussawi, and John Sedunov, 2021, The granular nature of large institutional investors, *Management Science* 67, 6629–6659.

Ben-David, Itzhak, Jiacui Li, Andrea Rossi, and Yang Song, 2022a, Ratings-driven demand and systematic price fluctuations, *The Review of Financial Studies* 35, 2790–2838.

Ben-David, Itzhak, Jiacui Li, Andrea Rossi, and Yang Song, 2022b, What do mutual fund investors really care about?, *Review of Financial Studies* 35, 1723–1774.

Berk, Jonathan B, and Richard C Green, 2004, Mutual fund flows and performance in rational markets, *Journal of political economy* 112, 1269–1295.

Busse, Jeffrey A, Tarun Chordia, Lei Jiang, and Yuehua Tang, 2021, Transaction costs, portfolio characteristics, and mutual fund performance, *Management Science* 67, 1227–1248.

Busse, Jeffrey A, Amit Goyal, and Sunil Wahal, 2010, Performance and persistence in institutional investment management, *The Journal of Finance* 65, 765–790.

Chan, Louis KC, Stephen G Dimmock, and Josef Lakonishok, 2009, Benchmarking money manager performance: Issues and evidence, *The Review of Financial Studies* 22, 4553–4599.

Chang, Yen-Cheng, Harrison Hong, and Inessa Liskovich, 2015, Regression discontinuity and the price effects of stock market indexing, *Review of Financial Studies* 28, 212–246.

Chen, Joseph, Harrison Hong, Ming Huang, and Jeffrey D Kubik, 2004, Does fund size erode mutual fund performance? the role of liquidity and organization, *American Economic Review* 94, 1276–1302.

Chen, Qi, Itay Goldstein, and Wei Jiang, 2010, Payoff complementarities and financial fragility: Evidence from mutual fund outflows, *Journal of financial economics* 97, 239–262.

Chernenko, Sergey, and Adi Sunderam, 2020, Do fire sales create externalities?, *Journal of Financial Economics* 135, 602–628.

Chinco, Alex, and Marco Sammon, 2023, The passive-ownership share is double what you think it is, *Available at SSRN 4188052* .

Cochrane, John H., 2011, Presidential address: Discount rates, *Journal of Finance* 66, 1047–1108.

Coles, Jeffrey L., Davidson Heath, and Matthew C. Ringgenberg, 2022, On index investing, *Journal of Financial Economics* 145, 665–683.

Coval, Joshua, and Erik Stafford, 2007, Asset fire sales (and purchases) in equity markets, *Journal of Financial Economics* 86, 479–512.

Duffie, Darrell, 2010, Presidential address: Asset price dynamics with slow-moving capital, *The Journal of finance* 65, 1237–1267.

Edelen, Roger, Richard Evans, and Gregory Kadlec, 2013, Shedding light on "invisible" costs: Trading costs and mutual fund performance, *Financial Analysts Journal* 69, 33–44.

Edelen, Roger M, 1999, Investor flows and the assessed performance of open-end mutual funds, *Journal of Financial Economics* 53, 439–466.

Elton, Edwin J, Martin J Gruber, and Christopher R Blake, 2014, The performance of separate accounts and collective investment trusts, *Review of Finance* 18, 1717–1742.

Evans, Richard B, and Rüdiger Fahlenbrach, 2012, Institutional investors and mutual fund governance: Evidence from retail–institutional fund twins, *The Review of Financial Studies* 25, 3530–3571.

Evans, Richard B, Juan-Pedro Gómez, and Rafael Zambrana, 2023, Mifid ii research unbundling: Cross-border impact on asset managers .

Evans, Richard B, Martin Rohleder, Hendrik Tentesch, and Marco Wilkens, 2022, Diseconomies of scale in quantitative and fundamental investment styles, *Journal of Financial and Quantitative Analysis* 1–29.

Fricke, Daniel, and Hannes Wilke, 2023, Connected funds, *The Review of Financial Studies* 36, 4546–4587.

Froot, Kenneth, and Melvyn Teo, 2008, Style investing and institutional investors, *Journal of Financial and Quantitative Analysis* 43, 883–906.

Gabaix, Xavier, and Ralph SJ Koijen, 2021, In search of the origins of financial fluctuations: The inelastic markets hypothesis, Technical report, National Bureau of Economic Research.

Gabaix, Xavier, and Ralph S.J. Koijen, 2022, In search of the origins of financial fluctuations: The inelastic markets hypothesis, Working paper, Harvard University.

Gabaix, Xavier, and Ralph SJ Koijen, 2024, Granular instrumental variables, *Journal of Political Economy* 132, 2274–2303.

Gerakos, Joseph, Juhani T Linnainmaa, and Adair Morse, 2021, Asset managers: Institutional performance and factor exposures, *The Journal of Finance* 76, 2035–2075.

Goldstein, Itay, Hao Jiang, and David T Ng, 2017, Investor flows and fragility in corporate bond funds, *Journal of Financial Economics* 126, 592–613.

Greenwood, Robin, and Marco Sammon, 2025, The disappearing index effect, *The Journal of Finance* 80, 657–698.

Greenwood, Robin, and David Thesmar, 2011, Stock price fragility, *Journal of Financial Economics* 102, 471–490.

Haddad, Valentin, Paul Huebner, and Erik Loualiche, 2024, How competitive is the stock market? theory, evidence from portfolios, and implications for the rise of passive investing, *American Economic Review* Forthcoming.

Haddad, Valentin, Paul Huebner, and Erik Loualiche, 2025, How competitive is the stock market? theory, evidence from portfolios, and implications for the rise of passive investing, *American Economic Review* 115, 975–1018.

Harris, Lawrence, and Eitan Gurel, 1986, Price and volume effects associated with changes in the S&P 500 list: New evidence for the existence of price pressures, *Journal of Finance* 41, 815–829.

Huang, Shiyang, Xu Lu, Yang Song, and Hong Xiang, 2025a, Remeasuring scale in active management, *Available at SSRN 4599484* .

Huang, Shiyang, Yang Song, and Hong Xiang, 2025b, Noise trading and asset pricing factors, *Management Science* 71, 6961–6978.

Jenkinson, Tim, Howard Jones, and Jose Vicente Martinez, 2016, Picking winners? investment consultants' recommendations of fund managers, *The Journal of Finance* 71, 2333–2370.

Jin, Dunhong, Marcin Kacperczyk, Bige Kahraman, and Felix Suntheim, 2022, Swing pricing and fragility in open-end mutual funds, *The Review of Financial Studies* 35, 1–50.

Jones, Howard, Jose Vicente Martinez, and Alexander Montag, 2022, Separate account v.s. mutual fund investors: Manager selection and performance, *Available at SSRN 3883499* .

Jones, Howard, Jose Vicente Martinez, and Alexander Montag, 2025, What twins tell us about the performance of separate accounts, mutual funds, and their investors, *Available at SSRN 3883499* .

Kacperczyk, Marcin, Clemens Sialm, and Lu Zheng, 2005, On the industry concentration of actively managed equity mutual funds, *Journal of Finance* 60, 1983–2011.

Koijen, Ralph SJ, Robert J Richmond, and Motohiro Yogo, 2024, Which investors matter for equity valuations and expected returns?, *Review of Economic Studies* 91, 2387–2424.

Koijen, Ralph SJ, and Motohiro Yogo, 2019, A demand system approach to asset pricing, *Journal of Political Economy* 127, 1475–1515.

Kyle, Albert S, and Anna A Obizhaeva, 2016, Market microstructure invariance: Empirical hypotheses, *Econometrica* 84, 1345–1404.

Li, Jiacui, 2022, What drives the size and value factors?, *Review of Asset Pricing Studies* 12, 845–885.

Li, Jiacui, and Zihan Lin, 2025, Price multipliers are larger at more aggregate levels, *Available at SSRN 4038664* .

Lou, Dong, 2012, A flow-based explanation for return predictability, *Review of Financial Studies* 25, 3457–3489.

Meketa Investment Group, 2019, Transition management, Technical report.

Nohel, Tom, Z. Jay Wang, and Lu Zheng, 2010, Side-by-side management of hedge funds and mutual funds, *The Review of Financial Studies* 23, 2342–2373.

Obizhaeva, Anna A, 2009, Portfolio transitions and stock price dynamics, *Available at SSRN 2591294* .

Obizhaeva, Anna A, 2012, Liquidity estimates and selection bias, *Available at SSRN 1178722* .

Pástor, L'uboš, and Robert F. Stambaugh, 2003, Liquidity risk and expected stock returns, *Journal of Political Economy* 111, 642–685.

Pástor, L'uboš, Robert F Stambaugh, and Lucian A Taylor, 2015, Scale and skill in active management, *Journal of financial economics* 116, 23–45.

Pavlova, Anna, and Taisiya Sikorskaya, 2023, Benchmarking intensity, *The Review of Financial Studies* 36, 859–903.

Peng, Cameron, and Chen Wang, 2023, Factor rebalancing, *Available at SSRN 3327849* .

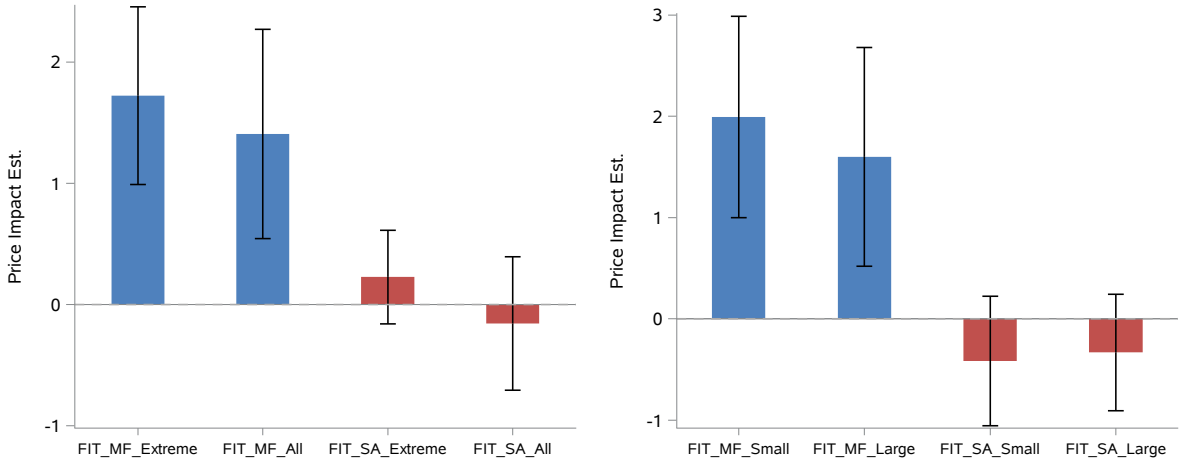
Shleifer, Andrei, 1986, Do demand curves for stocks slope down?, *Journal of Finance* 41, 579–590.

Song, Yang, 2020, The mismatch between mutual fund scale and skill, *The Journal of Finance* 75, 2555–2589.

Teo, Melvyn, and Sung-Jun Woo, 2004, Style effects in the cross-section of stock returns, *Journal of Financial Economics* 74, 367–398.

Wahal, Sunil, and Albert Y Wang, 2022, Flow diversification ([Sl]: SSRN).

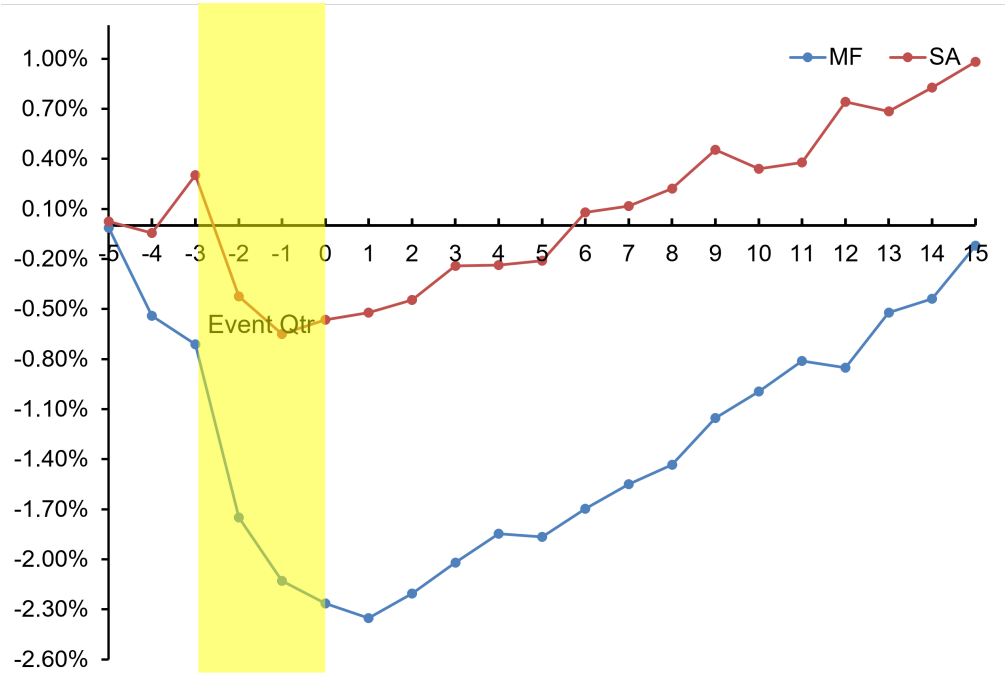
Wurgler, Jeffrey, and Ekaterina Zhuravskaya, 2002, Does arbitrage flatten demand curves for stocks?, *Journal of Business* 75, 583–608.



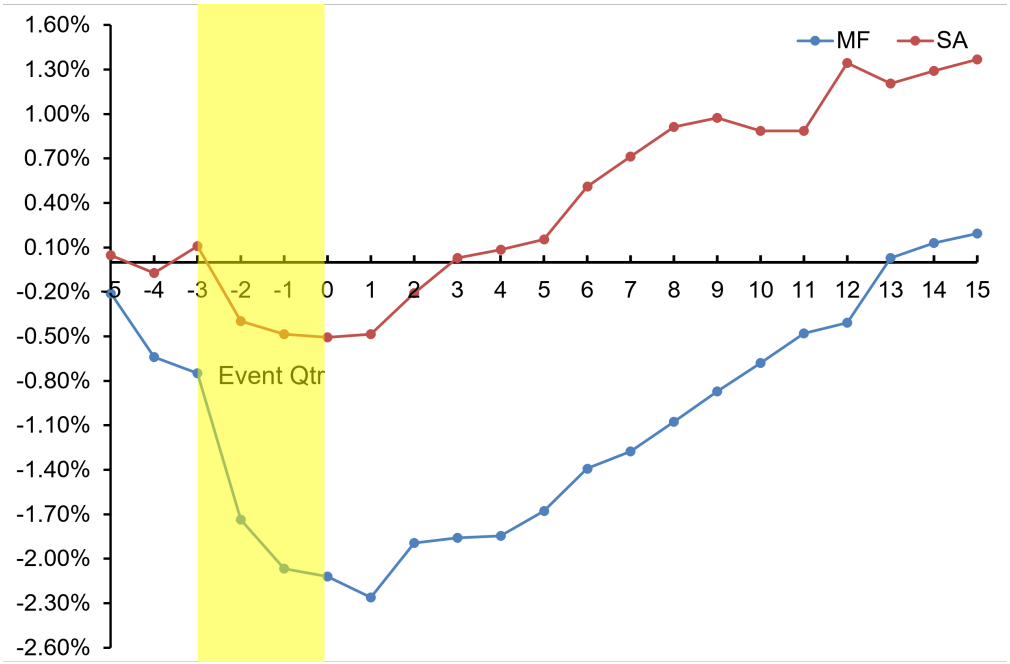
(a) Extreme FIT subsample

(b) Family size

Figure 2: **Heterogeneity in flow-induced price impact.** Panel (a) plots the estimated price impact of flow-induced trading together with 95% confidence intervals for the extreme-FIT subsample and the full sample. In each quarter, we rank stocks by the FIT measure (equation (3)) and define the extreme-FIT subsample as the union of the bottom and top 10% of stocks. We then estimate price impact by running a univariate regression of quarterly stock returns on the contemporaneous FIT measure. In the figure, *FIT_MF_Extreme* denotes the estimate from the extreme-FIT subsample, whereas *FIT_MF_All* denotes the estimate from the full sample (column 5 of Table 3). We plot the analogous estimates for separate-account FIT as well. Panel (b) plots the estimated FIT price impact and 95% confidence intervals when FIT is constructed separately from holdings of large and small fund families. In each quarter, we split mutual funds (and, separately, separate accounts) into two groups based on family-level AUM. We then reconstruct the FIT measure separately using holdings from large-family funds and small-family funds, and estimate the contemporaneous price impact of each measure in separate regressions. In the figure, *FIT_MF_Small* denotes the price impact estimate based on the FIT measure constructed from mutual funds in smaller families.



(a) Full sample



(b) Twin fund sample

Figure 3: **Fire-sale event CARs.** This figure plots the event-time cumulative abnormal returns (CAR) associated with mutual fund fire sale and separate account fire sale events during 2001.Q4-2024.Q4. In each calendar quarter, we define stocks under fire sale as the bottom 10% of stocks with the lowest *Pressure*. Then we track the average CAR of fire-sale stocks from one quarter before the fire sale to five quarters after. CAR is computed based on DGTW-adjusted returns. The horizontal axis indicates the event month, where months -2, -1, and 0 belong to the fire sale event quarter. The vertical axis indicates CAR. Panel (a) is based on the full sample of funds; Panel (b) is based on funds with twin vehicles.

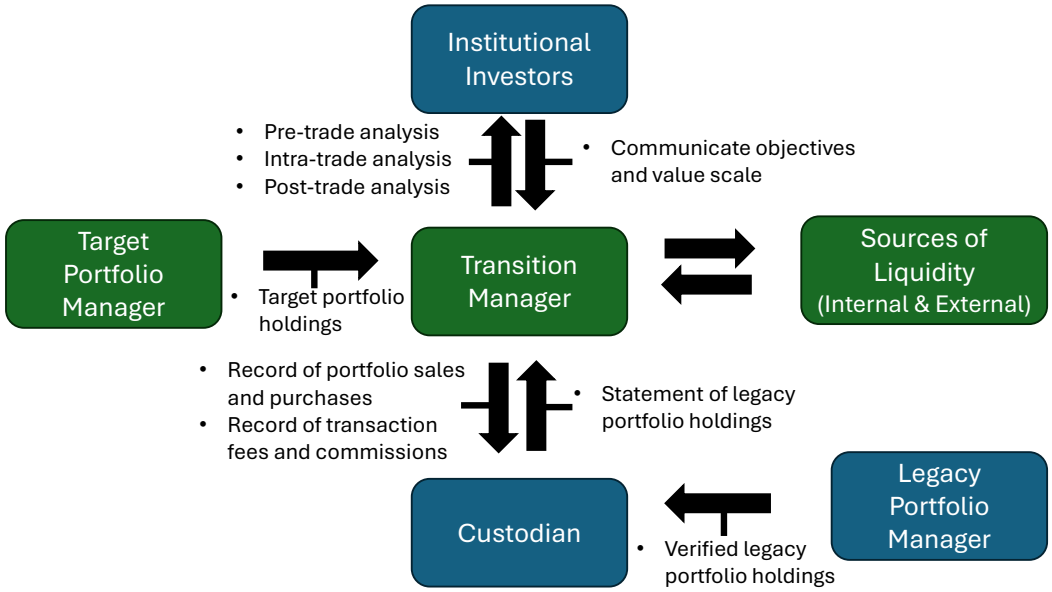


Figure 4: **Transition management process.**

Table 1: **Summary statistics.** Panel A reports summary statistics for mutual funds and separate account composites at the fund-quarter level from 2001Q4 to 2024Q4. Pct_Flow is quarterly percentage fund flow, defined in equation (1). Flow_Vol is the past eight-quarter rolling standard deviation of Pct_Flow. Flow_AR(1) is the first-order autocorrelation of Pct_Flow over the past eight quarters. Turnover is the self-reported annual portfolio turnover ratio. Cash is the percent of portfolio holdings allocated to cash. Panel B reports summary statistics of stock-quarter observations. NYSE Size Pctl is the stock's size percentile among NYSE stocks. FIT variables are flow-induced trading measures, as defined in Section 4.2. The superscript MF (SA) indicates a mutual-fund-based (separate-account-based) measure. σ is the standard deviation of daily stock return in a quarter. Pressure variables are fire-sale pressure measures, as defined in Section 4.3. $\sqrt{\text{G}}$ is the square root of fragility, as defined in Section 4.4.

Panel A: Fund-Quarter Observations

	Mean	STD	P10	P25	P50	P75	P90
Mutual Funds:							
Total Assets ($mi)	2028.30	14905.59	18.16	61.42	257.76	1028.86	3311.01
Gross Return (Qtr)	0.03	0.10	−0.10	−0.02	0.04	0.08	0.13
Age (yr)	15.18	12.98	2.75	6.00	12.17	20.67	29.83
Pct_Flow	0.02	0.30	−0.09	−0.05	−0.02	0.02	0.12
Flow_Vol	0.12	0.25	0.01	0.02	0.05	0.10	0.23
Flow_AR(1)	0.09	0.39	−0.40	−0.19	0.07	0.37	0.62
Turnover (%)	79.14	139.50	12.00	25.80	51.00	91.00	151.00
Cash (%)	2.82	12.68	0.00	0.54	1.77	3.78	6.87
Sep Accounts (Composite):							
Total Assets ($mi)	1374.96	8164.50	1.60	16.96	127.72	688.80	2428.40
Gross Return (Qtr)	0.03	0.10	−0.09	−0.01	0.04	0.08	0.13
Age (yr)	11.18	9.25	1.50	4.17	9.17	16.33	23.67
Pct_Flow	0.06	0.53	−0.12	−0.04	0.00	0.03	0.17
Flow_Vol	0.25	0.44	0.02	0.04	0.09	0.22	0.56
Flow_AR(1)	−0.07	0.37	−0.51	−0.31	−0.11	0.15	0.44
Turnover (%)	60.05	168.05	11.93	22.15	40.13	73.58	118.00
Cash (%)	2.89	7.58	0.00	0.34	1.70	3.56	6.34

Table 1 Continued

Panel B: Stock-Quarter Observations							
	Mean	STD	P10	P25	P50	P75	P90
NYSE Size Pctl (%)	28.02	29.16	0.39	3.03	16.47	47.29	77.03
Return (Qtr, %)	3.20	35.47	−28.57	−11.79	1.14	14.01	31.78
FIT^{MF} (%)	−0.04	0.60	−0.55	−0.24	−0.02	0.08	0.40
FIT^{SA} (%)	−0.17	3.70	−0.67	−0.28	−0.03	0.02	0.24
σ (%)	2.13	1.27	1.04	1.33	1.80	2.52	3.57
$\sqrt{\text{G}}^{\text{MF}}$ (%)	0.40	0.38	0.11	0.18	0.30	0.50	0.78
$\sqrt{\text{G}}^{\text{SA}}$ (%)	0.74	1.52	0.09	0.18	0.36	0.73	1.48
$\text{Pressure}^{\text{MF}}$ (%)	−0.03	1.10	−0.13	−0.04	−0.01	0.00	0.05
$\text{Pressure}^{\text{SA}}$ (%)	−0.07	1.97	−0.19	−0.07	−0.02	0.00	0.04

Table 2: **Fund holding position changes in response to flows.** This table reports results from Fama-MacBeth regressions. The unit of observation is a fund-stock-quarter, and the sample period is 2001Q4-2024Q4. The sample includes only each fund's holdings at the beginning of each quarter; new positions established during the quarter are excluded. The dependent variable is the percentage change in the fund's position in a holding stock, $Shares_{i,j,t}/Shares_{i.j.t-1} - 1$. The key independent variable is the fund's percentage flow in the quarter. We control for holding-stock characteristics measured at the previous quarter-end: Own is the fund's ownership of the holding stock; illiq is the stock's Amihud illiquidity measure, scaled by 10^8; and Weight is the stock's weight in the fund portfolio. The sample is split into inflow and outflow subsamples based on the sign of fund flow in the quarter. t-statistics in parentheses are calculated based on standard errors with Newey-West correction of four lags. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Mutual Funds				Separate Accounts			
	Inflow		Outflow		Inflow		Outflow	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Flow	0.700***	0.704***	0.748***	0.704***	0.784***	0.724***	0.934***	0.947***
	(16.40)	(15.73)	(16.90)	(14.57)	(57.70)	(38.31)	(77.34)	(63.36)
Flow×Own		−55.969***		25.519***		2.087		5.753***
		(−5.19)		(6.62)		(0.70)		(4.42)
Flow×illiq		−0.002		−0.000		−0.013***		−0.011**
		(−1.40)		(−0.03)		(−3.74)		(−2.56)
Flow×Weight		2.148		2.057		4.893***		−2.655***
		(1.33)		(1.34)		(9.36)		(−4.73)
Own		−1.640***		−1.055***		−0.184		−0.194
		(−4.41)		(−6.64)		(−0.67)		(−1.09)
illiq		0.000*		0.000		−0.002**		−0.001
		(1.68)		(0.14)		(−2.20)		(−1.64)
Weight		−4.851***		−4.192***		−3.090***		−2.886***
		(−15.14)		(−15.18)		(−8.37)		(−9.71)
No. Obs.	12,047,784	12,047,784	15,451,947	15,451,947	4,158,025	4,158,025	7,371,680	7,371,680
Adj. R^2	0.096	0.102	0.006	0.01	0.341	0.347	0.043	0.046

Table 3: **Price impact of Flow-induced trading.** This table reports results from Fama-MacBeth regressions. The unit of observation is stock-quarter, and the sample period is 2001Q4-2024Q4. The dependent variable is quarterly stock returns. Panel A reports the contemporaneous relation between stock returns and flow-induced trading. The key independent variables are flow-induced trading by mutual funds (FIT^{MF}) and separate accounts (FIT^{SA}) in the same quarter. Control variables include stock size, book-to-market ratio (BM), past 12-month returns skipping the most recent month (MOM), asset growth (AG), gross profitability (GP), and Amihud illiquidity (Illiq). Panel B reports the reversal effect of flow-induced trading. The key independent variables are past 12-quarter average FIT by mutual funds ($PastFIT^{MF}_{t-12:t-1}$) and separate accounts ($PastFIT^{SA}_{t-12:t-1}$). t-statistics in parentheses are calculated based on standard errors with Newey-West correction of four lags. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Contemporaneous Price Impact

	(1)	(2)	(3)	(4)	(5)	(6)
FIT^{MF}	1.4057***	1.5988***			1.5734***	1.6420***
	(3.19)	(5.24)			(4.09)	(5.80)
FIT^{SA}			−0.1566	0.0562	−0.3708	−0.1898
			(−0.56)	(0.27)	(−1.65)	(−1.03)
Size		0.0007		0.0004		0.0006
		(0.42)		(0.22)		(0.37)
BM		0.0108**		0.0109**		0.0107**
		(2.35)		(2.33)		(2.34)
MOM		0.0028		0.0038		0.0027
		(0.34)		(0.46)		(0.34)
AG		−0.0045**		−0.0046**		−0.0045**
		(−2.12)		(−2.16)		(−2.12)
GP		0.0275***		0.0275***		0.0275***
		(4.33)		(4.17)		(4.33)
Illiq		−950.57		−1405.9		−714.38
		(−0.31)		(−0.45)		(−0.24)
No. Obs.	316,515	316,515	316,515	316,515	316,515	316,515
Adj. R^2	0.0041	0.0331	0.0008	0.0309	0.0046	0.0334

Table 3 Continued

Panel B: Reversals

	(1)	(2)	(3)	(4)	(5)	(6)
$\text{PastFIT}^{\text{MF}}_{\text{t}-12:\text{t}-1}$	−1.9480**	−1.4409**			−1.7992**	−1.3854**
	(−2.19)	(−2.33)			(−2.04)	(−2.22)
$\text{PastFIT}^{\text{SA}}_{\text{t}-12:\text{t}-1}$			−0.2137	−0.2629	−0.1084	−0.1592
			(−0.71)	(−1.18)	(−0.32)	(−0.61)
Size		−0.0000		0.0002		−0.0001
		(−0.01)		(0.13)		(−0.08)
BM		0.0102**		0.0105**		0.0100**
		(2.30)		(2.24)		(2.26)
MOM		0.0037		0.0032		0.0031
		(0.45)		(0.39)		(0.38)
AG		−0.0047**		−0.0045**		−0.0045**
		(−2.23)		(−2.12)		(−2.11)
GP		0.0263***		0.0266***		0.0260***
		(4.16)		(4.10)		(4.14)
Illiq		−1346.6		−859.21		−705.96
		(−0.41)		(−0.28)		(−0.23)
No. Obs.	314,559	314,559	311,513	311,513	311,265	311,265
Adj. R^2	0.0027	0.0319	0.0008	0.0314	0.0034	0.0327

Table 4: **Fire-sale event CARs.** This table reports the event-time cumulative abnormal returns (CAR) associated with mutual fund fire-sale and separate account fire-sale events during 2001Q4-2024Q4. The test design follows Coval and Stafford (2007). In each calendar quarter, we define stocks under fire sale as the bottom 10% of stocks with the lowest *Pressure* from mutual funds or separate accounts (see Section 4.3 for definition). We then track the average event-time CAR for fire-sale stocks from one quarter before the fire-sale quarter to five quarters thereafter. Finally, we compute the time-series average event-time CAR and the associated t-values across the calendar quarters. The CAR is reported for each event month, where months -2, -1, and 0 belong to the fire-sale event quarter. The benchmark returns for CAR calculation are the equal-weighted average returns of stocks held by mutual funds at each time point.

	Mutual Funds		Separate Accounts	
Event Month	CAR	t-value	CAR	t-value
-5	-0.01%	-0.07	0.02%	0.12
-4	-0.54%	-2.04	-0.05%	-0.15
-3	-0.71%	-2.39	0.30%	0.85
-2	-1.75%	-5.96	-0.42%	-1.12
-1	-2.13%	-6.21	-0.65%	-1.60
0	-2.27%	-5.82	-0.57%	-1.24
1	-2.35%	-5.83	-0.52%	-1.03
2	-2.21%	-5.17	-0.45%	-0.78
3	-2.02%	-4.39	-0.24%	-0.39
4	-1.85%	-3.66	-0.24%	-0.35
5	-1.87%	-3.48	-0.21%	-0.30
6	-1.70%	-3.04	0.08%	0.11
7	-1.55%	-2.45	0.12%	0.16
8	-1.43%	-2.11	0.22%	0.30
9	-1.15%	-1.66	0.45%	0.57
10	-1.00%	-1.32	0.34%	0.42
11	-0.81%	-1.06	0.38%	0.49
12	-0.85%	-1.08	0.74%	0.93
13	-0.52%	-0.64	0.68%	0.84
14	-0.44%	-0.51	0.83%	1.01
15	-0.12%	-0.14	0.98%	1.19

Table 5: **Fragility and future stock volatility.** This table reports Fama-MacBeth regressions of one-quarter-ahead stock return volatility, σ_{t+1}, on the square root of mutual fund fragility and separate account fragility. The sample period is from 2002Q1 to 2024Q4, and the sample stocks include stocks with market capitalization above the NYSE median in each quarter. σ_{t+1} is the standard deviation of daily stock returns in quarter $t+1$, and $\sqrt{G_t}$ is the square root of fragility defined in Section 4.4. The control variable is the stock return volatility in quarter t. t-statistics in parentheses are calculated based on standard errors with Newey-West correction of four lags. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
$\sqrt{G_t}^{MF}$	0.530***	0.194***			0.511***	0.188***
	(7.727)	(6.686)			(6.622)	(5.874)
$\sqrt{G_t}^{SA}$			0.078***	0.025***	0.025	0.008
			(5.561)	(3.918)	(1.659)	(1.053)
σ_t		0.724***		0.735***		0.724***
		(35.758)		(37.051)		(35.646)
No. Obs	84,957	84,957	84,957	84,957	84,957	84,957
Adj. R^2	0.037	0.509	0.005	0.504	0.04	0.509

Table 6: **Price impact in the twin-fund sample.** Results in this table are based on mutual funds and separate accounts with twin vehicles. Panel A reports estimated price impact of FIT using the specification in Table 3. Panel B reports results from predicting stock return volatility by fragility using the specification in Table 5. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Price impact of FIT				
	(1)	(2)	(3)	(4)
FIT^{MF}	1.7468***		1.9887***	1.8017***
	(3.52)		(3.84)	(4.08)
FIT^{SA}		0.5626	−0.0419	0.0212
		(1.61)	(−0.12)	(0.08)
Size				0.0000
				(0.04)
BM				0.0070
				(1.32)
MOM				−0.0007
				(−0.08)
AG				−0.0038*
				(−1.76)
GP				0.0251***
				(3.36)
Illiq				5161.83
				(0.75)
No. Obs.	264,179	264,179	264,179	264,179
Adj. R^2	0.0027	0.0009	0.0032	0.0389
Panel B: Predict volatility by fragility				
	(1)	(2)	(3)	(4)
$\sqrt{G_t}^{MF}$	0.462***		0.443***	0.174***
	(6.913)		(6.382)	(6.026)
$\sqrt{G_t}^{SA}$		0.087***	0.045**	0.014
		(3.989)	(2.350)	(1.328)
σ_t				0.731***
				(36.901)
No. Obs	84,128	84,128	84,128	84,128
Adj. R^2	0.015	0.003	0.016	0.505

Table 7: **Market liquidity and price impact of FIT.** This table reports the price impact of flow-induced trading in subsamples classified by market liquidity. We use the market liquidity measure from Pástor and Stambaugh (2003) to split the 2001Q4-2024Q4 sample period into two halves. Within each subsample, we re-estimate the Fama-MacBeth regressions as in Table 3. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Mkt Liquidity:		Lower			Higher	
FIT^{MF}	1.9398***		1.9130***	1.2650**		1.3768***
	(4.21)		(4.42)	(2.63)		(3.24)
FIT^{SA}		0.1803	−0.0659		−0.0653	−0.3111*
		(0.65)	(−0.29)		(−0.30)	(−1.76)
Size	0.0018	0.0014	0.0017	−0.0004	−0.0007	−0.0005
	(0.75)	(0.59)	(0.71)	(−0.25)	(−0.38)	(−0.29)
BM	0.0085	0.0087	0.0085	0.0131***	0.0130***	0.0129***
	(1.29)	(1.29)	(1.28)	(2.79)	(2.78)	(2.80)
MOM	0.0028	0.0040	0.0028	0.0027	0.0036	0.0027
	(0.24)	(0.33)	(0.23)	(0.37)	(0.48)	(0.37)
AG	−0.0035	−0.0036	−0.0035	−0.0056**	−0.0056**	−0.0055**
	(−1.17)	(−1.21)	(−1.18)	(−2.17)	(−2.19)	(−2.16)
GP	0.0228***	0.0221***	0.0228***	0.0321***	0.0328***	0.0322***
	(2.97)	(2.91)	(2.98)	(3.82)	(3.66)	(3.81)
Illiq	2155.03	1636.90	2176.49	−3990.1	−4383.9	−3543.7
	(0.85)	(0.61)	(0.87)	(−0.79)	(−0.84)	(−0.75)
No. Obs.	158,139	158,139	158,139	158,376	158,376	158,376
Adj. R^2	0.0376	0.0351	0.038	0.0287	0.0268	0.0289

Table 8: **Summary statistics for transition programs.** This table reports summary statistics for transition programs. Panel A reports the distribution of program-level total transition volume (in $million), the number of distinct stocks traded in the program, and the total number of trades in the program. Panel B reports the distribution of program duration. The duration of a program is defined as the number of days between the first and last trades of the program. The column "Program%" reports the distribution by number of programs. The column "Volume%" reports the distribution by program-level trading volume.

Panel A: Program size and #trades

	Mean	P25	P50	P75	Max
Volume ($mi)	248	27	92	238	8,702
# Stocks Traded	237	27	96	270	3,161
# Trades	578	47	173	516	52,316

Panel B: Program duration

Days	Program%	Cumulative%	Volume%	Cumulative%
1	31.45	31.45	11.05	11.05
2	12.81	44.26	8.00	19.05
3-5	15.89	60.16	15.13	34.18
6-10	17.52	77.67	22.58	56.76
11-20	7.72	85.39	11.92	68.68
21-30	2.97	88.36	8.05	76.73
30+	11.64	100.00	23.27	100.00

Table 9: **Transition trade costs.** This table reports Fama-MacBeth regression results at the transition-trade level. The sample period is 2013-2021. The dependent variable is trade cost, defined in equation (8). Columns (1)–(3) use the previous close as the benchmark price; columns (4)–(6) use the current-day open. The key independent variable is trade size, defined as the trade dollar volume scaled by the stock's same-day dollar trading volume from CRSP. The controls are stock and program characteristics, including the natural logarithm of market capitalization, idiosyncratic volatility, Amihud illiquidity, daily turnover ratio, the inverse of stock price, NASDAQ-listed indicator, and the program's total dollar trading volume. We report results separately for all trades, buy trades, and sell trades. To estimate the coefficients, we first pool all trades in the same calendar month and run cross-sectional regressions. We then calculate the time-series average of the coefficient estimates and the associated t-statistics across months. t-statistics in parentheses are calculated based on standard errors with Newey-West correction of four lags. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Benchmark Price:	Previous Close			Current Open		
Trade Type:	All	Buy	Sell	All	Buy	Sell
	(1)	(2)	(3)	(4)	(5)	(6)
TradeSize	0.010	0.049	0.035	0.012	0.051	0.005
	(1.28)	(0.87)	(1.59)	(1.12)	(1.04)	(1.11)
Size	-0.000***	-0.000***	-0.000	-0.000***	-0.000***	-0.000**
	(-3.37)	(-3.29)	(-1.35)	(-6.31)	(-4.42)	(-2.56)
IVol	0.026***	-0.025	0.035***	0.030***	-0.018**	0.045***
	(4.29)	(-1.41)	(5.78)	(4.17)	(-2.02)	(7.11)
illiq	51012.7**	-146916	50268.4*	33281.6**	21336.6	108115*
	(2.24)	(-0.79)	(1.96)	(2.52)	(0.08)	(1.67)
Turnover	-0.017**	0.004	-0.008	-0.012	-0.004	-0.003
	(-2.17)	(0.22)	(-0.68)	(-1.29)	(-0.21)	(-0.35)
PRC_inverse	0.002	0.002	0.014	0.002	-0.000	0.012
	(0.92)	(0.44)	(1.23)	(1.00)	(-0.09)	(1.54)
Nasdaq	0.000	0.000	-0.000	0.000	0.000	0.000
	(0.38)	(0.42)	(-0.17)	(1.63)	(0.16)	(1.43)
Program_volume	0.000	0.000	0.000	0.000**	0.000	0.000
	(1.21)	(0.17)	(0.93)	(2.35)	(0.58)	(1.09)
No. Obs.	872,353	390,980	481,373	872,334	390,977	481,357
Adj. R^2	0.024	0.04	0.043	0.028	0.036	0.046

Table 10: **Program-level trade costs.** This table reports transition-program-level trade costs (%). We first compute trade-level trade costs using equation (8), where we use either the previous closing price or the current-day opening price as the benchmark. We then compute the program-level trade cost as the volume-weighted or equal-weighted average of trade costs across all trades in the program. In Panel A, we sort all programs into quintiles by total program volume and report the average program-level trade cost for each quintile. In Panel B, we sort all programs into four groups by program duration, defined as the number of days between the first and last trading dates of the program.

Panel A: Programs sorted by trading volume						
Benchmark	Weighting Scheme	1 (Small)	2	3	4	5 (Large)
Previous Close	Volume-weighted	0.11	0.04	0.09	0.04	0.06
Previous Close	Equal-weighted	0.11	0.06	0.09	0.06	0.09
Current Open	Volume-weighted	0.15	0.08	0.09	0.07	0.09
Current Open	Equal-weighted	0.16	0.09	0.08	0.08	0.10
Panel B: Programs sorted by duration						
Benchmark	Weighting Scheme	1 Day	2-5	6-10	>10	
Previous Close	Volume-weighted	0.08	0.07	0.04	0.06	
Previous Close	Equal-weighted	0.06	0.10	0.07	0.09	
Current Open	Volume-weighted	0.11	0.08	0.10	0.09	
Current Open	Equal-weighted	0.10	0.11	0.11	0.10	

Table 11: **Transition management and large separate-account outflows.** Panel A reports the time-series relation between the occurrence of separate account fire sales and the occurrence of transition programs during 2013Q3-2021Q4. We use the full-sample 10th percentile value of quarterly flows to define the fire sales for separate accounts or mutual funds in each quarter. SA_FireSale% (MF_FireSale%) is the number of separate account (mutual fund) fire sales in a quarter as a fraction of the number of existing separate accounts (mutual funds) in the quarter. Transit_Program% is the number of sales-oriented transition programs as a fraction of the number of existing separate accounts in the quarter. A program is defined as sales-oriented if the sales volume accounts for more than 50% of the total trading volume of the program. We perform a time-series regression of Transit_Program% on contemporaneous SA_FireSale% and MF_FireSale%, controlling for contemporaneous stock market liquidity, stock market return, and the lagged Transit_Program%. Standard errors are computed with a Newey–West adjustment of one lag. In Panel B, we match transition programs with separate account-quarter observations in our sample (see Appendix Section C). We define a fund-quarter as experiencing extreme outflows if the flow is lower than the 10th or 5th percentile values of flows in the quarter. We report the probability of experiencing extreme outflows among the matched separate account-quarters and the full sample. We also report the p-value from testing the equality of the probabilities of extreme outflows between the matched and full samples.

Panel A: Time-series regressions

	(1)	(2)	(3)
SA_FireSale%	0.045*		0.077**
	(1.93)		(2.02)
MF_FireSale%		0.023	−0.028
		(1.00)	(−0.95)
Mkt_Liquidity	0.033	0.030	0.033
	(1.29)	(1.19)	(1.26)
Mkt_Ret	−0.007	−0.003	−0.009
	(−0.78)	(−0.30)	(−0.90)
Sentiment	−0.001	−0.001	−0.001*
	(−1.67)	(−1.57)	(−1.75)
Lag Transit_Program%	0.801***	0.781***	0.761***
	(2.96)	(2.67)	(2.64)
Intercept	0.000	0.003	0.000
	(0.14)	(0.78)	(0.12)
No. Obs.	34	34	34
Adj. R^2	0.328	0.291	0.314

Panel B: Fire-sale probability among matched sample

	(1)	(2)	(3)
	Pr(Bot 10% Flow)	Pr(Bot 5% Flow)	Median Flow
Matched SA-Quarters	34.34%	22.22%	−5.73%
All SA-Quarters	10%	5%	−0.39%
p-value of testing equality:	<0.0001	<0.0001	<0.0001

Appendix

A Additional Results

In this section, we present supporting evidence and additional robustness results that complement our main findings.

As a robustness check for the FIT price impact analysis, we construct an alternative measure of flow-induced trading as the flow-induced shares demand scaled by the total number of shares of the stock held by all funds:

$$\text{FIT}_{j,t} = \frac{\sum_i \text{Shares}_{i,j,t-1} \times \text{Flow}_{i,t} \times \text{PSF}}{\sum_i \text{Shares}_{i,j,t-1}}. \tag{10}$$

We re-estimate the price-impact specification as in Table 3 using this alternative FIT measure and report the results in Table A.1. Consistent with previous results, Table A.1 shows that FIT^{MF} generates a significant price impact while FIT^{SA} generates virtually no price impact.

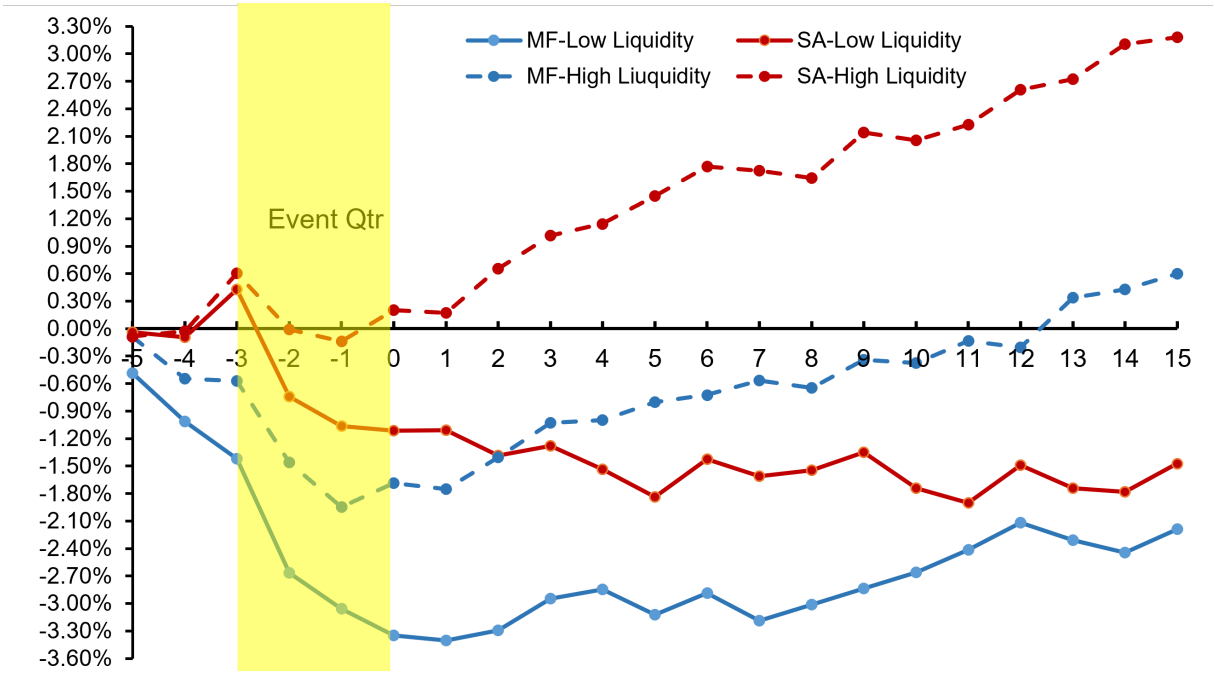
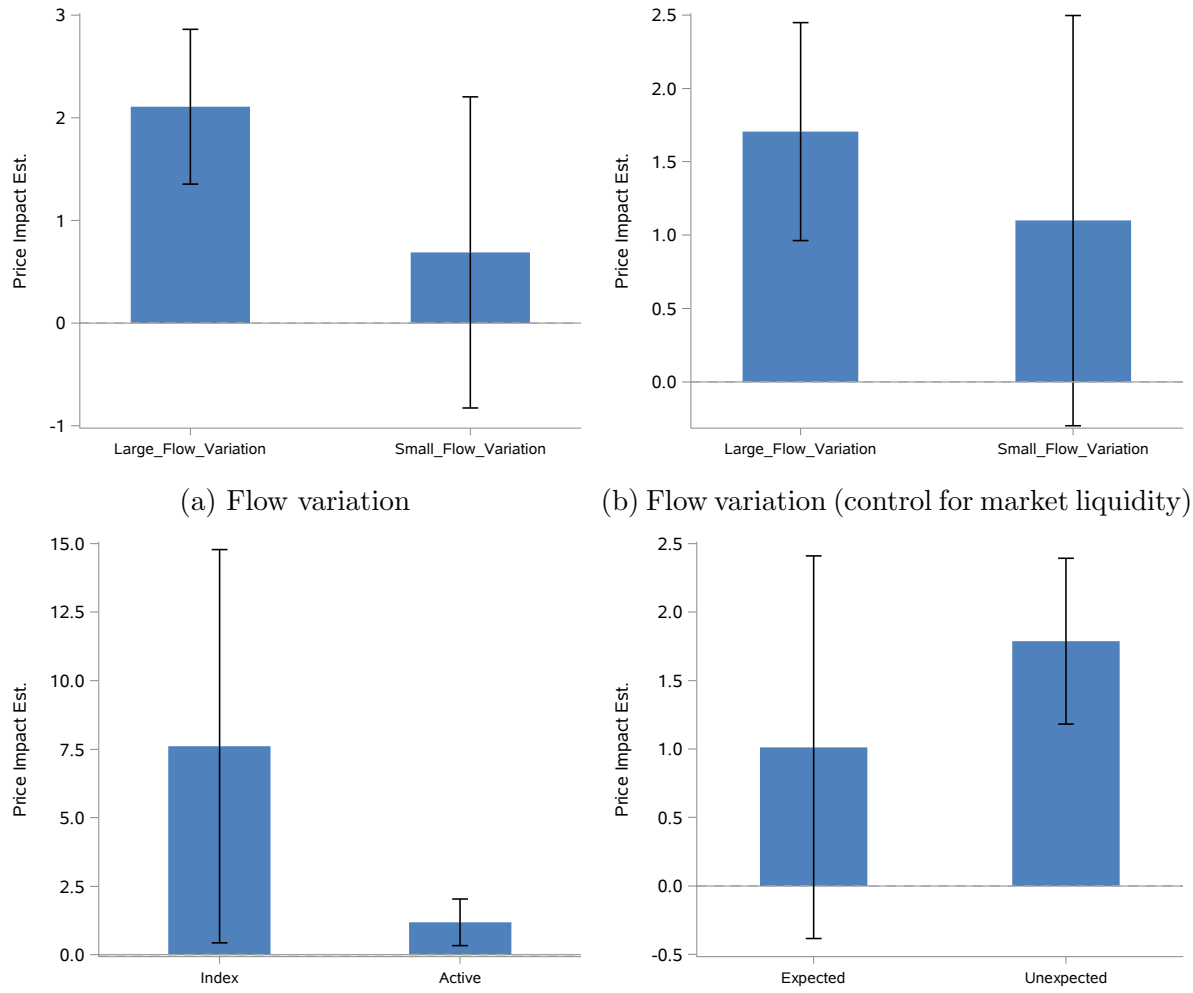


Figure A.1: **Fire-sale event CARs: low versus high market liquidity.** This figure plots event-time cumulative abnormal returns associated with mutual fund and separate account fire-sale events. The sample period, 2001Q4–2024Q4, is split into two halves based on market liquidity. We plot fire-sale event CARs separately in low- and high-liquidity periods.



(a) Flow variation

(b) Flow variation (control for market liquidity)

(c) Active versus index mutual funds

(d) Expected versus unexpected flows

Figure A.2: **Heterogeneity in mutual fund flow-induced price impact.** Panel (a) plots the estimated price impact of mutual fund flow-induced trading together with 95% confidence intervals in the subsamples with larger and smaller flow variations. We calculate the flow variation of each quarter by the standard deviation of mutual fund percentage flows in the quarter. Then, we split the sample period into two equal groups based on the quarterly flow variation. In each subsample, we estimate the price impact by running a univariate regression of quarterly stock returns on the contemporaneous FIT. In the figure, *Large_Flow_Variation* denotes the estimate from the subsample with larger flow variation, whereas *Small_Flow_Variation* denotes the estimate from the subsample with smaller flow variation. In Panel (b), we use the quarterly flow variation orthogonalized to market liquidity to split the sample and repeat the analysis in Panel (a). Specifically, we run a time-series regression of quarterly flow variation on the contemporaneous market liquidity measure of Pástor and Stambaugh (2003) and use the residual from the regression to split the sample. Panel (c) plots the estimated FIT price impact and 95% confidence intervals when FIT is constructed separately from active mutual funds and index mutual funds. Panel (d) plots the estimated FIT price impact and 95% confidence intervals when FIT is constructed separately from expected flows and unexpected flows. Specifically, we first estimate the full-sample relation between quarterly percentage fund flows and one-quarter-lagged percentage flow and fund returns via Fama-MacBeth regressions. Next, we compute expected flows for a fund-quarter based on the lagged fund returns and flows and the estimated relation. Unexpected flows are calculated as actual flows minus expected flows. Finally, we estimate the price impact by regressing quarterly stock returns on the contemporaneous FIT constructed by expected/unexpected flows, with stock-level controls included.

Table A.1: **Robustness check for the price impact of flow-induced trading.** In this table, we use an alternative measure of flow-induced trading (see equation (10)) and re-estimate the price impact estimations as in Table 3. Panel A reports the estimation results of contemporaneous price impacts of FIT, and Panel B reports the results from the reversals of FIT.

Panel A: Contemporaneous Price Impact						
	(1)	(2)	(3)	(4)	(5)	(6)
FIT^{MF}	0.2407***	0.2493***			0.2440***	0.2447***
	(4.54)	(5.61)			(5.20)	(6.05)
FIT^{SA}			−0.0038	0.0133	−0.0117	0.0044
			(−0.19)	(0.83)	(−0.63)	(0.30)
Size		0.0007		0.0003		0.0006
		(0.42)		(0.20)		(0.37)
BM		0.0109**		0.0109**		0.0108**
		(2.35)		(2.31)		(2.34)
MOM		0.0031		0.0038		0.0031
		(0.39)		(0.47)		(0.39)
AG		−0.0045**		−0.0046**		−0.0045**
		(−2.12)		(−2.15)		(−2.12)
GP		0.0281***		0.0274***		0.0281***
		(4.35)		(4.15)		(4.32)
Illiq		−1393.0		−1451.5		−1257.0
		(−0.44)		(−0.46)		(−0.40)
No. Obs.	316,515	316,515	316,515	316,515	316,515	316,515
Adj. R^2	0.0035	0.0328	0.0007	0.0309	0.0042	0.0332

Table A.1 Continued

Panel B: Reversals

	(1)	(2)	(3)	(4)	(5)	(6)
$\text{PastFIT}^{\text{MF}}_{\text{t}-12:\text{t}-1}$	−0.1948**	−0.1206*			−0.1860**	−0.1201*
	(−2.38)	(−1.88)			(−2.18)	(−1.76)
$\text{PastFIT}^{\text{SA}}_{\text{t}-12:\text{t}-1}$			−0.0258	−0.0013	−0.0184	0.0024
			(−0.78)	(−0.04)	(−0.52)	(0.07)
Size		0.0003		0.0002		0.0002
		(0.17)		(0.15)		(0.10)
BM		0.0104**		0.0106**		0.0103**
		(2.29)		(2.26)		(2.27)
MOM		0.0038		0.0031		0.0032
		(0.48)		(0.38)		(0.40)
AG		−0.0048**		−0.0045**		−0.0045**
		(−2.27)		(−2.08)		(−2.11)
GP		0.0265***		0.0268***		0.0263***
		(4.11)		(4.16)		(4.15)
Illiq		−1653.6		−897.44		−1057.1
		(−0.52)		(−0.29)		(−0.35)
No. Obs.	314,559	314,559	311,613	311,613	311,307	311,307
Adj. R^2	0.0014	0.0314	0.0007	0.0316	0.0021	0.0326

Table A.2: **Fire-sale event CARs: twin-fund sample.** This table reports event-time cumulative abnormal returns associated with mutual fund and separate account fire-sale events during 2001Q4-2024Q4. The test specification follows Table 4, but the sample is restricted to mutual funds and separate accounts with twin vehicles.

	Mutual Funds		Separate Accounts	
Event Month	CAR	t-value	CAR	t-value
-5	-0.21%	-1.27	0.05%	0.24
-4	-0.64%	-2.92	-0.07%	-0.26
-3	-0.75%	-2.94	0.11%	0.32
-2	-1.74%	-6.73	-0.40%	-1.01
-1	-2.07%	-6.64	-0.48%	-1.08
0	-2.12%	-5.77	-0.51%	-0.98
1	-2.26%	-5.95	-0.48%	-0.87
2	-1.89%	-5.20	-0.21%	-0.33
3	-1.86%	-4.43	0.03%	0.04
4	-1.85%	-4.18	0.08%	0.11
5	-1.68%	-3.64	0.15%	0.20
6	-1.39%	-2.92	0.51%	0.63
7	-1.28%	-2.43	0.71%	0.85
8	-1.08%	-1.96	0.91%	1.07
9	-0.87%	-1.55	0.97%	1.07
10	-0.68%	-1.14	0.89%	0.95
11	-0.48%	-0.82	0.89%	1.03
12	-0.41%	-0.65	1.34%	1.53
13	0.03%	0.04	1.20%	1.36
14	0.13%	0.17	1.29%	1.45
15	0.19%	0.24	1.37%	1.48

Table A.3: **Fragility and future stock volatility: subsamples by market liquidity.** This table reports Fama–MacBeth regressions predicting stock volatility with fragility in subsamples classified by market liquidity. We split the sample period into two equal halves based on the market liquidity measure of Pástor and Stambaugh (2003). Within each period, we re-estimate the regression as in Table 5. *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Market Liquidity:		Low			High	
$\sqrt{G}^{MF}$	0.194***		0.187***	0.195***		0.188***
	(5.463)		(4.926)	(5.347)		(4.708)
$\sqrt{G}^{SA}$		0.029***	0.013		0.021***	0.004
		(3.170)	(1.245)		(3.436)	(0.496)
Vol_Lag	0.764***	0.773***	0.763***	0.685***	0.697***	0.684***
	(23.225)	(23.811)	(23.121)	(39.506)	(42.184)	(39.582)
No. Obs	42,370	42,370	42,370	42,587	42,587	42,587
Adj. R^2	0.519	0.515	0.52	0.498	0.493	0.499

B Derivation of Fragility

In this section, we describe how we derive and estimate fragility. As in Greenwood and Thesmar (2011) (GT), we assume the following contemporaneous relationship between fund flow-induced trading and return of stock j:

$$r_{j,t} = \alpha + \lambda \frac{\sum_i \text{Shares}_{i,j,t-1}\text{Flow}_{i,t}\text{PSF}}{\text{Shrout}_{j,t-1}} + \varepsilon_{j,t}. \tag{11}$$

Here, $r_{j,t}$ is the return of stock j in quarter t, $\text{Shares}_{i,j,t-1}$ is the number of shares of stocks j held by fund i at the end of quarter $t-1$, $\text{Flow}_{\text{i,t}}$ is the percentage flow of fund i in quarter t, PSF is the partial scaling factor, and $\text{Shrout}_{j,t-1}$ is shares outstanding of stock j at the end of quarter $t-1$. The fraction, $\frac{\sum_i \text{Shares}_{i,j,t-1}\text{Flow}_{i,t}\text{PSF}}{\text{Shrout}_{j,t-1}}$, is effectively the FIT measure as in equation 3. λ is the price impact factor. The residual term, $\varepsilon_{j,t}$, has a conditional mean of zero and may capture other sources of variation of returns (e.g., news about fundamentals). The specification here is similar to the regression model we estimated as in equation 4.

Substituting fund ownership of stock j ($w_{i,j,t-1} = \frac{\text{Shares}_{\text{i,j,t-1}}}{Shrout_{j,t-1}}$) into equation 11, we get

$$r_{j,t} = \alpha + \lambda \sum_i w_{i,j,t-1}\text{Flow}_{i,t}\text{PSF} + \varepsilon_{j,t}. \tag{12}$$

Taking variance on both sides, we obtain the conditional variance of stock j at the end of quarter t as:

$$\text{Var}_t(r_{j,t+1}) = \lambda^2 W_{j,t}{}' E_t(\Omega_{t+1}) W_{j,t} + \text{Var}_t\left(\varepsilon_{j,t+1}\right) \tag{13}$$

Here, $E_t(\Omega_{t+1})$ is the conditional variance-covariance matrix of fund flows in quarter $t+1$ and $W'_{j,t} = [w_{1,j,t}, w_{2,j,t}, ..., w_{K,j,t}]$ is the vector of mutual fund ownership of stock j.

Similar to GT, we define the fragility of stock j in quarter t as

$$G_{j,t} = W_{j,t}{}' E_t(\Omega_{t+1}) W_{j,t}. \tag{14}$$

To estimate $E_t(\Omega_{t+1})$, we calculate the variance-covariance matrix of mutual fund flows using observations in the most recent eight quarters (including quarter t), and we require that a fund have no missing quarterly flows in the eight-quarter rolling window. The summary statistics of fragility and co-fragility are reported in Table 1.

C Transition Management Program Matching

In this section, we outline our methodology for linking transition management programs to separate accounts.

First, we restrict the sample to programs satisfying two constraints: (1) the execution occurs within a single calendar month, and (2) the portfolio comprises at least 10 unique common stocks. Share quantities are normalized using CRSP cumulative adjustment factors to ensure comparability between execution and reporting dates.

Second, we classify programs based on the *buy-sell ratio*, defined as the net change in principal divided by the total principal (Net Flow/Principal). Programs with an absolute ratio exceeding 0.90 are labeled *uni-directional*; otherwise, they are *bi-directional*. This distinction addresses the identification challenge specific to bi-directional reallocations: *in-kind transfers*, where overlapping positions between legacy and target portfolios are retained without generating trade records.

For *uni-directional* programs, we match net-sell programs against the Separate Account's previous-period holdings (within 180 days prior) and net-buy programs against subsequent-period holdings (within 180 days post). A candidate pair is confirmed as a

match if it meets four criteria: (1) matched securities represent at least 80% of the program's total principal; (2) the Pearson correlation and cosine similarity between adjusted trade weights and holding weights exceed 0.90 for both the full portfolio and the core subset (securities with weights $> 1\%$); (3) the fund's quarterly net flow to transition principal ratio falls within $[0.5, 2.0]$; and (4) at least 10 unique securities are matched.

For *bi-directional* programs, identification is complicated by overlapping positions absent from trade records. We implement a two-step procedure to address this. First, we apply a **pre-screening filter**: a candidate fund must hold at least 8 of the program's top 10 securities (ranked by trading volume) and report a quarterly net flow between $1\times$ and $5\times$ the transition's directional trading volume. Second, we model the matching as a robust regression problem using the Random Sample Consensus (RANSAC) algorithm. Unlike standard Ordinary Least Squares (OLS), RANSAC iteratively estimates parameters from random subsets of inliers, making it resilient to outliers—specifically, stationary holdings that generate no trade records. The model is specified as:

$$\Delta\text{Trade}_{i,p} = \beta_{sell} H_{i,f_{sell}} + \beta_{buy} H_{i,f_{buy}} + \epsilon_i \tag{15}$$

where $\Delta\text{Trade}_{i,p}$ represents the net change of security i in program p, $H_{i,f_{sell}}$ and $H_{i,f_{buy}}$ denote the holdings in the candidate sell and buy funds, and ϵ_i is the error term.

To prevent spurious fitting to zero-trade noise, we employ a weighted estimation scheme where securities with non-zero trading volume are assigned a relative importance weight of 100. We estimate this model using both share-based and value-based inputs. A match is validated if either specification meets the following criteria: (1) the weighted RANSAC regression yields an $R^2 > 70\%$; (2) the identified inlier securities explain more than 40% of the total transition trading volume; and (3) the regression coefficients are

economically consistent ($\beta_{sell} < 0$ and $\beta_{buy} > 0$).