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Investment Decisions Depend on Portfolio Disclosures

DAVID K. MUSTO*

ABSTRACT

A weekly database of retail money fund portfolio statistics is uneconomical for retail investors to observe, so it allows direct comparison of disclosed and undisclosed portfolios. This makes possible a more direct and unambiguous test for "window dressing" than elsewhere in the literature. The analysis shows that funds allocating between government and private issues hold more in government issues around disclosures than at other times, consistent with the theory that intermediaries prefer to disclose safer portfolios. Cross-sectional comparisons locate the most intense rebalancing in the worst recent performers.

MONEY MANAGERS ARE TYPICALLY REQUIRED to disclose their portfolios on prearranged dates. Whether investors receive new information depends on whether managers are influenced by the disclosure requirement. Managers can ignore the disclosure schedule when they invest, in which case the disclosed portfolios will be unbiased, albeit noisy, estimates of their investing strategies. Alternatively, they can plan their investing around the schedule by moving briefly into and out of the portfolios they prefer to disclose, thereby reducing or destroying the power of the disclosures to reveal anything meaningful. The goal of this paper is to establish which scenario best describes the behavior of money market mutual fund managers.

The idea that portfolio weights interact with disclosure dates surfaces now and again in the academic and trade literature, surviving on circumstantial evidence rather than direct observation. The odd behavior of asset prices around common disclosure dates, particularly the end of the year, comprises most of this evidence. Haugen and Lakonishok (1988) argue that riskier stocks and bonds and recent losing investments are cheap at year-end because disclosing intermediaries avoid them—a strategy known in the industry as *window dressing*—and Musto (1997) makes the same case for year-end and quarter-end price shifts in the money market. Despite this suggestive evidence, the incidence of window dressing remains an open question, reflecting the difficulty of detecting what managers have putatively tried to

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conceal. The empirical tests in this paper address the problem by analyzing the portfolio adjustments of retail money market funds around disclosure dates.

The primary data in question are the portfolio statistics collected weekly by the newsletter *Money Fund Report* (MFR). Several current statistics, including the allocation between government and private issues, are reported in this publication for a substantial majority of money funds. The power of the data to detect window dressing follows from three facts: (1) private issues are generally regarded as riskier than government issues, (2) the government/private allocation is disclosed to the general public only twice a year, and (3) at $1,195/year, the newsletter is uneconomical for almost all retail investors to observe. A strategy of holding more in lower-risk government securities around disclosures than before or after is effectively unobservable to the retail clientele but is easily detectable in the data.

The rest of the paper is in five sections. Section I covers the window dressing literature and some special considerations that apply to the money market, Section II describes the money fund database and the associated database of disclosure schedules, Section III designs and executes the empirical tests for window dressing, Section IV analyzes the cross section of window dressing activity, and Section V summarizes and concludes.

I. Background and Literature

It is well known that many portfolio managers have the opportunity to window dress. Under the Investment Company Act of 1940, mutual fund managers are required to disclose portfolios in complete detail as of the fiscal year-end and six months later, but they have no obligation to reveal positions held on other dates of the year. In principle, a manager can disclose a portfolio completely different from the one usually held. For an equity manager this will involve transactions costs approximately two times the average bid/ask spread, but for a money fund manager the transactions costs can be very low, or even zero. Money market instruments mature quickly, and the proceeds must be reinvested somewhere, so there is not necessarily a transactions cost to tilting from one sort of portfolio to another. As a result, money managers, and especially money fund managers, can easily window dress if they want to.

Whether fund managers are motivated to window dress turns on what they believe outsiders infer from disclosures. There is plenty of evidence that outsiders infer *something* from disclosures. Consumers pay for services such as *Morningstar* which report and analyze disclosed portfolios, and academics often use disclosed portfolios as proxies for undisclosed portfolios (e.g., Falkenstein (1996), Sias and Starks (1997)). If managers conclude that disclosures influence judgments about their professional behavior or ability, it is at least plausible that they want to window dress.

This motive is stronger when money funds are involved because there is little information outside of portfolio disclosures about a money fund's risk. In the standard performance-measurement paradigm, there are three main sources of variation across managed funds' returns: skill, noise, and risk profiles. Thus, investors trying to identify skilled managers by their realized returns are advised to net out their risk profiles first. Starting with Jensen (1969), the finance literature has developed performance measures that estimate risk profiles from regressions of returns on factors, without reference to the underlying portfolios. These measures are now staples of the fund profiles sold to investors. But money funds are different in a way that makes them relatively uninformative.

Money funds follow a peculiar accounting standard known as the *amortized cost method*, in which the reported value of a security under 60 days to maturity follows a straight-line amortization. For example, if a fund buys $1,000,000 face value of 30-day commercial paper (CP) for $994,000, it will value the paper at $996,000 in 10 days and $998,000 in 20 days, regardless of developments in interest rates and risk premia. The result is that the realized returns of a money fund are relatively uninformative about the risks it is exposed to. Suppose fund *A* holds only 30-day CP with a discount rate of 6 percent, and fund *B* holds only 30-day Treasury bills, also at 6 percent. If *A* and *B* make no transactions for the next 30 days, and the CP that *A* bought does not default, their returns will be precisely the same. If the default-risk premium jumps up, the market value of fund *A* will drop, but its advertised net asset value and yield will be unaffected so an outsider will have no idea that *A* is riskier than *B*. Defaults are very rare, so this sort of confusion is typical. The low information content of money funds' returns makes their portfolio disclosures relatively more important, which in turn strengthens the motive to window dress.

Empirical research has focused not on whether window dressing occurs but on whether it moves prices at the year-end. Haugen and Lakonishok (1988) argue that window dressing, and not tax-loss selling, may be responsible for the big January returns of small stocks and recent losers known as the January Effect. That is, values of the affected stocks go up after the year-end because institutions with year-end disclosures have stopped avoiding them, not because individuals have stopped selling them. Much of the evidence on window dressing comes from tests of this theory.

Some of the tests compare institutional and individual trading at the year-end. Dyl and Maberly (1992) find that odd-lot orders (i.e., not blocks of 100 shares), generally associated with individuals rather than institutions (e.g., Lakonishok and Maberly (1990)), are biased toward sales in the last days of the year. They conclude that at least some of the January Effect reflects tax-motivated selling and not window dressing. Ritter (1988) shows that individuals bias toward selling at year-end, and reaches the same conclusion. Sias and Starks (1997), looking at transactions data, find that in the last days of 1990 (relative to the first days of 1991) trades by institutions are

biased toward buying winners, which suggests window dressing. They do not, however, find any evidence of a bias toward selling losers, and their data do not indicate whether the institutions involved really had year-end disclosures.

Some other tests analyze patterns in disclosed portfolios. Lakonishok et al. (1991) study quarter-end portfolio reports by pension fund managers. These portfolios are all disclosed in the sense that the typical pension plan administrator can view his managers' portfolios at any point, not just year-ends or quarter-ends. As such, they cannot reveal anything about how disclosed portfolios compare with undisclosed portfolios. But Lakonishok et al. (1991) contend that administrators may pay more attention to year-end portfolios in performance evaluations, and that this induces managers to window dress during the fourth quarter. Lakonishok et al. find that both sales and purchases of recent losers are relatively high in the fourth quarter, and they take the sales as evidence of window dressing.

The database of Sias and Starks (1997) is similar to that of Lakonishok et al. (1991) in that it comes from public filings by institutional investors. For each stock, they have the aggregate investment by institutions, as reported in recent disclosures. They reason that institutions are relatively inactive in the stocks showing low aggregate investment. When they find that these stocks have relatively larger January Effects, they conclude that the January Effect does not reflect institutional activity such as window dressing. But their result could as easily be read the other way, because the stocks subject to window dressing should perforce be underrepresented in disclosures. There may be no way to resolve this ambiguity by studying public disclosures, but there are still some ways to gauge what intermediaries do differently when outsiders are not looking.

Musto (1997) shows that money market prices have something like a January Effect—big upward shifts for CP across the year-end, bigger shifts for riskier CP, no shifts for Treasury bills—and tests for window dressing on the Federal Reserve's quarter-end aggregate industry portfolios. These data allow some comparison between disclosed and undisclosed portfolios, because in some cases they reflect portfolios not disclosed individually to the public. For example, life insurers do not reveal their quarter-end money-market holdings to claimholders or regulators, but they do reveal them to the American Council of Life Insurers, which reports aggregates by instrument type to the Fed. Every life insurer has to disclose its December 31 portfolio, so estimating the effect of disclosures on CP holdings is simply a matter of comparing the year-end and quarter-end figures. Tests of this sort show the significant negative relationship between year-end CP investment and industry-wide disclosure events consistent with window dressing. They cannot, however, confirm that individual intermediaries are window dressing; the patterns in the aggregate data can in principle represent flows between institutions, rather than flows within the institutions' portfolios.

These papers improve our understanding of year-end price shifts and aggregate effects of institutional trading, but they also demonstrate the need for a direct comparison of the disclosed and undisclosed portfolios of indi-

vidual intermediaries. This sort of comparison may seem infeasible, but MFR makes it possible. Because the annual subscription price of MFR is $1,195, the publication is uneconomical for almost all retail investors,[1] and, thus, weekly statistics on retail funds are effectively undisclosed. These statistics are not complete risk profiles of the funds, but they do track default risk by showing the current allocation between government and private-issue securities. Testing for window dressing is therefore a simple matter of identifying the weeks proximate to semiannual disclosures, and then comparing the statistics from these weeks with the statistics from the other weeks. The data on institutional funds are useful as a comparison. The subscription price is more economical for institutions with millions of dollars to allocate, so the weekly statistics on institutional funds are *not* effectively undisclosed. Tests for window dressing should detect it in the first sample and not the second.

II. The Data

The tests below combine two databases on taxable money funds, one with weekly statistics on their portfolios, the other covering their fiscal year-ends (FYEs). Every week the manager of a money fund can report several statistics on his portfolio to MFR. These include the net asset value, recent 7-day and 30-day yields, and average maturity of portfolio holdings, each of which is reported in many newspapers. They also include the percent allocation to Treasury securities, other government issues, commercial paper, bankers' acceptances, and other money market categories, which are *not* reported in newspapers. Reports to MFR are voluntary, and the publishers estimate that they cover 95 percent by number and 98 percent by value of all money funds. Newsletters are dated Friday, but the portfolio statistics they contain refer to the previous Tuesday, which is the date we use here. The database covers the 504 consecutive Tuesdays from November 24, 1987, to July 15, 1997.

MFR sorts taxable funds by whether they are retail or institutional, and by whether they are government-only or unconstrained.[2] Institutional funds market to other institutional investors and have high minimum balances—a million dollars is typical. All other funds are considered retail, though some retail funds have minimum balances that make sense for only a few individuals (e.g., $100,000). Government-only funds restrict themselves to some combination of Treasury issues, Agency issues, and repos of Treasury issues. All other funds are considered unconstrained. Funds can and occasionally do change their types over the sample period. The tests below focus exclusively

[1] To put this number in perspective, consider that at the end of 1992 the average account at a taxable money fund (including institutional funds) was $22,600 (Cook and Duffield (1993)). The annual cost of MFR is more than 5 percent of this amount.

[2] MFR refers to taxable funds that are not government-only as either First Tier or Second Tier funds, depending on whether or not they are currently invested in any second-tier securities. I lump these two categories together as unconstrained funds.

on the unconstrained funds, which numbered 435 as of July 15, 1997: 280 retail funds managing $418,424 million, and 155 institutional funds managing $172,691 million.

A useful feature of the MFR data is that there is no look-ahead bias; data for one week are not retroactively altered or removed in response to events of later weeks. Every report by every fund for every week is included, regardless of whether the fund later folds, merges, or stops reporting. Missing observations are very rare; of the 150,768 reports by unconstrained funds (36,089 by institutional funds and 114,779 by retail funds), only 52 do not indicate the allocation to government securities, and there are only 60 missing reports by unconstrained funds.[3] 629 different unconstrained funds are included for at least one week.

The data on FYEs have several sources. The main one is the library of SEC filings on LEXIS/NEXIS, which identifies the FYE of almost every fund that operated in the last three years of the sample period. For funds that ceased operating before this period, FYEs are identified with data from Lipper Analytical Services, *Investment Dealer's Digest*, and the 1987–1991 volumes of *Investment Companies* from Wiesenberger Financial Services. These sources yield FYE data on 573 of the 629 unconstrained funds, including 406 of the 452 funds that are retail funds in at least one week and 205 of the 220 funds that are institutional in at least one week.[4] All of the 56 funds without FYE data no longer report to MFR, and many have dissolved. This biases the usable MFR data a little toward surviving funds.

Following the Investment Company Act of 1940, we assume that each fund discloses the portfolio it holds on its FYE, and also the portfolio it holds six months later. A few funds elect to disclose quarterly and for these funds our procedure will misclassify some disclosure dates as nondisclosure dates. To give a sense of the popular reporting schedules, Table I shows the distribution of fiscal year-ends and the associated assets under management for the unconstrained retail and institutional funds in the July 15, 1997 issue. The numbers vary considerably across months; by far the most popular schedule for retail funds is June 30/December 31, which accounts for 39 percent of the total by number and 37 percent by assets under management.

III. Tests for Window Dressing

Unconstrained funds ("funds," from here on) allocate throughout the year between government and private-issue securities. Table II presents a few summary statistics on these allocations (calculated for each fund individually and then averaged across funds), showing a mean of approximately 8 per-

[3] If a fund was an unconstrained fund in week i and week $j > i$ but did not report between i and j, I count $j - i - 1$ missing reports.

[4] Over the sample period, 43 unconstrained funds switched between retail and institutional and so are counted in both groups.

Table I

Fiscal Year-Ends of Unconstrained Money Funds Reporting to *Money Fund Report* as of 7/15/97

Money Fund Report is a weekly newsletter that covers a large majority of money funds. Unconstrained funds are those money funds that invest in the whole range of money market instruments, such as commercial paper and Treasury bills. They are not restricted to tax-favored or government-issued securities. Retail funds cater to individual investors, and institutional funds cater to institutional investors.

	Retail Funds		Institutional Funds	
Report Month	No.	Assets ($ billion)	No.	Assets ($ billion)
January	4	0.881	20	13.539
February	11	9.430	6	6.529
March	24	58.665	17	20.117
April	8	11.376	12	14.104
May	10	11.835	6	7.717
June	31	44.976	6	2.387
July	16	41.367	8	10.434
August	24	23.581	14	24.736
September	22	13.916	12	27.233
October	39	31.080	11	7.488
November	15	61.820	10	9.978
December	76	109.496	33	28.430
Total	280	418.424	155	172.691

cent and substantial time-series variation within both retail and institutional funds. The window dressing hypothesis predicts higher allocations to government securities during semiannual disclosure dates; the tests are designed to establish whether this occurs. For comparison, the tests are repeated on institutional funds, for which the window dressing hypothesis has no prediction.

A. Are Government Allocations Bigger during Disclosures?

A fund has two disclosure dates in a year, the ends of its second and fourth fiscal quarters. The empirical question is how its government investment on these dates compares with its government investment during the rest of the year. This comparison is very simple but it has to take into account that the MFR data cover only Tuesdays. Let $T(d)$ be the Tuesday closest to date d, and let the Tuesdays of year y start after $T(12/31/y - 1)$ and end with $T(12/31/y)$. So for example, $T(12/31/89) = 1/2/90$ and $T(12/31/90) = 1/1/91$, so the Tuesdays of 1990 run from 1/9/90 to 1/1/91. A fund's government allocation on a non-Tuesday disclosure date must be estimated from disclosures on nearby Tuesdays. My estimate is the allocation reported for the Tuesday

Table II

Summary Statistics of Allocations to Government Securities

For the 452 funds that are unconstrained retail funds in at least one week, the weeks in which any fund is an unconstrained retail fund are identified, and then the mean, median, minimum, maximum, standard deviation, and interquartile range of the allocations to government securities in those weeks are calculated. Finally, the average of each statistic across funds is calculated and reported below. This procedure is repeated for the 220 funds that are unconstrained institutional funds in at least one week. Unconstrained money funds are those money funds that invest in the whole range of money market instruments, such as commercial paper and Treasury bills. Retail funds are those that cater to individual investors, and institutional funds are those catering to institutional investors.

Statistic	Retail Funds (%)	Institutional Funds (%)
Mean	8.62	8.69
Median	7.24	7.78
Minimum	2.02	3.39
Maximum	27.90	22.41
Standard deviation	6.12	4.80
Interquartile range	7.60	6.75
Number of funds	452	220

closest to the disclosure date, rather than the last Tuesday on or before it or the first Tuesday on or after it because disclosure-related allocations are as likely to show up just before the disclosure date as just after.[5]

The estimate of fund f's disclosed government allocation in year y is denoted $DISC_{f,y}$ and defined as the average over the two Tuesdays closest to f's two disclosure dates in y. The estimate of f's undisclosed allocation in y, $UNDISC_{f,y}$, is the average over all other Tuesdays of y. Consider the Scudder Cash Investment Trust (SCIT), which has a June fiscal year. The value of *DISC* is 7.0 for 1990 because its government allocation was 2 percent on $T(6/30/90) = 7/3/90$ and 12 percent on $T(12/31/90) = 1/1/91$, and *UNDISC* is 1.56, the average over the 50 other Tuesdays of 1990. For every year y from 1988 to 1996 and for every fund f with data for all of y I let $Z_{f,y} = DISC_{f,y} - UNDISC_{f,y}$, and test across all years and retail funds whether Z is significantly positive. I then repeat the test on the sample of institutional funds. The results are in Table III.

The test shows that retail funds do in fact allocate more to government securities during disclosures. The point estimate is 0.296 percent of the portfolio on average, which is about 4 percent of the average undisclosed allocation to government securities (i.e., $0.296/7.678 \approx 0.04$), and is significantly

[5] Suppose a manager usually buys one-month CP and rolls it over at maturity, but that he wants to hold only Treasury bills on 7/31. He could just as easily roll into a one-month Treasury bill on 7/1 and hold it to maturity on 8/1 as he could roll into a one-month Treasury bill on 7/31 and hold it to maturity on 8/31. Either way he shows a portfolio of only Treasury bills, and he is in fact holding Treasuries instead of CP for exactly one month.

Table III

Disclosed Minus Undisclosed Government Allocations of Unconstrained Money Funds, 1988–1996

For every year y from 1988 to 1996, and for every retail fund f that reported portfolio statistics for each Tuesday of y, the average allocation to government-issue instruments is calculated first over the two Tuesdays closest to f's semiannual disclosures, the disclosed portfolios, and then over the remaining Tuesdays of y, the undisclosed portfolios. This yields 1,802 sets of two averages. The table reports the means of these two averages across the observations, the mean difference between disclosed and undisclosed averages, and the one-tailed t-statistic for the null hypothesis that the true mean is not positive. Unconstrained money funds are those money funds that invest in the whole range of money market instruments, such as commercial paper and Treasury bills. Retail funds are those that cater to individual investors, and institutional funds are those catering to institutional investors.

Portfolio Type	Mean Government Allocation (%)	No. of Observations	t-Statistic
Retail Funds			
Disclosed	7.974	1802	
Undisclosed	7.678	1802	
Difference	0.296	1802	3.75**
Institutional Funds			
Disclosed	7.229	523	
Undisclosed	7.096	523	
Difference	0.134	523	0.87

** Null hypothesis rejected at the 1 percent level.

positive at the usual levels. To put this number in perspective, 0.296 percent of the $418 billion managed by retail funds is approximately $1 billion. One interpretation is that someone who takes disclosed portfolios to be unbiased estimates of undisclosed portfolios overestimates the undisclosed allocation to government securities by this amount.

When retail funds are replaced by institutional funds the test statistic is no longer significant and the point estimate is less than one-half the size. This pattern supports the hypothesis that window dressing drives the reallocations detected in retail funds.

B. Do Investment Decisions Depend on Disclosure Events?

The results of Table III do not completely establish that disclosure events affect investment decisions because these events do not account for seasonal and other effects that can correlate the residuals. For example, the same result obtains if all funds hold more government securities in June or December, regardless of disclosure schedules, since a disproportionate number of funds disclose in June and December. To address this and other potential sources of interdependence, I modify the test design in a way that controls more precisely for the incidence of disclosures.

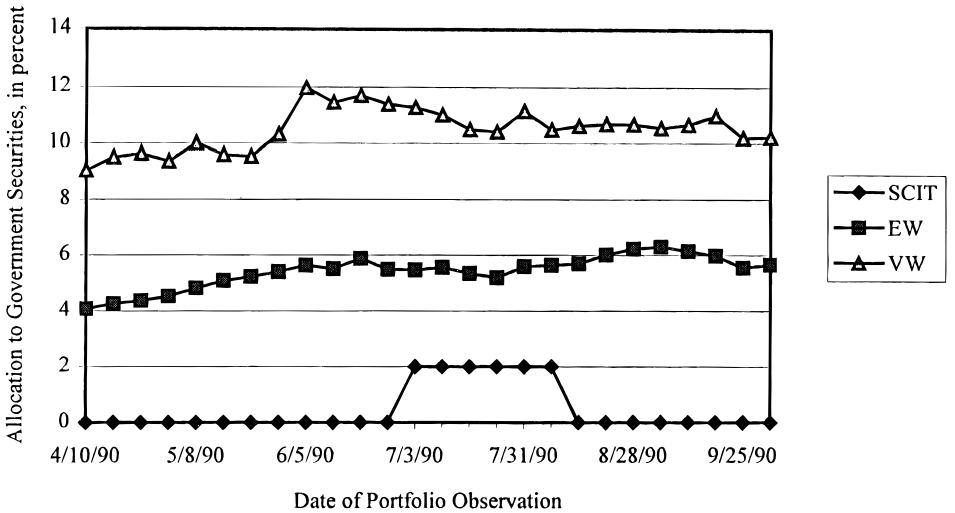

Figure 1. Scudder Cash Investment Trust for six months around a disclosure. SCIT is the fund's investment in government securities on 6/30/90, EW and VW are the equal-weighted and value-weighted, respectively, average investments on the indicated date of all money funds that allocate between government-issue and private-issue securities.

The test design is borrowed from the abnormal-return literature: compare a fund's allocations around a disclosure date with an index of funds with different disclosure dates. As before, most of the complications follow from using weekly data. For this test it is convenient to number the MFR issues from 1 to 504 (the 11/24/87 issue being number 1) and let $I(m)$ be the number of the MFR issue closest to the end of month m, and $G_{f,i}$ be the government allocation of fund f in issue i.

Suppose fund f has a disclosure at the end of month m, so that $G_{f,I(m)-12} \ldots G_{f,I(m)+13}$ are f's government allocations over the 26 weeks (i.e., approximately one-half year) surrounding a disclosure date, and $G_{f,I(m)}$ is the allocation closest to the disclosure. The previous test is almost equivalent to comparing $G_{f,I(m)}$ with the average of G_f over the 25 other weeks. For the new test I locate all funds that did *not* have disclosures at the end of m, let $X_{m,i}$ be the average allocation of these funds in issue i, and then compare $G_{f,I(m)} - X_{m,I(m)}$ with the average of $G_f - X_m$ over the other 25 weeks. In the event-study terminology, X is the market index and $G_{f,i} - X_{m,i}$ is f's "abnormal" government allocation on the date of issue i. The window dressing hypothesis is that the abnormal allocation is higher at disclosure time, which in this notation means $i = I(m)$.

An example illustrates this point. Figure 1 plots three variables from the six months surrounding 6/30/90. The line labeled SCIT is the government allocation of Scudder Cash Investment Trust, which had a disclosure on 6/30/90. This fund's government allocation was 2 percent on 7/3/90 and an average of 0.4 percent in the other weeks (i.e., 2 percent for 5 weeks and

0 percent for 20 weeks), for a difference of 2.0 − 0.4 = 1.6 percent. Meanwhile, the value-weighted index VW of funds that did not disclose on 6/30/90 (but which must have disclosed at some point in those six months) is 11.26 percent on 7/3/90 and 10.46 percent on average in the other weeks, for a difference of 0.80 percent, and the equal-weighted index EW is 5.48 percent on 7/3/90 and 5.41 percent otherwise, for a difference of only 0.07 percent. So SCIT's bias toward holding government securities at disclosure, after controlling for the market, is either 1.60 − 0.80 = 0.80 percent or 1.60 − 0.07 = 1.53 percent, depending on the choice of index.[6] The test for window dressing is to calculate these statistics for every disclosure of every retail fund and determine whether they are significantly positive. As before, the test is repeated on the sample of institutional funds. The details of the test are in the Appendix and the results are in Panel A of Table IV.

Retail funds' bias toward disclosing government holdings is positive and statistically significant relative to either index, and the point estimates of 0.20 percent and 0.27 percent are similar to the estimate from the previous test. Figure 2 plots these results in event-study fashion: the value for week i is the average of $G_{f,I(m)+i} - X_{m,I(m)+i}$ across the 3,783 disclosures with the indicated index. Some or all of the funds' preference for government securities during portfolio disclosures evidently relates directly to their fiscal years. And again, the test does not find a significant pattern when repeated on institutional funds. But before concluding that investment decisions interact with disclosure requirements, it is worth noting that funds have other responsibilities keyed to their fiscal years.

The IRS and the SEC both impose constraints on portfolio weights. The SEC constraints bind throughout the year, but some of the IRS constraints apply only to certain points in the fiscal year. In particular, a fund must obey the diversification standards of IRC 851(b)(4)(A) which do not apply to holdings of government securities and which apply *only* at the close of each quarter of the taxable year. Among other things, this regulation restricts the amount the fund can have invested with any one issuer. The semiannual disclosure dates analyzed above are two of the four dates per year when this standard applies, so the results above are consistent with a strategy of temporary portfolio adjustments, not to influence outsiders, but to satisfy the IRS.

The IRS regulation seems potentially important to the results, but there are at least two reasons why it is probably not. First, the SEC has a substantially similar diversification constraint (17 CFR § 270.2a-7(c)(4)(i)) that is always binding, so the marginal effect of the IRS constraint at quarter-ends may be very small. And second, when the tests of Panel A of Table IV are modified so that the control sample is the two Tuesdays closest to the adjacent quarter-ends (i.e., dates when the IRS regulation applies but portfolios are not disclosed), rather than the 25 adjacent Tuesdays, the test sta-

[6] The value-weighted index puts a very large weight on the portfolios of just a few funds; as of 7/15/97, 12 of the 280 retail funds managed more than half of the total assets, and the smallest 50 percent of retail funds managed only 3.3 percent of the total assets.

Table IV

Interaction between Fiscal Years and Government Allocations for Unconstrained Money Funds, 1988–1997

Details of the test design are given in the Appendix; results are given here. Panel A: For a fund f whose second or fourth fiscal quarter-end is proximate to the Tuesday of week w, let $G_{f,w-12} \ldots G_{f,w+13}$ be its allocations to government securities on the 26 Tuesdays around w and let $X_{w-12} \ldots X_{w+13}$ be an index of government allocations of all funds whose second or fourth fiscal quarter-ends are *not* proximate to w. Index weighting indicates whether the index is equal-weighted or weighted by assets under management, mean is the average of $(G_{f,w} - X_w) - (1/25)\sum_{i \neq 0}(G_{f,w+i} - X_{w+i})$ across quarters and funds, t-statistic is the one-tailed t-statistic for the null hypothesis that the true mean is not positive. Panel B: Same methodology as Panel A, except that the number subtracted from $G_{f,w} - X_w$ is not the average over the 25 adjacent weeks, but rather the average over the Tuesdays closest to the two adjacent quarter ends. Unconstrained money funds are those money funds that invest in the whole range of money market instruments, such as commercial paper and Treasury bills. Retail funds are those that cater to individual investors, and institutional funds are those catering to institutional investors.

Index Weighting	Mean	t-Statistic	No. of Observations
Panel A: Over 25 Adjacent Weeks			
Retail funds			
Equal	0.1982	3.58**	3783
Value	0.2662	4.77**	3783
Institutional funds			
Equal	0.1346	1.38	1146
Value	0.1137	1.17	1146
Panel B: Over Two Adjacent Quarter-Ends			
Retail funds			
Equal	0.3146	3.80**	3765
Value	0.3306	3.95**	3765
Institutional funds			
Equal	0.0746	0.56	1136
Value	0.0536	0.40	1136

** Null hypothesis rejected at the 1 percent level.

tistic is still significant for the retail funds but not the institutional funds, and point estimates are similar. These results are reported in Panel B of Table IV.

IV. The Cross-Section of Window Dressing Activity

As found in Tables III and IV, the point estimates of window dressing by the average fund are all in the neighborhood of 0.3 percent of the total portfolio. This can reflect the whole population of funds biasing toward government issues by 0.3 percent, or fewer funds biasing relatively more. The second scenario is perhaps more reasonable because 0.3 percent appears unlikely to

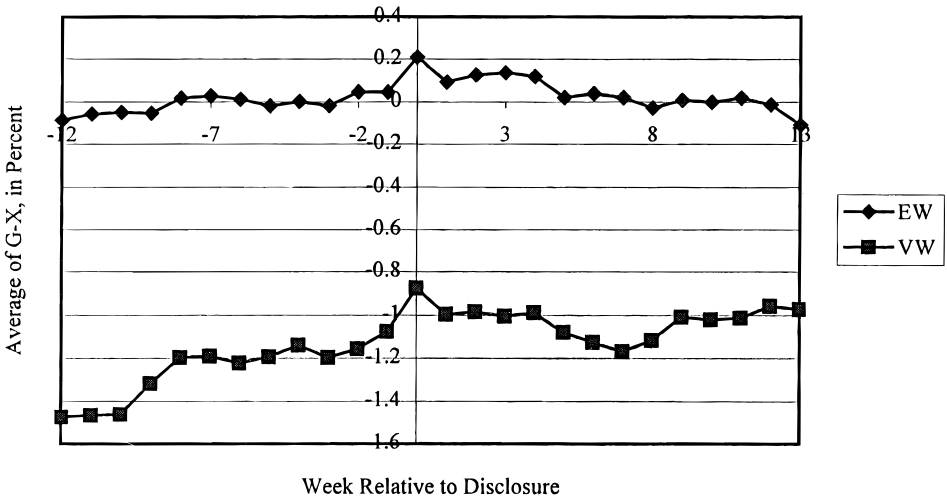

Figure 2. Abnormal government investment relative to disclosure. EW and VW are the equal-weighted and value-weighted, respectively, average allocations to government-issue securities of all money funds that allocate between government-issue and private-issue securities, measured at the indicated number of weeks from a portfolio disclosure.

materially alter investors' opinions. This section investigates whether the results represent more intense activity in a subset of funds by partitioning the 3,783 observations into groups that arguably have more or less incentive to window dress. We cannot necessarily observe the cross section of this incentive, but the literature on mutual-fund agency problems provides some guidance.

Brown, Harlow, and Starks (1996) investigate whether equity-fund portfolios are distorted by an agency problem. They argue that the convex relationship between past performance and net new investment creates an incentive to add variance, and that the incentive is stronger with younger and smaller funds. The shorter track record of younger funds increases investors' interest in their recent performance, and consequently the effect of improving investors' opinions of their recent performance. Similarly, smaller funds may need to grow to survive, and therefore have an added incentive to improve investors' opinions. They also gain a comparative advantage in adjusting portfolio weights from the smaller market impact of their trades. Brown et al. confirm empirically that variance-increasing adjustments decrease with fund age and size.

The MFR data do not indicate fund age, but they do report assets under management for each week, so testing the sensitivity of window dressing to fund size is straightforward. For each of the 3783 observations from Panel A of Table IV I calculate *LOGSIZE* = the log of fund size in the disclosure week, and let s_i be the standardized value of *LOGSIZE* (i.e., minus the mean

LOGSIZE and then divided by the standard deviation) for observation i. I then partition the observations by s_i, and recalculate the same statistics.[7] The results are presented in Panel A of Table V.

The tests do not detect a strong relationship between window dressing and fund size. The point estimates are indeed highest for the smallest funds, but they are second-highest for the largest funds and lowest in the middle. We can get a sense of the statistical significance of the relationship with a simple regression. Letting $WD_{EW,i}$ and $WD_{VW,i}$ be the estimates of window dressing relative to the equal- and value-weighted indices (respectively) for observation i of Panel A of Table IV, we get (t-statistics for difference from zero are in parentheses):

$$WD_{EW,i} = \underset{(3.58)}{0.1982} - \underset{(-1.74)}{0.0965} s_i + e_i, \qquad R^2 = 0.08\%; \tag{1}$$

$$WD_{VW,i} = \underset{(4.77)}{0.2663} - \underset{(-2.12)}{0.1183} s_i + e_i, \qquad R^2 = 0.12\%. \tag{2}$$

By this measure, the significance is marginal.

It is not too surprising that fund size does not have the effect it has in Brown et al. (1996), since their insights about equity fund sizes apply less well to the money market. It is not likely that smaller money funds need to grow to survive because the typical fund is offered by a fund family or bank that is almost certain to offer its customers a money fund no matter what. And the extra liquidity of smaller funds is a minor advantage when investments mature quickly anyway.

Another result in Brown et al. (1996) is that asset substitution is more intense among funds with worse recent returns, consistent with their need for a "home run" to bring in new funds. The cross section of past returns can also be important to window dressing. Investors do not know whether a low money-fund return reflects low risk or low performance. They may also not know whether it reflects high fees, though they can find out at relatively small cost. Investors are intuitively more suspicious of an underperforming fund's portfolio, which in turn increases the manager's incentive to show low risk.

To determine the relationship between window dressing and past performance, the observations of Panel A of Table IV are partitioned by recent yield as follows. First, I calculate the median annualized seven-day yield of all unconstrained retail funds in each week. Then for each of the 3,783 observations I calculate the past relative return as the average yield of the fund over the fiscal quarter (i.e., 13 weeks) ending with the disclosure week

[7] Pooling the observations of fund size across years is not problematic here because the distribution of fund sizes changes little over the sample period. For example, the median fund size was $257.7 million in the first week and $273.15 million in the last.

Table V

Cross Section of Window Dressing by Money Funds in Basis Points

This table refers to unconstrained money funds, which are those money funds that invest in the whole range of money market instruments, such as commercial paper and Treasury bills. Panel A: For a fund f whose second or fourth fiscal quarter-end is proximate to the Tuesday of week w, let $G_{f,w-12} \ldots G_{f,w+13}$ be its allocations to government securities on the 26 Tuesdays around w, let $X_{w-12} \ldots X_{w+13}$ be an index of government allocations of all funds whose second or fourth fiscal quarter-ends are *not* proximate to w, and let $s_{f,w}$ be the log of f's assets under management as of w standardized by subtracting the mean and dividing by the standard deviation of s across quarters and funds. Index weighting indicates whether the index is equal-weighted or weighted by assets under management, mean is the average of $(G_{f,w} - X_w) - (1/25)\Sigma_{i \neq 0}(G_{f,w+i} - X_{w+i})$ for indicated values of $s_{f,w}$, t-statistic is the one-tailed t-statistic for the null hypothesis that the true mean is not positive, and the number of observations is included in the statistics. Panel B: Observations are sorted instead by $r_{f,w}$, which is the average seven-day yield of f from $w - 12$ to w minus the average over the same period of the median fund yield.

Panel A: Sorted by Fund Size

Index Weights	$s_{f,w} < -2$	$-2 < s_{f,w} < -1$	$-1 < s_{f,w} < 0$	$0 < s_{f,w} < 1$	$1 < s_{f,w} < 2$	$2 < s_{f,w}$
Equal	57.75	29.90	28.51	3.69	21.30	36.97
(t-statistic)	(1.19)	(1.06)	(2.96)**	(0.54)	(2.36)**	(1.16)
Value	71.68	42.79	35.50	7.76	28.47	45.49
(t-statistic)	(1.45)	(1.52)	(3.66)**	(1.11)	(3.06)**	(1.46)
No. of observations	118	393	1356	1381	455	80

Panel B: Sorted by Past Performance

Index Weights	$r_{f,w} < -2$	$-2 < r_{f,w} < -1$	$-1 < r_{f,w} < 0$	$0 < r_{f,w} < 1$	$1 < r_{f,w} < 2$	$2 < r_{f,w}$
Equal	130.79	31.04	15.38	20.27	−4.30	−61.10
(t-statistic)	(1.44)	(1.99)*	(1.91)*	(2.58)**	(−0.31)	(−1.27)
Value	130.53	45.96	20.98	26.53	3.22	−65.46
(t-statistic)	(1.41)	(2.91)**	(2.57)**	(3.34)**	(0.22)	(−1.33)
No. of observations	74	425	1319	1477	374	88

*, ** Null hypothesis rejected at the 5 percent and 1 percent levels, respectively.

minus the average of the median yield over the same period.[8] I convert this relative return to a standardized return r_i (i.e., minus the mean and over the standard deviation of all observations) and repeat the tests of Panel A of Table V. The results are in Panel B of Table V.

The figures indicate a connection between relative returns and window dressing. The worst performers tilt more than average toward government instruments for disclosures, and the best performers do not. As above, we can gauge the significance of the relationship with simple regressions:

$$WD_{EW,i} = \underset{(3.39)}{0.1760} - \underset{(-4.12)}{0.2136} r_i + e_i, \qquad R^2 = 0.45\%; \tag{3}$$

$$WD_{VW,i} = \underset{(4.63)}{0.2435} - \underset{(-4.33)}{0.2279} r_i + e_i, \qquad R^2 = 0.50\%. \tag{4}$$

To put these numbers in perspective, consider that the standard deviation of relative returns is 0.27 percent; after a quarter when the average fund pays 5 percent, the models predict no window dressing by a fund that pays 5.25 percent, and 0.6 to 0.7 percent rebalancing by a fund that pays 4.50 percent.

V. Summary and Conclusion

The data for this paper are fiscal year-ends and weekly portfolio statistics of taxable money funds. The interesting feature of these data, and the inspiration for the paper, is that the weekly statistics on allocations to government securities are uneconomical for retail investors to observe. Thus, we perform a direct test of the window dressing hypothesis of Haugen and Lakonishok (1988) and others on retail money funds by comparing the semiannual portfolios that the clientele can observe with those that they cannot. Retail funds that allocate between government and nongovernment instruments can misrepresent their default risk to their clientele by holding more in governments at disclosure than before or after. Several tests in the event-study tradition confirm that portfolio weights do in fact follow this pattern. The unconditional average effect on fund portfolios is modest—approximately 0.3 percent of total holdings—but cross-sectional evidence locates more intense activity in the worse recent performers. It is also worth noting that this sample is biased toward funds that do not mind increased disclosure, which intuitively are those that window dress relatively less.

Much of the current finance literature investigates whether agency problems distort the investment decisions of financial intermediaries. The evidence presented here indicates that money fund managers do not manage their claimholders' money as they would manage their own. That is, a private individual managing his own money has no disclosure events to inter-

[8] I lose 26 observations due to missing yield figures for one or more weeks, leaving 3,757.

act with his investment decisions. This does not prove that consumers want their intermediaries to behave differently, or that fund managers incur extra transactions costs. What it shows is that the strategy known as window dressing is a real and measurable element of delegated money management.

Appendix: Details of Test Design for Table IV

Panel A: For a given month m between February 1988 and March 1997 I identify $I(m)$ and all money funds in the MFR database which for every issue from $I(m) - 12$ to $I(m) + 13$ have government allocation and assets-under-management data and are unconstrained retail funds. I divide these funds into two groups: those with fiscal quarters II or IV ending in month m, and all other funds. From the second group I create two indices, $EW_{m,i}$ and $VW_{m,i}$, where $EW_{m,i}$ is the equal-weighted average government allocation in issue i and $VW_{m,i}$ is weighted by assets under management reported in issue i, $i \in \{I(m) - 12, \ldots, I(m) + 13\}$. For a given fund f in the first group I calculate two statistics Z_{EW} and Z_{VW} as follows:

$$Z_{EW} = (G_{f,I(m)} - EW_{m,I(m)}) - \frac{1}{25} \sum_{i \neq I(m)} (G_{f,i} - EW_{m,i}), \tag{A1}$$

$$Z_{VW} = (G_{f,I(m)} - EW_{m,I(m)}) - \frac{1}{25} \sum_{i \neq I(m)} (G_{f,i} - VW_{m,i}). \tag{A2}$$

For every month I repeat this procedure for all funds in the first group, which yields 3,783 observations of Z_{EW} and of Z_{VW}. I then run the same test on institutional funds.

Panel B: Same methodology, except now

$$Z_{EW} = (G_{f,I(m)} - EW_{m,I(m)}) - \frac{1}{2} ((G_{f,I(m-3)} - EW_{m,I(m-3)}) + (G_{f,I(m+3)} - EW_{m,I(m+3)})), \tag{A3}$$

and

$$Z_{VW} = (G_{f,I(m)} - VW_{m,I(m)}) - \frac{1}{2} ((G_{f,I(m-3)} - VW_{m,I(m-3)}) + (G_{f,i(m+3)} - VW_{m,i(m+3)})), \tag{A4}$$

which yields 3,765 observations. Again, the test is repeated on institutional funds.

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