Can disclosure decrease price efficiency?*

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Abstract

We propose a novel measure of window dressing by fund managers—reduced price efficiency—and document returns reverse 28% more in the 30 days after funds record their holdings for required portfolio disclosures, consistent with an overall drop in price efficiency. Asset pricing anomalies also earn negative returns on disclosure dates, consistent with window dressing leading prices to diverge from fundamental value. We further link our findings to fund-level trades by showing that mutual fund managers are more likely to reverse trades initiated on disclosure days and less likely to pay commissions for information on those days. We also show that volume increases on disclosure days and that return reversals are largest among stocks with larger increases in volume, consistent with increases in demand for securities driving the observed distortions. Combined, these findings suggest that mandated fund disclosures have the unintended consequence of decreasing price efficiency in equity markets.

Key Words: Mandatory Disclosure, Return Reversal, Market Efficiency, Price Pressure

To facilitate flows of capital from investors to investment funds, the SEC requires funds to report holdings to investors on a quarterly basis. In addition to their widespread use by brokers, pension funds and investment advisors, numerous academic studies use these reported holdings to measure various aspects of ownership.[1] However, because these holdings, such as publicly-traded equities, can be traded quickly and at low cost, fund managers have the ability to disclose different positions than they typically hold on non-disclosure days. Such "window dressing" by fund managers, if present, would create a gap between reported holdings and actual day-to-day ownership and potentially impact the informational quality of portfolio disclosures and pricing efficiency more broadly. In this paper, we provide evidence that mandated mutual fund disclosures result in such window-dressing trades, and importantly, affect asset prices.

We hypothesize a gap between ownership and reported holdings will exist if fund managers temporarily shift their holdings when they have to disclose positions. The nature of the window dressing can be systematic, such as a desire by many funds to report holdings that garnered recent media coverage and earned positive returns, which could help attract funds (Solomon et al. 2014), or idiosyncratic,[2] such as an individual fund's desire to report positions consistent with its stated investment strategy. An empirical challenge to identifying window dressing in the cross-section

[1] For example, studies have used holdings to measure the (i) type of investment advisor (Bushee 1988; Bushee and Noe 2000), (ii) the extent to which holdings coincide with stated fund style [see Wermers (2011) for a review], (iii) the extent to which mutual funds herd (Grinblatt, Titman, and Wermers 1995; Wermers 1999), and (iv) the total level of institutional holdings (e.g., Grinstein and Michaely 2005; Hong and Kaperczyk 2009) or ownership by index and non-index mutual funds (e.g., Appel, Gormley, Keim 2016 and 2018).

[2] By idiosyncratic strategies, we mean that window dressing could cause some subset of fund managers to buy and others to sell the same security. Specific examples of idiosyncratic strategies include: (i) eliminating positions inconsistent with fund strategy as stated in the prospectus, (ii) eliminating toe-hold positions that may signal future holdings, (iii) minimizing risk assessments by rebalancing positions (Musto, 1997 and 1999), (iv) eliminating (increasing) positions that you do not (do) want to discuss with investors.

is that we can only observe reported holdings, rather than average fund ownership during the quarter. We circumvent this identification challenge by examining price changes around dates when funds must record their positions for future disclosures. There are two assumptions underlying this identification strategy: (i) funds concentrate their window dressing trades around such recording dates, and (ii) market liquidity does not adjust to the temporary shift in funds' desired portfolio holdings, such that these window dressing trades affect prices.[3]

We provide initial evidence of window dressing and its potential impact on prices by examining returns around portfolio disclosure dates for securities in two industries that have been subject to investor boycotts: tobacco and firearms (see appendix 1). If fund managers trade out of these securities to avoid reporting holdings in such companies, we might expect to find depressed prices around reporting days for stocks in these two industries. We find exactly that. Relative to other firms, tobacco (firearm) manufacturers exhibit returns on the two days preceding quarterly reporting that is, on average, 0.6% (0.4%) lower than their observed differential return on all other days. These lower returns are robust to controlling for firm characteristics and asset pricing anomalies, suggesting the lower returns arise because of reporting requirements. Moreover, among firearm manufacturers, we find an even larger differential in returns for reporting days that occur following a school shooting, consistent with these events increasing fund managers' incentive not to report positions in firearm makers. These findings suggest that at least a portion

[3] Given that market makers require compensation for providing liquidity (Nagel, 2012) and the evidence that liquidity does not sufficiently adjust to counteract price pressures generated by IPO lock-up expirations (Field and Hanka, 2001; Ertimur, Sletten, and Sunder, 2014), S&P 500 index changes (Chen, Noronha, and Singal, 2004), and mutual fund fire sales (Coval and Stafford, 2007), it seems likely that if window dressing incentives affect trading around disclosure dates, these trades will have at least a temporary effect on prices.

of the reduction in institutional holdings documented in prior literature for sin stocks (Hong and Kaperczyk, 2009), results from firms' systematically under-reporting ownership.[4]

To quantify the potential impact of reporting requirements on pricing more broadly, we next look for evidence of increased return reversals following disclosure dates. While we have clear predictions about the direction of price pressure in a handful of stocks (e.g., firearms and tobacco), this is not true when fund managers window dress for idiosyncratic reasons. However, if window dressing around disclosure dates pushes prices away from their intrinsic values, we would expect returns on disclosure days to have larger subsequent reversals as prices move back toward intrinsic value (Biais, Hillion and Spatt, 1999). An alternative hypothesis, is that reporting requirements enhance price discovery by increasing incentives for managers to identify and invest in under-valued securities. We test for increased price distortions (or increased "noise" in returns) around disclosure dates by regressing future returns on (i) daily returns, (ii) daily fund disclosures (the market value of securities disclosed on a day valued at last year's prices divided by the total market value of securities disclosed that entire year, valued at last year's prices), (iii) the interaction of daily returns and fund disclosures.

We find an economically large and statistically significant increase in return reversals on days when managers' must record their portfolio holdings. Our empirical estimates suggest the days with the most fund disclosures have 28% greater reversals over the subsequent 30 trading days than the average day. As returns reverse an average of 11%, the 28% increment equates to an over 250% increase in return reversals. Moreover, over half of the return reversals occur after

[4] In untabulated analyses, we also examine the effect of Morningstar's creation of a corporate social responsibility index on differential returns around reporting days. We find that firms with high CSR scores exhibit significantly higher returns around reporting days after the creation of the index.[5]

the first day subsequent to the reporting date suggesting that the distortions induced by disclosures persist for several days afterwards.

To better understand the increased reversals, we investigate the timing of the distortions in asset prices and whether there is an associated changed in liquidity. First, we find that return reversals are not isolated on the day of the disclosure; the day prior to when most funds disclose also has 10% larger return reversals than observed for non-disclosure days. We find insignificant differences on other days leading up to disclosure, suggesting that window-dressing trades associated with disclosures primarily occur on the recording day and the day beforehand. Second, we show that liquidity increases on days when many funds disclose. Since increased liquidity decreases the cost of trade, this would increase managers' incentives to concentrate disclosure-motivated trades in the short-window before portfolio disclosures (Admati and Pfleiderer 1988).

We conduct a series of tests that establish our reversal results likely relate to funds recording positions rather than some correlated event. First, using institution-level transactions data, we document that funds are more likely to reverse trades in the next week when those trades are entered into on a date the fund records its holdings. The shorter duration of holding suggests funds consider the trades executed on these days to have less information about intrinsic value. We also show that funds pay lower commissions on disclosure days, consistent with these trades being less motivated by information (Groysberg, Healy, and Maber, 2011). Second, we investigate the association between abnormal trading volume and return reversals. If the reversals are the result of funds initiating *more* trades with low information content, we would expect reversals to increase in abnormal volume. In contrast, if the reversals are associated with funds executing *fewer* informed trades, we would expect the reversals to be largest in stocks with less abnormal trading volume. We document that reversals are increasing with abnormal volume, suggesting it is the demand for securities rather than the supply of securities which drive the increase in return reversals. Third, we show that value stocks, those with high profitability, high book-to-market ratios and low asset growth, all exhibit significantly negative returns, consistent with lower price discovery (Engelberg, McLean, and Pontiff, 2017) on reporting days. The sell-off in value stocks suggests institutions face challenges articulating the investment thesis behind these positions, contributing to their negative returns. Fourth, we conduct a number of robustness tests that rule out alternative explanations for our results. Specifically, we find that (i) dividend payments, (ii) the distribution of paychecks, and (iii) variation in the supply of liquidity do not drive the asset pricing results we document.

In our final analysis, we show that front-running disclosure date reversals is highly profitable. To do this, we focus on quarter-end dates, which are known in advance and when most funds record positions for required disclosures. Using only the five hundred largest stocks in the economy, we show that a portfolio that front-runs the reversals by taking a long (short) position in firms with the smallest (largest) quintile of quarter-end returns beginning at the open of the following trading day earns 1.6% over the subsequent month. These returns are larger than the monthly returns to other well-known asset pricing anomalies (Bernard and Thomas, 1990; Fama and French, 2016).

This paper makes three key contributions. First, we examine the cross-section of stocks for window dressing and find substantial evidence of this behavior. In particular, we provide evidence of window dressing both in a sub-sample of stocks facing pressure from consumer advocates and more broadly using increased price reversals around disclosure days as a measure of window dressing. Our evidence is related to, but broader than, the prior literature's examination of whether funds sell losers and buy winners in response to disclosure requirements, which has produced mixed evidence (Lakonishok et al. 1991; Hu et al. 2014; Meier and Schaumburg 2004; Agarwal et al. 2014). While the disclosure of daily net asset values limits funds ability to fool investors about fund financial performance, window dressing has an ability to affect investors' inferences about other aspects of fund's investment strategy.

The second contribution is related to the literature on the information externalities of mandating disclosure. Previous studies implicitly assume these externalities are positive because information has an ameliorative effect on market functioning (Leuz and Wysocki 2016). For example, required disclosures could discipline managers into doing research and executing trades that accelerate information into price, improving price efficiency. If true, we would expect price changes on days of disclosure to exhibit momentum as subsequent investors trade in the same direction as the informed trades induced by disclosure (Campbell, Grossman, and Wang, 1993). However, we document the opposite; disclosure dates are associated with increased subsequent return reversals. These findings indicate that providing information about fund holdings might instead distort secondary market prices, which is not something (to our knowledge) previously considered by the literature. These distortions might also be of interest to regulators because they could plausibly affect the information extracted from prices (Hayek 1945), though we caution that our focus is on documenting a novel externality of mandatory disclosure, and we do not conduct tests which evaluate the efficiency of securities regulation more broadly.

Third, this paper uses novel variation in price efficiency to provide evidence of funds engaging in window dressing trades. Because of the multiplicity of reasons funds may want to show investors a different portfolio than the one that maximizes expected returns, we abstract away from the specific strategies and instead test for overall changes in price efficiency. The underlying assumption is that if fund managers trade into a stock for a reason other than they expect the security is undervalued, the price pressure exerted by these trades should reverse. Consistent with this, we document larger price reversals around disclosure days, suggesting disclosure requirements have substantial effects on the securities investors choose to hold.

1. Institutional Background and Literature Review

Institutional investors are subject to two mandatory ownership disclosure requirements by the SEC – Form 13F and Form N-CSR and N-Q (these latter two forms replaced Form N-30D in May 2004). While both disclosures are filed quarterly, Form 13-F filings are aggregated at a company level while Forms N-CSR and Form N-Q are filed at the individual fund level. Since mutual fund companies operate several mutual funds, each fund having a different fund manager – Form 13F filings are seen to be less informative than Forms N-CSR/Q. A second major difference between the two filings is that Form 13F is only filed by large investors (the SEC defines large investors as those with more than $100 million USD in holdings of equities, convertible bonds and exchange-listed options) and includes information only on large positions (defined as more than 10,000 shares and market value exceeding $200,000 USD)[5]. On the other hand, Forms N-CSR/Q are filed by all mutual funds regardless of the fund's size or the size or type of the holdings.

The SEC recognizes the disclosure of institutional investors' portfolio holdings through these various forms as a key aspect of the securities market regulation. These filings are meant to increase fund transparency to investors so that they can better monitor how their fund managers are investing. However, an increase in the transparency of portfolio holdings also increases the

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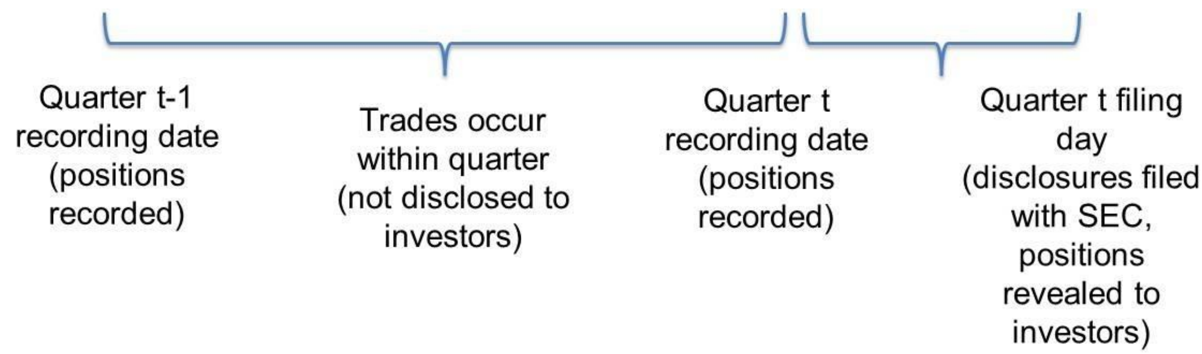
risk of revealing fund managers' investment strategies which could lead to copycat trades or front-running by competing funds. Keeping this in mind, the SEC allows funds to file their disclosure forms with a 60- (Forms N-CSR/Q) to 45-day delay (Form 13-F).



Above is a timeline depicting the recording of fund positions and subsequent revelation to investors. In our subsequent analyses, the dates on which we test for predictable returns correspond to date when fund positions are recorded rather than when they are disclosed.

Two prior studies that investigate the impact of these disclosure requirements on security prices are Carhart et al. (2002) and Hu et al. (2013). Both examine whether managers "portfolio pump" or inflate net asset values (NAVs) by placing buy orders just before disclosure. Specifically, Carhart et al. (2002) shows that funds' NAVs systematically rise on disclosure days and reverse the following day, suggesting that fund managers systematically trade around disclosure days to inflate performance. Analyzing institutional trades, Hu et al. (2013) find that funds tend to buy stocks in which they already hold large positions, which is also consistent with portfolio pumping, but that year-end price inflation is driven by a lack of institutional selling rather than buying.

Our subsequent findings expand on these existing studies by analyzing whether disclosures are associated with differences in medium-term pricing efficiency and whether these reversals are associated with liquidity and the direction of price movements. For example, both of these studies focus on short-term pricing increases, and Carhart et al. (2002) show stronger effects in more ***illiquid*** securities. In contrast, our paper documents differences in longer-term pricing efficiency and stronger effects in more ***liquid*** securities, thus providing evidence that portfolio pumping is not the only way portfolio disclosures impact price efficiency. Additionally, we expand on these studies by showing that disclosure-induced return reversals have similar magnitude for both negative and positive price movements. In other words, although fund disclosures may incentivize more funds to buy rather than sell, we show that disclosure-induced sales also seem to have less information content than the average sale.

Other studies investigate the specific strategies managers use to window dress (e.g. Meier and Schaumburg, 2004; Agarwal et al., 2014, etc.), or hold securities that they would not hold but for the reporting requirements. Strategies investigated by the prior literature include: (i) concealing risk (e.g., Musto, 1997 and 1999), (ii) attracting media attention (Solomon, Soltes, and Sosyura, 2014), and (iii) concealing fund strategy (e.g., Wermers, 2001). Several studies conclude that fund strategies such as 'herding' are not 'window dressing' because they tend to enhance rather than diminish price efficiency (e.g. Wermers 1999; Frank et al. 2004). We abstract away from the specific strategies and instead conduct tests designed to capture the aggregate effect of disclosure-induced trades on price efficiency. The basic idea is that there are a variety of ways portfolio managers would want to change their holdings on days when they are revealed to investors, and trading into these alternative holdings will affect prices.

Finally, there is also a literature on how the disclosure of fund holdings affects investor allocation decisions (e.g., Grinblatt and Titman, 1989 and 1993; Cohen, Coval, and Pástor, 2005) and stock liquidity and fund performance (Agarwal et al. 2015). The focus of our empirical work, however, is on how the obligation to disclose might also lower the information content of prices.

2. Hypothesis Development

We hypothesize that mutual fund investors will want to hold different positions when they have to disclose those positions than when they do not. Fund managers determine whether to hold security i at time t, by placing weight on their expectations of risk-adjusted returns (RAR_{it}) and weight on their expectations of positions that fund investors would prefer to hold ($IPREF_{it}$). Fund managers sell a stock when the utility managers obtain from holding it is lower than the utility managers can obtain from holding another security, less the trading costs of altering positions. Our central hypothesis is that on days when funds must report their portfolios the weight fund managers assign to $IPREF_{it}$ increases and RAR_{it} decreases, resulting in different trades than are observed on non-reporting days.

To provide a concrete example, we hypothesize that because cigarette makers sell an addictive carcinogenic product and lied to the public about both its addictiveness and tendency to cause cancer, investors will have a weak preference to own these securities. In appendix one, we provide examples of investor boycotts of tobacco stocks and prior literature documents low investor ownership (Hong and Kaperczyk 2009). We hypothesize investor demands will lead fund managers to exit cigarette positions intensively at portfolio reporting days and delay entering positions until after positions must be reported. The change in weights assigned to IPREF for these securities could be sufficiently strong that some funds will exit a position completely before disclosure and then re-enter the position immediately afterwards. Assigning more weight to investor preferences could also have more subtle effects, such as inducing managers to trim a cigarette position before disclosure (for example, to move it out of the top ten holdings). We hypothesize similar forces affect stock prices for firearm manufacturers, whose sale of rifles with high capacity magazines which can be converted from semi-automatic to automatic weapons, are commonly used in school shootings and are hypothesized to increase fatalities. We provide evidence the decline in value for firearm manufacturers is concentrated after school shootings.

Investor preferences over specific stocks can arise for a variety of reasons. We provide evidence on one systematic preference that we hypothesize will have similar effects on the portfolio holdings of most fund managers: a desire not to be an owner of tobacco stocks or firearm stocks. Investor preferences can also have idiosyncratic effects on fund holdings, so that window dressing could cause some subset of fund managers to buy and others to sell the same security. Specific examples of idiosyncratic strategies include: (i) eliminating positions inconsistent with fund strategy as stated in the prospectus, (ii) eliminating toe-hold positions that may signal future holdings, (iii) minimizing risk assessments by rebalancing positions (Musto, 1997 and 1999), (iv) eliminating (increasing) positions that you do not (do) want to discuss with investors.

Whether disclosure-induced trades will distort security prices or lead prices to converge toward fundamental value depends on how these trades are related to intrinsic value. If disclosure incentivizes a fund manager to temporarily trade into a position for reasons unrelated to intrinsic value (e.g., to cater to investor preferences), holding liquidity constant, the initial trade can exert price pressure, but since the price movement does not reflect intrinsic values, subsequent price movements will reverse out the price impact. Alternatively, if disclosure induces investors to adopt positions that better reflect their information about intrinsic values, we would expect more permanent price changes since the trade presumably pushes price towards its intrinsic value.

Whether disclosure-induced trades affect security prices will also depend how the provision of liquidity adjusts around disclosure dates. If liquidity providers anticipate fund managers will attempt to reverse positions entered into on disclosure days because these trades reflect window dressing, they will be more willing to supply liquidity. This will reduce the price pressure of trades and mitigate the impact of temporary trades on return reversals.

Because it is ex-ante unclear how fund managers and liquidity providers will react to disclosure requirements, it is necessary to test for their impact. We will primarily test for an effect of disclosure on price efficiency by examining whether there is a change in the magnitude of price reversals around days when fund managers record their positions. An increase in price reversals on these days would be indicative of a decrease in price efficiency, while a decrease in price reversals on disclosure days would suggest an increase in price efficiency. However, we will supplement these tests by examining how major asset pricing anomalies perform, under the assumption that asset pricing anomalies earning positive returns leads to a convergence toward fundamental values (Engelberg et al. 2018) and negative returns lead to a divergence from fundamental values (i.e., less price efficiency).

We also test how price reversals interact with liquidity in the cross-section. If investors initiate low information content trades in response to disclosures, we expect that fund managers will trade more heavily in stocks with low trading costs (i.e. those stocks which are more liquid and heavily traded). Because the benefits to window dressing are unlikely to vary systematically with stock liquidity, this suggests that any decrease in price efficiency caused by window dressing, ignoring any potential change in liquidity provision by market makers, should be greater among more liquid stocks. Thus, if disclosures induce less informative trades, we predict return reversals following mutual funds disclosures should be larger for more liquid stocks.

3. Data, Research Design, and Results

3.1. Data and Descriptive Statistics

We construct our dataset by obtaining prices of the securities held from the Center for Research in Security Prices (CRSP) database and limiting the sample to common shares (i.e. share code 10 or 11) that trade on the NYSE, AMEX or NASDAQ with a prior month-end market capitalization larger than ten million dollars. For all observations (at the firm-date level), we calculate return variables such as value weighted returns, market excess returns, lead/lagged returns, trading volume, etc. We require four variables identified in the asset pricing literature (Fama and French, 2016), that generate abnormal returns – operating profit, total asset growth, size (or market value of equity), and book-to-market ratio. We also include the past earnings surprise, a proxy for post-earnings announcement drift. For each of these factors, we generate percentile ranks for each month. We use two proxies for firm-level liquidity – the average ratio of the daily absolute return to the dollar trading volume on that day (commonly known as the Amihud (2002) illiquidity measure) and closing bid-ask spreads scaled by share price. As spreads are not available for all firm-dates, analyses using this variable have a more limited sample. We provide a full description of each variable in the appendix.

An empirical challenge to measuring fund ownership is that when funds window dress, reporting will systematically differ from ownership. To circumvent this empirical challenge, we calculate fund ownership at the aggregate level. If a fund sells one security and buys another security in response to reporting requirements, this will affect measures of reporting for the individual securities but not for the market. We obtain disclosed fund positions from the S12 and 13F master file from Thomson Financial mutual fund holdings database from 1982 to 2015. We begin the sample in 1982, the first year for which the S12 database includes month-end reporting. We begin by measuring the value of fund positions recorded on a particular day. Specifically, for each disclosed position, across both databases, we calculate the value of shares disclosed (using last year's prices). To calculate the value of positions disclosed at the date level, we then sum the value of these positions across all funds and firms by date. To calculate the value of positions disclosed at the firm-date level, we instead sum these values across all funds by firm-date. If a position is recorded on a non-trading day, we assign it to the most recent past trading day. While mutual funds choose their own reporting date, firms completing form 13-F (including mutual funds, insurance companies and pension funds) must record their holdings on quarter-end days. To avoid double-counting positions from mutual funds, we use 13-F position values for quarter-end days and S12 position values for non-quarter end days.

To calculate the aggregate percentage of portfolio holdings disclosed on a particular date (*AggrOwn*), we take the sum of position values recorded across all funds on a date and divide by the sum of positions recorded across all funds over the year. Specifically,

$$AggrOwn_t = \frac{\sum Market\ capitaliztion\ of\ all\ disclosed\ positions\ on\ date\ t}{\sum Market\ capitalization\ of\ all\ disclosed\ positions\ in\ a\ year}$$

An illustration will help make the calculation a bit more transparent. The combined market capitalization for all disclosed positions on 03/31/2009 (valued at 12/31/2008 prices) equals $7.99 trillion USD, and total market capitalization of disclosed holdings for all of 2009 equals $36.66

trillion USD. Thus, *AggrOwn* for 03/31/2009 equals 7.99/36.6 = 21.8%. In contrast, funds disclose $239 billion in positions on 02/28/2009; so, *AggrOwn* equals 0.239/36.65 = 0.65%.[6]

To facilitate interpretation of point estimates in our later regressions, we rescale *AggrOwn* using the sample average of *AggrOwn* across quarter-end days, which is 23.7%. This rescaling ensures that our later estimation coefficients can be interpreted as the average change in the outcome of interest that occurs for the typical level of disclosures on a quarter-end day.

For our trade tests, we use a proprietary dataset called Ancerno from Abel Noser Solutions, a financial services firm that provides trading cost analytics advice to institutional asset owners, managers, and brokers such as mutual funds and hedge funds. The observations from the Ancerno dataset allow us to observe trade level data such as the date of a transaction by a fund manager, the stock symbol of the trade, the number of shares traded, dollar principal traded, and any commissions paid. The dataset anonymizes the name of the trading institution/fund manager but identification codes for managers are provided which allows us to track an institution's trades across stocks and over time. In later tests, we merge this data with our *AggrOwn* measure and run cross-sectional tests to examine whether the serial correlation in trades by fund managers changes on days when most funds record portfolio holdings. We use commissions as a proxy for trades influenced by information. Our expectation is that if recording positions leads funds to make uninformed trades, we would expect a decline in commission dollars.

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[6] Two advantages of calculating our measures at the aggregate level are: (i) our measure is orthogonal to all firm-level variables that could be related to price formation, and (ii) there are issues with measuring the timing of portfolio holdings, and we expect the issues are less significant at the market level (Schwarz and Potter 2016).

3.2 Descriptive Statistics

We begin by first examining our main variables of interest by firm-date, computing the averages over (i) all quarter-end days, (ii) all non-quarter-end month-end days, and (iii) non-month-end days. We first show the average rescaled *AggrOwn* value is 98.2% on quarter-end days,[7] 3.6% on non-quarter-end month-end days, and 0.003% on non-month-end days. These findings confirm that most disclosures occur on quarter-end days, while some fund-level disclosures occur on non-quarter-end end-month days, and very few disclosures occur on non-month end days. We also split the sample (from 1982 to 2015) into three sub-periods: 1982-93, 1994-2004 and 2005-2015. From this, we see that the proportion of disclosures that occurs on quarter-end dates decreases in the later years of our sample.

[Insert Table 1 around here]

Table 1 also reports the daily liquidity (across both bid-ask spreads and Amihud liquidity), dollar volume, and absolute returns of firms by date. We percentile rank each of these daily values by month-year, pooling the entire sample, and then take the average for quarter-end, month-end and non-month-end days. On quarter-end days, stocks are more liquid relative to both non-quarter-end month-end days and non-month-end days. Moreover, we find that absolute returns tend to be higher on quarter-end days, consistent with either more information about fundamentals flowing into prices at the end of the quarter or with an increase in stock return noise. We also find that dollar volume increases on quarter-end days, suggesting demand for securities increases, consistent with the theory that recording positions leads funds to enter into additional transactions.

[7] The average rescaled end-of-quarter *AggrOwn* value differs from one because there are more firm-quarters in the later years of our sample, when more mutual funds report on non-quarter-end days.

Our subsequent tests will examine whether the quarter-end increase in returns correspond to increased information about intrinsic values.

4. Results

4.1 Do investor preferences affect portfolio reporting day returns?

We start our tests by examining whether tobacco and firearm stocks exhibit negative returns on portfolio reporting days, two industries which have been the subject of investor boycotts (see Appendix One). We hypothesize funds will wish to avoid reporting holdings of cigarette stocks, because cigarettes are an addictive product which cause heart disease and cancer and the CEOs of these companies lied about these risks. We hypothesize funds will wish to avoid reporting holdings of firearm companies because large magazine clips and the convertibility of semi-automatic into automatic weapons facilitates mass shootings.

We test whether fund manager preferences not to hold these securities affects asset prices by regressing returns over the day of portfolio reporting and the day before on (i) the percentage revenue generated from Tobacco SIC codes, (ii) the percentage of revenue generated from Firearm SIC codes (both SIC codes defined in the appendix) and our variables of interest, the interaction of aggregate ownership with percentage of tobacco and firearm revenue. The prediction that fund managers sell these holdings in response to reporting requirements predicts negative returns for these stocks. In all specifications, we also include (i) date fixed effects and (ii) controls for asset pricing anomalies.

$$Ret(t-1,t) = \alpha_i + \beta_1 Tobacco + \beta_2 Tobacco * AggrOwn + \beta_3 Firearm + \beta_4 Firearm * AggrOwn + \varepsilon_i, \quad (1)$$

In Table 2 column (1), we show $Tobacco * AggrOwn$ loads with a statistically significant negative coefficient (t=-3.96). The coefficient magnitude suggests that tobacco stocks on average lose sixty basis points over the two day quarterly reporting window. We find the main effect on tobacco stocks is significantly positive, consistent with prior research's findings that tobacco stocks on average earn higher risk-adjusted returns (Hong and Kaperczyk 2009). We also find negative and significant coefficient on $Firearm * AggrOwn$, consistent with reporting triggering sales of firms in this industry as well. In column (2), we report similar results including firm fixed effects.

We provide additional evidence on our hypothesized mechanism by examining whether major school shootings trigger an increase in the reporting induced sell-off of firearm stocks. School shootings are often accompanied by prolonged periods of public pressure and boycotts of firearm stocks and we hypothesize similar forces will affect the share market around portfolio disclosure.[8] We define a day as affected by a school shooting if it occurs within three years of one of the top ten school shootings in American history.[9] To test whether school shootings have an incremental effect on firearm stocks, we supplement equation (1) including a post-school shooting indicator and fully interacting it with AggrOwn and Firearms. In column (3), we find our variable of interest School*AggrOwn*Firearms loads with a significantly negative coefficient,

[8] Mass school shootings were uncommon before the 'Columbine' shooting of April 20, 1999 but have occurred more frequently since. Seven (nine) of the ten largest school massacres (shooting massacres) occurred after 1999. The only mass school shooting that occurred before 1999 occurred at the University of Texas in 1966.

[9] Perhaps the best example of a school shooting affecting ownership is Cerberus, a private equity group, owned the company that manufactured the gun used in the Sandy Hook School shooting through one of its funds. Three days after the shooting, Cerberus announced that it planned to sell the gun manufacturer. However, it had difficulty finding a buyer. Cerberus bought out LPs who no longer wanted to be owners of a gun manufacturer, effectively removing the position from its fund (https://www.stltoday.com/business/local/after-sandy-hook-cerberus-vowed-to-sell-gun-maker-what/article_a5d41748-748e-52b8-9a3b-9bd61a5bb0ca.html).

suggesting school shootings depress reporting returns of firearm stocks by 0.5%. In column (4), we show these results are robust to including firm fixed effects.

Finally, we conduct a number of untabulated robustness checks that validate our results relate to the makers of large consumer brands that have been the subject of most public pressure. First, we find that for Philip Morris, the most visible tobacco company, the average return on quarter-end reporting days is significantly lower than the average non-quarter-end reporting day (We take the average of Altria and Philip Morris International stock prices, for quarters after its corporate restructuring). Second, we repeat our analyses coding Tobacco and Firearm as indicators set to one if the parent company controls a major consumer brand.[10] We hypothesize consumer brands will generate attention from investors and thus stimulate window dressing trade. We find statistically significant results using an indicator for major consumer brands.

4.2 How does reporting affect price efficiency?

Our previous set of tests suggest that disclosures affect asset prices but leave uncertain as to whether they enhance or decrease price efficiency. In this section, we examine whether trades that occur on days when funds record portfolio positions have lower price discovery. Following past market microstructure research, we examine this question by ascertaining whether security returns reverse in the period following days when many funds record the positions that they will subsequently disclose to investors. Trades that do not reflect information about the intrinsic value

[10] There are two major manufacturers of firearms that are publicly listed, Sturm Ruger & Co and Smith & Wesson (which recently changed its name to American Outdoor Brands). Both manufacture semi-automatic rifles with high capacity magazines. Many firearms manufacturers, such as Browning and Remington, are private for the entirety of our sample. Manufacturers of most major American cigarette brands were public for most of our sample (RJR Nabisco, the maker of Winston and Camel, was private for two years after the 1989 KKR leveraged buyout and their operations were sold to international companies, Japanese Tobacco and British American Tobacco in the early 2000s). In addition to Phillip Morris and RJR, we also consider Lorillard Tobacco, maker of True and Newport to be a major consumer brand. They were a part of the Loews Corporation until 2007.

of a security by definition will not impound information about long-term value into prices. If disclosure requirements induce ‘uninformed’ or ‘noise’ trades (e.g., via window-dressing), holding liquidity constant, then these uninformed trades would cause the price of the asset to go up (in the case of uninformed buying) or down (in the case of uninformed selling) temporarily followed by reversals when the price of the asset is ‘reset’ to its original fundamental value (Campbell et al., 1993; Biais et al., 1999). In other words, if fund disclosures reduce price discovery, future returns should be more negatively associated with contemporaneous returns on disclosure days. We thus estimate the following model:

$$Ret(t+1, t+s)_i = \alpha_i + \beta_1.AggrOwn + \beta_2.Ret(t) + \beta_3.AggrOwn * Ret(t) + controls + \varepsilon_i, \quad (2)$$

where ‘s’ indexes trading days and can take values from 1 to 30 depending on the specification. A negative sign on the coefficient on the interaction term (β_3) would indicate that returns that occur on portfolio recording days exhibit greater subsequent reversals. We include firm and date fixed effects in all regressions. When doing so, *AggrOwn* is dropped since it is collinear with the date fixed effects. Standard errors are double-clustered by firm and month.

We begin our analysis by assessing the extent to which returns reverse over the course of the sample by estimating model (1) after omitting *AggrOwn* and its interactions. The estimates of this regression are found in column 1 of Table 3. Consistent with prior studies documenting return reversals, we find *Ret(t)* loads with a highly significant coefficient of -11%.

Estimating model (2), we find strong evidence of larger return reversals on days with fund reporting. This is shown in Table 3, column (2), where *Ret(t)*AggrOwn,* loads with a statistically significant value of -27.7% (*t*=-7.8). This estimate indicates that reversals increase, on average, by about 27.7 percentage points on days where the amount of positions recorded is similar to that of an average quarter-end date. We also observe the coefficient on *Ret(t)* declines to -10.9%, suggesting fund reporting can explain 5% (1-10.9/11.4) of daily return reversals (Lehmann 1990; Jegadeesh 1990).[11]

The pricing distortion that is associated with disclosures does not dissipate quickly. This is shown in column (3), where we re-estimate model (1) after computing the dependent variable as the cumulative return from Day 2 until Day 30, *Ret(t+2, t+30)*. Excluding the next trading day (*t*+1) allows our regression coefficients to capture reversals that occur more than one day after the date of interest. Doing this, we find a coefficient on *Ret(t)*AggrOwn* of -14.2% (*t*=5.0). As the coefficients are more than double the coefficient on *Ret(t)*, our analysis suggests the positions recorded on the average quarter-end day have the effect of more than tripling return reversals from Day 2 until Day 30. The finding also shows that price distortions occurring on recording days do not dissipate as quickly as has been suggested by previous work (Carhart et al., 2002).

The observed reversals do not appear to be driven solely by portfolio pumping. In column (4)–(5), we re-estimate model (1) including an indicator variable for days with negative market-adjusted returns (*NegRet(t)*) and fully interact it with the other variables in model (1). If our results are explained by portfolio pumping (Carhart et al. 2002; Hu et al. 2013), we would expect to observe the incremental reversals associated with disclosures only when returns are positive; in other words, the triple interaction *NegRet(t)*Ret(t)*AggrOwn* would load with a significant coefficient equal and opposite to the coefficient on *Ret(t)*AggrOwn*. Contrary to this prediction, we document an insignificant coefficient on the triple interaction (column 4). In column (5), we

[11] In column (2), we obtain a similar coefficient on our variable of interest including or excluding firm and date fixed effects. Our coefficients are also unaffected by including day of the week indicators interacted with daily returns and including indicators for whether returns fall within the three day EA window, interacted with returns.

show that when we remove the next trading day from the dependent variable, we still observe an insignificant coefficient on *NegRet(t)*Ret(t)*AggrOwn*, suggesting similar incremental reversals for negative and positive returns. Overall, our results are consistent with an increase in both low-information buys and low-information sells when funds report positions.

The increase in reversals also extends to the day before funds record their holdings. This is shown in column (6) where we include the one- and two-day prior returns in the regression specification and interact those returns with *AggrOwn*, to investigate whether our results are consistent with funds making low-information-content trades in anticipation of having to reveal positions. We find *Ret(t-1)*AggrOwn* loads with a marginally significant negative coefficient (-10.7%; *t*-stat=1.9) while *Ret(t-2)*AggrOwn* loads with an insignificant negative coefficient. The significant coefficient on the prior days' returns interacted with *AggrOwn*, also provides evidence that the price distortions generated by requiring disclosure do not unwind after a single day.

[Insert Table 3 around here]

In order to provide additional evidence on the duration of return reversals, we re-estimate equation (1) thirty times, each time incrementing the time index, *s*, by 1 day. This allows us to construct a time series of the return reversal from day 1 to day 30 following portfolio disclosures. We create Figure (1) by plotting the coefficients on *Ret(t)* and *AggrOwn*Ret(t)* from these thirty regressions. Figure (1) shows that the coefficient on contemporaneous returns, *Ret(t)*, reaches -11% within ten trading days following the disclosure day and remains constant thereafter. However, the coefficient on *AggrOwn *Ret(t)* drops to -20% in the first ten trading days, and continues to fall by another eight percentage points over the subsequent twenty-five trading days.

This finding indicates that, on average, the reversal in returns observed following portfolio disclosures is both larger and longer in duration than non-disclosure day reversals.

[Insert Figure 1 around here]

Finally, in Table 4, we examine how disclosure day return reversals have evolved over time by splitting our sample into three sub-periods by decades (1982-93; 1994-2004 and 2005 onwards). We find that reporting-day reversals, while significantly negative, were smaller in magnitude in our earliest sub-period. Reporting-day reversals increased in magnitude from 13.3% to 35.5%, as we move from our earliest sub-period to the subsequent one. Since 2005, reporting induced reversals have averaged 21.5%. The increase in return reversals since the 1980s could be attributable to market level changes that led to an increase in overall stock liquidity, such as the introduction of electronic trading, reduction in ticker size etc. Increases in liquidity lower the costs of trading, making it less costly for fund managers to engage in window-dressing.

[Insert Table 4 around here]

4.3 Linking return reversals to fund behavior

While the larger return reversals associated with aggregate disclosures are consistent with low-information-content trades initiated in anticipation of disclosures, one potential concern is that another variable correlated with position recording by funds, which predominantly occurs at month and quarter end, drives these findings. In this section, we present three tests that suggest the disclosure-driven fund trading activity drives return reversals.

4.3.1 Return reversals and market quality

Our hypothesis is that position reporting requirements induce funds to make low-information-content trades, which affects the information impounded into price. This hypothesis implies that funds make trades they would not otherwise make and incur transaction costs that they would otherwise seek to avoid. Our theory has two testable implications: (i) the incremental reversals associated with fund reporting should be larger in more liquid stocks, where the costs of transacting are lower[12] and (ii) return reversals should be correlated with incremental trading volume, because fund reporting requirements increase the demand for liquidity. These tests can also differentiate our results from an alternative hypothesis, that reporting requirements of financial intermediaries reduce the supply of liquidity and thereby increase price impact.

We provide evidence on how stock liquidity and trading volume are associated with disclosure date return reversals by fully interacting the specification in our first section (equation 1) with characteristics of the firm's equity market, *FirmChar*. Specifically, we estimate

$$
\begin{aligned}
Ret(t+1,t+s)_i = \quad & \alpha_i + \beta_1.AggrOwn + \beta_2.Ret(t) + \beta_3.FirmChar + \beta_4.AggrOwn * Ret(t) + \\
& \beta_5.AggrOwn * FirmChar + \beta_6.FirmChar * Ret(t) + \\
& \beta_7.AggrOwn * FirmChar * Ret(t) + Controls + \varepsilon_i
\end{aligned}
\tag{3}
$$

In our first set of tests, we select two firm-level proxies for illiquidity: spreads and Amihud (2002) illiquidity. We calculate spreads as the ask minus the bid, scaled by price, and we calculate Amihud (2002) illiquidity as the ratio of absolute value of returns divided by the dollar volume of

[12] Specifically, if fund managers wish to report a portfolio different from the portfolio they wish to hold, they would determine their portfolio allocations at the time of disclosure trading off this preference with the trading costs of doing so (Admatti and Pfleiderer 1988). Trading costs are negatively associated with stock liquidity, so the costs of rebalancing in response to disclosure will vary with liquidity, but the reporting preferences will not. Given this, we expect a higher fraction of disclosure induced trades in more liquid stocks, and if the greater return reversals on disclosure days are driven by window-dressing, we might also expect to observe the disclosure day reversals to be differentially larger for more liquid stocks.

trades. For both variables, we measure illiquidity as the average over the month, so that these variables capture firm characteristics, rather than trading conditions on a particular day. Specifically, for each firm-month, we take the average of daily values and then percentile rank each variable in the cross-section: *RankMonthlyAmihud* and *RankMonthlyBidAsk*. We expect that if funds concentrate their low information content trades in more liquid stocks, the coefficient on triple interaction term between *AggrOwn*, *Ret(t)* and *FirmChar* (β_7) should be significantly positive when *FirmChar* equals either *RankMonthlyAmihud* or *RankMonthlyBidAsk*.

[Insert Table 5 around here]

As we expect, incremental price reversals are larger among more liquid stocks. This is shown in Columns (1) and (2) of Table 5, in which we find the triple interaction *Ret(t)*AggrOwn*FirmChar* loads positively when spreads and Amihud (2002), respectively, are used as proxies for illiquidity. Our coefficient estimates for spreads (Amihud) imply moving from a 25th percentile ranked firm for illiquidity to a 75th percentile ranked firm would decrease incremental return reversals by 12.0 (11.6) percentage points. This finding differs from results from the extant literature on price pumping (Carhart et al. 2002). The stock price noise associated with portfolio disclosures that we document is largest in the most liquid stocks, while the increase in closing prices documented in Carhart et al. (2002) is instead largest in the most *illiquid* stocks.

In columns (3) and (4), we test whether reversals are associated with the demand for or supply of liquidity, as proxied by trading volume. If reversals are driven by a demand for liquidity, we would expect that reversals to be larger when there is more dollar volume. If reversals are driven by market makers withdrawing from the market, we would expect the absence of counterparties would lead reversals to be larger among stocks with lower volume. To measure dollar volume, we use the percentile rank of the firm's average daily dollar volume over the month, *RankDolVol*. In column (3), we find the triple interaction *Ret(t)***AggrOwn***RankDolVol* loads with a marginally significant negative coefficient, consistent with increased demand for transactions leading to reversals. In column (4), to address concerns that these results are driven by investors trading in the security on normal, rather than reporting days, we include the rank of trading dollar volume over the entire month as a control and fully interact it with *Ret(t), AggrOwn, and Ret(t)***AggrOwn.* Including controls for monthly dollar volume, allows the coefficient on daily dollar volume to capture the impact of abnormal volume on reversals. We continue to find a statistically significant coefficient, consistent with volume increasing reversals.

4.3.2 Fund level trades and fund disclosures

To examine how fund managers trading behavior changes around disclosure days, our next set of tests tracks the trades of funds over time. Our analysis allows us to examine when managers build and reverse positions, so we can provide corroborative evidence that the reversals we observe in equity returns are driven by the behavior of fund managers. We conduct two main analyses using this data: (1) we examine whether managers are more likely to reverse trades entered into on reporting days. A willingness to reverse trades suggests the trades were motivated by temporary motives (e.g., window dressing) rather than long-lived information. And (2), we use commissions as a proxy for trades motivated by information because of the Wall Street convention to pay commissions in exchange for research and examine how commissions vary with fund reporting.

To examine whether fund managers are more likely reverse trades initiated on reporting days, we obtain trade-level data from Ancerno, which allows us to track managers' trades over time. We test for window dressing by examining whether fund managers are more likely to reverse positions entered into on disclosure days relative to positions entered on other days during the quarter. Specifically, we test this by estimating

$$Trade(t+1, t+7)_i = \alpha_i + \beta_1 . AggrOwn * Trade(t) + controls + \varepsilon_i, \qquad (4)$$

where *Trade* either measures the sign of trades (i.e., buy = 1 and sell = -1) or the sign of the trade multiplied by the absolute value of the log of the dollar volume. We measure all values at the fund-firm-date level and include date and firm fixed effects as controls.

Consistent with funds being more likely to reverse trades initiated on reporting dates, we find that interaction between *AggrOwn* and *Trade(t)* loads negatively. This is shown in Table 6. When we measure trades using direction (both independent and dependent variables) we find that the trades on days with a level of disclosures similar to that of an average quarter-end date are 10% more likely to be reversed in the subsequent week (Table 6, column 1; *t*-stat=4.1). We obtain similar results when including institution fixed effects, which ensures that the observed increase in trade direction changes is driven by within-institution trades (column 2). In columns (3) and (4), we examine the economic significance of these reversals by estimating regressions using dollar volume. We continue to find a significant coefficient estimate (-13%). Because the main effect on *LogDolTrade(t)* is positive, this suggests funds are less likely to follow up positions taken on aggregate reporting days by executing similar trades in the future.[13]

[Insert Table 6 around here]

[13] In untabulated analyses, we find that our findings are robust to: (i) including fund trades at date *t* on which the fund does not trade in the window from *t*+1 to *t*+7, (ii) extending the window over which we measure the dependent variable from the next week to the next six weeks, which demonstrates the reversals are not temporary, and (iii) summing trades across managers, which shows the tendency to reverse trades does not cancel out in aggregate.

4.4.3 Fund level trades and commissions

Institutional investors, such as those captured in the Ancerno database, pay commissions to brokerages at least in part, in exchange for information. To provide additional evidence on the information in fund trades, we examine whether commissions vary with fund reporting on both per share and per dollar basis. To do this, we include month-year fixed effects and day of the week fixed effects to control for trends in brokerage commissions unrelated to fund reporting and estimate the following specification:

$$Commisions(t)_i = \alpha_i + \beta_1 . AggrOwn + controls + \varepsilon_i . \qquad (5)$$

In column (1) of Table 7, we find that per dollar of transaction commissions decline 0.01% with our measure of fund reporting. Because the average commission per dollar of trade is 0.1%, the reduction amounts to a decline of 9.8%. In column (2) of Table 7, we find that per share commissions decline 0.3 cents per share. Since the average commission is a little under three cents per share, this decline results in a 10.2% reduction in commissions per share.

[Insert Table 7 around here]

4.4.4 Asset pricing anomaly returns and fund disclosures

Our next set of tests use anomaly returns to provide additional evidence that fund reporting induces prices to move away from fundamental values. In particular, if larger anomaly returns reflect prices converging towards their fundamental value (Engelberg et al. 2018), a negative anomaly return would be consistent with lower price discovery.

We then interact these percentile ranks with our disclosure measures and run the following regression model:

$$Ret(t-1,t) = \alpha_i + \beta_1.RankOP + \beta_2.RankCMA + \beta_3.RankME + \beta_4.RankBTM + \beta_5.RankSUE + \beta_6.AggrOwn + \beta_7.RankOP * AggrOwn + \beta_8.RankCMA * AggrOwn + \beta_9.RankME * AggrOwn + \beta_{10}.RankBTM * AggrOwn + \beta_{11}.RankSUE * AggrOwn + Controls + \varepsilon_i \quad (6)$$

where *RankOP*, *RankCMA*, *RankME*, *RankBTM*, and *RankSUE*, are the percentile ranks for operating profit, total asset growth, market value of equity, book-to-market ratio, and standardized unexpected earnings respectively. We construct all of our percentile ranks so that if the anomalies earn returns positive returns, as shown in the prior literature, the coefficients should be positive. Because we require non-missing values for all asset pricing variables, our sample is somewhat smaller than in the main analysis.

[Insert Table 8 around here]

We present our results in Table 8, both without (column (1)) and with firm fixed effects (column (2)). We have two main results. First, strategies commonly thought of as value strategies such as book-to-market, operating profit or low asset growth earn substantially more negative returns than on non-reporting days. This finding is consistent with fund managers having difficulty articulating their value to retail investors. We find an insignificantly positive coefficient on post-earnings announcement drift, which is commonly thought of as an attention-related anomaly.

4.5 Fund disclosures and the supply of liquidity

Our central result is that returns on disclosure days exhibit greater reversals. In this section, we examine how liquidity responds to the increase in uninformed trades. While our return reversal tests suggest liquidity provision does not adjust sufficiently in response to disclosure, these set of tests enable us to assess whether it responds at all during disclosure days. If we observe some liquidity response, this could help explain why funds concentrate their reporting induced trades around the reporting date, because doing so minimizes trading costs (Admati and Pfleiderer 1988).

We first examine the effect of fund reporting (*AggrOwn*) on two proxies for stock illiquidity – the closing bid-ask spread scaled by the stock price and price impact, as measured as in Amihud (2002). Higher levels of both measures reflect a lower level of liquidity. In order to reduce the effect of outliers and facilitate interpretation, we use the daily percentile ranks of both liquidity proxies (*RankDailyBidAsk* or *RankDailyAmihud*).

Specifically, we estimate the following models:

$$RankDailyBidAsk\ (or\ RankDailyAmihud) = \alpha_i + \beta_1 . AggrOwn + controls + \varepsilon_i \quad (7)$$

[Insert Table 9 around here]

We find that liquidity increases around fund disclosures. In columns 1 and 2 of Table 9, we observe that both illiquidity measures – spread and Amihud price impact – are significantly negatively associated with *AggrOwn*. Since both proxies are negatively associated with liquidity, this indicates that liquidity increases around fund reporting. However, the observed shift in liquidity is economically small. Relative to a day where no disclosures occur (i.e., *AggrOwn* = 0), liquidity is about 0.67 to 0.94 percentile ranks higher on an average quarter-end day. Taken together with our earlier results, this analysis suggests liquidity responds to fund reporting, but the response is not sufficient to minimize the noise in returns.

In columns (3) and (4), we examine whether dollar volume and/or absolute returns increase with fund reporting. We percentile rank both variables, to facilitate comparison of coefficients. We find both volume and absolute returns increase. The increases are consistent with fund reporting triggering an increase in trading activity.

4.6 Can investors trade profitably on disclosure day reversals?

We next assess whether an understanding that returns on fund reporting days have less information about intrinsic values and thus subsequently reverse would enable investors to trade profitably, and we benchmark these potential profits against those earned by existing asset pricing anomalies.

To test whether portfolio reporting can be used to construct viable trading strategies, we restrict our sample in a number of ways. First, because trading on quarter end day returns requires knowledge of the closing market price, we construct future returns using monthly returns from the open of the first day of the month until the close on the last trading day of the month. Because open prices are only available on CRSP beginning in 1992, this requirement limits the sample to only years after 1992. Second, we limit the sample to the five hundred largest stocks that meet our sample selection criteria and have data available to construct the asset pricing factors. Imposing these requirements ensures the stocks we trade are highly liquid.

To benchmark the profitability of this possible trading strategy, we construct five asset pricing variables. First, we calculate size, operating profit, total asset growth, and book-to-market value of equity (Fama and French 2016). Second, we include seasonally adjusted unexpected earnings to benchmark our anomaly against post-earnings announcement (Bernard and Thomas

1990). To ensure comparability of the profitability of trading strategies involving each factor, we percentile rank all independent variables, including portfolio reporting day returns, by month. Moreover, we sort the resulting percentile ranks so that all coefficients, other than our variable of interest, would be expected to have a positive association with future returns (i.e. large firms have low values for size, because they would be expected to earn low returns).

[Insert Table 10 around here]

Our findings suggest the existence of a profitable trading strategy around disclosure days. This is shown in Table 10. In column (1), we select only monthly returns for January, April, July and October. These months follow a quarter end day, when most fund disclosures take place (see Table 1). Regressing monthly returns on only the percentile rank of quarter end day returns, we find that disclosure day returns load with a highly significant coefficient of -0.018 (*t*=4.1). Our linear model would thus suggest a firm with returns in the bottom percentile would outperform the top percentile by 1.8% over the month. In column (2), we replace returns on the quarter end date with returns on the quarter end date and the day before, *Ret(t-1, t),* and find a slightly larger coefficient -0.020 (*t*=3.84). In untabulated analyses, we find that taking a long (short) position in securities with the highest (lowest) quintile of returns generates a monthly return of 1.6% (*t*=3.24), a fairly large return in securities of such large size.

The profitability of this trading strategy is robust to controlling for other asset pricing factors. This is shown in Table 10, column (3), where we include controls for the five asset pricing variables discussed above. The coefficient on quarter-end returns remains large and statistically significant after controlling for these additional factors. Moreover, the coefficient for recording day returns is more than twice that of the second largest coefficient (operating profit).

Consistent with fund disclosures driving the significant reversals, we find smaller reversals in months not after quarter end dates. This is shown in Table 10, column (4), where instead of selecting months after quarter end dates, we select months after non-quarter-end dates (i.e. February, March, May, June, August, September, November and December). For months after non-quarter-end dates, we find insignificant reversals that are about one-fourth as large as those reported in columns (2)–(3), when using quarter-end dates.

The profitability of the trading strategy is not limited to just the largest stocks. In column 5, we present results again for quarter-end months, but using all firms instead of just the 500 largest stocks. We find a slightly larger coefficient than columns (2)–(3), which is again larger than all other included asset pricing anomalies, although the difference is smaller.

4.7 Robustness tests

Thus far, our findings illustrate that fund reporting requirements are associated with low-information-content trades and return reversals. However, a number of other events occur with greater frequency around quarter-end days. In this section, we conduct additional tests to show that these factors are unlikely to explain the greater reversals on dates that funds record holdings.

4.7.1 Dividend payments

Firms tend to pay dividends more frequently on quarter-end days than on non-quarter-end days and these dividend payments could plausibly generate reversals through the re-investment of dividends. If this explains the reversals associated with *AggrOwn*, we would expect (i) the interaction of an indicator set equal to one when the firm distributes a dividend and returns to load with a negative coefficient and (ii) substantial attenuation on the interaction of *AggrOwn* and

Ret(t). In Table 11, column 1, we modify model (1) by including both a dividend payment indicator and its interaction with returns and find the interaction does not load. Moreover, we obtain a similar coefficient for *AggrOwn*Ret(t)* as obtained in our earlier estimations, which is inconsistent with dividend payments driving our results.

4.7.2 Paychecks

In a contemporaneous paper, Etula, Rinne, Suominen, and Vaittinen (2017) argue payment cycles caused by the need for pension funds and companies to settle obligations in cash causes the price of debt and equity to temporarily increase and subsequently decline following days of cash settlements. While these authors document a different asset pricing phenomena than we document in this paper, we do not believe the cash settlement explanation also explains the returns phenomena we document in this paper for several reasons.

First, a large number of funds settle obligations around the 15th of the month when no fund disclosures take place. If such payment cycles cause our price distortions, we should observe the noise in returns to increase around this time as well. To test this, we create a mid-month indicator variable that takes a value of 1 for the 14^{th}, 15^{th} and 16^{th} of each month (and zero otherwise). We interact this with the returns to examine whether return reversals take place on these dates. Column 2 from Table 11 shows no distortions taking place around these dates.

Second, we observe return reversals on quarter-end dates are four times larger than return reversals on non-quarter-month-end dates. Such quarterly spikes cannot be explained by cash disbursements, which Etula et al. (2017) document follow a monthly payment cycle.

Third, cash disbursements tend to be made at both the beginning-of-month and end-of-month with similar frequencies. To examine whether beginning of month disbursements cause returns to reverse, in untabulated analyses we create a beginning of month indicator variable that takes a value of one for the first date of month (and zero otherwise) and interact this variable with stock returns on the date of fund disclosure. In untabulated analyses, we do not see any increase in return reversals around month-start dates which suggests that our results cannot be explained by uninformed trades initiated in response to the receipt of cash.

4.7.3 The availability of arbitrage capital

Asset pricing theory suggests that limits to arbitrage allow mispricing. If quarter-end reporting requirements of financial institutions leads to a drawdown in arbitrage capital, this could generate reversals at quarter-end days, but for a different reason than fund reporting.

If this were driving our results, we would expect reversals to be larger when less arbitrage capital is available and larger reversals when arbitrage capital is lower. To examine this possibility, we use the noise measure constructed by Hu, Pan and Wang (2013) as a proxy for market liquidity and test whether return reversals are larger in magnitude during times of less arbitrage capital.[14] Column 3 of Table 11 presents this test. We find no evidence that reversals decrease when overall market liquidity is higher (or vice-versa). In untabulated analyses, we also find an insignificant coefficient on the triple interaction term *Ret(t)*AggrOwn*Noise* when we fully interact the Hu et

[14] The noise measure is constructed by first calculating a daily smooth zero-coupon yield curve. This yield curve is then used to estimate the model yield for all publicly traded bonds on that day. Any deviation from the model yield is taken as 'noise' for that security. The individual noise measures for all bonds are then aggregated by calculating the root mean squared error to obtain the final noise measure. We thank Grace Xing Hu for making this measure publicly available.

al. measure with model (1). Overall, we conclude the availability of arbitrage capital plays at most a second order effect in generating the asset pricing patterns we document.[15]

[Insert Table 11 around here]

5. Conclusion

We find that mandatory disclosure of portfolio holdings by funds is associated with greater distortions in security prices, leading to larger return reversals on dates that funds record their holdings. We find these distortions continue to affect market prices beyond the first trading day following the recording date, adding to the evidence of short duration effects in Carhart et al. (2002). Because prices feedback into asset allocation decisions, we regard this as evidence of negative externalities from disclosure. Our findings on the returns to asset pricing anomalies also suggest that mandatory disclosure of portfolio holdings leads to lower price discovery. In addition, the effects we document are larger in liquid stocks rather than illiquid stocks, consistent with the possibility that liquidity induces funds to execute trades without information about intrinsic values. This is unusual as most asset pricing anomalies decrease with size and liquidity.

Our study makes three important contributions to the literature. First, we add to the literature on the unintended consequences of regulation. Our findings suggest that requiring additional disclosures from mutual funds feeds back into and distorts asset prices. Second, we add to the literature by providing a novel measure of 'window dressing' trades. We argue that funds

[15] A related possibility is that investment banks terminate repurchase agreements (repos) with hedge funds, so as to avoid reporting equity securities on their balance sheet, in advance of quarter-end days on which they close their own books. However, repos for equity securities tend to be quite long in duration (Machiavelli and Pettit 2018). In addition, discussion with representatives of the federal reserve suggest no difference in the magnitude of equity repos carried on the books at quarter-end days compared to non-quarter end days, inconsistent with this explanation.

likely follow a number of distinct strategies to prepare their positions for review by investors and the one common element of such trades is that they do not impound information about fundamentals into price generating reversals. Third, we show that liquidity increases when funds disclose, offering a possible explanation for the concentration of such trades immediately before reporting, as trading at these times consumes less capital.

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Appendix 1 – Examples of Investor Boycotts

	Date and Source	Details
1	24th May 1990 – New York Times	Harvard and City University of New York eliminate stocks of tobacco companies from their investment portfolios. President Derek Bok of Harvard University, in a letter dated 18th May disclosed that Harvard had decided on the divestment in September 1989 and completed the stock sale in March 1990.
2	16th June 2000 – University of Michigan	The University of Michigan Board of Regents voted at its June 15-16 meeting to divest the University of its holdings in tobacco manufacturing companies. Robert Kasdin, U-M executive vice president and chief financial officer, will instruct the University's investment managers to sell all relevant stocks within the next 10 months. **In divesting itself of the tobacco-related investments, the U-M joins several other institutions—Wayne State, Harvard and Northwestern universities—and public pension funds in California, Florida, Maryland, Texas and New York.**
3	18th December 2012 – Wall Street Journal	Within hours of Friday's shooting at Sandy Hook Elementary School in Connecticut, executives at Cerberus Capital Management LP made the call that Cerberus would put Freedom Group up for sale. Freedom Group is one of the nation's biggest makers of guns and ammunition including the Bushmaster rifle that was used in the shooting at the school.
4	12th April 2013 - CalSTRS	In December 2012, following the Sandy Hook Elementary School tragedy in Connecticut, CalSTRS board member and California State Treasurer, Bill Lockyer, issued a call for the fund to divest from companies which manufacture firearms and high-capacity magazines that are illegal for sale to, or possession by, the general public in the state of California. As of December 31, 2012, the total market value of CalSTRS holdings in Sturm Ruger and Smith & Wesson was approximately $3 million, which represented 0.3 basis points of the Global Equity portfolio.

5	23rd May, 2016 – Bloomberg	Axa SA, France’s largest insurer, said it will stop investing in tobacco and divest all of its $2 billion dollars of assets in the industry. Axa didn’t disclose its tobacco investments. According to data compiled by Bloomberg, its holdings include stakes in Philip Morris International Inc., British American Tobacco Plc and Altria Group Inc. “This decision has a cost for us, but the case for divestment is clear: the human cost of tobacco is tragic; its economic cost is huge,” Deputy Chief Executive Officer Thomas Buberl, said in the statement. “It makes no sense for us to continue our investments within the tobacco industry.”
6	15th July, 2016 – Pensions & Investments	New York City Employees’ Retirement System’s board voted to become the first of the city’s five retirement systems to divest its holdings of some gun retailers, said Scott M. Stringer, the fiduciary for the five city pension funds, in an e-mail.
7	22nd August, 2018 – Pensions & Investments	Yale University's board of trustees has adopted a policy prohibiting its $27.2 billion endowment from investing in retail outlets that market and sell assault weapons. The university announced in a statement on Tuesday that the policy was adopted by the board following a recommendation by the board's Committee on Investor Responsibility.
8	22nd February, 2018 - CNBC	New Jersey state lawmakers on Thursday moved to restrict the state's public pensions from investing in the stocks of gun manufacturers. State pensions that own stocks of gun makers, and to a lesser extent, gun retailers, came under criticism after the Feb. 14 shooting, in which 17 people died. After the mass shooting at a concert in Las Vegas last year, California Treasurer John Chiang urged the state's teacher and public employee pensions to sell their holdings of companies that sell assault weapons, ammunition and gun accessories.

Appendix 2 – Variable Definitions

Variable Name	**Variable Description**
Fund Disclosure Variables	
AggrOwn	Ratio of the portfolio position values disclosed on a date to the total value of portfolio positions disclosed during the year. Portfolio disclosures are calculated using Thomson mutual fund holdings (S12) on non-quarter- end days and institutional fund holdings (13f) on quarter-end days. Portfolio positions are split-adjusted and valued at last years' prices. To ease interpretation, we divide this ratio, by the average ratio observed across all quarter-end days in our sample (23.7%), so that the coefficient in estimations can be interpreted as the average change observed in the outcome of interest when disclosure levels equal those on an average quarter-end date.
Tobacco	Percentage of revenue earned from tobacco industry segments (Fama-French 48 industry code = 5), and 0 otherwise
Firearms	Percentage of revenue earned from firearm industry segments (SIC codes: 3482, 3484), and 0 otherwise
School	Indicator variable that takes a value of 1 if a school shooting took place in the three years prior to the disclosure day and 0 otherwise
Return Variables	
Ret(t-2)	Returns two day priors to the day we measure position recording
Ret(t-1)	Returns one day prior to the day we measure position recording
Ret(t)	Returns the day we measure position recording
RankRet(t)	Percentile rank of returns on the day we measure position recording
Ret(t+1, t+30)	Cumulative market-adjusted returns on the security held from day 1 to day 30 following the day we measure position recording
Ret(t+2, t+30)	Cumulative market-adjusted returns on the security held from day 2 to day 30 following the day we measure position recording.

NegRet(t)	Indicator variable that takes a value of 1 if market-adjusted returns on date *t* are negative, and 0 otherwise.
OpenCloseRet	Monthly returns, adjusted so that the returns are cumulated from the open on the first of the month until the close on the last trading day of the month.
Trading Characteristic Variables	
RankMonthlyAmihud	For each firm-month, we take the average of all daily values and then percentile rank all averages in the cross-section. Ranking average values enables our rank to provide a firm-measure of illiquidity unaffected by fund reporting. Amihud illiquidity is calculated as the average of daily (abs(ret)/(dollar volume)) over the month.
RankMonthlyBidAsk	For each firm-month, we take the average of all daily values and then percentile rank all averages in the cross-section. Ranking average values enables our rank to provide a firm-measure of illiquidity unaffected by fund reporting. We compute bid-ask spreads as ask minus bid scaled by price.
RankMonthlyDolVol	For each firm-month, we take the average of daily dollar volume and then percentile rank all averages in the cross-section. Ranking average values enables our rank to provide a firm-measure of illiquidity unaffected by fund reporting.
RankDailyBidAsk	Percentile rank across firms of firm's daily bid-ask spread. Percentile ranks calculated over all firm-dates within a month.
RankDailyAmihud	Percentile rank across firms of firm's daily Amihud liquidity measure. Percentile ranks calculated over all firm-dates within a month.
RankDailyDolVol	Percentile rank across firms of firm's daily dollar volume. Percentile ranks calculated over all firm-dates within a month.
RankDailyABSRet	Percentile rank across firms of firm's absolute returns. Percentile ranks calculated over all firm-dates within a month.
TradeDir(t)	Fund-level direction of trading on date *t*.
TradeDir(t+1,t+7)	Fund-level direction of trading during the week following day *t*.
LogDolTrade(t)	Fund-level direction of trade on date *t* multiplied by the absolute value of the log dollar volume of trade on that date.

LogDolTrade(t+1,t+7)	Fund-level direction of trade during the week following day *t* multiplied by the absolute value of the log dollar volume of trade during that week.
CommDolVol(t)	Commissions earned per dollar volume of trading.
CommPerShare(t)	Commissions earned per share of trading.
DivPay	Indicator variable set equal to one if the firm pays a dividend
Noise	Noise measure constructed by first calculating a daily smooth zero-coupon yield curve. This yield curve is then used to estimate the model yield for all publicly traded bonds on that day. Any deviation from the model yield is taken as 'noise' for that security. The individual noise measures for all bonds are then aggregated by calculating the root mean squared error to obtain the final noise measure. We thank Grace Xing Hu for making this measure publicly available.
MidMonth	Indicator variable set equal to one if date falls on the 14th, 15th or 16th of the month, to correspond with the distribution of paychecks mid-month.
Factor Variables	
RankCMA	Monthly percentile rank of firm's asset growth
RankME	Monthly percentile rank of firm's market value of equity
RankOP	Monthly percentile rank of firm's operating profit
RankBTM	Monthly percentile rank of firm's book to market ratio
RankSUE	Monthly percentile rank of firm's most recent earnings surprise, occurring at least one week before the trading date. We calculate the earnings surprise as the most recent quarter's income before extraordinary items (IBQ) minus IBQ for the same time last year and scaled by the standard deviation of IBQ over the most recent eight quarters.
Date Variables	
Non-QTR Month end	Indicator variable that takes a value of 1 for the last trading day of the month, so long as that day is not also a quarter-end date.
Non-Month end	Indicator variable that takes a value of 1 for non-month end dates.
QTR end	Indicator variable that takes a value of 1 for the last trading date of a quarter (e.g., March 31st if March 31st falls on a weekday, March 29th if March 31st falls on a Sunday and March 30th if March 31st falls on a Saturday).

Table 1 – Descriptive Statistics

Sample	AggrOwn	Rank BidAsk	Rank Amihud	Rank DolVol	Rank ABSRet
QTR End	98.2%	49.1%	48.9%	50.9%	50.9%
Non-QTR Month End	3.6%	49.5%	49.2%	49.8%	49.5%
Non-Month End	0.003%	49.5%	49.5%	49.5%	49.5%

QTR End	AggrOwn
1982-1993	103.0%
1994-2004	95.5%
2005-2015	97.5%

Table 2 – Sin Stocks and Fund Disclosures

	(1) Ret(t-1,t)	(2) Ret(t-1,t)	(3) Ret(t-1,t)	(4) Ret(t-1,t)
Tobacco	0.001*	-0.002**	0.001*	-0.002**
	(1.769)	(-1.996)	(1.764)	(-1.995)
Tobacco*AggrOwn	-0.006***	-0.006***	-0.006***	-0.006***
	(-3.294)	(-3.317)	(-3.294)	(-3.317)
Firearms	0.001	0.005	0.001*	0.005
	(1.333)	(1.154)	(1.748)	(1.224)
Firearms*AggrOwn	-0.005**	-0.005**	-0.003	-0.003
	(-1.995)	(-1.983)	(-1.188)	(-1.178)
School			0.000	0.000
			(0.000)	(0.000)
School*Firearms			-0.001	-0.001
			(-0.755)	(-0.984)
School*Firearms*AggrOwn			-0.006***	-0.006***
			(-3.010)	(-3.034)
Date FE	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	Yes
Asset pricing factors	Yes	Yes	Yes	Yes
Asset pricing factors*AggOwn	Yes	Yes	Yes	Yes
Definition of tobacco and firearms	%Revenue	%Revenue	%Revenue	%Revenue
Clustered S.E	Firm & Month	Firm & Month	Firm & Month	Firm & Month
Observations	21,470,595	21,470,595	21,470,595	21,470,595
Adjusted R-squared	0.021	0.024	0.021	0.024

This table reports regressions of future returns on daily returns, %of revenue derived from firearm and tobacco industries, aggregate fund disclosures, and the interaction of fund disclosures and %revenue. Robust standard errors are clustered by month and firm. T-statistics are reported in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels for two-tailed tests respectively. We exclude all observations before 1985 because segment disclosures are only available from 1985 onwards.

Table 3 – Return reversals and fund disclosures

	(1) Ret(t+1,t+30)	(2) Ret(t+1,t+30)	(3) Ret(t+2,t+30)	(4) Ret(t+1,t+30)	(5) Ret(t+1,t+30)	(6) Ret(t+2,t+30)
Ret(t)	-0.114***	-0.109***	-0.051***	-0.034**	0.002	-0.113***
	(-13.974)	(-12.973)	(-6.731)	(-2.166)	(0.138)	(-12.452)
Ret(t)*AggrOwn		-0.277***	-0.142***	-0.321***	-0.152***	-0.288***
		(-7.811)	(-5.020)	(-6.284)	(-3.907)	(-8.323)
NegRet(t)*Ret(t)				-0.187***	-0.119***	
				(-6.350)	(-4.040)	
NegRet(t)*Ret(t)*AggrOwn				0.057	0.001	
				(0.751)	(0.015)	
Ret(t-1)						-0.064***
						(-6.853)
Ret(t-2)						-0.047***
						(-4.925)
Ret(t-1)*AggrOwn						-0.107*
						(-1.908)
Ret(t-2)*AggrOwn						-0.045
						(-0.874)
Date FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Day of week*Ret(t) FE	No	No	Yes	Yes	Yes	Yes
NegRet(t) & NegRet(t)*AggOwn	No	No	No	No	Yes	Yes
Clustered S.E	Firm & Month	Firm & Month	Firm & Month	Firm & Month	Firm & Month	Firm & Month
Observations	22,931,436	22,931,436	22,931,436	22,931,436	22,931,436	22,931,436
Adjusted R-squared	0.056	0.056	0.054	0.056	0.055	0.056

This table reports regressions of future returns on daily returns (Ret(t)), the % of aggregate fund disclosures made on that day scaled by its sample average on quarter-end days (AggrOwn), and the interaction of disclosures and daily returns. Robust standard errors are clustered by month and firm. t-statistics are reported in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

Figure 1

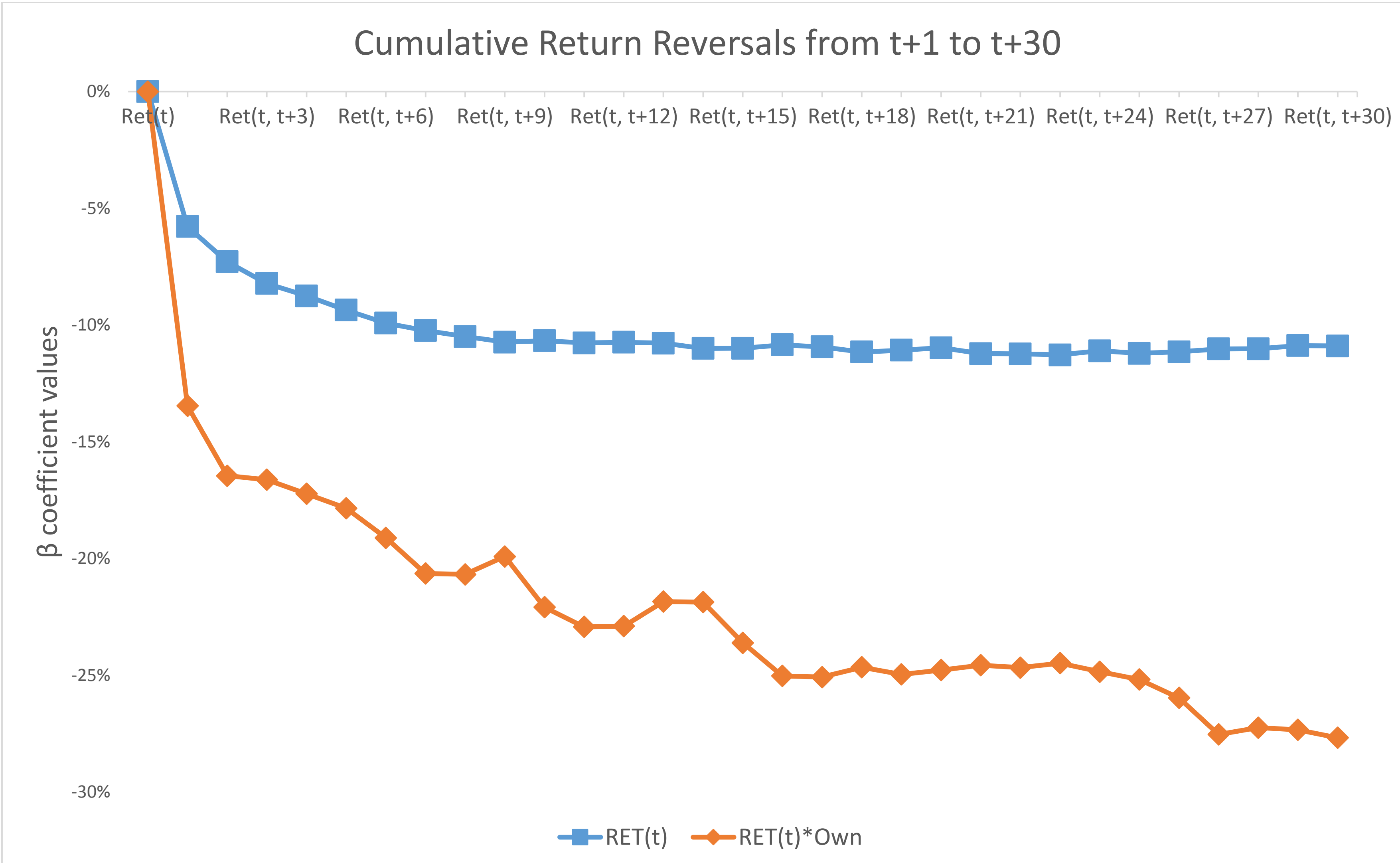
Cumulative Return Reversals from t+1 to t+30
β coefficient values
0%
-5%
-10%
-15%
-20%
-25%
-30%
Ret(t)
Ret(t, t+3)
Ret(t, t+6)
Ret(t, t+9)
Ret(t, t+12)
Ret(t, t+15)
Ret(t, t+18)
Ret(t, t+21)
Ret(t, t+24)
Ret(t, t+27)
Ret(t, t+30)
RET(t)
RET(t)*Own

Table 4 – Return Reversals and Fund Disclosures by Decade

	(1)	(2)	(3)
	Ret (t+1, t+30)	Ret (t+1, t+30)	Ret (t+1, t+30)
Ret(t)	-0.169***	-0.102***	-0.090***
	(-22.907)	(-6.918)	(-8.929)
Ret(t)*AggrOwn	-0.133***	-0.355***	-0.215***
	(-4.961)	(-6.794)	(-2.687)
Year	1982-1993	1994-2004	1995-2015
Date FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Clustered S.E	Firm & Month	Firm & Month	Firm & Month
Observations	6,416,245	9,007,074	7,508,117
Adj. R-squared	0.065	0.071	0.055

This table reports regressions of future returns on daily returns (Ret(t)), the percentage of aggregate fund disclosures made on that day scaled by its sample average on quarter-end days (AggrOwn), and their interaction. Robust standard errors are clustered by month and firm. t-statistics are reported in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively

Table 5 - Return Reversals and Firm Characteristics

	(1) Ret(t+1, t+30)	(2) Ret(t+1, t+30)	(3) Ret(t+1, t+30)	(4) Ret(t+1, t+30)
Ret(t)	0.060***	0.062***	-0.248***	-0.246***
	(3.031)	(3.553)	(-32.202)	(-33.245)
Ret(t)*AggrOwn	-0.430***	-0.407***	-0.193***	-0.212***
	(-4.480)	(-4.440)	(-5.141)	(-5.931)
FirmChar	0.027***	0.054***	-0.042***	-0.051***
	(4.226)	(7.772)	(-9.880)	(-22.070)
Ret(t)*FirmChar	-0.301***	-0.302***	0.282***	0.271***
	(-13.331)	(-15.447)	(17.676)	(7.038)
AggrOwn*FirmChar	0.005	0.004	0.003	0.075***
	(1.078)	(0.953)	(0.681)	(11.637)
Ret(t)*AggrOwn*FirmChar	0.240**	0.223**	-0.147*	-0.243*
	(2.162)	(2.065)	(-1.844)	(-1.902)
Date FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Characteristic	Rank (Monthly Avg. Spread)	Rank (Monthly Avg. Amihud)	Rank (Daily DolVol)	Abnormal Volume
Clustered S.E.	Firm & Month	Firm & Month	Firm & Month	Firm & Month
Observations	18,852,609	22,931,436	22,931,436	22,931,436
Adj. R-Squared	0.060	0.058	0.058	0.058

This table reports regressions of future returns on daily returns (Ret(t)), % of aggregate fund disclosures made on that day scaled by its sample average on quarter-end days (AggrOwn), firm-level characteristics (FirmChar), and the interaction of these variables. The firm-level characteristics used in columns 1-3 are the monthly percentile rank of a firm's bid-ask spread (RankBidAsk), monthly percentile rank of a firm's Amihud illiquidity measure (RankAmihud), percentile rank of a firm's daily dollar trading volume (RankDailyDolVol), respectively. In column (4), we present coefficients computed using RankDailyDolVol and include RankMonthlyVol fully interacted with Ret(t) and AggrOwn to allow our coefficient to capture incremental daily volume unexplained by monthly volume. Robust standard errors are clustered by month and firm. T-statistics are reported in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

Table 6 - Fund Positions and Fund Disclosures

	(1) TradeDir (t+1, t+7)	(2) TradeDir (t+1, t+7)	(3) LogDolTrade (t+1, t+7)	(4) LogDolTrade (t+1, t+7)
TradeDir(t)	0.596***	0.559***		
	(50.102)	(42.064)		
TradeDir(t)*AggrOwn	-0.100***	-0.100***		
	(-4.076)	(-4.306)		
LogDolTrade(t)			0.735***	0.708***
			(73.437)	(65.361)
LogDolTrade(t)*AggrOwn			-0.132***	-0.136***
			(-5.979)	(-6.403)
Date FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Institution FE	No	Yes	No	Yes
Clustered S.E.	Firm & Month	Firm & Month	Firm & Month	Firm & Month
Observations	73,370,053	73,370,053	73,370,053	73,370,053
Adj. R-squared	0.378	0.409	0.427	0.453

This table reports regressions of trade direction for dates t+1 through t+7 onto trade direction on date t (TradeDir(t)), % of aggregate fund disclosures made on that day scaled by its sample average on quarter-end days (AggrOwn), and the interaction of fund disclosures and trade direction on date t. Robust standard errors are clustered by month and firm. t-statistics are reported in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

Table 7 – Fund Positions and Commissions

	(1) CommDolVol(t)	(2) CommPerShare(t)
AggrOwn	-0.00009***	-0.00296***
	(-5.62785)	(-7.20007)
Day of week FE	Yes	Yes
Month-Year FE	Yes	Yes
Clustered S.E.	Month-Year	Month-Year
Observations	3,271	3,271
Adj. R-squared	0.929	0.891

This table reports regressions of commissions paid on the % of aggregate fund disclosures made on that day scaled by its sample average on quarter-end days (AggrOwn). In column 1, the outcome variable is commissions earned per dollar volume of trading, and in column 2, the outcome variable is commissions earned per share of trading. Robust standard errors are clustered by month and firm. t-statistics are reported in parenthesis. *** denotes statistical significance at the 1% level.

Table 8 - Asset pricing anomalies and returns around fund disclosures

	(1)	(2)
	Ret(t-1,t)	Ret(t-1,t)
RankOP	0.000	0.000*
	(0.645)	(1.788)
RankCMA	0.001***	0.001***
	(8.942)	(5.302)
RankME	0.002***	0.010***
	(9.721)	(19.402)
RankBTM	-0.000	0.001***
	(-0.393)	(5.043)
RankSUE	0.001***	0.000***
	(6.255)	(2.901)
RankOP*FirmOwn	-0.003***	-0.003***
	(-4.471)	(-4.535)
RankCMA*FirmOwn	-0.002***	-0.002***
	(-3.253)	(-3.250)
RankME*FirmOwn	0.005***	0.005***
	(4.021)	(4.037)
RankBTM*FirmOwn	-0.004***	-0.004***
	(-4.711)	(-4.737)
RankSUE*FirmOwn	0.000	0.000
	(0.476)	(0.522)
Date FE	Yes	Yes
Firm FE	No	Yes
Clustered S.E.	Firm & Month	Firm & Month
Observations	22,931,436	22,931,436
Adj. R-squared	0.021	0.024

This table reports regressions of two day returns (Ret(t-1,t)) on percentile ranks of asset pricing anomalies, percentage of aggregate fund disclosures made on that day scaled by its sample average on quarter-end days (AggrOwn), and the interaction of fund disclosures and anomalies. Robust standard errors are clustered by month and firm. T-statistics are reported in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

Table 9 – Liquidity Tests

	(1) Rank DailyBidAsk	(2) Rank DailyAmihud	(3) Rank DailyDolVol	(4) Rank DailyABSRet
AggrOwn	-0.585*	-0.861***	1.744***	1.483***
	(-1.865)	(-4.709)	(6.730)	(3.856)
Firm FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Clustered S.E.	Firm & Month	Firm & Month	Firm & Month	Firm & Month
Observations	18,628,881	22,931,436	22,931,436	22,931,436
Adj. R-squared	0.501	0.682	0.764	0.078

This table reports regressions of illiquidity, volume, and daily absolute returns on fund disclosures, as measured using the percentage of aggregate fund disclosures made on a day scaled by its sample average on quarter-end days (AggrOwn). The outcomes analyzed in columns 1-4 are Rank-DailyBidAsk, Rank-DailyAmihud, Rank-DailyDolVol, and Rank-DailyABSRet, respectively. Robust standard errors are clustered by month and firm. T-statistics are reported in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels for two-tailed tests respectively.

Table 10 – Return Predictions

	(1) OpenCloseRet	(2) OpenCloseRet	(3) OpenCloseRet	(4) OpenCloseRet	(5) OpenCloseRet
RankRet(t)	-0.018***				
	(-4.097)				
RankRet(t,t-1)		-0.020***	-0.019***	-0.005	-0.025***
		(-3.848)	(-3.683)	(-1.581)	(-5.815)
RankOP			0.009**	0.011***	0.007
			(2.039)	(3.837)	(1.273)
RankINV			-0.001	0.003	0.013***
			(-0.334)	(1.202)	(3.971)
RankME			-0.006*	0.011***	0.018**
			(-1.969)	(3.445)	(2.054)
RankBTM			0.003	0.008*	0.005
			(0.454)	(1.820)	(0.674)
RankSUE			0.001	0.007***	0.010**
			(0.251)	(3.609)	(2.422)
Date FE	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	Yes	Yes	Yes
Firm selection	500 largest	500 largest	500 largest	500 largest	All
Month selection	QTR end	QTR end	QTR end	Non-QTR Month-end	QTR end
Clustered Standard Errors	Firm & Month	Firm & Month	Firm & Month	Firm & Month	Firm & Month
Observations	49,821	49,821	49,299	98,509	395,384
Adj. R-squared	0.161	0.161	0.186	0.179	0.111

This table reports regressions of future returns on daily returns, %of aggregate fund disclosures made on that day, and the interaction of fund disclosures and daily returns. Robust standard errors are clustered by month and firm. T-statistics are reported in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels for two-tailed tests respectively

Table 11 – Robustness Tests

	(1) Ret (t+1, t+30)	(2) Ret (t+1, t+30)	(3) Ret (t+1, t+30)
Ret(t)	-0.109***	-0.109***	-0.094***
	(-12.934)	(-12.633)	(-7.955)
Ret(t)*AggrOwn	-0.276***	-0.276***	-0.290***
	(-7.805)	(-7.962)	(-7.744)
DivPay	-0.003***		
	(-9.998)		
Ret(t)*DivPay	-0.021		
	(-1.198)		
PayCheck		0.000	
		(0.000)	
Ret(t)*PayCheck		0.005	
		(0.188)	
Noise			0.000
			(0.000)
Ret(t)*Noise			-0.003
			(-1.161)
Date FE	Yes	Yes	Yes
Firm FE	No	Yes	Yes
Clustered S.E	Firm & Month	Firm & Month	Firm & Month
Observations	22,931,436	22,931,436	20,138,514
Adj. R-squared	0.056	0.056	0.057

This table reports regressions of future returns on daily returns (Ret(t)), the % of aggregate fund disclosures made on that day scaled by its sample average on quarter-end days (AggrOwn), and the interaction of disclosures and daily returns. Robust standard errors are clustered by month and firm. T-statistics are reported in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% levels for two-tailed tests respectively