Disclosure costs beyond simple mimicry: Machine learning and reverse-engineering strategies[*]

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Abstract

I study disclosure costs through a new channel: reverse-engineering of strategies, where disclosed trades enable outsiders to infer how fund managers combine characteristics into trading rules. I exploit a rare natural experiment that shuts down simple portfolio mimicry. Following the unexpected disclosure, fund performance declines persistently by 36 bps per month, and outsider front-running increases. I verify the mechanism by showing that simple machine-learning techniques can uncover strategies that significantly predict funds' subsequent trades. To assess the social cost on market efficiency, I develop a model with strategic interaction between a fund manager and an outsider. Contrary to Huddart, Hughes, and Levine (2001), the manager deters reverse-engineering not by randomizing trades but by sluggishly updating strategies. Consequently, increasing disclosure frequency from quarterly to monthly reduces price informativeness by 3.4%, an effect equivalent to a 40% increase in noise trading. Overall, the results highlight that disclosure frequency is central to reverse-engineering risk; delayed disclosure is ineffective at mitigating it.

Keywords: Portfolio Disclosure, Machine Learning, Reverse-engineering, Abel Noser
JEL Codes: G12, G14, G18, G23

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1 Introduction

Disclosure is a cornerstone of modern institutions, shaping accountability, trust, and the efficient allocation of resources. However, advances in data science and machine learning pose a threat that disclosed information enables outsiders to reverse-engineer underlying strategies. This goes beyond simple mimicry of past actions: it targets the deeper decision-making logic and allows forecasts of future actions. For example, clinical trial disclosures, reflecting experimental efficacy and timing of phase transitions, allow competitors to infer R&D pipelines and forecast market entry (Cognition Solutions, 2025). Likewise, routine corporate disclosure, including financial statements, can be analyzed to infer rivals' inventory management strategies for strategic benchmarking (AMMEX, 2018). These cases illustrate a growing tension: disclosure brings benefits in transparency, but modern technology can raise disclosure costs by revealing proprietary strategies.

This tension is especially salient in asset management. On the one hand, funds are increasingly required to disclose their portfolio holdings over time. For example, the U.S. Securities and Exchange Commission (SEC) is planning to increase the disclosure frequency for mutual funds from quarterly to monthly (SEC, Release No. IC-35308).[1] The stated objectives include: (i) protecting investors by reducing information asymmetries; (ii) enabling timelier regulatory oversight; and (iii) improving market efficiency by allowing the market to incorporate holdings information into asset prices.

On the other hand, although portfolio disclosure is intended to report *outcomes* of past trades with a lag,[2] it may allow outsiders to reverse-engineer the underlying trading *rules*—how active fund managers map asset characteristics into trading decisions—and then anticipate managers' current or even subsequent trades. Since trading strategies are a fund's core intellectual property for generating alpha, managing risk, and differentiating products, revealing strategies beyond portfolio holdings can lead to unintended consequences that run counter to regulatory objectives. First, it can harm fund investors by increasing the risk of predation or front-running, thereby reducing fund performance. Second, it can undermine effective oversight if reported holdings are strategically altered by fund managers to prevent strategy leakage. Third, it can weaken market efficiency by reducing incentives to develop new strategies, limiting the incorporation of new private information into prices.

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[1]Initially, the effective date for the amendments was November 17, 2025. The Commission subsequently delayed the effective date to November 17, 2027.

[2]The Investment Company Act of 1940 mandates that individual mutual funds disclose their portfolio holdings quarterly in Forms N-CSR and N-Q with a delay of no longer than 60 days.

To shed light on these unintended consequences, this paper examines how disclosure affects fund performance, trading behavior, and market efficiency through the channel of strategy leakage, where disclosed portfolios (or trades) enable outsiders to reverse-engineer underlying trading strategies. A central empirical challenge here is to distinguish between the unintended effects of strategy leakage and the intended effects of portfolio disclosure. For example, following a regulatory shock that increases the frequency of mandatory disclosure, fund performance can be affected because outsiders simply copy disclosed holdings (i.e., the channel of portfolio mimicking), independently of any reverse-engineering of the manager's trading strategies.

I address this challenge by exploiting an exogenous, non-regulatory release of historical trades. Specifically, Abel Noser, a consulting firm located in New York, unexpectedly sold one-year-lagged, trade-level data for a few hundred U.S. funds in 2002 (Hu, Jo, Wang, and Xie, 2018). Abel Noser had obtained these data when performing trading-cost analyses for those client funds. The one-year lag in data provision is crucial for ruling out portfolio mimicking. Because the one-year-lagged portfolios have already been public and stale by the time due to regulatory portfolio disclosure, which requires a maximum lag of 60 days. Outsiders would not have incentives to copy those old trades. Moreover, even if some copying occurred, most positions would likely have been closed within a year, further limiting any effects due to portfolio mimicking. Therefore, the plausible channel would be that granular disclosure allows outsiders to reverse-engineer clients' trading strategies.

Based on this setting, I first examine how disclosure affects fund performance through strategy leakage. I focus on active equity mutual funds because (i) strategy leakage is most relevant for active managers and (ii) mutual funds are central to policy debates over disclosure regulation. I employ a difference-in-differences (DiD) approach to estimate the impact on client funds' risk-adjusted returns around the year of data release (i.e., 2002). In particular, I construct the treated group using the pre-2002 client funds—those that joined Abel Noser before the event—to ensure the exogeneity of the shock.

A valid DiD estimation requires the control group (non-client funds) to provide a credible counterfactual for the treated group (client funds). To ensure comparability, I use propensity score matching (PSM) to select non-client funds with similar ex-ante characteristics as the control group. The results are robust to alternative control constructions, including varying the matching thresholds and sorting on different sets of pre-specified fund characteristics.

The estimates show that, even with a one-year lag, releasing past trades significantly reduces the performance of client funds by 36 bps per month, indicating a sizable impact

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from strategy leakage. Conceptually, strategy leakage can reduce fund performance in two situations: (i) if clients follow rule-based, unprofitable strategies (e.g., characteristic-based noise trading), leakage enables outsiders to anticipate their trades and earn profits from their price impact, which in turn harms clients; or (ii) if clients employ profitable proprietary strategies, leakage increases competition and erodes clients' alpha. In my setting, the second situation is more plausible: client funds are, on average, large and historically profitable, with an average pre-period alpha of 45 bps per month. My findings align with prior evidence on cross-sectional variation in mutual fund skill (Cremers and Petajisto, 2009; Jiang and Zheng, 2018; Kacperczyk, Sialm, and Zheng, 2005, 2008). Moreover, a fund-level heterogeneity test shows that higher-skill clients experience larger performance declines after the release of trades, suggesting that strategy leakage is particularly consequential for skilled funds.

I conduct four tests to validate the strategy-leakage mechanism. First, I conduct a placebo test focusing on client funds whose trading data was released after the 2002 event year, leveraging the one-year lag between fund inception in Abel Noser and data release. Consistent with the hypothesis of strategy leakage, these "placebo-treated" funds exhibit no significant change in performance around the event year prior to their data release.

Second, I exploit fund heterogeneity using triple-difference analyses. I find that performance declines are larger for client funds whose released trading data (i) spans a longer period or (ii) covers more stocks. These patterns are consistent with the interpretation that large samples—either in the time or cross-sectional dimension—enable outsiders to reverse-engineer trading strategies with greater precision.

Third, I document an increase in front-running by outside predators—equity hedge funds. Following Chen, Da, and Huang (2019), I identify equity hedge funds' long positions from 13F filings and use short interest to proxy for short positions. After the data release, the correlation between equity hedge funds' positions and clients' subsequent trades rises significantly, consistent with intensified front-running by outside predators. For comparison, I also examine hedge funds whose primary focus is not U.S. equities, including "Fixed Income Arbitrage," "Fund of Funds," "Global Macro," and "Emerging Markets." Because these funds are unlikely to engage in arbitrage in the U.S. equity market, I do not find a similar increase in correlation for their equity holdings.

Fourth, I evaluate the feasibility of reverse-engineering trading strategies from an outsider's perspective using a machine-learning method. Conceptually, a trading strategy is a function mapping from asset characteristics to trading decisions (direction and quantity). Uncovering strategies, therefore, is to estimate a function with characteristics

as X and observed trades as Y. I employ a simple yet effective method—regression trees—because their hierarchical structure plausibly reflects managers' step-by-step, criterion-based decision processes.[3] Moreover, regression-tree implementations were readily available at the time of the event (e.g., in R, Java, and SAS during the 2000s), indicating that technology would not impede reverse engineering. I use a real-time training procedure and account for the one-year data lag when uncovering strategies. In out-of-sample tests, the uncovered strategies significantly predict subsequent client-fund trades (t-stat of 4.6 for buys and 5.8 for sells, with stock-by-year fixed effects), confirming the feasibility of uncovering strategies from past trades.

The ML training-testing framework enables a counterfactual analysis of how disclosure frequency affects strategy leakage. For each disclosure frequency (quarterly, bi-monthly, monthly, bi-weekly, weekly), I aggregate actual trades to the target horizon, retrain the ML models to uncover strategies, and evaluate out-of-sample trading predictability. Two implications emerge. First, more frequent disclosure leads to stronger trading predictability, indicating greater strategy leakage. Second, while strategy leakage is limited under quarterly disclosure, it becomes substantially more severe with monthly disclosure: trading predictability roughly quadruples when moving from quarterly to monthly reporting, as measured by standardized regression coefficients.

While the preceding analyses establish the causal impact of disclosure on fund performance and validate the strategy-leakage mechanism, an important question remains: how does strategy leakage affect fund trading behavior and market efficiency in equilibrium? Studying this empirically is challenging for two reasons. First, although treating a subset of funds enables causal inference for performance, it is difficult to assess market-wide effects under counterfactual regimes where all funds face intensified leakage (e.g., with more frequent disclosure). Second, without theoretical guidance, an empirical study of trading behavior lacks a clear organizing framework. To address these challenges, I develop a simple model with strategic interaction, derive a testable implication for trading behavior, and calibrate the model to the data to evaluate the effect on market efficiency.

The model has infinite periods. In each period, an asset pays a dividend that is a linear function of observable characteristics plus noise. A fund manager and an outsider, both endogenously updating trading strategies to maximize expected profits, trade with noise traders each period. A trading strategy is a coefficient vector mapping from characteristics

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[3]For example, Sandy Sanders, manager of John Hancock Fundamental All Cap Core Fund (JFCIX), describes a Seven-Step Stock Picking Process—(1) competitive advantage, (2) good industry, (3) growth drivers, (4) financial statement analysis, (5) management team, (6) valuation analysis, (7) risks—consistent with a tree-like evaluation sequence (Investor's Business Daily).

to trades. There is a time-varying "true" strategy embedded in the dividend process that would be the manager's optimal strategy absent the outsider. While both the manager and the outsider observe characteristics, only the manager knows the true strategy; the outsider attempts to learn it from the manager's past trades.

The core trade-off is between the manager's current vs. future profits: deploying a better trading strategy increases current profits but facilitates the outsider's learning, thereby eroding the manager's future profits. This trade-off implies that the manager may strategically respond in equilibrium.

My model yields a counterintuitive implication for strategic response. Huddart, Hughes, and Levine (2001) demonstrate that when an informed trader must disclose past trades, he optimally adds random noise to trades to deter portfolio mimicking. By analogy, one might expect the manager to add random noise either to trades or to strategies (thus, frequently update strategies) to deter strategy leakage. However, I show that such randomization is never optimal in this case. Instead, the optimal response is sluggish updating.

This is because, unlike portfolio mimicking, strategy leakage arises from learning across multiple trades and periods. Adding random noise increases variance but does not create bias in the outsider's estimates. Sluggish updating, by deliberately sticking to outdated strategies, biases the learner away from the current true strategy. A cost-benefit analysis shows that, for an equivalent reduction in current profits, creating bias via sluggish updating always deters strategy leakage more effectively than increasing variance via randomization. In other words, the variance that needs to be added to prevent learning can be too high to maintain the trade's underlying profitability.

I empirically test this model-generated implication on fund trading behavior. Sluggish updating predicts higher persistence in trading strategies. To measure persistence, I estimate client funds' trading strategies year by year and compute the correlations of trading strategies across adjacent years. Consistent with sluggish updating, strategy persistence rises steadily after the data release: the year-over-year correlation increases from 0.85 in the pre-period to 0.93 by the end of the sample.

Finally, I study market efficiency—measured by price informativeness, the extent to which equilibrium prices reflect fundamentals. Strategy leakage has two opposing effects: it can enhance informativeness by improving the outsider's information, but it can reduce informativeness by inducing the manager to update sluggishly. To assess the net effect, I calibrate the model. The key parameter is the outsider's learning precision, which proxies for disclosure frequency (higher precision corresponds to more frequent disclosure). I

calibrate this precision via a bootstrap procedure using the released trades.

The calibration indicates that the negative force dominates over a wide range. Increasing disclosure frequency from quarterly to monthly reduces price informativeness by 3.4%, an impact equivalent to a 40% increase in noise trading. Thus, more frequent disclosure can backfire, undermining the regulatory goal of improving market efficiency.

Related Literature

First, my study contributes to empirical studies of the effects of trade (or portfolio) disclosure. For example, Agarwal, Mullally, Tang, and Yang (2015) studies the impact of mandatory portfolio disclosure on mutual funds' performance; Shi (2017) extends the study to hedge fund performance; Hagenberg (2025) finds that for U.S. insurers, an increase in the transparency of investment transactions increases portfolio similarity. While these studies emphasize portfolio mimicking as the main mechanism, I identify and quantify a distinct channel—strategy leakage. In contrast to the view that reporting delays effectively mitigate portfolio mimicking, I show that strategy leakage can persist even under a one-year reporting delay.

Second, I contribute to the literature on how the adoption of AI and machine learning technologies shapes the functioning of financial markets (Dou, Goldstein, and Ji, 2025; Dugast and Foucault, 2018, 2025; Farboodi and Veldkamp, 2020). Specifically, Dou, Goldstein, and Ji (2025) examine how AI-powered informed speculators learn trading strategies within a model-free, autonomous reinforcement learning framework when they face sophisticated investors who trade smartly and strategically against them. While their study focuses on AI-powered trading execution strategies based on informative trading signals, I provide complementary empirical evidence that machine learning methods can facilitate the learning of trading strategies from disclosed trades to construct such informative signals. I further show that the frequency of trade disclosures is a key determinant of the precision of this learning process.

Third, my study contributes to the study of information disclosure and strategic trading. Huddart, Hughes, and Levine (2001) show that when an informed trader is required to disclose past trades, she will add a noise component (i.e., randomization) to prevent full revelation of information. Yang and Zhu (2019) derives the condition of such randomization and finds that randomization is not only possible but also likely. Different from these papers studying under the situation of portfolio mimicking, I show that to deter strategy mimicking, the informed trader would follow a completely different

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strategic behavior—never randomizing but sluggishly updating. The intuition is that deterring strategy mimicking requires introducing bias for outsiders to learn, but simply adding noise will not achieve this goal. In terms of the effect on price informativeness, Huddart, Hughes, and Levine (2001) show that the negative effect from randomization does not dominate the positive effect from information. However, in my setting, I show that the negative effect from sluggish updating will dominate, which leads to unintended consequences of increasing disclosure frequency.

More generally, my study contributes to the literature on the role of intermediaries and the implications for asset pricing (Dou, Kogan, and Wu, 2025; Goldman and Slezak, 2003; Haddad, Huebner, and Loualiche, 2025; He and Krishnamurthy, 2013; Kaniel and Kondor, 2013; Koijen and Yogo, 2019). I show that fund managers' strategic responses aimed at deterring strategy leakage can reduce price informativeness as disclosure frequency increases. My result highlights an unintended consequence that runs counter to regulatory objectives of enhancing market transparency.

Lastly, my study is related to the research based on Abel Noser data, e.g., Eisele, Nefedova, Parise, and Peijnenburg (2020); Gormley, Kaplan, and Verma (2022); Jame (2018); Puckett and Yan (2011). Unlike prior papers that study institutional investors' trading behavior based on Abel Noser data, my contribution is to use the data-sales event to identify strategy leakage and examine the impact.

The rest of the paper is organized as follows. Section 2 provides institutional background. Section 3 presents empirical identification of the impact on fund performance. Section 4 sheds light on the mechanism. Section 5 presents a model to study the equilibrium effect of strategy leakage. Section 6 provides additional tests. Section 7 concludes.

2   Institutional Background

The institutional background in this section mainly follows the survey paper, Hu, Jo, Wang, and Xie (2018). Abel Noser Solutions (originally named ANcerno), located in New York, is a firm providing transaction cost analysis to hundreds of institutional clients, including Fidelity, Vanguard, and Putnam. Due to its business, when institutional clients, e.g., mutual funds, execute trades, the trading data can be directly sent to Abel Noser through an Order Delivery System for transaction cost analysis. In 2002, Abel Noser started to sell its clients' historical trading data. The annual subscription fee was $500 and later gradually increased over the years. The timing of the data sales is primarily because a PhD intern working on his doctoral dissertation about institutional trading

cleaned the data, making it ready for sale.

When providing the trading data, Abel Noser replaced fund identities with numeric codes, i.e., *clientcode* and *clientmgrcode*. The original data was anonymous, i.e., no institution identity information was provided. One may consider uncovering identities by comparing cumulative trading to holdings changes from public 13F filings, as in the algorithm of Hu, Ke, and Yu (2009). However, this exercise involves non-trivial work. Abel Noser provides data on a quarterly basis with a time lag of one year.

The data provided by Abel Noser contains fund numeric codes: *clientcode* and *clientmgrcode*. A classification code is included to identify the type of institutional clients: plan sponsors (*clienttypecode* = 1), investment managers (*clienttypecode* = 2), and brokers (*clienttypecode* = 3). Abel Noser data do not contain many broker clients, thus the data on *clienttypecode* = 3 is few. Each client trading execution corresponds to one observation in the Abel Noser data. The variables contain specific information for each transaction, including the *symbol*, *CUSIP*, *side*, *price*, *volume*, and timestamps. *Symbol* and *CUSIP* identify the stock traded. *Side*, *price*, and *volume* specify whether the trade is a buy or sell, the execution price, and the number of shares traded.

During 2011-2012, the Abel Noser data included a *MasterManagerXref* file containing cross-reference identity information for sample institutions. This file was only available during this period and not for data subscribers before or after. With this cross-reference file, data users can match the Abel Noser daily trading data with other databases by fund name. Shortly after the revelation of institutional identity information, in 2012, Abel Noser removed the fund numeric codes from the data. Thus, the Abel Noser data after that is a pooled sample of trades, and users cannot separately identify and track trades for different institutions. In 2017, Abel Noser completely stopped providing data.

3 Empirical Identification

In this section, I exploit the shock—Abel Noser sold its clients' one-year-lagged trade-level data in 2002—to examine the causal impact of disclosure on fund performance through the channel of strategy leakage. The one-year lag for the released trades helps rule out the alternative explanation of portfolio mimicking for two reasons. First, because the one-year lagged portfolios have already been publicly available through regulatory portfolio disclosures, we expect data buyers not to have an incentive to follow stale information. Second, despite following, the impact on clients through portfolio mimicking would be limited, because over the one year, it is likely that clients have already closed their

positions. Taken together, the exogenous release of one-year-lagged trades facilitates the identification of strategy leakage.

3.1   Data and sample

I obtain data from several sources. Mutual fund returns are from CRSP, and fund holdings are from the Thomson Reuters S12 database. I link the two databases using the MFLINKS database. Following standard practice in the mutual fund literature, I drop funds with last-quarter-end TNA below $5 million. To focus the analysis on U.S. domestic equity mutual funds, I exclude international funds (IOC = 1), municipal bond funds (IOC = 5), bond & preferred funds (IOC = 6), and metals funds (IOC = 8) based on the investment objective code (IOC). To focus the analysis on active funds, I further exclude index funds based CRSP index flag (i.e., *crsp_index_flag* = 'B', 'D', or 'E') as well as a name search (i.e., fund names include 'INDEX', 'INDX', 'IDX', or 'S&P'). To obtain gross (before-expense) fund returns, I add the monthly expense ratio (variable *exp_ratio* divided by 12) back to the monthly fund returns reported in CRSP.

I obtain institutional detailed trades from the Abel Noser data, which spans the period from 1998 to 2011 and includes 1,039 unique institutions.[4] I am able to access the cross-reference identity information provided by Abel Noser, which is only available during 2011-2012. Based on the name information, I match Abel Noser clients to funds in Thomson Reuters/CRSP by jointly considering (i) fund names and (ii) the similarity of trading behavior. The details of the matching procedure are described in Appendix A. It finally yields 151 matched mutual funds. Table 1 Panel A presents the number of matched funds by the beginning years of Abel Noser clients. I conduct most of the analyses in this paper with a period ending in 2007, to avoid contamination due to the 2008-2009 global financial crisis.

I construct stock characteristics using stock returns, prices, and trading volumes from CRSP; accounting information and short interest from Compustat; analyst recommendations and forecasts from IBES; and institutional ownership from Thomson Reuters. Construction of characteristics follows the procedure in Green, Hand, and Zhang (2017) and Hou, Xue, and Zhang (2020). The list of stock characteristics is in Appendix B.

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[4]One institution (identifier: *clientcode*) may include multiple funds (identifier: *clientmgrcode*).

3.2 Identification strategy

I employ difference-in-differences (DiD) regressions to estimate the impact on client funds' return after Abel Noser sold their trading data in 2002. I use the funds that became Abel Noser's clients before 2002 as the treated group. Because these funds would not expect Abel Noser to sell their trading data in 2002, such an empirical design helps achieve the exogeneity of the shock. The control group is the non-client funds. The regression specification is given by

$$\Delta R_t = \beta_0 + \beta_1 \times \mathbb{I}_{\{\text{year} \geq 2002\}} + \epsilon_t, \tag{1}$$

where ΔR_t is the average treated-minus-control (risk-adjusted) fund returns in month t, and $\mathbb{I}_{\{\text{year} \geq 2002\}}$ is a zero-one indicator of the post-period. β_1 captures the treatment effect on fund performance due to trading data release.

A potential concern of the above estimation method is the self-selection issue. Because funds endogenously choose whether to become Abel Noser's clients, it raises a concern that the client funds might systematically differ from the control group. To deal with the concern, I construct the control group by conducting propensity score matching (PSM) to pick funds with characteristics similar to the clients. Table 1 Panel B reports the balance test on fund characteristics.

For PSM, I estimate the propensity scores for each fund by cross-sectionally regressing a zero-one indicator of client funds on pre-period fund characteristics (including TNA, age, fund α, fund holding scores, volatility, turnover ratio, and expense ratio) via logistic regression. I use k-nearest neighbors matching by matching each client fund to k (e.g., $k = 20$) non-client funds with the closest propensity scores in each month. Then, I calculate the performance difference between each client fund and the average of its matched non-client funds. The dependent variable in Eq.(1), ΔR_t, is the average of the performance differences across all client funds in month t.

Alternatively, I construct the control group via sorting as a robustness check. I first cross-sectionally sort funds on characteristics (e.g., double sort on TNA and past α), and then match each client fund to the non-client funds in the same sorting group. Once the control group is constructed, I calculate ΔR_t as mentioned before.

3.3 Disclosure effect on fund performance

A. Baseline results

Table 2 Panel A shows the estimation returns of Eq.(1) with PSM and $k = 20$ nearest neighbors. After Abel Noser sold its clients' trading data, despite the one-year lag, the client funds' risk-adjusted returns significantly dropped by around 36 bps per month compared to similar non-client funds. The magnitudes of the performance drop are similar across different risk-adjustment models: 38.5 bps (t-stat=2.07) for excess return, 40.8 bps (t-stat=2.52) for CAPM α, 30.5 bps (t-stat=3.25) for Fama and French (1993) three-factor (FF3) α, and 33.8 bps (t-stat=4.19) for Carhart (1997) four-factor (CH4) α.

The results are robust for alternative ways of constructing the control group. As the results in Table 3 show, varying the number of neighbors in PSM to 10 or 50, as well as picking the control group by sorting on different characteristics, all generate similar and significant treatment effects.

To examine the timing of impact on fund performance, I separately estimate the treatment effects for each year using the following regression:

$$\Delta R_t = \sum_{l=-4}^{5} \beta_l \times \mathbb{I}_{\{\text{year } l\}} + \epsilon_t, \tag{2}$$

where $l = 0$ denotes the event year, i.e., 2002. Figure 1 presents the estimated time series of β_l for CH4-adjusted returns. As a benchmark, I set the coefficient in 2001, i.e., one year before the event, as zero. There are several key patterns in the figure. First, all coefficients in the pre-period are insignificant and close to zero, which supports that the parallel trends assumption tends to hold. Second, precisely from the year Abel Noser started providing data, the performance of client funds began to drop, which is consistent with the causal interpretation. Finally, the performance decrease persists until the end of the sample period.

B. Placebo test

I conduct a placebo test to further address the concern about the selection on the unobservables. For example, funds favoring Abel Noser may share some common unobservable features, which could be related to their performance drop in 2002. To rule out this explanation, I construct a placebo-treated group by (i) using all of Abel Noser's clients (including those who joined in the post-period) and (ii) focusing on the returns before

their trading data was released.[5] For example, suppose there is a fund that joined Abel Noser in January 2003. Given the one-year-lag rule of providing data, the trading data of this fund would not be released until at least January 2004. In this case, I drop the observations after January 2004 for this fund. Therefore, if the previously estimated performance drop indeed comes from the trading data release, instead of the alternative explanation, we would not see any effect for this placebo test, as only the pre-date-release fund returns are included.

Table 2 Panel B shows the results of the placebo test. As expected, the performance changes around 2002 for the clients before their data release are close to zero and statistically insignificant. Figure 2 plots the time series of effects for these placebo-treated client funds. Again, all coefficients in both pre- and post-periods are statistically insignificant under 95% confidence intervals. These results help rule out the alternative explanation and suggest that trading data release is likely to be the reason for the performance drop.

C. Heterogeneity

To support the trading strategy leakage mechanism, I exploit fund-level variation. Specifically, if trading strategy leakage is the true mechanism, we would expect client funds to experience a larger performance drop (i) if their released trading data facilitates outside investors to learn their strategies better, or (ii) if outside investors have stronger incentives to learn their strategies. Therefore, I hypothesize that the client funds' performance will decline more if (i) their trading data spans a longer period, (ii) contains trades on more stocks, or (iii) client funds have better skills.

I test the hypotheses by conducting a triple-differences analysis. The regression specification is

$$\Delta R_{i,t} = \beta_0 \times \mathbb{I}_{\{\text{year} \geq 2002\}} + \beta_1 \times \mathbb{I}_{\{\text{year} \geq 2002\}} \times \mathbb{I}_{\{\text{fund category}\}} + \text{FE} + \epsilon_{i,t}, \tag{3}$$

where $\Delta R_{i,t}$ is client fund i's risk-adjusted fund returns in month t minus the average from the matched non-client funds, and β_1 is the coefficient we focus on. I construct the measures of $\mathbb{I}_{\{\text{fund category}\}}$ only using pre-period information to alleviate concern about endogeneity issues.

I define three zero-one indicators to reflect fund categories. First, an indicator of

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[5]To obtain a valid DiD estimation, I require funds in the sample to have at least 12 observations in the post-period.

"longer period" ($\mathbb{I}_{\{\text{longer period}\}}$) equals one if, until the end of 2001, the period of being Abel Noser's client is longer than the sample median. Second, an indicator of "more stocks" ($\mathbb{I}_{\{\text{more stocks}\}}$) equals one if, until the end of 2001, the number of stocks traded by a client fund is more than the sample median. Finally, an indicator of "better skill" ($\mathbb{I}_{\{\text{better skill}\}}$) equals one if the fund CH4 alpha in the pre-period is higher than the sample median.

Table 4 reports the triple-differences analysis for the three tests. Consistent with my hypotheses, first, clients whose trading data spans a longer period or contains trades on more stocks suffer larger performance drops after data release, as a large sample (either in time or stock dimension) facilitates outside predators to uncover trading strategies with better precision. Second, clients with better skills experience larger performance drops after data release, as outside predators have stronger incentives to uncover more profitable strategies. Overall, the variation tests suggest that the documented effects come from trading strategy leakage.

D. Trading returns

Besides using fund returns, I conduct a test using trading returns. The advantage of this test is that the sample size is larger, because there is no need to match Abel Noser data to CRSP to obtain fund returns. As in previous tests, I focus on active funds, thus I select clients based on Abel Noser's classification code, i.e., investment managers (*clienttypecode* = 2), here.[6] Similar to the baseline test, here I also focus on the clients who joined Abel Noser before 2002. After the above sample selection, the sample contains 1,015 funds (or accounts), and its total trading volume is about 32 times the volume in the baseline matched sample. The sample size is much larger because it not only contains unmatched mutual funds in the baseline sample, but also contains other types of funds, e.g., hedge funds.

I calculate trading returns by tracking stock returns after each trade in the following several days and weighing them by the dollar trading volume. Specifically, for each fund-month, I calculate the dollar-volume-weighted risk-adjusted return as

$$R^{trade} = \frac{\sum_j Buy_j \times R^k_{[d+1,d+h]}}{\sum_j Buy_j} - \frac{\sum_j Sell_j \times R^k_{[d+1,d+h]}}{\sum_j Sell_j}, \tag{4}$$

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[6]Abel Noser classifies clients into three types: plan sponsors (*clienttypecode* = 1), investment managers (*clienttypecode* = 2), and brokers (*clienttypecode* = 3). Plan sponsors are usually passive pension funds. Investment managers include both mutual funds and hedge funds. In the Abel-Noser/CRSP matched sample, all funds' *clienttypecode* are 2.

where Buy_j or $Sell_j$ is dollar trading volume for trade j, and $R^k_{[d+1,d+h]}$ is stock k's cumulative risk-adjusted return over day $[d+1, d+h]$. Then, I regress the client funds' average trading return on the post-period indicator to estimate the change in performance.

Table 5 reports the results of the impact on clients' trading returns.[7] I set the horizon (h) to be 5, 10, or 20 trading days. Again, there are significant drops in trading returns across different specifications. Figure 3 plots the time series of the impact, which is consistent with the dynamics in baseline results. Taken together, I provide causal evidence that Abel Noser's selling trading data adversely affects its clients' performance. Despite Abel Noser providing data with a one-year lag, the negative impact on fund returns is non-negligible and persistent.

3.4 Evidence on increase in outsiders' predatory trading

I provide evidence on the increase in outsiders' predatory trading due to the release of clients' trading data. As institutionalized arbitrageurs in the financial market, hedge funds might be thought of as potential predators (Chen, Hanson, Hong, and Stein, 2008). Predatory trading, also known as "front-running", implies hedge funds may already hold (short) a stock before client funds buy (sell) the same stock (Brunnermeier and Pedersen, 2005). Given that the release of data facilitates hedge funds to learn more about Abel Noser's clients, we would expect an immediate increase in hedge funds' predatory trading activities against client funds right after the trading data release.

To examine how predatory trading activities evolved around 2002, I conduct the following regression and separately estimate for long and short sides:

$$DTV_{k,t+1} = \beta \times POS_{k,t} + \sum_{l \in \{1999,2000,2002,2003\}} \delta_l \times POS_{k,t} \times \mathbb{I}_{\{year\ l\}} + \mu_{k,y} + \epsilon_{k,t+1}. \quad (5)$$

$DTV_{k,t+1}$ is the client funds' aggregate buy (for long-side estimation) or sell (for short-side estimation) dollar volume on stock k in month $t+1$. To be consistent with the treated group in the fund performance test, I focus on the client funds that joined Abel Noser before 2002.

For long-side estimation, $POS_{k,t}$ is aggregate hedge fund holdings ($HFHD$) based on the hedge fund list from Chen, Da, and Huang (2019).[8] As hedge funds report holdings on a quarterly basis, I include only the months of the quarter ends in the regression, i.e., t

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[7]The sample here starts from 1999 because the data in 1998 are sparse, which makes the calculated trading returns noisy.

[8]I remove the hedge funds that are also Abel Noser's clients when constructing $HFHD$.

is the month of March, June, September, or December. For short-side estimation, I follow the literature to use monthly short interest ($SINT$) as a proxy for hedge funds' short positions (Chen, Hanson, Hong, and Stein, 2008; Chen, Da, and Huang, 2019; Jiao, Massa, and Zhang, 2016).

To control for slow-moving stock characteristics, e.g., stock size, I add stock-by-year fixed effects, $\mu_{k,y}$, to the regression. Coefficient β captures the *level* of predatory trading activities in 2001, which is the benchmark, and δ_l captures the *change* of predatory trading activities in year l relative to the benchmark.

Table 6 shows the estimation results of Eq.(5) for long and short sides, and Figure 4 visualizes the dynamics. There are several key observations. First, β coefficients are significant: $\beta = 0.299$ (*t*-stat=4.69) for the long side and $\beta = 0.106$ (*t*-stat=1.84) for the short side, reflecting hedge funds tend to trade before the client funds even before Abel Noser selling data. This result is consistent with the notion that hedge funds, on average, are smart arbitragers. Second, δ_l coefficients in the pre-period (1999 and 2000) are small and insignificant, which suggests no substantial change in predatory trading activities before the data release. This result supports the parallel trend assumption. More importantly, consistent with the hypothesis, there are significant surges in predatory trading activities for both long and short sides immediately after Abel Noser's selling data in the post-period (2002 and 2003). The hedge funds' predatory trading intensity almost double (triple) in 2002 relative to 2001: $\delta_{2002} = 0.252$ (*t*-stat=2.27) for the long side and $\delta_{2002} = 0.216$ (*t*-stat=2.28) for the short side.

Taken together, the evidence suggests that the release of trading data, even with a one-year lag, facilitates outsiders, such as hedge funds, to uncover strategies and conduct predatory trading, which in turn hurts client funds' performance.

4   Uncovering Trading Strategies via Machine Learning

In this section, I further evaluate the strategy leakage mechanism by testing whether trading strategies can be uncovered from the released trades. I first present an illustrative example of uncovering a specific trading strategy, and then extend to a general approach using a machine-learning (ML) method. I also conduct counterfactual analyses to examine how disclosure frequency influences the extent of strategy leakage.

4.1 An example of uncovering the Golden Cross trading strategy

I begin with an illustrative example of uncovering a specific trading strategy: Golden (Death) Cross. The Golden (Death) Cross is a momentum-type trading strategy that buys (sells) assets when a relatively short-term moving average line crosses above (below) a long-term moving average line. Before exploring the trading data, we do not know (i) *whether* client funds use such a strategy, and (ii) if so, *what horizons* of moving averages they use. Therefore, the goal of the strategy-uncovering exercise is not only qualitative (e.g., types of strategies), but also quantitative (e.g., parameters of strategies).

The procedure of uncovering strategies is as follows: First, I construct multiple pairs of short-term vs. long-term moving-average prices, for example, all binary combinations from horizons of 5 days, 10 days, 15 days, 20 days, and so on. Then, I examine at which horizon pair clients' trades exhibit the strongest discontinuity pattern around the cross-point. Specifically, if clients tend to discontinuously buy (sell) more when the x-day moving-average price merely crosses above (below) the y-day moving-average price, it would suggest that clients are likely following the short-term-x-and-long-term-y Golden (Death) Cross strategy. On the contrary, if under any pair of horizon combinations, there is no discontinuity pattern, it would suggest that clients are not following the Golden (Death) Cross strategy.

The results are displayed in Figure 5. In this example, I focus on the strategy for the aggregate client funds. To visualize the discontinuity, I plot the net trading volume (on the y-axis) against the ratio of the short-term moving average to the long-term moving average prices (on the x-axis). A ratio larger (smaller) than one indicates the short-term moving average line is above (below) the long-term moving average.[9] In the left panel, there is a clear discontinuous trading pattern around the point where MA10 crosses MA20. In contrast, in the right panel, we do not see such discontinuity when changing the horizons to MA15 and MA30. Taken together, these suggest that client funds are likely using MA10 and MA20 to conduct the Golden (Death) Cross strategy, instead of using MA15 and MA30.

4.2 General approach of uncovering trading strategies

Motivated by the previous example, I consider a general approach to uncovering trading strategies from the released trades. Conceptually, a trading strategy is a function mapping

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[9]Results are similar if I strictly consider the case of cross above or below, i.e., the ratio *changes* from smaller than one to greater than one or vice versa.

from asset characteristics to trading decisions (i.e., trading directions and quantities). And uncovering trading strategies is a process to reverse engineer: (i) the characteristics used, e.g., MA10 and MA20 for the Golden Cross strategy; (ii) the combination rule, e.g., whether MA10 crosses above or below MA20; and (iii) the trading decisions assigned, e.g., buy if crosses above. Therefore, a natural approach is to utilize machine-learning techniques to efficiently select/combine a large number of characteristics to search for the underlying trading strategies.

Among various machine learning methods (e.g., Lasso, clustering, trees, neural networks), regression trees are a suitable approach for uncovering trading strategies. This is because, first, regression trees are a supervised-learning method that can efficiently explore the underlying relationship between stock characteristics and observed trades. Second, regression trees can easily incorporate non-linear and interactive relations, which allows for the uncovering of some complicated strategies. Additionally, the structure of regression trees is simple and intuitive—similar to multidimensional sorting, which provides an advantage in terms of transparency and interpretability compared to methods like neural networks. Finally, regression trees, introduced by Quinlan (1986), were implementable at the time of the release of trading data—there were existing tree-algorithm packages (such as in R, Java, and SAS) in 2002. This suggests that technology would not be a constraint for implementing my approach to uncover strategies at that time. My results would also serve as a lower-bound estimation, given more recent technological developments such as transformers and large language models (e.g., ChatGPT).

I construct samples for training trees as stock-week panels: for the observation of stock k and week w, the dependent variable is the aggregate client funds' buy (or sell) dollar volume on that stock from week $w + 1$ to $w + 4$ scaled by the stock's past dollar volume. I focus on aggregate client funds because predatory trading requires price impact from the targets. I cumulate the client funds' trading volume over four weeks to reduce noise. Scaling by the stock's trading volume is to control for stock size and liquidity. I transform the dependent variable into a cross-sectional percentage ranking when training the regression trees. For predictors, I construct 61 stock characteristics, including past returns, liquidity measures, betas, fundamentals, industry classification, analyst forecasts, etc. The list of predictors is in Appendix B. As a single tree can be a "weak learner", I follow the literature (e.g. Gu, Kelly, and Xiu, 2020) and employ an ensemble method of Gradient Boosted Regression Trees (GBRT) to combine outcomes from many trees.

4.3 Trading predictability

I follow a real-time training-testing procedure and update regression trees on a quarterly basis. At the end of quarter q, given that Abel Noser's data is provided with a one-year lag, I construct the training sample with the one-year gap spanning three years, i.e., from quarter $q - 15$ to $q - 4$. After training, for the weeks in quarter $q + 1$, I input the most recent stock characteristics to the trees to predict client funds' following trades. Based on the outputs from regression trees, I define $BuyScore_{k,w}$ and $SellScore_{k,w}$ as the prediction scores for client funds' next-week buy and sell trades, respectively, on stock k at the end of week w.

To evaluate the uncovered trading strategies, I test the out-of-sample trading predictability, that is, whether the uncovered trading strategies can predict the subsequent trades of client funds. Specifically, I estimate the following regression:

$$NetDTV_{k,w+1} = \beta_1 \times BuyScore_{k,w} + \beta_2 \times SellScore_{k,w} + \text{FE} + \epsilon_{k,w+1}, \tag{6}$$

where $NetDTV_{k,w+1}$ is the aggregate client funds' net dollar volume on stock k in week $w + 1$ scaled by the stock's past dollar volume, $BuyScore_{k,w}$ and $SellScore_{k,w}$ are outputs from trees based on the stock characteristics in week w. For better interpretability on coefficients, I transform $NetDTV_{k,w+1}$ to cross-sectional percentage rank (ranging from 0 to 100), and scale $BuyScore_{k,w}$ and $SellScore_{k,w}$ by their full-sample standard deviations.

Table 7 reports the estimation results of Eq.(6). The testing period is from 2002 to 2007. As it shows, across the specifications with different fixed effects, *BuyScore* (*SellScore*) can always positively (negatively) predict, with strong significance, client funds' net trades in the following week. In column (3), I add the stock-by-year fixed effects, which control for the slow-moving stock characteristics. The coefficients under this specification imply that one-std. increase in *BuyScore* (*SellScore*) corresponds to 1.2 (2.1) pp., with t-stat of 4.60 (5.82), increase (decrease) in the cross-sectional ranking of net trading next week. This suggests the extracted trading strategies can capture relatively high-frequency trading dynamics within a stock-year.

In sum, using a simple machine-learning method—similar to multidimensional sorting and only requiring a little computational power—I show that it is feasible to uncover trading strategies with significant out-of-sample trading predictability.

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4.4 Predatory portfolios

I next examine the profits an outsider could earn by predating client funds using the uncovered strategies. Broadly speaking, predatory trading takes two forms depending on the informativeness of client funds' trades (Chen, Hanson, Hong, and Stein, 2008): First, if client trades are non-informative, the predatory trading involves "liquidity provision"—trading in the *opposite* directions to gain from client funds' price impact. Second, if client trades are informative, the predatory trading involves "front-running"—trading in the *same* direction to gain from the information in those trades. In the pre-period, client funds earned positive alphas, i.e., 45 bps per month, indicating that their trades were informative. Consequently, a profit-seeking outsider would be inclined to trade in the same direction as the uncovered strategies, i.e., to front-run rather than provide liquidity.

I evaluate the profitability of front-running client funds by constructing a long-short "predatory" portfolio based on the uncovered strategies. At the end of each week, I sequentially sort stocks on *BuyScore* and *SellScore* into 5×5 portfolios, and go long (short) stocks in the group of high-*BuyScore*-low-*SellScore* (low-*BuyScore*-high-*SellScore*), i.e., to long (short) the stocks that client funds are predicted to buy (sell). *BuyScore* and *SellScore* are generated from the uncovered trading strategies inputted with recent stock characteristics as described before. To ensure the results are not driven by small stocks, I only keep large stocks with market equity above the 20th-percentile NYSE breakpoint (Hou, Xue, and Zhang, 2020).

I regress the long-short portfolio return on common factors over the testing period (i.e., 2002-2007) and report the average weekly abnormal returns in Table 8. As the results show, the predatory strategy based on the uncovered trading strategies earns significant alpha that cannot be explained by CAPM, Fama and French (1993) three-factor model, and Carhart (1997) four-factor model. The results are robust for either equally or value-weighted. More importantly, the magnitude of the profitability is large and matches the magnitude of client funds' loss. For example, the CH4 alpha of the predatory strategy with equally weighted stocks in the portfolio achieves $21.2 \times 4 = 84.8$ bps per month (*t*-stat=3.27). As in the previous tests of trading predictability, here I also include the one-year lag of data feeding when training ML models.

Overall, the results suggest that it is not only feasible to backout trading strategies to predict client funds' trades, but also highly profitable to conduct predatory trading based on the uncovered trading strategies.

4.5 Disclosure frequency and strategy leakage

The ML training-testing framework enables a counterfactual analysis of how disclosure frequency affects the extent of trading strategy leakage. This is an important question with implications for optimal disclosure policy. To answer the question, I re-examine trading predictability and the predatory-portfolio alphas at each alternative disclosure frequency. Specifically, for each frequency (quarterly, bi-monthly, monthly, bi-weekly, or weekly), I aggregate the actual trades to the target frequency, retrain the ML models to uncover strategies, and evaluate out-of-sample predictability and alphas of predatory portfolios using the same testing procedure.

Figure 6 reports trading predictability—measured as the coefficient difference between *BuyScore* and *SellScore* as in Table 7 column 3—and the CH4 alphas of predatory portfolios. Consistent with intuition, higher disclosure frequency implies more serious strategy leakage. Two reasons drive this pattern: (i) more frequent disclosure yields more observations for uncovering strategies, and (ii) finer granularity reveals trades that would be unobserved at lower frequency (e.g., round trips).

An interesting finding is that the *monthly* frequency emerges as a crucial threshold: both trading predictability and predatory-portfolio alphas shift from statistically insignificant to significant at monthly disclosure frequency. This implies that increasing disclosure frequency from quarterly to monthly—as in the SEC's new rule, Release No. IC-35308, effective November 2027—can materially intensify strategy leakage, making it a key consideration for policy design.

4.6 Returns of client funds vs. Returns of predatory portfolios

I provide evidence that the losses of the client funds are consistent with the gains from the predatory portfolios. I calculate the correlation coefficient by regressing the client funds' returns (i.e., the treated-minus-control risk-adjusted fund returns, ΔR_t, in Eq.(1)) on the returns of the predatory long-short portfolio (as described in Section 4.4). Because predatory trading is to exploit client funds, we expect a negative correlation between these two.

Table 9 reports the correlation coefficients. To obtain correlation coefficients from the regression, I scale both the client funds' returns and long-short portfolio returns to have the same standard deviation, e.g., the std. of the market portfolio. Consistent with the hypothesis, returns of client funds and returns of the predatory long-short portfolio exhibit a significantly negative correlation. The average correlation across

different specifications is -0.33. The negative correlation suggests that the client funds'
performance drop is indeed through the trading strategy leakage channel.

5   A Model with Strategic Interaction

My previous analyses suggest that past released trades can lead to trading strategy
leakage, which has a negative impact on active funds. This section further addresses two
questions: how fund managers strategically respond in equilibrium, and what are the
market-wide implications of strategy leakage? I develop a model incorporating strategic
interaction between fund managers, who seek to deter strategy leakage, and outside
learners.

5.1   The model

A. Environment

The economy I analyze has infinite periods. In each period, an asset generates a payoff

$$d_{t+1} = \boldsymbol{\theta}_t' \boldsymbol{z}_t + \epsilon_{t+1}, \tag{7}$$

where $\boldsymbol{z}_t$ is a K-dim vector of asset characteristics, $\boldsymbol{\theta}_t$ is a vector of loadings on character-
istics, and ϵ_{t+1} is an unpredictable component. I assume $\boldsymbol{z}_t \overset{iid.}{\sim} \mathcal{N}(\boldsymbol{0}, \boldsymbol{I}_K)$, where $\boldsymbol{I}_K$ is an
identity matrix. The vector of loadings, $\boldsymbol{\theta}_t$, evolves in an AR1 process:

$$\boldsymbol{\theta}_t = \rho \boldsymbol{\theta}_{t-1} + \sqrt{1 - \rho^2} \boldsymbol{u}_t. \tag{8}$$

Without loss of generality, I assume $\boldsymbol{u}_t \overset{iid.}{\sim} \mathcal{N}(\boldsymbol{0}, K^{-1} \boldsymbol{I}_K)$ such that $\mathbb{E}(\boldsymbol{\theta}_t' \boldsymbol{\theta}_t) = \mathbb{E}(\boldsymbol{u}_t' \boldsymbol{u}_t) = 1$.
The random variables $\boldsymbol{u}_t$, $\boldsymbol{z}_t$, and ϵ_{t+1} are mutually independent to each other.

B. Players

There are three players trading assets in the economy: strategy developer, outside learner,
and noise trader. In each period t, both the developer and the learner can observe
characteristics $\boldsymbol{z}_t$, but only the developer knows $\boldsymbol{\theta}_t$. The learner tries to learn $\boldsymbol{\theta}_t$ from the
developer's past trades. The demand functions for the developer (D_t^d) and the learner

(D_t^l) are given by

$$D_t^d = \frac{\boldsymbol{\beta}_t' \mathbf{z}_t - p_t}{\lambda}, \quad D_t^l = \frac{\boldsymbol{\gamma}_t' \mathbf{z}_t - p_t}{\lambda}, \tag{9}$$

where $\boldsymbol{\beta}_t$ and $\boldsymbol{\gamma}_t$ are trading strategies that are optimally chosen by the developer and the learner, p_t is the asset price determined by market clearing, and λ is a constant parameter.[10] Because I normalize $\mathbb{E}(\boldsymbol{\theta}_t' \boldsymbol{\theta}_t) = 1$ in Eq.(8), which implies $\lim_{K \to \infty} \boldsymbol{\theta}_t' \boldsymbol{\theta}_t = 1$, I then assume both the developer and the learner choose strategies $\boldsymbol{\beta}_t$ and $\boldsymbol{\gamma}_t$ from the space $\{\boldsymbol{\theta} : \boldsymbol{\theta}' \boldsymbol{\theta} = 1\}$, i.e., with the same scale to the fundamental loadings. The noise trader's demand is given by $\xi_t \overset{iid.}{\sim} \mathcal{N}(0, \sigma_\xi^2)$. I normalize the asset supply to zero. The market clearing condition is

$$D_t^d + D_t^l + \xi_t = 0, \tag{10}$$

which implies the asset price:

$$p_t = \frac{\boldsymbol{\beta}_t' \mathbf{z}_t + \boldsymbol{\gamma}_t' \mathbf{z}_t + \lambda \xi_t}{2}. \tag{11}$$

Given the trading strategies, the expected profits before the realization of $\mathbf{z}_t$ for the developer (Π_t^d) and the learner (Π_t^l) are given by

$$
\begin{aligned}
\Pi_t^d &= \mathbb{E}_t[(d_{t+1} + p_{t+1} - p_t)D_t^d] &= \frac{\boldsymbol{\theta}_t' \boldsymbol{\beta}_t - \boldsymbol{\theta}_t' \boldsymbol{\gamma}_t}{2\lambda} + \frac{\lambda \sigma_\xi^2}{4}, \\
\Pi_t^l &= \mathbb{E}_t[(d_{t+1} + p_{t+1} - p_t)D_t^l] &= \frac{\boldsymbol{\theta}_t' \boldsymbol{\gamma}_t - \boldsymbol{\theta}_t' \boldsymbol{\beta}_t}{2\lambda} + \frac{\lambda \sigma_\xi^2}{4}.
\end{aligned}
\tag{12}
$$

C. Strategic interactions

To study the strategic interactions, let's first consider the learner's decision. The learner chooses the trading strategy, $\boldsymbol{\gamma}_t$, based on an updated belief about the developer's strategy by optimally combining prior belief and new observations. Specifically, at each period t, the learner incrementally observes the developer's trades in period $t-1$, which provides a noisy estimation on $\boldsymbol{\beta}_{t-1}$:

$$\hat{\boldsymbol{\beta}}_{t-1} = \boldsymbol{\beta}_{t-1} + \hat{\boldsymbol{e}}_{t-1}. \tag{13}$$

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[10]λ is the risk-aversion coefficient multiplied by the variance of payoff for mean-variance investors.

The error term $\hat{e}_{t-1}$ comes from the process of uncovering strategies, e.g., regressing the developer's trades on characteristics. I assume $\hat{e}_t \overset{iid.}{\sim} \mathcal{N}(0, \sigma_e^2 K^{-1} I_K)$ (such that $\hat{e}_t' \hat{e}_t \approx \sigma_e^2$ when K is large) and it is independent to other random variables. The learner updates her beliefs by combining prior beliefs and the new estimate $\hat{\beta}_{t-1}$:

$$b_t = (1 - r_t^*) b_{t-1} + r_t^* \hat{\beta}_{t-1}, \tag{14}$$

and then chooses the trading strategy by normalizing (i.e., to satisfy $\gamma_t \in \{\theta : \theta'\theta = 1\}$):

$$\gamma_t = \frac{b_t}{\sqrt{b_t' b_t}}. \tag{15}$$

The learner optimally chooses the learning rate, r_t^*, to maximize the expected profits in future periods. I lay out the optimization problem in detail in Section 5.2.

Next, let's turn to the developer's decision. The developer knows that the learner is trying to mimic his strategy. Thus, he may strategically respond. Note that, without the learner, the optimal trading strategy for the developer is $\beta_t = \theta_t$. When strategic incentive arises, the developer may (i) reduce the speed of updating strategy by relying more on his past strategy β_{t-1}, or (ii) add random noise to his trade or strategy. To accommodate these two potential strategic behaviors, I consider the following decision-making rule for the developer:

$$\beta_t = x_t^* \theta_t + y_t^* \beta_{t-1} + n_t^* v_t, \tag{16}$$

where (x_t^*, y_t^*, n_t^*) are decision variables optimally chosen by the developer to maximize future expected profit subject to $\beta_t \in \{\theta : \theta'\theta = 1\}$, and the random component, $v_t \overset{iid.}{\sim} \mathcal{N}(0, K^{-1} I_K)$, is independent to other random variables. In this formulation, adding random noise v_t to the strategy is equivalent to adding random noise $v_t' z_t$ to trades.

5.2 Equilibrium characterization

I consider a stationary equilibrium, in which the optimal decision variables, r_t^* for the learner and (x_t^*, y_t^*, n_t^*) for the developer, are invariant over time. Note that under such an equilibrium, the trading strategies β_t and γ_t still keep changing, only the underlying rules of strategy evolution remain constant. In a stationary equilibrium, the expected profits defined in Eq.(12) are also constant and can be calculated using the following lemma.

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Lemma 1. *Given $(r_t, x_t, y_t, n_t) = (r, x, y, n)$ for $\forall t$, the restriction $\beta_t \in \{\theta : \theta'\theta = 1\}$ implies $n^2 = 1 - \left(\frac{1+\rho y}{1-\rho y}\right) x^2 - y^2$, and we have*

$$\lim_{K \to \infty} \theta_t' \beta_t = \frac{x}{1 - \rho y}, \tag{17}$$

$$\lim_{K \to \infty} \theta_t' \gamma_t = \frac{x}{1 - \rho y} \frac{\frac{r\rho}{1-\rho(1-r)}}{\sqrt{\frac{r(1+\sigma_e^2)}{2-r} + \frac{2r}{2-r} \left[A \frac{\rho(1-r)}{1-\rho(1-r)} + (1 - A) \frac{y(1-r)}{1-y(1-r)}\right]}}, \tag{18}$$

where $A = \frac{x^2 \rho}{(1-\rho y)(\rho - y)}$.

Based on Lemma 1, a stationary equilibrium is characterized by (r^*, x^*, y^*, n^*) such that:

- Given the learner's decision, $r = r^*$, the developer optimally chooses (x^*, y^*, n^*) to maximize his expected profits (proportional to $\theta_t' \beta_t - \theta_t' \gamma_t$) in each period:

$$(x^*, y^*, n^*) = \underset{0 \leq x,y,n \leq 1}{\operatorname{argmax}} \frac{x}{1 - \rho y} \left[1 - \frac{\frac{r\rho}{1-\rho(1-r)}}{\sqrt{\frac{r(1+\sigma_e^2)}{2-r} + \frac{2r}{2-r} \left[A \frac{\rho(1-r)}{1-\rho(1-r)} + (1 - A) \frac{y(1-r)}{1-y(1-r)}\right]}}\right]$$

$$\text{s.t.} \quad \left(\frac{1 + \rho y}{1 - \rho y}\right) x^2 + y^2 + n^2 = 1$$

- Given the developer's decision, $(x, y, n) = (x^*, y^*, n^*)$, the learner optimally chooses r^* to maximize her expected profits (proportional to $\theta_t' \gamma_t$) in each period:

$$r^* = \underset{0 < r \leq 1}{\operatorname{argmax}} \frac{\frac{r\rho}{1-\rho(1-r)}}{\sqrt{\frac{r(1+\sigma_e^2)}{2-r} + \frac{2r}{2-r} \left[A \frac{\rho(1-r)}{1-\rho(1-r)} + (1 - A) \frac{y(1-r)}{1-y(1-r)}\right]}}$$

Due to the strategic interaction between the two players, the optimal trading strategy for the developer may deviate from θ_t, i.e., y^* or n^* is not zero. The trade-off is that although such deviation reduces the profit directly from the payoff, it can deter revealing the true strategy to the learner, which benefits the developer by trading at a better price. One key result of the developer's optimal decision is as follows.

Proposition 1. *For any $0 < r \leq 1$, the developer's best response satisfies $n^* = 0$.*

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Recall that the developer can strategically deviate in two ways: (i) slow down updating $(y > 0)$ and/or (ii) combine a random strategy $(n > 0)$. Proposition 1 states that combining a random strategy is never optimal. The intuition is that combining a random strategy will not create estimation bias for the learner while slowing down updating will. Therefore, conditional on the same cost of the deviation, the developer benefits more by tilting toward the past strategy, compared to combining a random strategy. Figure 7 illustrates this point. The x-axis is the developer's expected dividend payoff, $\mathbb{E}[\theta_t'\beta]$, and the y-axis is the learner's expected dividend payoff, $\mathbb{E}[\theta_t'\gamma]$. The blue dotted line represents the developer's deviation by combining a random strategy, and the red solid line is for tilting toward the past strategy. The plot shows that conditional on the same level of $\mathbb{E}[\theta_t'\beta]$, i.e., same cost, tilting toward the past strategy is always more effective in reducing the learner's expected dividend payoff, which benefits the developer more.

Given that the developer's best response satisfies $n^* = 0$, the restriction $\left(\frac{1+\rho y}{1-\rho y}\right) x^2 + y^2 + n^2 = 1$, derived from $\beta_t \in \{\theta : \theta'\theta = 1\}$, implies that the developer only needs to choose y, with x to be determined by the restriction given the value of y. Therefore, the developer's best response can be effectively summarized by a policy function $y = P^d(r; \rho, \sigma_e)$. Similarly, let $r = P^l(y; \rho, \sigma_e)$ denote the learner's policy function. The equilibrium implies the fixed point of the following system:

$$
\begin{aligned}
y^* &= P^d(r^*; \rho, \sigma_e), & (19) \\
r^* &= P^l(y^*; \rho, \sigma_e). & (20)
\end{aligned}
$$

Figure 8 plots the policy functions, $P^d(r; \rho, \sigma_e)$ and $P^l(y; \rho, \sigma_e)$, and the equilibrium represented by their intersection point. There are several findings. First, $P^d(r; \rho, \sigma_e)$ exhibits an inverted U-shape, i.e., y achieves the highest value when r is in the middle and y approaches zero when r is near the two ends. This reflects the developer's trade-off between maintaining profitability vs. deterring learning. When the learner's r is around an ideal level (i.e., neither too small nor too large), the developer's motive of deterring learning dominates, and it suggests a larger deviation relative to θ_t is optimal. On the contrary, when the learner's r is extreme (i.e., close to zero or one) and less effective for learning, the developer's motive of maintaining profitability dominates, and it suggests a smaller deviation relative to θ_t is optimal.

Second, $P^l(y; \rho, \sigma_e)$ is increasing in y. The intuition is that the learner chooses the learning rate to balance the trade-offs between more timely information (larger r) vs. more robust estimation (smaller r). When y is larger, the developer's strategy is more

persistent, which mechanically reduces the timeliness and increases the robustness of the learner's estimation. Therefore, to balance, the learner tends to choose a larger r as a response to a larger y.

Third, both $P^d(r; \rho, \sigma_e)$ and $P^l(y; \rho, \sigma_e)$ tend to increase when the learner's precision σ_e^{-1} increases. This is because higher precision increases the developer's need for deterring learning and also allows the learner to put more weight on the recent information. Finally, the intersection suggests the uniqueness of the equilibrium. And the equilibrium y^* and r^* tend to increase as the learner's precision σ_e^{-1} increases.

5.3 Calibration

I set baseline parameters for numerical comparative statics. I interpret one period in the model as one quarter. There are four parameters to be calibrated: ρ, λ, σ_ξ, and σ_e. First, ρ is the AR1 coefficient of θ_t, which captures the persistence of the economy. Giglio, Kelly, and Kozak (2024) estimate a rich affine model of equity portfolios and obtain the annual persistence of state-space dynamics as 0.72 (Table II, factor PC1). I calibrate $\rho = 0.72^{1/4} = 0.921$.

Second, λ and σ_ξ are parameters related to asset demands. Because proportionally changing λ and σ_ξ only has a scaling effect in the model, WLOG, I normalize λ to be one. Given $\lambda = 1$, the proportion of noise trading volume to total trading volume is

$$\frac{\mathbb{E}(|\xi_t|)}{\mathbb{E}(|D_t^d|) + \mathbb{E}(|D_t^l|) + \mathbb{E}(|\xi_t|)} = \frac{\sigma_\xi}{\sqrt{\sigma_\xi^2 + 2[1 - \mathbb{E}(\beta_t' \gamma_t)]} + \sigma_\xi}. \tag{21}$$

Bloomberg Intelligence estimates that retail trading volumes account for around 25% of the total trading volumes in the U.S. equity market at the end of 2021 (Financial Times, 2021). Therefore, under a common interpretation that retail trading are proxies for noise trading, I calibrate $\sigma_\xi = 0.344$.

Finally, σ_e is the standard deviation of the error term when learning trading strategies. For example, a higher disclosure frequency corresponds to a lower σ_e (or a higher σ_e^{-1}) as outsiders can learn better with more granular observations. The correlation between the developer's strategy β_t and its noisy estimation $\hat{\beta}_t$ is

$$\text{Corr}(\beta_t' z_t, \hat{\beta}_t' z_t) = \frac{1}{\sqrt{(1 + \sigma_e^2)}}. \tag{22}$$

I calibrate σ_e using a bootstrap method to fit trading strategies based on the Abel Noser

data. Specifically, similar to the exercise in Section 4, I first use decision trees to estimate a trading strategy—by fitting trades with stock characteristics—as β_t in Eq.(22). To calibrate σ_e under a given disclosure frequency, e.g., monthly, I then collapse the trades to the given frequency, generate bootstrapped samples by randomly drawing observations with replacement, and estimate trading strategies from the bootstrapped samples as $\hat{\beta}_t$ in Eq.(22). Finally, by matching the correlations, I obtain σ_e^{-1} to be 0.304, 0.564, and 1.126 for quarterly, monthly, and weekly disclosure frequency, respectively. I summarize the calibration results in Table 11.

5.4 Comparative statics

I conduct comparative statics analyses to explore the implications of the learning precision, σ_e^{-1}, on the equilibrium decisions and outcomes. An increase in σ_e^{-1} captures the change that facilitates the learner to learn about the developer's strategy, for example, increasing the frequency of mandatory portfolio disclosure.

5.4.1 Effects on the trading strategies

Figure 9 shows equilibrium decisions (i.e., developer's x, y, n and learner's r) as functions of σ_e^{-1}, for various values of ρ. The patterns are similar across the four panels. First, n^* is always zero, meaning that adding noise is not an optimal response for the developer to deter strategy leakage, as Proposition 1 states. Second, as σ_e^{-1} increases, the developer reduces the speed of updating trading strategy toward θ_t (i.e., x^* decreases) and increases the stickiness to the last-period strategy (i.e., y^* increases). This suggests that the developer impedes the learner's learning by sluggish updating. Third, as ρ increases (from panel A to panel D), the magnitude of decrease in x^* and increase in y^* as σ_e^{-1} changing is larger. This is because higher ρ suggests the process of θ_t is more persistent, and sluggish updating is less costly for the developer. Finally, the equilibrium learning rate r^* increases as σ_e^{-1} increases. Intuitively, the learner puts more weight on the recent signal when the signal's precision is higher.

Figure 10 provides a visualization of the evolution of trading strategies in equilibrium. Each point represents a strategy (as a strategy is defined as a vector), and each path represents a process of strategies, e.g., θ_t, β_t, and γ_t. To plot the figure, I first set the process of θ_t as given and then generate the paths of β_t and γ_t based on the developer's and learner's equilibrium decisions. Panel A corresponds to the situation with a low learning precision, and panel B is for a high learning precision. As the figure shows,

when the precision is low, the developer's strategy (β_t) tracks more closely to the true strategy (θ_t), as the developer anticipates that the learner cannot learn the strategy very fast. When the precision is high, the developer strategically avoids tracking θ_t too closely and makes his strategy path more slow-moving. At the same time, the learner can follow the developer more closely due to the high precision. However, there is still a distance between the learner's strategy (γ_t) and the true one (θ_t) because of the developer's strategic response.

Overall, the analysis suggests that an increase in the learning precision (e.g., due to an increase in disclosure frequency) can reduce the incentives for funds to develop new strategies, as they have stronger motives for strategically responding to the trading strategy leakage.

5.4.2 Effects on the price informativeness

To analyze market-level implications of trading strategy leakage, I focus on the payoff-price sensitivity, i.e., how the equilibrium price can reflect fundamental information, which is the coefficient κ_1 by regressing dividend payoff d_{t+1} on equilibrium price p_t:

$$d_{t+1} = \kappa_0 + \kappa_1 p_t + \varepsilon_{t+1}. \tag{23}$$

The payoff-price sensitivity is given by

$$\kappa_1 = \frac{Cov(d_{t+1}, p_t)}{Var(p_t)} = \frac{\mathbb{E}[\theta_t' \beta_t] + \mathbb{E}[\theta_t' \gamma_t]}{1 + \mathbb{E}[\beta_t' \gamma_t] + \frac{\lambda^2 \sigma_\xi^2}{2}}. \tag{24}$$

Intuitively, the payoff-price sensitivity is positively related to the expected inner product between fundamental loadings, θ_t, and their strategies for the developer ($\mathbb{E}[\theta_t \beta_t]$) and the learner ($\mathbb{E}[\theta_t \gamma_t]$), as price incorporates their information through trading. The payoff-price sensitivity is negatively related to the strategy similarity between the developer and the learner ($\mathbb{E}[\beta_t \gamma_t]$) and the amount of noise trading (σ_ξ^2) because these factors increase the variance of the price.

Figure 11 plots the components $\mathbb{E}[\theta_t \beta_t]$ (panel A), $\mathbb{E}[\theta_t \gamma_t]$ (panel B), $\mathbb{E}[\beta_t \gamma_t]$ (panel C), and the payoff-price sensitivity (panel D) as functions of the learning precision σ_e^{-1}. The solid line represents equilibrium outcomes *with* strategic interaction, while the dashed line is for the case *without* strategic interaction by assuming the developer always plays $\beta_t = \theta_t$. There are several findings. First, in equilibrium, $\mathbb{E}[\theta_t \beta_t]$ decreases when σ_e^{-1} increases. This is because the developer strategically reduces the speed of updating his

strategies when the learner can learn faster. Second, $\mathbb{E}[\boldsymbol{\theta}_t \boldsymbol{\gamma}_t]$ increases when σ_e^{-1} increases. As a comparison, if the developer always plays the true strategy $\boldsymbol{\theta}_t$, the learner's $\mathbb{E}[\boldsymbol{\theta}_t \boldsymbol{\gamma}_t]$ will increase even more. This suggests that the developer's sluggish updating is effective in the sense of reducing $\mathbb{E}[\boldsymbol{\theta}_t \boldsymbol{\gamma}_t]$ relative to the case without strategic response. Third, the strategy similarity between the developer and the learner, $\mathbb{E}[\boldsymbol{\beta}_t \boldsymbol{\gamma}_t]$, increases when σ_e^{-1} increases. Moreover, the increase is larger relative to the case without strategic response. This is because the developer's strategy becomes more persistent due to sluggish updating, and the learner is more likely to recover $\boldsymbol{\beta}_t$ based on the developer's past trades.

Finally, panel D plots how payoff-price sensitivity changes as σ_e^{-1} increases. On the one hand, the increase in $\mathbb{E}[\boldsymbol{\theta}_t \boldsymbol{\gamma}_t]$ tends to improve the payoff-price sensitivity, as the learner can learn more about fundamental information and affect the price via trades. On the other hand, the decrease in $\mathbb{E}[\boldsymbol{\theta}_t \boldsymbol{\beta}_t]$ due to the developer's strategic behavior and the increase in $\mathbb{E}[\boldsymbol{\beta}_t \boldsymbol{\gamma}_t]$ tend to reduce the payoff-price sensitivity. Putting together, the numerical solutions show that the overall effect can be negative in a wide range, i.e., better learning precision induces worse payoff-price sensitivity. As a comparison, when there is no strategic behavior, the payoff-price sensitivity will be monotonically increasing as learning precision increases. This result highlights the unintended consequence of frequent disclosure on the market level—due to the incentive of alleviating the impact from strategy leakage, the asset price can become less informative in terms of the payoff-price sensitivity.

5.5 Empirical test of the counterintuitive model implication

My model produces a counterintuitive implication: the strategy developer deters strategy leakage not by adding random noise, but by sluggish updating. I empirically test the implication based on the released trades of client funds.

Sluggish updating implies higher persistence in the trading strategies. To measure persistence, I estimate trading strategies of client funds year by year and compute the correlations of the uncovered strategies across adjacent years. To align with the model, I aggregate client funds into a representative agent when calculating these strategy correlations.

Figure 12 plots the time series of the trading strategy persistence. The correlations are always above 0.8 in the figure, suggesting that, on average, trading strategies are quite persistent. More importantly, consistent with the model implication of sluggish updating, there is a steady rise in persistence following the data release. The year-over-

year correlation increases from 0.85 in the pre-period to 0.93 by the end of the sample. Overall, this test confirms the key implication of my model.

6   Additional Results

I present additional results in this section. First, I examine whether client funds quit Abel Noser after the release of data. Then, I examine what happens after two reverse shocks: (i) Abel Noser removed fund numeric codes from the data in 2012, and (ii) Abel Noser completely stopped providing the data in 2017.

6.1   Client funds' other response

A potential response for client funds is to quit Abel Noser. To examine whether this is a major response, I estimate the changes in quit rates using the following regression:[11]

$$IsEndYear_{i,y} = \beta_0 + \beta_1 \times \mathbb{I}_{\{Year=2002\}} + \beta_2 \times \mathbb{I}_{\{Year>2002\}} + \text{BegYear FE} + \epsilon_{i,y}. \quad (25)$$

The dependent variable, $IsEndYear_{i,y}$, equals one if year y is the last year for fund i being Abel Noser's client, and zero otherwise. I add the beginning-year fixed effects to control for ages. The coefficient β_1 (β_2) captures the change in quit rate during 2002 (from 2003 to 2007) relative to the rate during the pre-period.

The estimation results are in Appendix Figure A1. Although there was a slight increase in the quit rate, around 3 percent, in the event year, this increase is not statistically significant. This could be because, given that past trades were already released, the marginal benefits from quitting Abel Noser could be small. Additionally, it may suggest that client funds have a strong demand for Abel Noser's services, for example, to demonstrate to their investors about "best execution" for their transactions. In short, there is no evidence that quitting Abel Noser is a major response for client funds.

6.2   Reverse shocks

I examine what happens after reverse shocks. I consider two events: (i) Abel Noser removed fund numeric codes from the data in 2012, and (ii) Abel Noser completely

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[11]To calculate the quit rate with the denominator equal to the number of clients in that year, fund i is included in the sample only during its period of being a client. This is to ensure that the number of observations in year y equals the number of clients in that year.

stopped providing the data in 2017. Similar to the analysis in Section 3.3, I conduct the following regression over the period 2002-2020:

$$\Delta R_t = \beta_0 + \beta_1 \times \mathbb{I}_{\{year \geq 2012\}} + \beta_2 \times \mathbb{I}_{\{year \geq 2017\}} + \epsilon_t, \tag{26}$$

where ΔR_t is the average differenced risk-adjusted fund return in month t between the treated and control groups. The treated group is the funds that became Abel Noser's clients in or after 2002. The control group is constructed via PSM.

Table 10 reports the estimated treatment effects. First, the effect due to removing fund numeric codes in 2012 is insignificant. This could be because (i) outside investors can still learn strategies using aggregate trades from client funds, as the exercise in Section 4.3, or (ii) the event that Abel Noser leaked the linktable in 2011 mixes the effect since two events are close to each other.

Second, the effect of completely stopping providing the data in 2017 is significantly positive. For example, the client funds improved their FF3-alpha (CH4-alpha) by 7.4 (6.6) bps per month with t-stat of 2.04 (1.89) after Abel Noser stopped providing data. The positive effect of the reverse shock supports the causal interpretation. The afterward 6-7 bps of performance improvement, compared to the initial 36 bps of performance drop, implies an irreversible hurt to these client funds.

7   Conclusion

This paper studies how disclosure affects fund performance and market efficiency via a new channel: strategy leakage. I empirically identify strategy leakage by exploiting a quasi-natural experiment in which Abel Noser Solutions, a trading-cost-analysis company, began selling its clients' historical trading data with a one-year lag. Despite the one-year lag in data provision, it reduces the performance of client funds by 36 bps per month for years. By adopting a simple machine learning method and examining different disclosure frequencies, I find that releasing monthly trades is a crucial cutoff, as uncovered strategies can be statistically significant (or insignificant) in predicting client funds' trades and achieving predatory profits when the frequency exceeds (falls below) monthly.

To study how fund managers strategically respond in equilibrium and the market-wide implications of strategy leakage, I develop a model incorporating the strategic interaction between the strategy developer and the outside learner. Contrary to Huddart, Hughes, and Levine (2001), the developer deters strategy leakage not via playing a

random strategy but via sluggish updating. Consequently, when the learning precision for the outsider increases, the persistence of the developer's strategy increases, and the price informativeness can decrease. Overall, my study highlights the limited role of reporting delay and the unintended consequences of increasing disclosure frequency via the channel of strategy leakage.

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Table 1: Summary statistics. This table presents the summary statistics for the matched mutual fund sample of Abel Noser's clients over the period of 1998 to 2007. I use MFLINKS tables to match fund returns from CRSP and fund holdings from the Thomson Reuters S12 database. Then, I match the funds to their trades from the Abel Noser dataset by (i) fund names and (ii) comparing cumulative trading to holding changes. Panel A presents the numbers of matched funds classified by the beginning year they became Abel Noser's clients. Panel B presents the balance test on fund characteristics between the treated group (i.e., Abel Noser's client funds) and the control group (i.e., non-client funds). The control group is constructed via propensity score matching (PSM) with 20 nearest neighbors.

Panel A: Number of matched funds	
Beginning year of becoming Abel Noser's clients	# matched funds
1998-2001	39
2002	11
2003	19
2004	15
2005	32
>2005	35
Total	151

Panel B: Balance test on fund characteristics

Characteristics	Treated	Control	Diff.	t-stat
TNA ($ million)	2,141	2,480	-338	[-0.41]
Age (years)	11.103	11.472	-0.369	[-0.22]
Fund CH4 α (%)	0.344	0.212	0.133	[0.79]
Hscore: SZ	3.785	3.812	-0.026	[-0.45]
Hscore: BM	1.216	1.195	0.021	[0.19]
Hscore: MOM	2.662	2.688	-0.026	[-0.37]
Idio. volatility (%)	2.021	1.921	0.100	[0.51]
Turnover ratio	1.109	1.167	-0.058	[-0.38]
Expense (%)	1.412	1.412	0.001	[0.01]

Table 2: Disclosure effect on client funds' return. This table presents the difference-in-differences analysis on fund returns around 2002, i.e., the year when Abel Noser started to sell its clients' historical trading data. The sample period is from 1998 to 2007. In Panel A, the treated group is the funds that became Abel Noser's clients before 2002. In Panel B, the treated group (placebo) is Abel Noser's client funds but only includes the returns before their trading data is released (the release date for a client fund is one year after it became a client). For both Panels A and B, the control group is the non-client funds selected by propensity score matching (PSM). I estimate propensity scores using logistic regression with fund characteristics of TNA, age, past alpha, hscore_sz, hscore_bm, hscore_mom, idiosyncratic volatility, turnover ratio, and expense ratio. I use k-nearest neighbors matching with $k = 20$. I report in this table the estimated coefficient β_1 of the following regression: $\Delta R_t = \beta_0 + \beta_1 \times \mathbb{I}_{\{year \geq 2002\}} + \epsilon_t$, where the dependent variable, ΔR_t, is the average risk-adjusted fund return in month t for the treated group minus that for the control group. To obtain risk-adjusted fund returns, I estimate fund beta using 60-month rolling regressions. Fund returns are before-expense and expressed in percentage points. t-statistics, shown in brackets, are computed based on standard errors with Newey-West corrections of 12 lags (months). *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Risk-adjusted fund returns			
	Excess return	CAPM alpha	FF3 alpha	CH4 alpha
Panel A: Impact on client funds				
Treatment effect	-0.385**	-0.408**	-0.305***	-0.338***
	[-2.07]	[-2.52]	[-3.25]	[-4.19]
Observations	120	120	120	120
Panel B: Impact on client funds before data release (placebo)				
Treatment effect (placebo)	-0.046	-0.029	-0.011	0.042
	[-0.87]	[-0.52]	[-0.16]	[0.61]
Observations	120	120	120	120

Table 3: Alternative construction of control group. This table presents the results of robustness tests using different methods of constructing the control group. I consider PSM with 10 or 50 nearest neighbors, single sorting on fund TNA (5 groups), and double sorting on fund TNA and Age/Alpha/Turnover (5×5 groups) as alternative methods. The sample construction and estimation procedure are the same as those in Table 2 Panel A, except for the way of constructing the control group as indicated here. *t*-statistics, shown in brackets, are computed based on standard errors with Newey-West corrections of 12 lags (months). *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

| Control group | Risk-adjusted fund returns | | | |
	Excess return	CAPM alpha	FF3 alpha	CH4 alpha
PSM: 50 nearest neighbors	-0.325*	-0.353**	-0.274***	-0.274***
	[-1.72]	[-2.12]	[-2.97]	[-3.44]
PSM: 10 nearest neighbors	-0.396*	-0.467***	-0.378***	-0.399***
	[-1.90]	[-2.68]	[-3.02]	[-3.50]
Sort by TNA	-0.392**	-0.419**	-0.331***	-0.345***
	[-2.19]	[-2.31]	[-3.88]	[-4.39]
Sort by TNA & Age	-0.376**	-0.400**	-0.321***	-0.325***
	[-2.09]	[-2.22]	[-3.41]	[-3.72]
Sort by TNA & Alpha	-0.356*	-0.417**	-0.340***	-0.340***
	[-1.78]	[-2.20]	[-3.89]	[-4.16]
Sort by TNA & Turnover	-0.267**	-0.312**	-0.291***	-0.300***
	[-2.08]	[-2.12]	[-3.99]	[-4.05]

Table 4: Triple-differences analysis. This table presents the triple-differences analysis on fund returns exploiting client funds variation over the period from 1998 to 2007. I report the coefficients from fund-month panel regressions: $\Delta R_{i,t} = \beta_0 \times \mathbb{I}_{\{year \geq 2002\}} + \beta_1 \times \mathbb{I}_{\{year \geq 2002\}} \times \mathbb{I}_{\{fund\ category\}} + FE + \epsilon_{i,t}$, where $\Delta R_{i,t}$ is client fund i's CH4-adjusted fund returns in month t minus the average from the matched non-client funds. I consider the following variation in fund characteristics: $\mathbb{I}_{\{longer\ period\}}$ equals one if, until the end of 2001, the period of a fund being Abel Noser's client is longer than the sample median, and zero otherwise. $\mathbb{I}_{\{more\ stocks\}}$ equals one if, until the end of 2001, the number of stocks traded by a client fund is more than the sample median, and zero otherwise. $\mathbb{I}_{\{better\ skill\}}$ equals one if the fund CH4 alpha in the pre-period is higher than the sample median, and zero otherwise. t-statistics, shown in brackets, are calculated via bootstrap with 500 replications. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Treated-minus-control CH4-adjusted fund returns					
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{I}_{\{\geq 2002\}}$	-0.152		-0.176		-0.092	
	[-1.21]		[-1.24]		[-0.76]	
$\mathbb{I}_{\{\geq 2002\}} \times \mathbb{I}_{\{longer\ period\}}$	-0.669***	-0.796***				
	[-2.94]	[-3.37]				
$\mathbb{I}_{\{\geq 2002\}} \times \mathbb{I}_{\{more\ stocks\}}$			-0.476**	-0.467**		
			[-2.18]	[-2.13]		
$\mathbb{I}_{\{\geq 2002\}} \times \mathbb{I}_{\{better\ skill\}}$					-0.795***	-0.815***
					[-3.65]	[-3.75]
Observations	2,755	2,755	2,755	2,755	2,755	2,755
Fund FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE		Yes		Yes		Yes

Table 5: Disclosure effect on trading return. This table presents the changes in client funds' dollar-volume-weighted risk-adjusted trading returns after Abel Noser started to sell data in 2002. The sample period is from 1999 to 2007. For each fund-month, I calculate the dollar-volume-weighted risk-adjusted return as $R^{trade} = (\sum_j Buy_j \times R^k_{[d+1,d+w]})/(\sum_j Buy_j) - (\sum_j Sell_j \times R^k_{[d+1,d+w]})/(\sum_j Sell_j)$, where Buy_j or $Sell_j$ is dollar trading volume for trade j, and $R^k_{[d+1,d+h]}$ is stock k's cumulative risk-adjusted return over day $[d+1, d+h]$ with $h = 5, 10, 20$. I report in this table the estimated coefficient β_1 of the following regression: $R^{trade}_t = \beta_0 + \beta_1 \times \mathbb{I}_{\{year \geq 2002\}} + \epsilon_t$, where the dependent variable, R^{trade}_t, is the client funds' average dollar-volume-weighted risk-adjusted trading return in month t. Returns are expressed in percentage points. t-statistics, shown in brackets, are computed based on standard errors with Newey-West corrections of 12 lags (months). *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

Horizon (h)	Dollar-volume-weighted risk-adjusted trading returns			
	Excess return	CAPM alpha	FF3 alpha	CH4 alpha
5 trading days	-0.134**	-0.212***	-0.179***	-0.187***
	[-2.05]	[-4.30]	[-3.75]	[-3.63]
10 trading days	-0.323***	-0.422***	-0.386***	-0.394***
	[-3.94]	[-5.25]	[-5.20]	[-4.68]
20 trading days	-0.482***	-0.604***	-0.508***	-0.528***
	[-4.80]	[-6.40]	[-4.76]	[-4.88]
Observations	108	108	108	108

Table 6: Hedge funds' predatory trading around the disclosure. This table presents the coefficients of regressing client funds' following trades on hedge funds' positions over the period from 1999 to 2003. I separately estimate for the long vs. short sides. The regression specification is $DTV_{k,t+1} = \beta \times POS_{k,t} + \sum_{l \in \{1999,2000,2002,2003\}} \delta_l \times POS_{k,t} \times \mathbb{I}_{\{year\ l\}} + \mu_{k,y} + \epsilon_{k,t+1}$. $DTV_{k,t+1}$ is the client funds' aggregate buy or sell dollar volume on stock k in month $t+1$, and I focus on the client funds that joined Abel Noser before 2002, i.e., consistent with the treated group in the test of fund returns. $POS_{k,t}$ is the hedge funds' aggregate dollar value of holding or short selling on stock k at the end of the month t. I apply $\log(1+x)$ transformation on both $DTV_{k,t+1}$ and $POS_{k,t}$. $\mu_{k,y}$ is the stock-year fixed effects. For the long side, I calculate aggregate hedge fund holdings ($HFHD$) based on the hedge fund list from Chen, Da, and Huang (2019). As hedge funds disclose holdings on a quarterly basis, I only include the quarter-end months in the regression. For the short side, I follow the literature to use short interest ($SINT$) as a proxy for hedge funds' short positions. All variables are winsorized cross-sectionally at the 1st and 99th percentiles. t-statistics, shown in brackets, are double clustered at the stock and month levels. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Client funds' buy		Client funds' sell
$HFHD$	0.299***	$SINT$	0.106*
	[4.69]		[1.84]
$HFHD \times \mathbb{I}_{\{1999\}}$	-0.080	$SINT \times \mathbb{I}_{\{1999\}}$	-0.076
	[-1.01]		[-1.11]
$HFHD \times \mathbb{I}_{\{2000\}}$	-0.070	$SINT \times \mathbb{I}_{\{2000\}}$	-0.006
	[-0.98]		[-0.08]
$HFHD \times \mathbb{I}_{\{2002\}}$	0.252**	$SINT \times \mathbb{I}_{\{2002\}}$	0.216**
	[2.27]		[2.28]
$HFHD \times \mathbb{I}_{\{2003\}}$	0.250**	$SINT \times \mathbb{I}_{\{2003\}}$	0.203**
	[2.33]		[2.32]
Observations	59,872	Observations	125,855
Stock $\times$ Year FE	Yes	Stock $\times$ Year FE	Yes

Table 7: Trading predictability of the uncovered strategies. This table presents the coefficients of a stock-week panel regression to test the out-of-sample trading predictability of the uncovered trading strategies from a machine-learning method. The testing period is from 2002 to 2007. The dependent variable is a cross-sectional percentage rank (ranging from 0 to 100) on client funds' aggregate net dollar volume in each week scaled by the past four-week average stock-level dollar volume. The key explanatory variables, *BuyScore* and *SellScore*, are obtained from the Gradient Boosting Regression Tree (GBRT) to predict client funds' following trades. To train the GBRT, I use 61 stock characteristics to fit client funds' aggregate buy or sell trading volume. Hyper-parameters, such as the number of layers, are determined by ten-fold cross-validation. I follow a real-time training and testing procedure: (i) training data is provided with a one-year lag; (ii) I train GBRT every quarter with a three-year rolling window; and (iii) after training, I input the most recent stock characteristics to GBRT to predict client funds' following trades. For better interpretability of the regression coefficients, I scale *BuyScore* and *SellScore* by their full-sample standard deviations to reflect the per-standard-deviation effect. *t*-statistics, shown in brackets, are double clustered at the stock and week levels. *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)
BuyScore	2.532***	1.396***	1.242***
	[10.98]	[5.68]	[4.60]
SellScore	-3.101***	-1.517***	-2.120***
	[-12.86]	[-5.05]	[-5.82]
Observations	465,353	465,334	465,265
Week FE	Yes	Yes	Yes
Stock FE		Yes	
Stock × Year FE			Yes

Table 8: Predatory portfolios based on the uncovered strategies. This table presents the weekly abnormal returns following a predatory strategy of buying (selling) stocks that client funds are predicted to buy (sell) over the period from 2002 to 2007. I drop small stocks based on the 20th percentile of the market equity for NYSE stocks. *BuyScore* and *SellScore* are obtained from the Gradient Boosting Regression Tree (GBRT) as described in Table 7. At the end of each week, I sequentially sort stocks on *BuyScore* and *SellScore* into 5 × 5 portfolios, and long (short) stocks in the group of high-*BuyScore*-low-*SellScore* (low-*BuyScore*-high-*SellScore*). EW (VW) indicates equally weighted (value-weighted) in the portfolio formation. I regress the long-short portfolio return on common factors and report the average abnormal returns, i.e., the intercepts. All returns are expressed in percentage points. *t*-statistics, shown in brackets, are computed based on standard errors with Newey-West corrections of 12 lags (weeks). *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

Portfolio weight	Long-short portfolio abnormal returns (weekly)			
	Excess return	CAPM alpha	FF3 alpha	CH4 alpha
EW	0.264**	0.300***	0.275***	0.212***
	[2.26]	[3.19]	[3.05]	[3.27]
VW	0.289*	0.321**	0.284**	0.204**
	[1.84]	[2.22]	[2.02]	[2.04]
Observations	313	313	313	313

Table 9: Returns of client funds vs. Returns of predatory portfolios. This table presents the correlation coefficients from a monthly time-series regression of the treated-minus-control risk-adjusted fund returns (i.e., ΔR_t in Table 2) on the returns of the predatory long-short portfolio (as described in Table 8) over the period from 2002 to 2007. For better interpretability of the regression coefficients, I scale both the client funds' returns and predatory portfolio returns such that their standard deviations are equal to that of the market portfolio. t-statistics, shown in brackets, are computed based on standard errors with Newey-West corrections of 12 lags (months). *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Client funds' risk-adjusted returns (relative to the control group)			
	Excess return	CAPM adj.	FF3 adj.	CH4 adj.
Predatory portfolio (EW)	-0.538***	-0.322***	-0.303**	-0.338**
	[-5.73]	[-2.72]	[-2.21]	[-2.59]
Predatory portfolio (VW)	-0.551***	-0.488***	-0.440***	-0.464***
	[-8.19]	[-6.04]	[-5.29]	[-5.67]
Observations	72	72	72	72

Table 10: Reverse shocks. This table presents the difference-in-differences analysis on fund return exploiting two reverse shocks: (i) Abel Noser removed fund numeric codes from the data in 2012, and (ii) Abel Noser completely stopped providing the data in 2017. The sample period is from 2002 to 2020. The regression specification is $\Delta R_t = \beta_0 + \beta_1 \times \mathbb{I}_{\{year \geq 2012\}} + \beta_2 \times \mathbb{I}_{\{year \geq 2017\}} + \epsilon_t$, where ΔR_t, is the average risk-adjusted fund return in month t for the treated group minus that for the control group. The treated group is the funds that became Abel Noser's clients in or after 2002. The control group is constructed via PSM with 20 nearest neighbors, i.e., the same procedure as in Table 2. I report the coefficients β_1 and β_2, which capture the treatment effects. Fund returns are before-expense and expressed in percentage points. t-statistics, shown in brackets, are computed based on standard errors with Newey-West corrections of 12 lags (months). *, **, *** denote significance at the 10%, 5%, and 1% level, respectively.

	Treated-minus-control risk-adjusted returns			
	Excess return	CAPM alpha	FF3 alpha	CH4 alpha
$\mathbb{I}_{\{year \geq 2012\}}$	0.027	0.031	0.016	0.020
	[0.44]	[0.48]	[0.24]	[0.30]
$\mathbb{I}_{\{year \geq 2017\}}$	0.068*	0.110***	0.074**	0.066*
	[1.93]	[2.72]	[2.04]	[1.89]
Observations	228	228	228	228

Table 11: Parameter calibration. This table presents the parameter calibration of the model. ρ is the AR1 coefficient of θ_t, which captures the persistence of the economy states. λ is the scaler in the demand equations of the developer and the learner. σ_ξ is the standard deviation of noise trading. σ_e (σ_e^{-1}) is the standard deviation of the error term when learning trading strategies (learning precision) for the learner.

| Variable	Calibration method	Value
ρ	persistence of state-space dynamics (Giglio, Kelly, and Kozak, 2024)	0.921
λ	normalize to one	1.000
σ_ξ	retail / total trading volumes $\approx$ 25% (Financial Times, 2021)	0.344
σ_e^{-1} (quarterly)	ML method and bootstrap	0.304
σ_e^{-1} (monthly)	ML method and bootstrap	0.564
σ_e^{-1} (weekly)	ML method and bootstrap	1.126
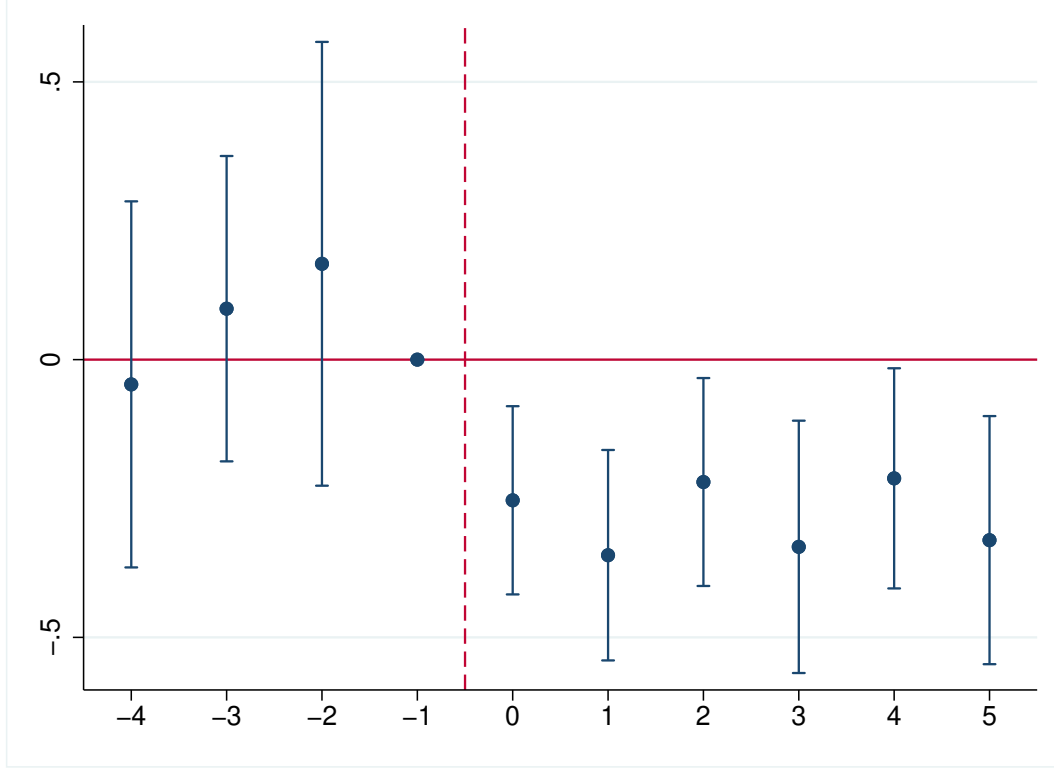
Figure 1: Time series of disclosure effect on client funds' return. This figure plots the time series of coefficients from the difference-in-differences analysis on Carhart-four-factor-adjusted fund returns. Period 0 in x-axis corresponds to 2002, i.e., the year when Abel Noser started to sell its clients' historical trading data. The treated group is the funds that became Abel Noser's clients before 2002. The sample construction and estimation procedure are the same as those in Table 2 Panel A, except here the regression specification is $\Delta R_t = \sum_{l=-4}^{5} \beta_l \times \mathbb{I}_{\{\text{year } l\}} + \epsilon_t$. I plot the time series of estimated coefficients $\{\beta_l\}$ benchmarking period $l = -1$ as zero. The vertical dashed line splits the pre- and post-periods. The vertical bars around point estimations represent 95% confidence intervals. The standard errors are calculated with Newey-West corrections of 12 lags (months).
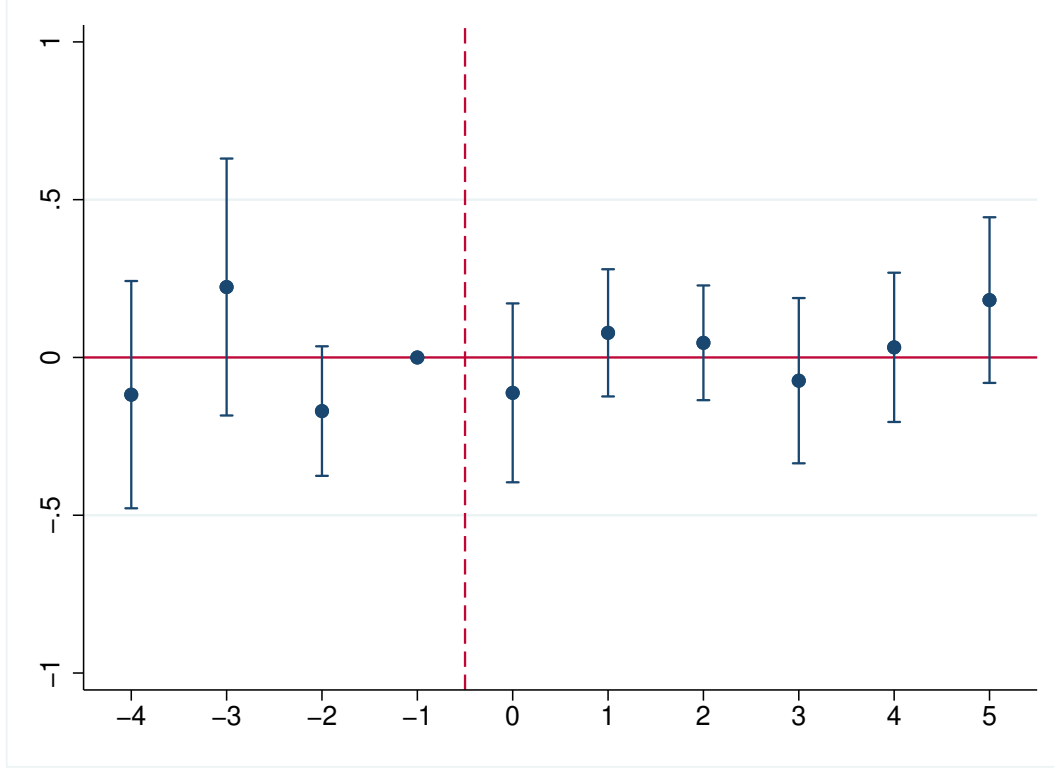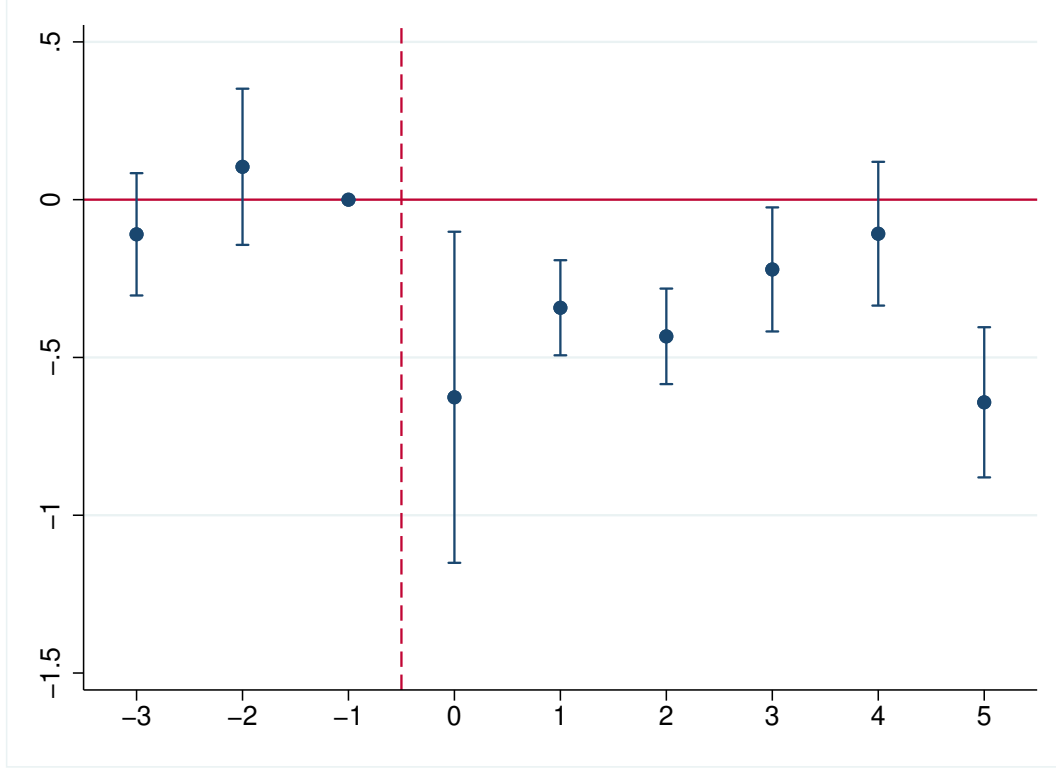
Figure 2: Time series of disclosure effect on client funds' return (placebo). This figure plots the time series of coefficients from the difference-in-differences analysis on Carhart-four-factor-adjusted fund returns. Period 0 in *x*-axis corresponds to 2002, i.e., the year when Abel Noser started to sell its clients' historical trading data. The treated group (placebo) is Abel Noser's client funds but only includes the returns before their trading data is released (the release date for a client fund is one year after it became a client). The sample construction and estimation procedure are the same as those in Table 2 Panel B, except here the regression specification is $\Delta R_t = \sum_{l=-4}^{5} \beta_l \times \mathbb{I}_{\{\text{year } l\}} + \epsilon_t$. I plot the time series of estimated coefficients $\{\beta_l\}$ and benchmark period $l = -1$ as zero. The vertical dashed line splits the pre- and post-periods. The vertical bars around point estimations represent 95% confidence intervals. The standard errors are calculated with Newey-West corrections of 12 lags (months).

Figure 3: Time series of disclosure effect on trading return. This figure plots the time series of coefficients by regressing client funds' average 10-day trading returns on zero-one indicators of years. The sample construction and estimation procedure are the same as those in Table 5, except here the regression specification is $R_t^{trade} = \sum_{l=-3}^{5} \beta_l \times \mathbb{I}_{\{year\ l\}} + \epsilon_t$. I plot the time series of estimated coefficients $\{\beta_l\}$ and benchmark period $l = -1$ as zero. The vertical dashed line splits the pre- and post-periods. The vertical bars around point estimations represent 95% confidence intervals. The standard errors are calculated with Newey-West corrections of 12 lags (months).
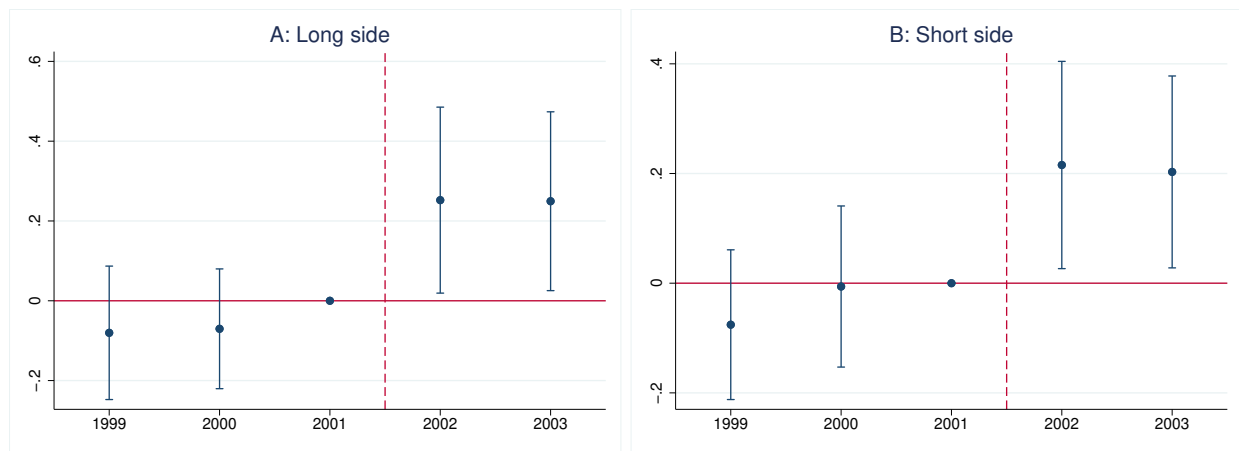
Figure 4: Hedge funds' predatory trading around the disclosure. This figure visualizes the estimation in Table 6 with panel A (panel B) for the long side (short side). I normalize the coefficients of 2001 (i.e., one year before Abel Noser started to sell data) to zero as benchmarks. The vertical dashed line splits the pre- and post-periods. The vertical bars around point estimations represent 95% confidence intervals. The standard errors are double-clustered at the stock and month levels.
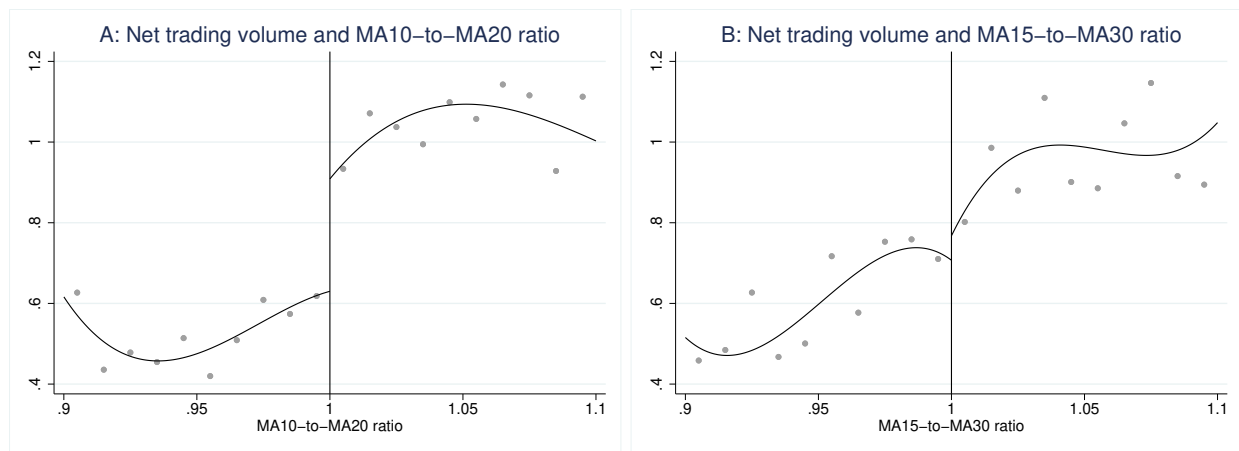
Figure 5: Uncover trading strategy: Golden (Death) Cross. This figure shows an example of examining whether client funds are employing the Golden (Death) Cross trading strategy, i.e., buy (sell) stocks if a short-term moving-average price crossover up (down) through a long-term moving-average price. I sort stocks by the ratio of the short-term and long-term moving-average prices at the end of each week and examine the net trading volume from client funds in the following week. The client funds' weekly net trading volume is scaled by the past four-week average stock-level dollar volume and displayed in percentage points in the y-axis. In panel A (B), the sorting variable in the x-axis is the ratio of 10-day-over-20-day (15-day-over-30-day) moving-average prices. The dots are the average net trading volume within each bin. Bins are evenly spaced over the range from 0.9 to 1.1. The lines are polynomial fits with an order of three.
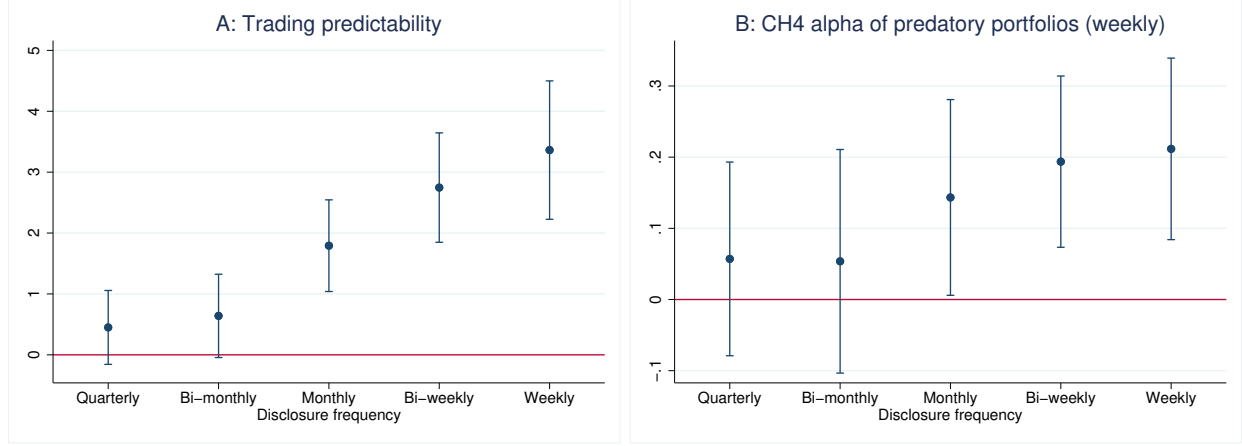
Figure 6: Disclosure frequency and strategy leakage. This figure shows how disclosure frequency affects trading strategy leakage. In panel A, I examine the out-of-sample trading predictability under different disclosure frequencies of client funds' portfolios. The sample period, training-testing scheme (e.g., three-year rolling window, quarterly update, and a one-year lag), and regression specification are the same as in Table 7. The only difference is that client funds' trades are observed in different frequencies when training GBRT to backout trading strategies. Panel A plots the coefficient difference, $\beta_{BuyScore} - \beta_{SellScore}$, with Week FE and Stock $\times$ Year FE, i.e., the specification in Table 7 Column (3). The vertical bars around point estimations represent 95% confidence intervals. In panel B, I examine the abnormal returns based on trading prediction under different disclosure frequencies. The sample period, sample construction, and portfolio formation are the same as in Table 8. The only difference is that *BuyScore* and *SellScore* are from the models that are trained under different frequencies of trades. Panel B plots the weekly CH4 alphas from long-short portfolios with equally-weighted stocks. The vertical bars around point estimations represent 95% confidence intervals. In panel A, the estimations are from stock-week panel regressions, and the standard errors are double clustered at the stock and week levels. In panel B, the estimations are from time-series regressions, and I report the Newey-West standard errors with 12 lags (weeks).
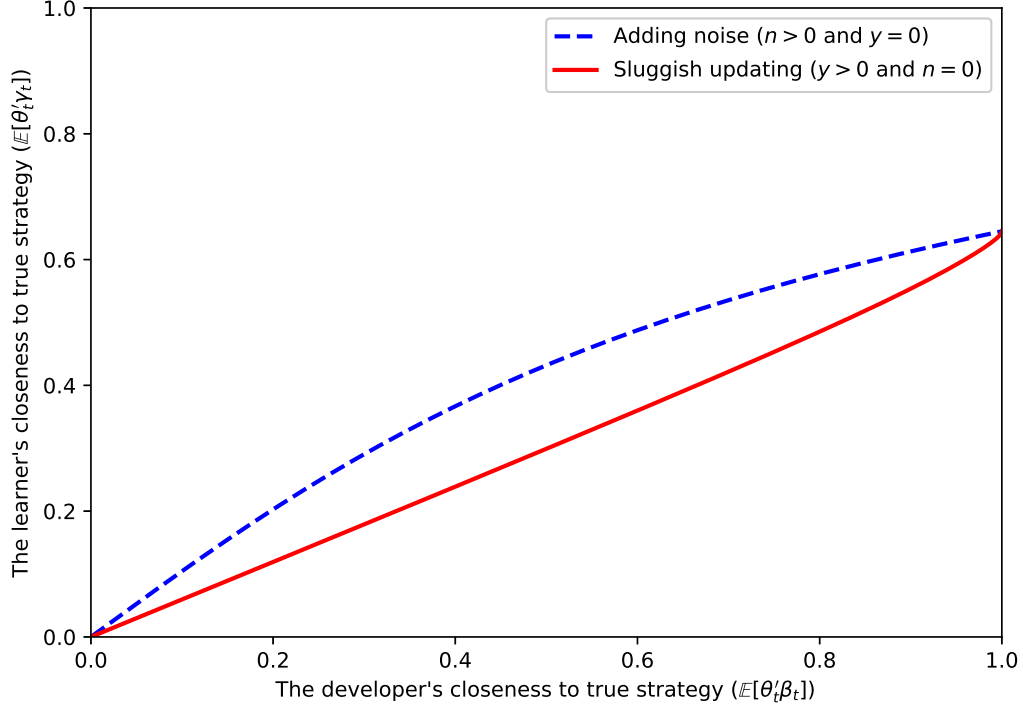
Figure 7: Strategic deviation: Adding noise vs. Sluggish updating. This figure illustrates the developer's trade-off analysis for each type of strategic deviation: adding noise vs. sluggish updating. The x-axis is the developer's expected dividend payoff, $\mathbb{E}[\theta_t'\beta_t]$, reflecting the cost of the strategic deviation, and the y-axis is the learner's expected dividend payoff, $\mathbb{E}[\theta_t'\gamma_t]$, reflecting the benefit of the strategic deviation. The blue dotted line represents the case of adding noise (i.e., $n > 0$ and $y = 0$), and the red solid line is for sluggish updating (i.e., $y > 0$ and $n = 0$). I draw the lines by varying the developer's strategy and considering the optimal learning rate for the learner at each point. I set $\rho = 0.921$ and $\sigma_e^{-1} = 0.5$.
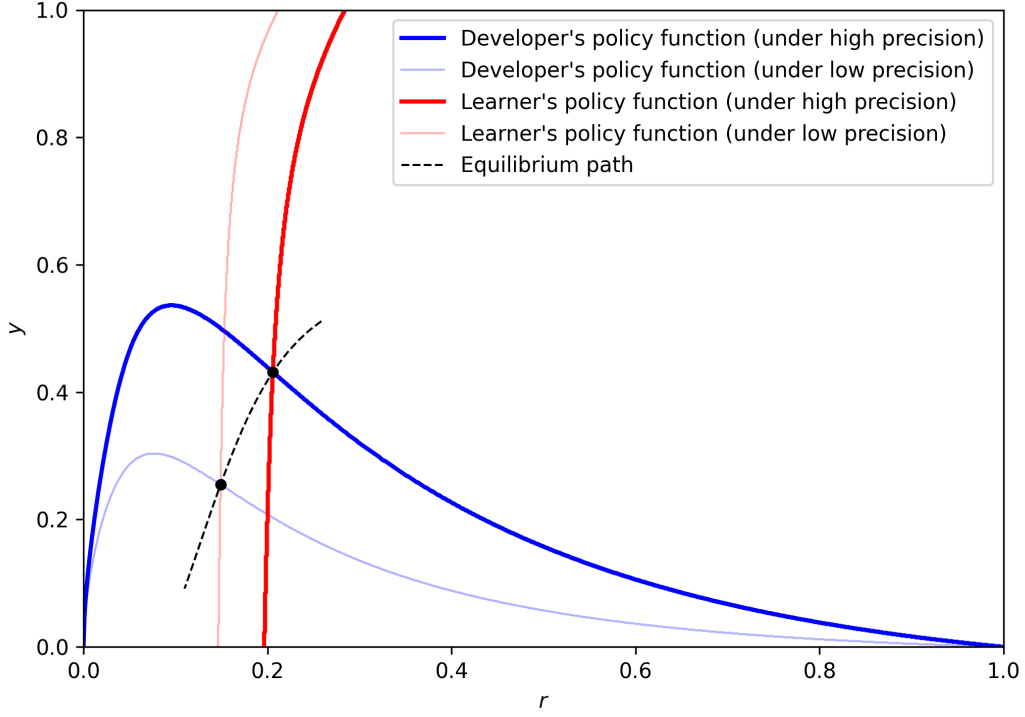
Figure 8: Best responses and equilibrium. This figure plots the developer's and the learner's policy functions and the equilibrium generated by their intersection. The x-axis is the learner's decision variable r, and the y-axis is the developer's decision variable y (note that Proposition 1 shows $n^* = 0$ and x is uniquely determined by the constraint $\left(\frac{1+\rho y}{1-\rho y}\right) x^2 + y^2 = 1$ given the value of y). The blue line represents the developer's policy function mapping from r to y, and the red line represents the learner's policy function mapping from y to r. The black points indicate the equilibrium, and the black dotted line shows the equilibrium path when the learner's precision σ_e^{-1} changes. I set $\sigma_e^{-1} = 0.5$ for the case of high precision, $\sigma_e^{-1} = 1/3$ for the case of low precision, and $\rho = 0.921$.
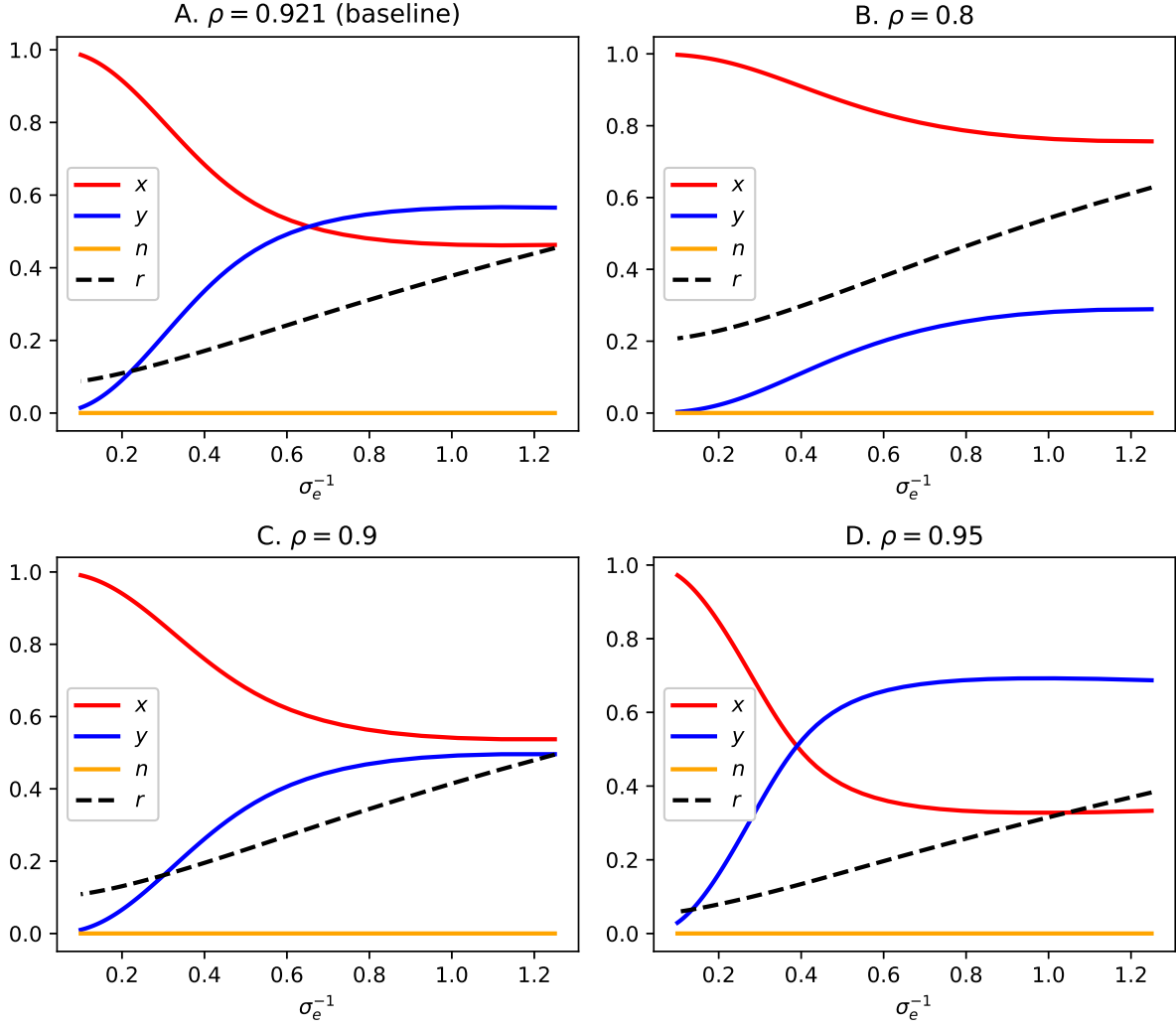
Figure 9: Equilibrium decisions as functions of the learner's precision. This figure plots the developer's decision variables, x, y, n, and the learner's decision variable, r, as functions of the learner's precision, σ_e^{-1}. I set $\rho = 0.921, 0.8, 0.9, 0.95$ in Panel A, B, C, and D, respectively.
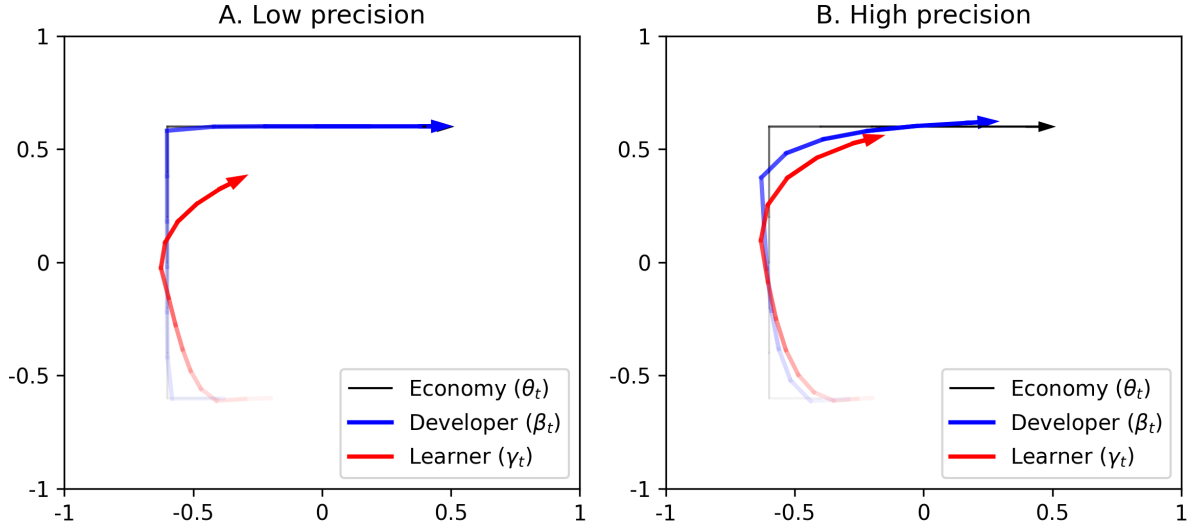
Figure 10: An illustration of the evolution of trading strategies. This figure visualizes the evolution of the trading strategies, including the true strategy θ_t (black line), the developer's strategy β_t (blue line), and the learner's strategy γ_t (red line) under low or high learning precision σ_e^{-1}. I set the dimension $K = 3$ and plot their first two elements in the figure (recall that θ_t, β_t, and γ_t are vectors with unit length). To generate the strategy paths, I first set the process of θ_t as shown in the figure. Then, I generate the paths of β_t and γ_t based on the developer's and learner's equilibrium decisions. I set $\hat{e}_t$ to zero to reflect the average path of the learner's strategy. I set $\sigma_e^{-1} = 0.2$ for the case of low precision, $\sigma_e^{-1} = 1$ for the case of high precision, and $\rho = 0.921$.
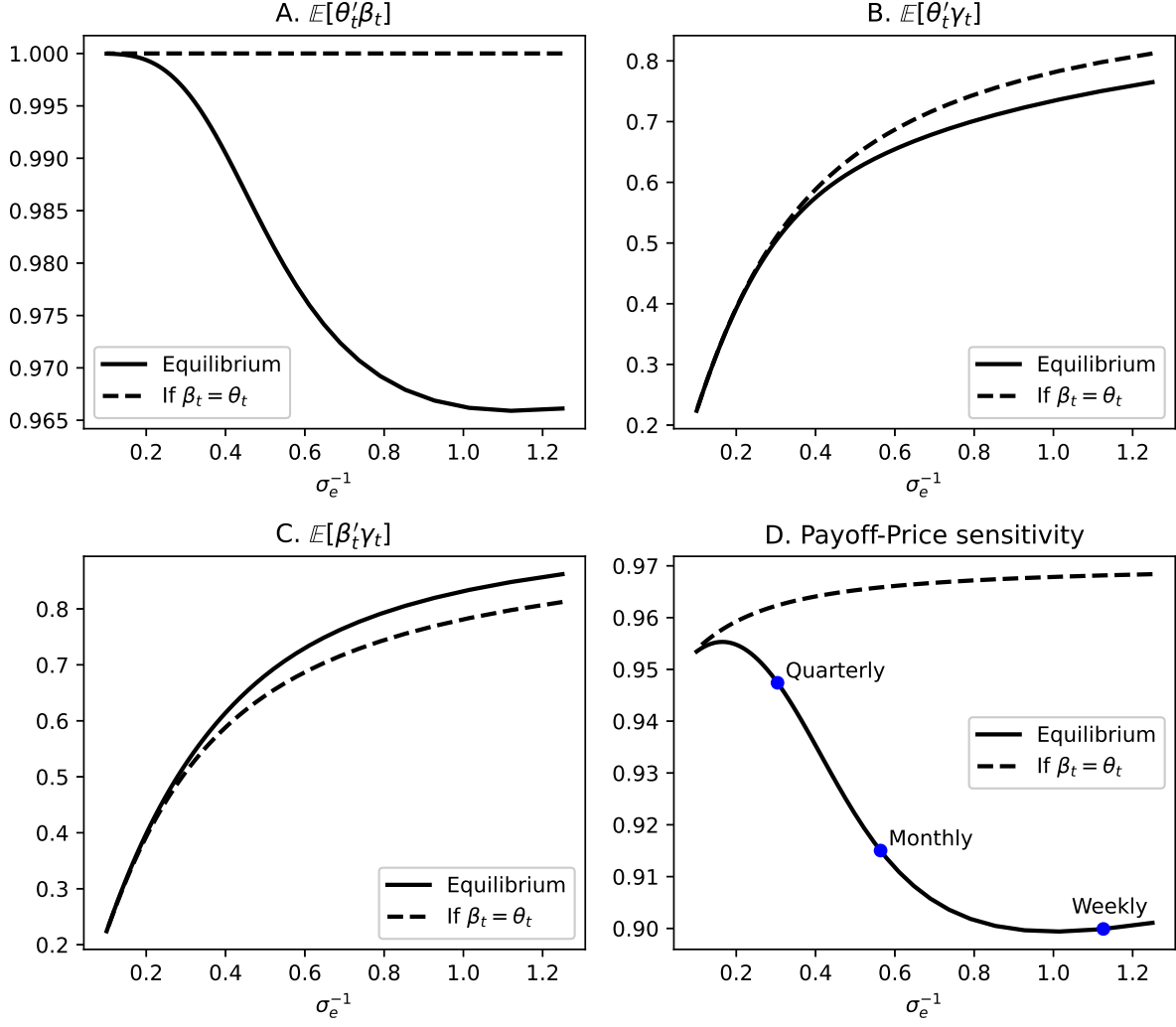
Figure 11: Equilibrium outcomes as functions of the learner's precision. This figure plots the developer's expected dividend payoff $\mathbb{E}[\theta_t'\beta_t]$ (in Panel A), the learner's expected dividend payoff $\mathbb{E}[\theta_t'\gamma_t]$ (in Panel B), the inner product between the two players' strategies $\mathbb{E}[\beta_t'\gamma_t]$ (in Panel C), and the payoff-price sensitivity (in Panel D) as functions of the learner's precision, σ_e^{-1}. The solid line represents equilibrium outcomes with strategic interaction, while the dashed line is for the case without strategic interaction by assuming the developer never deviates from the true strategy, i.e., $\beta_t = \theta_t$. I set $\lambda = 1$, $\sigma_\xi = 0.344$, and $\rho = 0.921$.
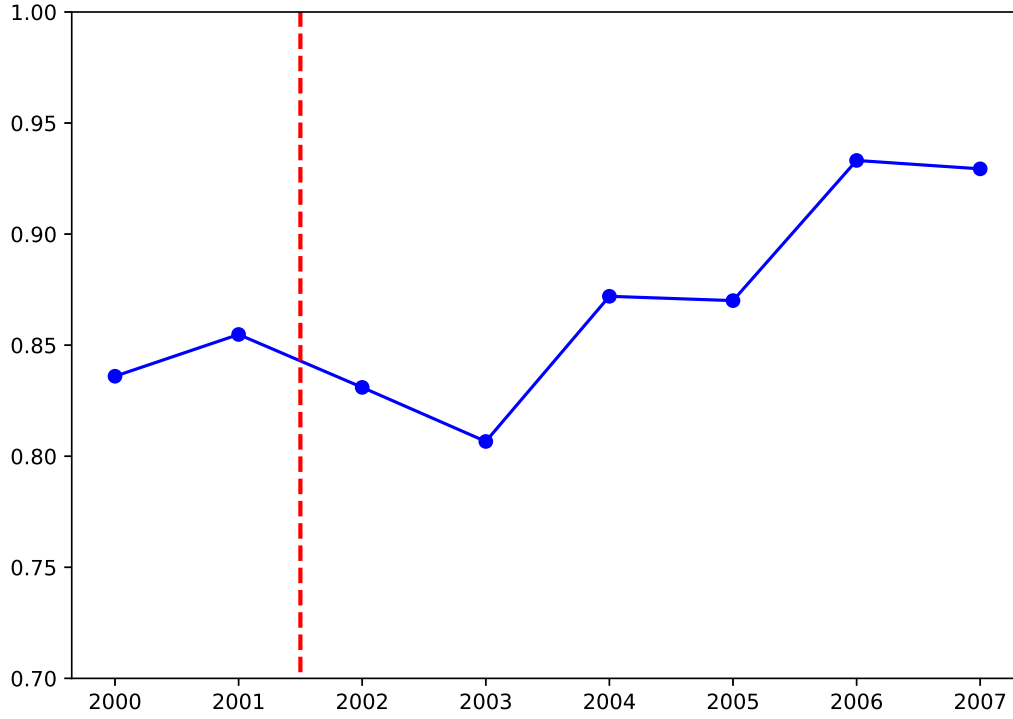
Figure 12: Client funds' trading strategy persistence. This figure plots the time series of the correlations of trading strategies between the current year and the previous year for the aggregate client funds. I estimate trading strategies as a function, $f(X)$, mapping stock characteristics X to trades. To calculate the correlations of trading strategies, first I use the Gradient Boosting Regression Tree (GBRT) to fit aggregate buy or sell trading volumes with (past) stock characteristics year-by-year. The sample construction and estimation procedure are similar to those mentioned in Table 7, but here I estimate on a year-by-year basis instead of using a rolling window. Let $f_y^{buy}(X)$ and $f_y^{sell}(X)$ denote the fitted trading strategies in year y for buy and sell trades, respectively. I calculate the trading strategy correlation across two adjacent years using $(Corr[f_y^{buy}(X), f_{y-1}^{buy}(X)] + Corr[f_y^{sell}(X), f_{y-1}^{sell}(X)])/2$, where I randomly draw X from the sample, i.e., to maintain the correlation structure among stock characteristics. The vertical dashed line splits the pre- and post-periods.

Appendix for "Disclosure costs beyond simple mimicry: Machine learning and reverse-engineering strategies"

Xudong Wen

A   Matching Abel Noser data to other databases

I match Abel Noser clients to funds in Thomson Reuters/CRSP by jointly considering (i) names and (ii) the similarity of trading behavior. The procedure proceeds as follows.

First, I generate a list of potential matches by performing fuzzy name matching between Abel Noser client names (variable *manager* and *reportedmanager*) to fund names in CRSP (variable *fund_name*, *mgmt_name*, and *mgr_name*) as well as in Thomson Reuters (variable *fundname* in the s12names table). The purpose of this step is to identify candidate pairs for further refinement. To ensure inclusivity at this stage, I apply a relatively low threshold for fuzzy matching and retain all pairs with a name match, for example, a match is recorded if *reportedmanager* in the Abel Noser data and *mgmt_name* in CRSP are similar.

Second, I refine the set of matched pairs by assessing the similarity of their trading behaviors. Specifically, let $\Delta S_{i,t,k}^{TFN}$ denote the quarterly share changes, as reported by Thomson Reuters, for fund i in quarter t, and stock k, and let $\Delta S_{j,t,k}^{AN}$ denote the cumulative share changes from Abel Noser data for client j in quarter t and stock k. I compute a percentage deviation score for each stock as $|\Delta S_{j,t,k}^{AN} - \Delta S_{i,t,k}^{TFN}|/|\Delta S_{i,t,k}^{TFN}|$. For a pair to be considered a match, I require that at least 80% of stocks have deviation scores below 10% or at least 60% of stocks have deviation scores below 1%. Applying this criterion yields a set of 91 matched mutual funds.

Finally, I supplement the matched list by adding funds with almost exactly matched names, defined as cases where both variables *manager* and *reportedmanager* can be matched with a fuzzy-matching score above 95 (a perfect match will score 100). Incorporating those funds allows me to study clients' strategic behaviors of submitting orders to Abel Noser. This step adds 60 matched mutual funds, resulting in a final total of 151 matched mutual funds.

B Characteristics list

Table A-1 provides the list of characteristics used in the machine-learning exercise of uncovering trading strategies.

Table A-1: Characteristics list

No.	Variable	Label	Description
1	sz	size	log(last-month market capitalization)
2	ret_1w	past-1-week cumulative return	cumulative return in the past one week
3	ret_2w	past-2-week cumulative return	cumulative return in the past two weeks
4	ret_4w	past-4-week cumulative return	cumulative return in the past four weeks
5	ret_8w	past-8-week cumulative return	cumulative return in the past eight weeks
6	ret_12w	past-12-week cumulative return	cumulative return in the past twelve weeks
7	dolvol_1w	past-1-week total dollar volume	log(1 + sum of dollar volume in the past one week)
8	dolvol_2w	past-2-week total dollar volume	log(1 + sum of dollar volume in the past two weeks)
9	dolvol_4w	past-4-week total dollar volume	log(1 + sum of dollar volume in the past four weeks)
10	turnover_1w	past-1-week average turnover	average of turnover ratios in the past one week
11	turnover_2w	past-2-week average turnover	average of turnover ratios in the past two weeks
12	turnover_4w	past-4-week average turnover	average of turnover ratios in the past four weeks
13	baspread_1w	past-1-week average bid-ask spread	average of bid-ask spread (divided by the middle price) in the past one week
14	baspread_2w	past-2-week average bid-ask spread	average of bid-ask spread (divided by the middle price) in the past two weeks
15	baspread_4w	past-4-week average bid-ask spread	average of bid-ask spread (divided by the middle price) in the past four weeks
16	amihud_1w	past-1-week average Amihud ratio	average of Amihud ratios in the past one week
17	amihud_2w	past-2-week average Amihud ratio	average of Amihud ratios in the past two weeks
18	amihud_4w	past-4-week average Amihud ratio	average of Amihud ratios in the past four weeks

No.	Variable	Label	Description
19	beta_mkt	market beta	estimate the four betas with daily returns and a one-year rolling window
20	beta_smb	SMB beta	estimate the four betas with daily returns and a one-year rolling window
21	beta_hml	HML beta	estimate the four betas with daily returns and a one-year rolling window
22	beta_mom	MOM beta	estimate the four betas with daily returns and a one-year rolling window
23	bm	book-to-market ratio	book value of equity (SEQQ, or CEQQ + PSTKQ, or ATQ - LTQ) divided by last-month market value of equity
24	dm	debt-to-market ratio	debt value (DLCQ + DLTTQ) divided by last-month market value of equity
25	cp	cash-flow-to-price ratio	cash flows (IBQ + DPQ, or IBQ if DPQ is missing) divided by last-month market value of equity
26	sp	sales-to-price ratio	sales (SALEQ) divided by last-month market value of equity
27	ep	earning-to-price ratio	earnings (IBQ) divided by last-month market value of equity
28	roa	return on assets	earnings (IBQ) divided by last-quarter total assets (ATQ)
29	roe	return on equity	earnings (IBQ) divided by last-quarter book value of equity
30	agr	asset growth	annual percent change in total assets (ATQ)
31	meanrec	mean analyst recommendations	mean analyst recommendations
32	chgrec	change of analyst recommendations	change of analyst recommendations relative to the past-one-quarter average
33	chgfeps_ltg0	change of EPS forecasts: LTG growth	change of EPS forecasts relative to the past-one-quarter average, FPI = 0

3

(continued)

No.	Variable	Label	Description
34	chgfeps_ann1	change of EPS forecasts: annual	change of EPS forecasts relative to the past-one-quarter average, FPI = 1
35	chgfeps_qtr1	change of EPS forecasts: 1st quarter	change of EPS forecasts relative to the past-one-quarter average, FPI = 6
36	chgfeps_qtr2	change of EPS forecasts: 2nd quarter	change of EPS forecasts relative to the past-one-quarter average, FPI = 7
37	chgfeps_qtr3	change of EPS forecasts: 3rd quarter	change of EPS forecasts relative to the past-one-quarter average, FPI = 8
38	chgfeps_qtr4	change of EPS forecasts: 4th quarter	change of EPS forecasts relative to the past-one-quarter average, FPI = 9
39	ino_s12	institutional ownership (s12)	shares held by s12 institutions divided by shares outstanding
40	chg_ino_s12	change of institutional ownership (s12)	quarterly change of the above institutional ownership (s12)
41	ino_s34	institutional ownership (s34)	shares held by s34 institutions divided by shares outstanding
42	chg_ino_s34	change of institutional ownership (s34)	quarterly change of the above institutional ownership (s34)
43	ino_hf	institutional ownership (hedge fund)	shares held by hedge funds divided by shares outstanding
44	chg_ino_hf	change of institutional ownership (hedge fund)	quarterly change of the above institutional ownership (hedge fund)
45	sint	short interest	short interest divided by shares outstanding
46	chg_sint	change of short interest	monthly change of short interest
47	gics_energy	industry indicator: energy	equals one if two-digit GICS code = 10, and zero otherwise
48	gics_materi	industry indicator: materials	equals one if two-digit GICS code = 15, and zero otherwise
49	gics_indust	industry indicator: industrials	equals one if two-digit GICS code = 20, and zero otherwise

4

(continued)

(continued)

No.	Variable	Label	Description
50	gics_condis	industry indicator: consumer discretionary	equals one if two-digit GICS code = 25, and zero otherwise
51	gics_consta	industry indicator: consumer staples	equals one if two-digit GICS code = 30, and zero otherwise
52	gics_health	industry indicator: health care	equals one if two-digit GICS code = 35, and zero otherwise
53	gics_financ	industry indicator: financials	equals one if two-digit GICS code = 40, and zero otherwise
54	gics_infotc	industry indicator: information technology	equals one if two-digit GICS code = 45, and zero otherwise
55	gics_commun	industry indicator: communication services	equals one if two-digit GICS code = 50, and zero otherwise
56	gics_utilit	industry indicator: utilities	equals one if two-digit GICS code = 55, and zero otherwise
57	gics_reales	industry indicator: real estate	equals one if two-digit GICS code = 60, and zero otherwise
58	week_mst	month-beginning indicator	equals one if the week is the month beginning, and zero otherwise
59	week_med	month-ending indicator	equals one if the week is the month ending, and zero otherwise
60	week_qst	quarter-beginning indicator	equals one if the week is the quarter beginning, and zero otherwise
61	week_qed	quarter-ending indicator	equals one if the week is the quarter ending, and zero otherwise

C Other empirical results

C.1 Separating treated vs. control groups

In Table A-2, I repeat the main difference-in-differences analysis by separating the treated and control groups. The results confirm that the treatment effect indeed comes from the treated group. And taking the difference between the treated and the control groups helps improve statistical power.

Table A-2: Disclosure effect on fund performance: Treated vs. Control group

	Risk-adjusted fund returns			
	Excess return	CAPM alpha	FF3 alpha	CH4 alpha
Treated - Control	-0.385**	-0.408**	-0.305***	-0.338***
	[-2.07]	[-2.52]	[-3.25]	[-4.19]
Treated	-0.259	-0.461	-0.368*	-0.394**
	[-0.30]	[-1.20]	[-1.98]	[-2.00]
Control	0.126	-0.053	-0.063	-0.055
	[0.17]	[-0.21]	[-0.46]	[-0.38]

C.2 Robustness: stricter matching requirement

In the main results, the treated funds are from the list of 151 matched mutual funds, as identified through the matching procedure described in Section A. Here, I consider a stricter matching requirement by excluding the supplementary matches added in the third step. Specifically, I restrict the treated funds to the subset of 91 mutual funds matched using only the initial two steps. As Table A-3 shows, results are robust for the stricter matching requirement.

Table A-3: Disclosure effect: A sub-sample with stricter matching requirement

	Risk-adjusted fund returns			
	Excess return	CAPM alpha	FF3 alpha	CH4 alpha
Treatment effect	-0.316**	-0.353***	-0.384***	-0.474***
	[-2.33]	[-3.87]	[-4.64]	[-4.37]
Observations	120	120	120	120

C.3 Robustness: uncovering trading strategies based on OLS

In the main results, I uncover trading strategies based on a non-linear machine-learning method: Gradient Boosted Regression Trees (GBRT). As a robustness check, here I consider uncovering strategies based on linear regression. The results on trading predictability are shown in Table A-4, and the results on predatory profitability are in Table A-5. Overall, it suggests that uncovering trading strategies, despite using linear regressions, is still feasible and profitable.

Table A-4: Trading predictability: Uncovering strategies based on OLS

	(1)	(2)	(3)
BuyScore	3.955***	2.800***	1.703***
	[11.16]	[6.63]	[3.05]
SellScore	-4.690***	-3.562***	-3.851***
	[-12.71]	[-7.26]	[-5.33]
Observations	465,353	465,334	465,265
Week FE	Yes	Yes	Yes
Stock FE		Yes	
Stock x Year FE			Yes

Table A-5: Predatory profitability: Uncovering strategies based on OLS

	Long-short portfolio abnormal returns (weekly)			
Portfolio weight	Excess return	CAPM alpha	FF3 alpha	CH4 alpha
EW	0.202	0.260**	0.278***	0.199**
	[1.38]	[2.42]	[2.65]	[2.13]
VW	0.251	0.323**	0.366**	0.282**
	[1.29]	[2.23]	[2.43]	[2.27]
Observations	313	313	313	313

C.4 Quit rates for client funds

I estimate the quit rates over the period 1998-2007 using the following fund-year panel regression:

$$IsEndYear_{i,y} = \beta_0 + \beta_1 \times \mathbb{I}_{\{Year=2002\}} + \beta_2 \times \mathbb{I}_{\{Year>2002\}} + \text{BegYear FE} + \epsilon_{i,y}.$$

To calculate the quit rate with the denominator equal to the number of clients in that year, fund i is included in the sample only during its period of being a client. This is to ensure that the number of observations in year y equals the number of clients in that year. $IsEndYear_{i,y}$ equals one if year y is the last year for fund i being a client, and zero otherwise. BegYear FE is the fixed effects on the first year of being a client. The coefficient β_1 (β_2) captures the change in quit rate during 2002 (from 2003 to 2007) relative to the one during the pre-period. The vertical bars around point estimations represent 95% confidence intervals. The standard errors are clustered at the year level.

Figure A-1 plots the estimation results. Overall, there is no evidence that quitting Abel Noser is a major response for client funds. This could be because client funds have a strong demand for Abel Noser's services, for example, to demonstrate to their investors about "best execution" for their transactions. Also, given that past trades were already released, the marginal benefits of quitting Abel Noser could be small.
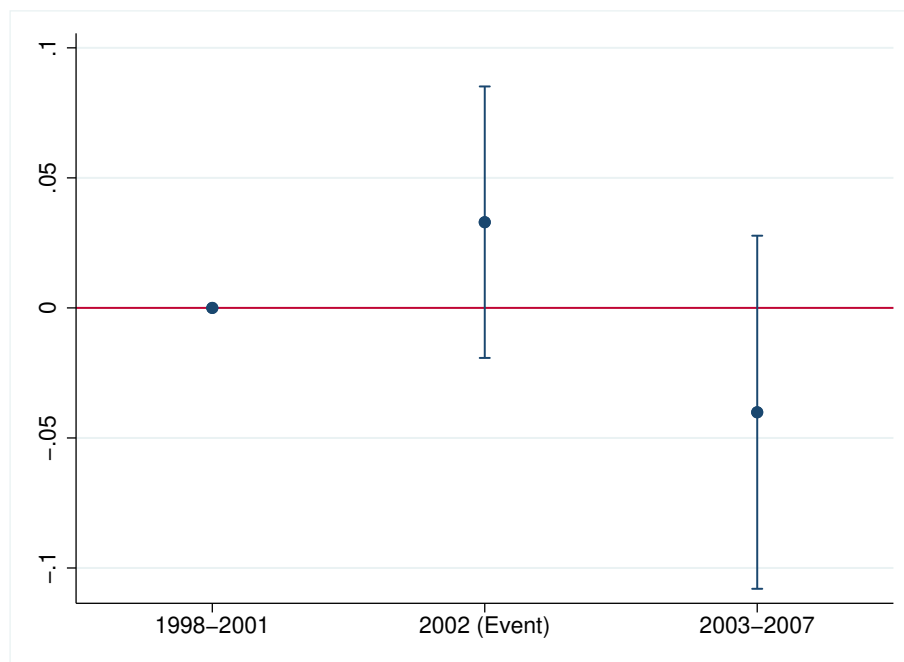

Figure A-1: Quit rates for client funds

D   Details related to the model

D.1   Proof of Lemma 1

Consider the system with the following stationary processes:

$$\boldsymbol{\theta}_t = \rho\boldsymbol{\theta}_{t-1} + \sqrt{1-\rho^2}\boldsymbol{u}_t, \tag{A-1}$$

$$\boldsymbol{\beta}_t = x\boldsymbol{\theta}_t + y\boldsymbol{\beta}_{t-1} + z\boldsymbol{v}_t, \tag{A-2}$$

$$\boldsymbol{b}_t = (1-r)\boldsymbol{b}_{t-1} + r(\boldsymbol{\beta}_{t-1} + \hat{\boldsymbol{e}}_{t-1}), \tag{A-3}$$

$$\boldsymbol{\gamma}_t = \frac{\boldsymbol{b}_t}{\sqrt{\boldsymbol{b}_t'\boldsymbol{b}_t}}, \tag{A-4}$$

where $\boldsymbol{u}_t, \boldsymbol{v}_t \overset{iid.}{\sim} \mathcal{N}(\mathbf{0}, K^{-1}\boldsymbol{I}_K)$ and $\hat{\boldsymbol{e}}_t \overset{iid.}{\sim} \mathcal{N}(\mathbf{0}, \sigma_{\hat{e}}^2 K^{-1}\boldsymbol{I}_K)$. The error terms $\boldsymbol{u}_t$, $\boldsymbol{v}_t$, and $\hat{\boldsymbol{e}}_t$ are mutually independent to each other. Note that for $k \geq 0$, we have

$$\mathbb{E}(\boldsymbol{\theta}_t'\boldsymbol{\theta}_{t-k}) = \rho^k. \tag{A-5}$$

Define the series of moments

$$f_k = \mathbb{E}(\boldsymbol{\theta}_t'\boldsymbol{\beta}_{t-k}), \tag{A-6}$$

$$\varphi_k = \mathbb{E}(\boldsymbol{\beta}_t'\boldsymbol{\beta}_{t-k}). \tag{A-7}$$

Multiplying $\boldsymbol{\beta}_{t-k}$ on both sides of Eq.(A-1) and taking expectation give

$$f_k = \rho f_{k-1}. \tag{A-8}$$

Multiplying $\boldsymbol{\theta}_t$ on both sides of Eq.(A-2) and taking expectation give

$$f_0 = x + y f_1. \tag{A-9}$$

Combining Eq.(A-8) and Eq.(A-9) gives

$$f_k = \frac{x\rho^k}{1 - \rho y}. \tag{A-10}$$

Therefore, we have

$$\lim_{K \to \infty} \boldsymbol{\theta}_t'\boldsymbol{\beta}_t = f_0 = \frac{x}{1 - \rho y}. \tag{A-11}$$

Multiplying β_{t-k} on both sides of Eq.(A-2) and taking expectation give

$$\varphi_k = x f_k + y \varphi_{k-1}. \tag{A-12}$$

Because (x, y, z) is chosen to ensure $\mathbb{E}(\beta_t' \beta_t) = 1$, we have $\varphi_0 = 1$. Recursively substituting Eq.(A-12) gives

$$
\begin{aligned}
\varphi_k &= x f_k + y x f_{k-1} + y^2 \varphi_{k-2} \\
&= x f_k + y x f_{k-1} + y^2 x f_{k-2} + y^3 \varphi_{k-3} \\
&= x f_k + y x f_{k-1} + y^2 x f_{k-2} + \cdots + y^{k-1} x f_1 + y^k \varphi_0 \\
&= \frac{x^2}{1 - \rho y} (\rho^k + y \rho^{k-1} + y^2 \rho^{k-2} + \cdots + y^{k-1} \rho) + y^k \\
&= \frac{x^2 \rho}{(1 - \rho y)} \frac{(\rho^k - y^k)}{(\rho - y)} + y^k \\
&= A \rho^k + (1 - A) y^k, \tag{A-13}
\end{aligned}
$$

where

$$A = \frac{x^2 \rho}{(1 - \rho y)(\rho - y)}. \tag{A-14}$$

Because of the restriction $\mathbb{E}(\beta_t' \beta_t) = 1$, (x, y, z) needs to satisfy

$$
\begin{aligned}
\mathbb{E}(\beta_t' \beta_t) &= \mathbb{E}([x \theta_t + y \beta_{t-1} + z v_t]'[x \theta_t + y \beta_{t-1} + z v_t]) \\
1 &= x^2 + y^2 + z^2 + 2xy f_1 \\
1 &= x^2 + y^2 + z^2 + \frac{2x^2 y \rho}{1 - \rho y}, \tag{A-15}
\end{aligned}
$$

which implies

$$z^2 = 1 - \left(\frac{1 + \rho y}{1 - \rho y} \right) x^2 - y^2. \tag{A-16}$$

To calculate $\lim_{K \to \infty} \theta_t' \gamma_t$, we need $\mathbb{E}(\theta_t' b_t)$ and $\mathbb{E}(b_t' b_t)$. First, note that

$$b_t = r[(\beta_{t-1} + \hat{e}_{t-1}) + (1 - r)(\beta_{t-2} + \hat{e}_{t-2}) + (1 - r)^2(\beta_{t-3} + \hat{e}_{t-3}) + \cdots]. \tag{A-17}$$

Therefore, we have

$$
\begin{aligned}
\mathbb{E}(\boldsymbol{\theta}_t' \boldsymbol{b}_t) &= r[f_1 + (1-r)f_2 + (1-r)^2 f_3 \cdots] \\
&= \frac{xr}{1-\rho y}[\rho + (1-r)\rho^2 + (1-r)^2\rho^3 + \cdots] \\
&= \frac{xr\rho}{(1-\rho y)[1-\rho(1-r)]}.
\end{aligned}
\tag{A-18}
$$

and

$$
\begin{aligned}
\mathbb{E}(\boldsymbol{b}_t' \boldsymbol{b}_t) &= r^2(1+\sigma_e^2)[1 + (1-r)^2 + (1-r)^4 + \cdots] + \\
&\quad 2r^2\varphi_1[(1-r) + (1-r)^3 + (1-r)^5 + \cdots] + \\
&\quad 2r^2\varphi_2[(1-r)^2 + (1-r)^4 + (1-r)^6 + \cdots] + \cdots \\
&= \frac{r^2(1+\sigma_e^2)}{1-(1-r)^2} + \frac{2r^2}{1-(1-r)^2}[\varphi_1(1-r) + \varphi_2(1-r)^2 + \cdots] \\
&= \frac{r^2(1+\sigma_e^2)}{1-(1-r)^2} + \\
&\quad \frac{2r^2}{1-(1-r)^2}A[\rho(1-r) + \rho^2(1-r)^2 + \cdots] + \\
&\quad \frac{2r^2}{1-(1-r)^2}(1-A)[y(1-r) + y^2(1-r)^2 + \cdots] \\
&= \frac{r(1+\sigma_e^2)}{2-r} + \frac{2r}{2-r}\left[A\frac{\rho(1-r)}{1-\rho(1-r)} + (1-A)\frac{y(1-r)}{1-y(1-r)}\right]. \tag{A-19}
\end{aligned}
$$

Finally, we have

$$
\lim_{K\to\infty}\boldsymbol{\theta}_t'\boldsymbol{\gamma}_t = \frac{\mathbb{E}(\boldsymbol{\theta}_t'\boldsymbol{b}_t)}{\sqrt{\mathbb{E}(\boldsymbol{b}_t'\boldsymbol{b}_t)}} = \frac{x}{1-\rho y}\frac{\frac{r\rho}{1-\rho(1-r)}}{\sqrt{\frac{r(1+\sigma_e^2)}{2-r} + \frac{2r}{2-r}\left[A\frac{\rho(1-r)}{1-\rho(1-r)} + (1-A)\frac{y(1-r)}{1-y(1-r)}\right]}}, \tag{A-20}
$$

where $A = \frac{x^2\rho}{(1-\rho y)(\rho - y)}$.

D.2 Proof of Proposition 1

I prove by contradiction, that if $z > 0$, there is always a profitable deviation for the developer. Let's begin with a decision (x, y, z) with $z > 0$ and satisfying the constraint

$\left(\frac{1+\rho y}{1-\rho y}\right) x^2 + y^2 + z^2 = 1$. The developer's objective function is given by

$$F(x,y,z;r) = \frac{x}{1-\rho y}\left[1 - \frac{\frac{r\rho}{1-\rho(1-r)}}{\sqrt{\frac{r(1+\sigma_e^2)}{2-r} + \frac{2r}{2-r}\left[A\frac{\rho(1-r)}{1-\rho(1-r)} + (1-A)\frac{y(1-r)}{1-y(1-r)}\right]}}\right], \quad \text{(A-21)}$$

where $A = \frac{x^2\rho}{(1-\rho y)(\rho-y)}$.

If $F(x,y,z;r) < 0$, then (x,y,z) cannot be the best response, because the developer can always play $(1,0,0)$, which gives

$$\begin{aligned}
F(1,0,0;r) &= 1 - \frac{\frac{r\rho}{1-\rho(1-r)}}{\sqrt{\frac{r(1+\sigma_e^2)}{2-r} + \frac{2r}{2-r}\frac{\rho(1-r)}{[1-\rho(1-r)]}}} \\
&> 1 - \frac{\frac{r\rho}{1-\rho(1-r)}}{\sqrt{\frac{r}{2-r} + \frac{2r}{2-r}\frac{\rho(1-r)}{[1-\rho(1-r)]}}} \\
&= 1 - \frac{\frac{r\rho}{1-\rho(1-r)}}{\sqrt{\frac{r}{2-r}\frac{[1+\rho(1-r)]}{[1-\rho(1-r)]}}} \\
&= 1 - \frac{\rho\sqrt{r(2-r)}}{\sqrt{1-\rho^2(1-r)^2}} \\
&= 1 - \frac{\sqrt{\rho^2(2r-r^2)}}{\sqrt{(1-\rho^2)+\rho^2(2r-r^2)}} \\
&> 0. \qquad\qquad\qquad\qquad\qquad\qquad \text{(A-22)}
\end{aligned}$$

Therefore, we only need to consider the decision (x,y,z) with $F(x,y,z;r) \geq 0$. Next, let's consider an alternative decision $(x',y,0)$ satisfying the constraint

$$\left(\frac{1+\rho y}{1-\rho y}\right)(x')^2 + y^2 = 1, \qquad\qquad \text{(A-23)}$$

which gives $x' = \sqrt{\frac{(1-\rho y)(1-y^2)}{1+\rho y}}$. By construction, $x' > x$ because $(x')^2 - x^2 = \left(\frac{1-\rho y}{1+\rho y}\right)z^2 > 0$. To compare $F(x,y,z;r)$ and $F(x',y,0;r)$, first, we have

$$\frac{x'}{1-\rho y} > \frac{x}{1-\rho y}. \qquad\qquad \text{(A-24)}$$

Next, for the term $A\frac{\rho(1-r)}{1-\rho(1-r)} + (1-A)\frac{y(1-r)}{1-y(1-r)}$, there are three possibilities:

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- If $y = \rho$, then it collapses to $\frac{\rho(1-r)}{1-\rho(1-r)}$, which is irrelevant to x and z;

- If $y < \rho$, we have $A(x') > A(x)$ and $\frac{\rho(1-r)}{1-\rho(1-r)} > \frac{y(1-r)}{1-y(1-r)}$, which implies $A(x')\frac{\rho(1-r)}{1-\rho(1-r)} + [1 - A(x')]\frac{y(1-r)}{1-y(1-r)} > A(x)\frac{\rho(1-r)}{1-\rho(1-r)} + [1 - A(x)]\frac{y(1-r)}{1-y(1-r)}$;

- If $y > \rho$, we have $A(x') < A(x)$ and $\frac{\rho(1-r)}{1-\rho(1-r)} < \frac{y(1-r)}{1-y(1-r)}$, which implies $A(x')\frac{\rho(1-r)}{1-\rho(1-r)} + [1 - A(x')]\frac{y(1-r)}{1-y(1-r)} > A(x)\frac{\rho(1-r)}{1-\rho(1-r)} + [1 - A(x)]\frac{y(1-r)}{1-y(1-r)}$.

Taken together, we have

$$
A(x')\frac{\rho(1-r)}{1-\rho(1-r)} + [1 - A(x')]\frac{y(1-r)}{1-y(1-r)} \geq
$$
$$
A(x)\frac{\rho(1-r)}{1-\rho(1-r)} + [1 - A(x)]\frac{y(1-r)}{1-y(1-r)} \tag{A-25}
$$

Therefore, given that $F(x,y,z;r) \geq 0$, Eq.(A-24) and Eq.(A-25) imply

$$
F(x',y,0;r) > F(x,y,z;r), \tag{A-26}
$$

which finishes the proof.