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ABSTRACT

We hypothesize that later sunsets and clock changes disrupt sleep, impairing investor's information processing and trading performance. Using geographic and seasonal sunset differences and daylight saving time transitions, we find that later sunset predicts lower abnormal returns on retail trades. Later sunset is also associated with weaker trading responses to earnings announcements and stronger tilts against earnings surprises. Later sunset is more negatively associated with performance for announcement-stock trades than for other trades. Together, these findings indicate that environmental factors influence the information processing and trading outcomes of investors.

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1. Introduction

We hypothesize that sleep disruption reduces investors' mental alertness, making them less effective at processing information and more prone to predictable trading mistakes. This is motivated by research documenting that sleep loss reduces the cognitive resources available for noticing relevant information and evaluating its implications and impairs decision-making and task performance.¹

To the extent that stock returns are unpredictable, it would be surprising that impaired information processing would result in systematic underperformance. However, there is evidence that the purchases of retail investors systematically underperform their sales (Odean 1999; Barber and Odean 2000). Furthermore, stock returns are partly predictable, and investors sometimes trade on the wrong side of return anomalies. For example, earnings surprises positively predict subsequent returns—a phenomenon known as post-earnings-announcement drift (Bernard and Thomas 1989, 1990), so trading as a contrarian to earnings surprises would predictably be unprofitable. Sleep disruption could impair performance by making investors less effective at avoiding such mistakes.

Much of behavioral economics and finance models have focused on static heuristics and biases, though an exception is the literature on the effects of mood variation on stock returns (Hirshleifer and Shumway 2003; Kamstra, Kramer, and Levi 2003; Edmans, Garcia, and Norli 2007; Edmans et al. 2022). Our focus on impairment of cognitive capacity for processing information differs from mood-based effects centered on changes in the perceived attractiveness of risky assets and tolerance for risk.

A growing literature in economics documents that sleep is a meaningful input into cognitive performance and economic decisions. Schilbach, Schofield, and Mullainathan (2016) treat sleep deprivation as a tax on cognitive bandwidth that distorts judgment and choice. Bessone et al. (2021) find that extending nighttime sleep among low-income workers in Chennai did not improve cognition or productivity, while workplace naps did, and discuss sleep timing and quality as possible explanations. Gibson and Shrader (2018) and Giuntella and Mazzonna (2019) use sunset time and U.S. time-zone borders to identify effects of sleep on earnings and productivity.

¹ Such effects are reported in Pilcher and Huffcutt (1996), Dinges et al. (1997), Harrison and Horne (2000), Van Dongen et al. (2003), Alhola and Polo-Kantola (2007), Lim and Dinges (2010), and Hudson et al. (2020).

Our empirical approach is motivated by the relationship between sleep and evening light. Later sunsets can delay sleep when work and other morning obligations remain fixed, reducing sleep duration or shifting biological timing relative to social schedules (Roenneberg, Kumar, and Merrow 2007; Wittmann et al. 2006).). Time-zone boundaries and Daylight Saving Time (DST) transitions also change when daylight occurs by the local clock. Our proxies do not distinguish these dimensions of sleep, so we use “sleep disruption” as an umbrella term and interpret each estimate as the reduced-form effect of the exposure.

Following Gibson and Shrader (2018) and Giuntella and Mazzonna (2019), we perform refined tests using these exposures to test whether sleep-disrupting schedules impair trading performance. The prediction is that performance will be poorer at later sunsets.

Retail investors provide a useful target for testing this hypothesis. Their geographic dispersion exposes them to different sunset times and clock schedules, and brokerage records can be used to observe their trading decisions and subsequent returns. Our tests apply retail trading records from 1991–1996.

We evaluate trading decisions using subsequent abnormal returns on portfolios formed from investor net purchases and sales. The main geographic and seasonal tests use monthly returns. Daylight Saving Time and earnings-announcement analyses use 21-day forward returns to capture performance immediately after the relevant trades.

We test the hypothesis that sleep disruption impairs performance in four contexts that generate different patterns of daylight timing relative to social schedules. The designs examine changes in sunset within ZIP codes over time relative to contemporaneous changes elsewhere, variation across time-zone boundaries, seasonal differences across latitudes, and abrupt clock adjustments. Each addresses different sources of geographic or temporal confounding. Section 3 develops their identifying assumptions and validation tests.

The first test concerns whether trading performance within a ZIP deteriorates when its sunset becomes later relative to other locations. We use ZIP-month portfolios with ZIP and year-month fixed effects, comparing within-ZIP changes with contemporaneous changes elsewhere. Consistent with the hypothesis, a one-hour later sunset is associated with 29.3 basis points lower subsequent monthly trading performance ($t = -2.50$). The estimate is robust to controlling for the seasonal affective disorder measure of Kamstra, Kramer, and Levi (2003), whose predicted effect

falls in fall and winter rather than spring and summer. Much of the identifying variation reflects latitude-dependent seasonality, linking this test to the North–South comparison below.

The second test approach examines whether performance is worse where evening light falls later relative to clock-based schedules using a regression discontinuity design (RDD). Nearby households on opposite sides of a time-zone boundary experience similar astronomical daylight, but sunset occurs about an hour later by the clock on the eastern side. The sleep-disruption hypothesis therefore predicts lower performance on that side. The estimated discontinuity in monthly Fama-French 3-factor trading performance is -40.4 basis points ($p = 0.074$).

The hypothesis also predicts a larger adverse border effect when sunsets are already late. An additional hour of evening light should interfere more with sleep when it extends closer to bedtime. The estimated summer-minus-winter difference in the border effect is -0.939 percentage points per month ($p = 0.049$), supporting this prediction. Section 3.2 explains how the seasonal comparison also addresses additive permanent cross-border differences.

Households and time-zone borders are not randomly located, so verifying geographic comparability is essential. To address such endogeneity, Section 3.2 reports household-characteristic comparisons, additional income controls and robustness checks, within-state estimates, placebo boundaries, and bandwidth sensitivity. Income is discontinuous at the boundary, but the performance estimate changes little after controlling for income and remains negative in both income subgroups. These checks support the conclusion of lower abnormal trading performance on the eastern, later-sunset side of the time-zone boundary

Our third approach uses the seasonal reversal in sunset differences across latitudes. Northern locations have relatively later sunsets in summer and earlier sunsets in winter, so the sleep disruption hypothesis predicts that the relative performance of northern investors deteriorates from winter to summer. Comparing portfolios within the same time zone and similar longitude ranges, we estimate a North $\times$ Summer interaction of -64.4 basis points per month ($t = -2.36$). This supports the predicted seasonal pattern in a sample extending beyond the time-zone borders.

The fourth approach examines an abrupt potential source of sleep disruption, building on the approach of Kamstra, Kramer, and Levi (2000). The spring Daylight Saving Time transition moves clock-based schedules earlier relative to biological time and shortens the overnight interval for someone maintaining the same clock-based sleep schedule. This motivates a prediction of

poorer performance after the spring adjustment. The fall transition lengthens that interval while also changing the schedule, giving an ambiguous prediction.

We compare changes in trading performance for DST-observing households with changes for households in nonobserving locations on the same dates. The spring DiD estimate for 21-day forward trading performance is -3.973 percentage points ($p = 0.021$), consistent with the predicted deterioration. The fall estimate (for which the prediction is ambiguous) is statistically insignificant. Section 3.4 discusses the comparatively large spring magnitude and its sensitivity to the small control portfolio.

The hypothesis that sleep disruption impairs information-processing also predicts effects on trading decisions in response to news. An investor who neglects an announcement does not trade on it, while an investor who notices the news but processes it badly may make a poorly considered trade. Both can be viewed as possible failure of attention—either complete inattention, or insufficient attention. We therefore examine trading responses, the allocation of trades across good- versus bad-news earnings surprises, and the performance of executed announcement-stock trades.

Consistent with an inattention effect, we find that later sunset is associated with a lower probability of trading held stocks during the earnings-announcement window relative to surrounding days. Furthermore, later sunset tends to increase investor’s contrarian trading tilt, consistent with poorer cognitive processing. On household-days with trading in any stock, a one-hour later sunset reduces positive-minus-negative net purchases across earnings surprise categories by \$132 ($t = -4.43$). Finally, consistent with this being a problem of information processing, the sunset–performance relationship is more negative for announcement-stock trades than for other trades, with an interaction of -0.311 percentage points over 21 trading days ($t = -2.06$).

Sleep disruption can affect trading for reasons other than impairment of attention to news, such as preference effects on attraction to lottery-type stocks or on the realization of winners versus losers. We do not find evidence of sleep disruption intensifying other biases. These tests suggest that sleep disruption particularly degrades the processing of arriving news.

The novel feature of this study is that it connects sleep-related cognitive impairment to differences in investor trading performance. Studies of DST document delayed market responses to earnings news and changes in analyst forecasts (Kleppe et al. 2024; Gonzalez and Li 2024; Bazley, Cuculiza, and Pisciotta 2024). This study provides complementary evidence about investor

trading decisions and subsequent performance and further identification using geographic and seasonal differences in sunset times.

The paper also contributes to research on environmental influences on financial markets, including sunshine and seasonal daylight (Saunders 1993; Hirshleifer and Shumway 2003; Kamstra, Kramer, and Levi 2003). Our hypothesis emphasizes the cognitive resources available for evaluating information. The earnings-news tests connect this proposed pathway to observable trading responses, allocation across surprises, and subsequent returns.

Our research contributes to the literature on the influence of sleep on economic decisions (Schilbach, Schofield, and Mullainathan 2016; Bessone et al. 2021; Gibson and Shrader 2018; Giuntella and Mazzonna 2019). This paper makes two new contributions to this literature. First, we study the effect of sleep disruption on individual investor trading behaviors and outcomes. Second, we provide novel evidence on the economic and psychological channels through which this occurs.

Our paper is related to deHaan and Glover (2024), who use U.S. time-zone borders to study how stock market access affects retail investment outcomes. They find that less overlap between trading hours and investors' waking day curbs active retail trading and raises realized capital gains reported on tax returns. We ask a different question, whether sleep disruption affects the quality of retail trades, using transaction-level data. Our hypothesis offers a complementary explanation for how daily schedules can affect investment outcomes through sleep-related changes in information processing. Section 3.2 discusses the evidence and the market-access explanation.

Finally, the paper examines determinants of the cognitive resources available for investing decisions. Limited attention models describe how information-processing constraints affect financial decisions and market outcomes (Hirshleifer and Teoh 2003). Empirical studies examine how distracting news or competing activities divert attention (DellaVigna and Pollet 2009; Hirshleifer, Lim, and Teoh 2009; Peress and Schmidt 2020; Hirshleifer and Sheng 2022). Our hypothesis concerns the tightness of the constraint rather than substitution. Sleep disruption may reduce the resources available for processing information, making predictable trading mistakes more likely.

2. Data and Measures

We next discuss data on retail investor trading and location. Then, in Subsection 2.3, we discuss data on local sunset time. Finally, we describe our monthly calendar-time trading performance measure in Subsection 2.4.

2.1 Retail Investor Data

The primary dataset used in this study is a panel of stock trades by households spanning six years from 1991 to 1996, provided by a large discount brokerage firm. This retail investor dataset has been widely used in the finance literature, starting with Barber and Odean (2000). The dataset includes daily trading records and monthly holding records for 77,995 households. The key information for this study is the location of each household. Therefore, we only consider households with valid coordinates derived from their ZIP code information. We convert ZIP codes into coordinates mainly using the 1990 Census U.S. Gazetteer, following Seasholes and Zhu (2010), and supplement these coordinates using SASHELP.ZIPCODE file and USPS ZIP code database. The locations of 52,051 households are identified.

Daily stock information is obtained from CRSP (Center for Research in Security Prices). Using the historic CUSIP codes, we combine the trading records with stock information and identify 10,601 different common stocks with valid price information. To adjust stock returns for risk, we obtain returns of the market portfolio, risk-free rate, and the SMB and HML factors from Kenneth French’s website. Additionally, we include the daily CRSP value-weighted index total return.

As we focus on the stock trading performance of retail investors, the final sample is restricted to households that trade common stocks with valid price information. Furthermore, to exploit the discontinuity design of time zones, we restrict the sample to households located in the contiguous United States. After filtering the sample, 41,131 households remain with their location and common stock trading records. The base sample has no household minimum-trade requirement. Each analysis applies the eligibility and return-data requirements described in its corresponding section.

Figure 1 maps the 41,131 households in the final sample. Table 1 summarizes their distribution and trading activity across the four time zones. Households are concentrated in the Eastern and Pacific zones, and purchases exceed sales in each zone.

2.2 Data on Distance from the Time-Zone Borders

We combine the ZIP-code coordinates of households with the U.S. time-zone boundary shapefile in ArcGIS to calculate distance to the nearest boundary. *SignedDistance* of a household to its closest time-zone border is defined to be negative west of the selected boundary and positive east. Where a location lies between two boundaries, we retain the distance with the smaller absolute value, assigning each household to one boundary. Figure 2 illustrates the discontinuity in clock-time sunset at the time-zone border, and Table 2, Panel B, reports the distance distribution.

2.3 Local Sunset Time Data

Our proxy for sleep disruption of investors is motivated by the finding that sleep is influenced positively or negatively by the presence of sunlight at different times of day. To identify the location of retail investors, we use the ZIP code information of each household to obtain the corresponding longitude and latitude coordinates. Using the solar algorithms outlined in Meeus (1991), we calculate daily sunset at each household's ZIP-code coordinates, accounting for local time zones and DST observance. Sunset is expressed in decimal hours of local clock time. In the ZIP-month estimation sample, it is averaged over the evenings preceding trades. As reported in Panel A of Table 2, it has a mean of 18.756 hours, or about 6:45 p.m. in local time. This measure captures the timing of evening light that motivates the sleep disruption hypothesis.

2.4 The Trading Performance Measure

We aggregate trades into calendar-time portfolios to measure the subsequent performance following investor decisions. In this section we define the monthly measure used in the ZIP-month and North–South analyses. Equation (1) gives the weight on stock i for investor group h in formation month m ,

$$w_{ihm} = \frac{\overline{p_{ihm}} \times \Delta q_{ihm}}{\sum_i |\overline{p_{ihm}} \times \Delta q_{ihm}|}, \quad (1)$$

Here the price term $\overline{p_{ihm}}$ is the trade quantity-weighted average transaction price of stock i by the investors in group h during month m ; Δq_{ihm} is the net shares traded in stock i by the investor group h during month m ; $\sum_i |\overline{p_{ihm}} \times \Delta q_{ihm}|$ represents the sum of the absolute dollar values of

the group’s net trades across stocks during month m , after purchases and sales of the same stock have been offset. The weights therefore measure the composition of net trading decisions rather than gross transaction volume. We use actual trades during the month rather than changes in portfolio weights because changes in portfolio weights can also result from price movements without trades. The absolute weights sum to one; the signed weights need not. Section 3.2 explains the time series alpha measure for the RDD, and Sections 3.4 and 4.1.3 define the 21-day forward-return tests.

We calculate the trading value-weighted monthly return for each investor group over the subsequent month as follows:

$$TradePerform_{h,m \rightarrow m+1} = \sum_i w_{ihm} (R_{i,m+1} - \widehat{R}_{i,m+1}), \quad (2)$$

where $\widehat{R}_{i,m+1}$ is computed from a Fama-French three-factor model. For each stock, factor loadings are estimated using the prior 36 months of monthly returns, from $m - 36$ through $m - 1$, so that only information available before the portfolio formation month is used. These estimated betas are then applied to the realized Fama-French factors in month $m + 1$ to obtain $\widehat{R}_{i,m+1}$.

A purchase contributes positively when the stock subsequently earns a positive abnormal return, and a sale contributes positively when the stock earns a negative abnormal return. *TradePerform* combines these signed contributions to evaluate the group’s net trading decisions. It differs from the return on total account wealth and from a buy-minus-sell strategy whose two legs are separately normalized. Related trade-based measures are used by Chen, Jegadeesh, and Wermers (2000). Table 2 reports the distributions for our different portfolio units and horizons.

3. Main Results: The Sleep Disruption Effect

In this section, we test the effect of sleep disruption on retail investors’ trading performance using four empirical approaches: (1) panel regression on sunset time with ZIP code and year-month fixed effects, (2) a regression discontinuity design (RDD) around time-zone borders with a seasonal difference-in-discontinuity test, (3) a difference-in-differences across latitudes and seasons, and (4) difference-in-differences analysis based on daylight saving time changes.

3.1 Sleep and Trading Performance: Panel Regression

Our first test examines whether performance deteriorates when a location's sunset becomes later relative to other locations. We aggregate trades into ZIP-month portfolios, aligning the observation unit with the local sunset exposure and accounting for the correlated trading of households in the same neighborhood (Pool, Stoffman, and Yonker 2015), and measure subsequent-month abnormal performance using Equation (2). Equation (3) is a panel regression that relates this outcome to Sunset and the stated controls. We estimate

$$TradePerform_{h,m \rightarrow m+1} = \alpha + \beta_1 Sunset_{h,m} + \beta_2 SAD_{h,m} + \gamma_h + \gamma_m + \varepsilon_{h,m}, \quad (3)$$

where $TradePerform_{h,m \rightarrow m+1}$ is defined as above, $Sunset_{h,m}$ is the average sunset time at the ZIP code's location on the day before each trade, averaged within the ZIP-month. SAD is the corresponding average of nighttime hours minus 12 during fall and winter and zero during spring and summer, following Kamstra, Kramer, and Levi (2003). The coefficient γ_m denotes year-month fixed effects, which absorb shocks common to all ZIP codes a given month. γ_h denotes ZIP-code fixed effects, which absorb additive time-invariant differences across ZIP codes². Identification therefore comes from changes in sunset within a ZIP relative to contemporaneous changes in other ZIPs. The households contributing trades and their portfolio weights can change across months, so this is a within-ZIP portfolio comparison rather than a within-household comparison. Standard errors are clustered by year-month or two-way clustered by ZIP-code and year-month, as indicated in Table 3.

We hypothesize that later sunset time causes later sleep times, thereby reducing mental alertness and decision-making capacity on the following day. Our hypothesis implies a negative coefficient on the sunset time variable, β_1 in Equation (3), indicating that a later sunset time is associated with lower trading performance.

We include SAD to account for that seasonal mood pattern. SAD is zero during spring and summer, while $Sunset$ continues to vary, so the measures are not identical. This control addresses SAD but does not absorb every possible geographically varying seasonal influence on performance.

² Related studies include inherent stock preferences (Døskeland and Hvide 2011; Grinblatt and Keloharju 2001; Seasholes and Zhu 2010) and trading propensities (Barber and Odean 2001; Grinblatt, Keloharju, and Linnainmaa 2012; Korniotis and Kumar 2011).

More generally, the panel and North–South analyses both exploit cross-sectional differences in the seasonal movement of sunset, so they are related tests.

Consistent with the hypothesis, Table 3 reports a negative Sunset coefficient across specifications. With SAD and two-way clustering, a one-hour later sunset is associated with a 29.3-basis-point lower monthly abnormal return ($t = -2.50$). Section 3.4 compares this economically substantial estimate with the other designs and published trading performance benchmarks.

3.2 Sleep Disruption and Trading Performance: An RDD Test

The border design tests whether trading performance falls where evening light occurs later relative to clock-based schedules. Figure 2 shows the discontinuity in local sunset time at the time-zone boundaries. The sleep-disruption hypothesis predicts a negative performance discontinuity on the later-sunset side. Interpreting that discontinuity requires comparable potential outcomes near the boundary, an assumption examined through the diagnostics below.

A distance-group portfolio comprises households sharing a ZIP within one of nine geographic cells, formed from the three boundaries and three latitude bands. We construct monthly returns from each group’s net trades. The median group contains two households and 38 trades over the 71-month sample period, while the mean group contains 4 households and 113.65 trades. Aggregation therefore leaves substantial portfolio-specific return variation.

Specifically, we estimate a monthly time-series Fama-French three-factor regression for each group, with the intercept representing the group’s risk-adjusted performance, α_h , to align with its cross-sectional feature:

$$\begin{aligned} \sum_i w_{ihm} R_{i,m+1} - \frac{1}{N_{g,m}} \sum_{h \in g} \sum_i w_{ihm} R_{i,m+1} - R_{f,m+1} \\ = \alpha_h + \beta_h MKT_{m+1} + s_h SMB_{m+1} + h_h HML_{m+1} + \varepsilon_{h,m+1}, \end{aligned} \quad (4)$$

where Equation (4) subtracts the geographic-cell monthly mean from each group’s raw return and then applies the displayed risk free and factor adjustments. Its intercept is the cell-adjusted performance measure used in the RDD. Estimation requires at least 12 usable monthly returns; no-trade months are excluded. This leaves 6,698 groups with an average of 37.94 usable months and a standard deviation of 18.77.

To test discontinuity at time-zone borders, we estimate

$$\begin{aligned}
TradePerform_h &= \alpha + \beta LateSide_h + \theta SignedDistance_h \\
&+ \delta LateSide_h \times SignedDistance_h + \varepsilon_h,
\end{aligned} \tag{5}$$

where $LateSide_h$ equals one east of the boundary, and its coefficient estimates the performance jump at $SignedDistance$ equal to zero. We implement the local linear RDD in `rdrobust`, allowing separate slopes and using an MSE-optimal bandwidth and triangular kernel. Tables report robust bias-corrected p -values and bandwidths, following Calonico, et al. (2019) and Calonico, Cattaneo, and Titiunik (2014). Standard errors are clustered by 10-km signed-distance bin.

Table 4 reports a full-sample FF3 discontinuity of -0.404 percentage points per month ($p = 0.074$), using a 170.6-kilometer bandwidth. The excess-return and CAPM estimates are also negative, at -0.531 ($p < 0.001$) and -0.448 ($p = 0.027$). The sign is consistent across benchmarks, while statistical precision differs. Figure 3 displays the FF3 discontinuity, using binned means chosen by the data-driven procedure of Calonico, Cattaneo, and Titiunik (2015) and local linear fits on each side of the border. Figures IA.1 and IA.2 report bandwidth sensitivity and the alternative return benchmarks.

As discussed in the introduction, U.S. time-zone borders are not randomly assigned, so the RDD approach is still subject to endogeneity. We address this concern with a set of validation tests. Table 5 examines discontinuities in available household characteristics and trading measures, aggregated to distance groups. Six of the seven are insignificant. However, reported income is higher on the east side of the boundary ($p < 0.001$). Total trades, stocks traded, and average holdings are measured during the sample and are not all predetermined relative to long-standing time-zone exposure. These tests describe cross-border differences rather than establishing balance in every pre-exposure characteristic.

If higher-income investors are better traders (Bui et al. 2022), the imbalance runs against our findings. Nevertheless, we examine the income issue in two ways. Separate RDD estimates for below and above median income are -0.644 ($p = 0.058$) and -1.342 ($p = 0.010$), using each subgroup's MSE-optimal bandwidth. Both remain negative, though insignificant under a common bandwidth check (Table IA.1, Panel B, Columns (2) and (3)). Adding measured income to the pooled specification leaves the pooled RDD estimate virtually unchanged, from -0.404 to -0.394 (Table IA.1). These findings reduce concern that the income variable explains the result.

The Internet Appendix also reports density tests, placebo-cutoff tests at ± 150 and ± 300 km and a donut-RD specification that excludes observations within 30 km of the cutoff (Tables IA.2-IA.4). The density test is sensitive to polynomial order and under the second-order specifications rejects continuity of the running-variable density; the estimated density discontinuity reverses sign across polynomial orders, indicating that this diagnostic is sensitive to the local specification. Placebo cutoffs show no comparable performance discontinuities, and the donut estimates have the same qualitative pattern.

Second, we focus on households who are in the same state but in different time zones. Among the 13 states in the contiguous U.S. that span two time zones during our sample period, 10 states have sufficient sample from both time zones in proximity to time-zone borders. Because this subsample is small, with 145 distance groups on the west side of the borders, we estimate the discontinuity at the full-sample bandwidth rather than at a bandwidth chosen within the subsample (Table IA.5). RDD analysis of these 10 states indicates that households on the later sunset side have lower trading performance: -1.20 percent per month for excess returns ($p < 0.001$), -0.79 for CAPM alphas ($p = 0.011$), and -0.57 for FF3 alphas ($p = 0.177$). These estimates reduce concern that the performance difference is attributable to crossing state boundaries.

Third, we run a placebo test at pseudo-time-zone borders. We define three vertical pseudo-time-zone borders, north to south. These borders divide the original time zones and perfectly align with state borders (Figure IA.3). We then estimate the same discontinuity (Table IA.6). In contrast with the results using the real time-zone borders, being on the east side of these pseudo-time-zone borders has no significant effect on trading performance. The placebo results provide no evidence of comparable jumps at the selected state boundaries, which reduces concern that the RDD result is driven by crossing state boundaries rather than time-zone boundaries.

As predicted above, the adverse border effect should be larger in summer, when sunsets are already late, than in winter. An additional delay in evening darkness may interfere more with sleep when it occurs close to bedtime, while morning obligations remain fixed. We therefore compare the discontinuity across the two seasons. This comparison also addresses alternative explanations based on permanent cross-border differences whose effects on performance are constant throughout the year.

Table 6 reports summer and winter FF3 discontinuities of -1.108 percentage points per month ($p < 0.001$) and -0.373 ($p = 0.457$; Columns (1) and (2)). The formal comparison estimates

a single RDD on each group’s summer-minus-winter alpha, yielding -0.939 ($p = 0.049$; Column (3)). This supports the predicted larger adverse effect in summer. This rules out explanations for our results based on a discontinuity whose effect is time invariant. It does not rule out unobserved permanent differences whose effect on performance vary seasonally, though we do not have an obvious candidate in mind.

An alternative market access channel (deHaan and Glover 2024) predicts that local trading hours affect retail trading and performance. Several findings weigh against market access as the primary explanation of our results. First, their mechanism operates partly through reduced active trading. We find no significant discontinuity in our trading-activity measures at the time-zone border (Table 5) and no significant relation between Sunset and monthly turnover (Section 4.2). Second, the difference in local-clock market hours across a time-zone boundary persists throughout the year, whereas the performance discontinuity is substantially larger in late-sunset months (Table 6). This seasonal pattern is difficult to reconcile with a market-access effect that is constant over the year, nor is there an obvious reason to expect market access to have strong seasonal variation. Third, the panel and North–South designs exploit variation in sunset timing that, for their principal identifying variation, is not accompanied by changes in local market hours. Taken together, these findings are not readily explained by market access alone.

3.3 Trading Performance across Latitude and Seasons

The latitude comparison tests a different seasonal prediction of the sleep disruption hypothesis. Northern locations have relatively later sunsets in summer and earlier sunsets in winter, so the relative performance of northern investors should deteriorate from winter to summer. Figure 4 illustrates the sunset reversal for Buffalo and Miami, which have similar longitudes but different latitudes.

We classify households as northern or southern by latitude, and within each group form monthly portfolios separately by time zone and three-degree longitude bin, so that northern and southern households are compared at similar east–west positions and the remaining sunset difference is driven by latitude. Table 2, Panel D, reports the resulting gap: northern households’ sunset is later by 29.46 minutes on average in April–September and earlier by 19.86 minutes in October–March. For each North $\times$ time-zone $\times$ longitude bin $\times$ month portfolio we compute *TradePerform* and estimate

$$\begin{aligned}
TradePerform_{h,m \rightarrow m+1} &= \alpha + \beta_1 North_h + \beta_2 Summer_m + \beta_3 North_h \times Summer_m \\
&+ \gamma_{TZ \times LongitudeBin} + \gamma_m + \varepsilon_{h,m+1},
\end{aligned} \tag{6}$$

where $North_h$ identifies portfolios in the top latitude quartile, with the bottom quartile forming the comparison group, and $Summer$ equals one for April through September and zero for October through March. The coefficient on $North \times Summer$ measures the change in their performance gap across seasons and is predicted to be negative. The level difference in either season can also reflect persistent differences between the groups. Standard errors are clustered by year-month.

Table 7 reports the results. In Column (3), the $North \times Summer$ interaction is -64.4 basis points per month ($t = -2.36$), which supports the predicted deterioration in relative northern performance. The estimated winter gap is $+35.3$ basis points, and the implied summer gap is -29.1 basis points. Our seasonal hypothesis is about the significant interaction, rather than whether either seasonal level is separately significant.

3.4 The Effect of Daylight Saving Time Changes

Our DST tests are based upon an abrupt change in clock-based schedules, which complement tests based on gradual seasonal and persistent geographic exposures. Building on the DST natural experiment literature, including Kamstra, Kramer, and Levi (2000, 2002), we examine individual trading performance around the transitions. The narrow event windows are helpful for ruling out alternative slowly changing influences, and the comparison group that does not observe DST addresses common event-window changes.

There is no expectation that spring and fall DST transitions will have symmetric effects. Barnes and Wagner (2009) find that the spring shift results in 40 minutes less sleep, while the fall change, despite the extra hour, does not produce a significant increase in sleep. Smith (2016) finds that the spring transition raises fatal vehicle crashes, consistent with impaired alertness in the following days. The two transitions are therefore not equivalent treatments. The spring transition removes an hour of sleep and shifts the schedule against the biological clock, so the two effects reinforce each other and the predicted effect on performance is negative. The fall transition shifts the schedule but adds an hour, so the effects oppose and the prediction is ambiguous.

We form daily portfolios for DST-observing and nonobserving households and evaluate their subsequent 21-day performance. We use 4,583 households that trade at least once in both the pre- and post-transition windows of at least one DST event.

For each group h and trading day t within the five trading days before or after a DST transition, we form a portfolio of the group's net stock trades.³ Similar to Equation (1), we assign each stock i a signed value weight, w_{iht} . Using these daily weights, we calculate each group's trade-value-weighted abnormal return over the subsequent 21 trading days:

$$TradePerform_{h,t \rightarrow t+21} = \sum_i w_{iht} (R_{i,t+1 \rightarrow t+21} - \hat{R}_{i,t+1 \rightarrow t+21}), \quad (7)$$

where $R_{i,t+1 \rightarrow t+21}$ is the compounded stock return over the subsequent 21 trading days, and $\hat{R}_{i,t+1 \rightarrow t+21}$ the corresponding compounded return implied by the Fama–French three-factor model. For each stock, factor loadings are estimated using the prior 250 trading days, requiring at least 126 valid daily observations and excluding the trade date t .

Table IA.7 reports the single-group comparisons. The Spring transition is followed by an approximately 1.0 percentage point deterioration in 21-day performance ($p = 0.108$). The fall estimate is +0.38 and insignificant ($p = 0.551$), consistent with the ambiguous prediction for a transition that shifts the schedule but adds an hour of sleep. A before–after comparison cannot separate the clock adjustment from other contemporaneous changes, so our main test adds a comparison group that does not observe DST.

The control group consists of households in Arizona and the Indiana counties that did not observe DST during our sample period. These households face the same market dates but no local clock adjustment. The DiD estimates the treated-minus-control change under the assumption that the groups would otherwise have experienced comparable changes in performance. Our sleep-disruption hypothesis predicts a negative spring interaction. For the fall change the prediction is ambiguous, since the schedule shifts but an hour of overnight hours is added.

To test this idea, we run the following DiD regression:

³ The event day (Day 0), which falls on the DST transition Sunday, is excluded from the analysis. For each DST transition event, we examine five consecutive trading days before the transition and five consecutive trading days after the transition. In most cases, this corresponds to Monday through Friday preceding the DST transition Sunday and Monday through Friday following it.

$$\begin{aligned}
TradePerform_{h,t \rightarrow t+21} &= \alpha + \beta_1 AfterDST_t + \beta_2 ObservingDST_h \\
&+ \beta_3 AfterDST_t \times ObservingDST_h + X'_t \delta + \gamma_{DSTevent} + \gamma_{dayofweek} \\
&+ \varepsilon_{ht},
\end{aligned} \tag{8}$$

where $AfterDST_t$ equals one if a trade date t is within 5 trading days after daylight saving time transitions and zero if within 5 trading days before the transitions. The variable $ObservingDST$ equals one if the portfolio consists of households in counties that observe daylight saving time and zero otherwise. Controls include the CRSP value-weighted index return and the standard deviation of daily market returns, measured over the same 21-day horizon as the outcome (the prior 21 days as a robustness check). The regression includes event and day-of-week fixed effects, the latter accounting for systematic weekday patterns in returns (French 1980).

The regression includes event and day-of-week fixed effects. Equation (8) is the baseline specification. Table 8 also reports versions adding market return and volatility over the subsequent or preceding 21 trading days. Because the daily portfolio returns overlap within each event window, standard errors are clustered by DST event. With only six spring and six fall events, we report wild cluster bootstrap p -values using the Webb (2023) six-point weights.

Table 8 reports the DiD results. The spring interaction term is -3.973 percentage points over 21 trading days ($p = 0.021$), consistent with the predicted deterioration. The fall interaction is -3.094 ($p = 0.190$), negative but imprecisely estimated.

In Column (1), the control portfolio improves by 2.977 points, implying a treated-group change of about -0.996 points. Unlike the DiD, that before–after change does not remove the counterfactual change represented by the controls. The large relative estimate depends substantially on the small control portfolio. The control portfolio contains only 43.4 households and 115 trades per event window, so its change is imprecisely measured. Section 5 reports its sensitivity to weighting, including an equal-household-weighted DiD of -0.879 ($p = 0.445$).

The DST test evaluates 21-day performance around an abrupt transition. The designs differ in exposure and return horizon. The panel coefficient is 29.3 basis points per month per hour of later sunset. The border discontinuity is -40.4 basis points per month for an approximately one-hour clock discontinuity. The $North \times Summer$ interaction is -64.4 basis points per month for a

seasonal swing in the North–South sunset gap of 49.32 minutes, or about -78.4 basis points per month per hour under linear rescaling.

For comparison, Odean (1999) reports a FF3 buy-minus-sell alpha of approximately -25 basis points per month. Coval, Hirshleifer, and Shumway (2021) report an out-of-sample spread of about 7.5 percentage points annually, or 62 basis points per month between strategies based on high- and low-performing investor quintiles. Our estimates are economically substantial relative to these benchmarks.

4. Mechanisms

We next examine whether the performance relationship is accompanied by trading decisions consistent with impaired information processing. Section 4.1 studies responses to earnings news. Section 4.2 examines alternative explanations based on variations in broader trading activity, and tilts toward lottery, attention-grabbing, and high-past-gain stocks. Our outcome variables are based on subsequent returns without any direct measurement of commissions or execution costs.

4.1 Attention and Cognitive Processing

A well-documented consequence of impaired sleep in neuroscience and psychology is the reduction of attention. A classic study by Pilcher and Huffcutt (1996) finds that sleep deprivation leads to significant impairments in cognitive performance, including attention, memory, and decision-making. They show that sleep-deprived individuals often perform tasks at a slower pace and make more errors. We refer to this as a loss of mental alertness. Dinges et al. (1997) explore how sleep deprivation affects attention and vigilance, critical components of cognitive alertness. Their study finds that even moderate sleep loss leads to lapses in attention, increased reaction times, and reduced vigilance. Research on circadian rhythms shows that human alertness levels fluctuate naturally throughout the day based on our biological clock. Dijk and Lockley (2002) argue that disruptions to the circadian rhythm, such as shift work or jet lag, can impair cognitive alertness and performance. Recent work in economics confirms these patterns in field settings: Bessone et al. (2021) find that workplace naps that improve sleep continuity raise performance on attention-intensive cognitive tasks, while merely extending fragmented nighttime sleep does not.

There is also extensive evidence that sleep loss impairs cognitive processing (Harrison and Horne 2000; Durmer and Dinges 2005; Killgore 2010; Lim and Dinges 2010) and taxes cognitive bandwidth (Schilbach, Schofield, and Mullainathan 2016). Sleep loss alters activity in the prefrontal cortex and related networks that support attention, executive function, and working memory (Krause et al. 2017). This impairs belief updating and decision-making. Specifically, individuals whose sleep is restricted rely more on simple heuristics and make less accurate Bayesian inferences (Dickinson and McElroy 2019). Furthermore, sleep-deprived individuals tend to make poor use of feedback in their decisions (Whitney et al. 2015). Whitney et al. further find that this failure is not explained by attentional lapses, so the effects go beyond simply failing to perceive a stimulus.

An investor may notice a news announcement but devote insufficient attention to its implications. Sleep-related impairment can therefore reduce both the likelihood of a news-triggered trade, for lack of noticing the news, and the quality of the trades that do occur. This partial attention interpretation encompasses both trading responses and poorer executed-trade performance.

Earnings announcements are a central and recurring source of public information with observable release dates and magnitudes. Our first prediction is that later sunset weakens the additional trading response around those releases. Section 4.1.1 tests this using trading and nontrading observations.

We further hypothesize that conditional on trading around news announcements, effective information processing helps investors avoid trading against the surprise. As documented in the post-earnings-announcement drift literature, returns continue to drift in the direction of the earnings surprise for weeks after the announcement (Ball and Brown 1968; Bernard and Thomas 1989, 1990). So trading in alignment with the surprise is on average profitable, whereas trading in opposition to it is unprofitable. Section 4.1.2 tests the prediction that later sunset shifts relative net purchases to be more contrarian against the earnings surprise.

Finally, we hypothesize that impairment of information processing is especially deleterious when information is arriving, so that the negative sunset–performance relationship is more pronounced for announcement-stock trades than for other trades. Section 4.1.2 tests this prediction.

4.1.1 Probability of Trading on Earnings Announcement Days

We obtain earnings data from the CRSP/Compustat Merged database and I/B/E/S, spanning from 1991 to 1996. We compile a comprehensive list of earnings announcement dates from both databases. In instances where I/B/E/S provides a different announcement date than Compustat, we follow the approach of DellaVigna and Pollet (2009) and choose the earlier date. We restrict announcements to trading days. Because the event window includes the announcement date, some same-day trades may precede the release.

We infer holdings from monthly position statements by trade records. A position recorded at month-end is treated as held through the following month, ensuring that a sale during that month remains in the sample. After the closure of a position, this rule can continue to classify the stock as held for the rest of the month. So the resulting measure is an approximation to daily holdings.

We use observations from five trading days before to five trading days after each announcement. The announcement window comprises the release date and the next two trading days. The remaining nearby days provide the benchmark for comparison. This keeps the comparison close in time and makes the panel computationally manageable.

The resulting panel contains 31,607,126 household–stock–day observations covering 39,440 households and 5,783 stocks, resulting in 403,146 household–stock pairs. 175,342 of these observations involve a trade. Within the panel, a household trades a given held stock on 0.697% of days inside that announcement window and on 0.500% of the surrounding days outside it. Because the sample consists of household-stock pairs already held, the test captures sales and additions to existing positions, not the extensive margin of buying an announcing stock for the first time.

Using a stratified Cox proportional hazards model, we estimate

$$\lambda_{hit} = \lambda_{0,it} \exp(\beta_1 \text{Sunset}_{h,t-1} + \beta_2 \text{EWindow}_{it} + \beta_3 \text{Sunset}_{h,t-1} \times \text{EWindow}_{it} + \beta_4 \text{SAD}_{ht} + \beta_5 \text{TradeRate}_h) \quad (9)$$

We estimate this model of the daily hazard that household h trades stock i on day t . EWindow equals one on the announcement day of stock i and the two subsequent trading days; Sunset is the demeaned local sunset time, in hours, at household h 's ZIP code on the preceding evening; SAD is the seasonal affective disorder measure at the same ZIP-day; and TradeRate is household h 's log trading frequency over its time in the sample to control for heterogeneity in

household trading propensity. Standard errors are clustered by ZIP code. The sample consists of household-stock-day observations in which the household holds the stock and the day falls within five trading days of one of that stock's announcements.

Strata are stock-days, so $\lambda_{0,it}$ is a stock-day-specific baseline hazard left unrestricted. It nonparametrically absorbs variation at the stock-day level, the announcement itself and its surprise, the stock's liquidity and price movement, and market conditions, seasonality, and calendar effects common to the day. Because *EWindow* varies only at the stock-day level, its main effect is subsumed by the baseline and is not separately identified. Identification therefore comes entirely from variation across households holding the same stock on the same day. β_3 measures how the announcement response itself varies with local sunset.

Table 9 reports the estimates. The announcement-window interaction is negative in both Cox specifications and in the Poisson specification with household and stock-day fixed effects. The interaction $EWindow \times Sunset$ is -0.043 in Column (1) ($z = -2.42$) and -0.063 ($z = -3.08$) in Column (2) with $EWindow \times SAD$ added. Based on Column (1), an hour of later sunset reduces the hazard of trading a held stock during the three-day announcement window by about 4% more than on the surrounding days. The Sunset main effect is insignificant ($z = -1.06$). Column (3) gives a similar interaction, -0.0611 ($z = -3.01$), in a Poisson specification that also controls for persistent household differences and stock-day fixed effects; its smaller reported sample reflects the estimator's exclusion of all-zero fixed-effect groups rather than a different panel.⁴ These findings are consistent with the hypothesis that later sunset is associated with a lower probability of buying or selling an earnings announcement stock on or immediately after an earnings announcement day.

We require investors to hold the stocks because retail investors do not typically short sell stocks. Thus, they have to hold the stock in order to sell it. As a robustness check, we also conduct a Poisson pseudo-maximum-likelihood analysis that removes the holding restriction and includes all stocks with observed earnings announcements, regardless of whether a household has previously held or traded them. For computational tractability, we aggregate the unrestricted sample to household–trading-day–*EWindow* cells and estimate Poisson models of the number of

⁴ The reported observation counts differ across columns because the estimators treat non-informative groups differently. Columns (1) and (2) count all 31,607,126 household-stock-day observations in the input panel, including stock-day strata in which no household traded; such strata contribute nothing to the partial likelihood but remain in the count. Column (3) is estimated with *ppmlhdfc*, which drops fixed-effect groups with an all-zero outcome, whose fixed effects are not identified, and any singletons created by that removal, retaining 13,549,449 observations. The difference reflects the treatment of all-zero groups, not missing data or a different underlying panel.

distinct eligible stocks traded, using the number of eligible stocks as exposure and including household and trading-date $\times$ EWindow fixed effects (Table IA.8). Results are consistent with Column (3) of Table 9 with the holding restriction.⁵

4.1.2 Direction of Trading Earnings Announcement Stocks

We now examine whether later sunset is associated with lower net purchases of positive-SUE stocks relative to negative-SUE stocks. The post-earnings-announcement drift literature documents that returns continue to drift in the direction of the earnings surprise for months after the announcement (Ball and Brown 1968; Bernard and Thomas 1989, 1990). In consequence, trading in the direction of the surprise is the profitable response to earnings news. Not doing so has an opportunity cost, and trading as a contrarian to earnings surprises is on average even more costly.

We use the I/B/E/S consensus forecast to compute standardized unexpected earnings (SUE) and classify announcement stocks into good- and bad-news categories. We then test whether the trades of investors with later sunset times are less aligned with the sign of the surprise, in the sense of buying less after positive SUE (good news) and selling less after negative SUE (bad news).

For each household-day with trading in any stock, we calculate net purchases of stocks announcing within $[t - 2, t]$ in SUE deciles 7–10 (good news), and net purchases in deciles 1–4 (bad news).⁶ *NetPurchase* is the dollar value purchased minus the dollar value sold, and is set to zero for a SUE category in which the household does not trade. We therefore test whether later sunset shifts net purchases of positive-SUE stocks relative to negative-SUE stocks via either a shift in participation trading or, conditional on trading, in the direction and amount traded. We then calculate the difference in net purchases of the positive-SUE stocks and the negative-SUE stocks, *Diff(NetPurchase)* in thousands of dollars. The sample mean of *Diff(NetPurchase)* is -0.0746 thousand dollars, or $-\$74.60$. This descriptive statistic, reported here rather than in the regression table, indicates lower mean net purchases of positive-SUE stocks than negative-SUE stocks.

We conduct the following regression:

⁵ When we split earnings announcements by analyst forecast disagreement (Gargano and Rossi 2018), the *EWindow* $\times$ *Sunset* coefficient is negative in both groups, although its statistical significance varies with the specification (Table IA.9).

⁶ Observations in deciles 5 and 6 are excluded from the sample because the sign of their SUE can be ambiguous. SUE deciles are constructed based on the distribution of SUE in each quarter.

$$\begin{aligned}
& Diff(NetPurchase_{h,t}) \\
& = \alpha + \beta Sunset_{h,t-1} + X'_{ht}\delta_1 + \gamma_h + \gamma_{yearquarter} + \gamma_{dayofweek} + \varepsilon_{ht},
\end{aligned} \tag{10}$$

where controls include *SAD*, past 21-day market return, and past 21-day market volatility, household, year-quarter, and day-of-week fixed effects. Standard errors are two-way clustered by ZIP code and trading day.

Table 10 reports the result. In Columns (1) and (2), the coefficient for *Sunset* is negative and significant at the 1% level, indicating that later sunset strengthens the contrarian tilt for earnings surprise. A one-hour later sunset reduces net purchases of positive-SUE stocks relative to negative-SUE stocks by \$132 ($t = -4.43$ in Column (2)). Columns (3) and (4) further decompose the relative trading response, by separately examining net sales of positive SUE stocks and net purchase of negative SUE stocks. Both margins contribute to the effect, with the purchase response to negative earnings news somewhat larger in magnitude.

4.1.3 Trading Performance around Earnings Announcements

To examine whether attention and cognitive processing serve as a channel for the lower trade performance of the later sunset investors, we examine whether the negative association between trade performance and sunset time is stronger around earnings news. We separate announcement-window trades from other trades. Announcement trades represent 6.7% of gross trading dollars.

The timing of the monthly return test is not fine-tuned to measuring performance in relation to earnings announcements. Returns begin in the following month, so an announcement-stock trade made early in the formation month can be evaluated only after several weeks of post-announcement returns (which tend to reflect a high fraction of total PEAD) have already occurred. So to more accurately capture the announcement effect, we instead use returns covering the 21 trading days immediately following each trade.

For each household-day-portfolio, we estimate the following regression:

$$\begin{aligned}
& TradePerform_{h,EAttrade,t \rightarrow t+21} \\
& = \alpha + \beta_1 EAttrade_{ht} + \beta_2 Sunset_{h,t-1} \\
& + \beta_3 (EAttrade_{ht} \times Sunset_{h,t-1}) + X'_{ht}\delta_1 + EAttrade_{ht}(X'_{ht}\delta_2) \\
& + \gamma_h + \gamma_{yearquarter} + \gamma_{dayofweek} + \varepsilon_{ht},
\end{aligned} \tag{11}$$

where *EAttrade* is an indicator equal to one for the earnings-announcement portfolio and zero for the other-trades portfolio. *Sunset* is local sunset time, in hours, at the household's ZIP code on the preceding evening and is demeaned at the sample mean. Controls include *SAD*, future 21-day market return, and future 21-day market volatility. All specifications include household, year-quarter, and day-of-week fixed effects. Because the 21-day return windows overlap, trades on adjacent dates can share 20 of 21 subsequent return days. We therefore two-way cluster standard errors by ZIP code and year-month, and in addition compute Driscoll–Kraay (1998) standard errors to address broader cross-sectional and temporal dependence.⁷

The main coefficient of interest is on the interaction term $EAttrade \times Sunset$. As Table 11 indicates, the additional announcement-trade coefficient is -0.311 ($t = -2.06$) in Column (3).⁸ This provides stronger evidence of a more negative sunset–performance relationship around earnings news. In contrast, the sunset-performance relationship on non-announcement trades is indistinguishable from zero (0.051 , $t = 0.52$).⁹ The evidence indicates that sleep disruption degrades investor trade performance particularly during earnings announcements.¹⁰

In summary, the results on earnings-announcement-related trading probability, SUE-related trading direction, and trading performance support the interpretation that later sunset is associated with less use and poorer use of earnings news. The findings on these different aspects of the decision process are consistent with sleep disruption impairing attention to and effective cognitive processing of news.¹¹

⁷ The Driscoll–Kraay estimator sums the moment conditions across all household-day observations on each trading date and applies a Bartlett-kernel covariance to the resulting daily series, with maximum lags of 21, 42, and 63 trading days (one, two, and three months; 21 days equals the holding period of the dependent variable). The estimation sample is that of Column (3) (783,414 household-day observations over 1,497 trading days).

⁸ The interaction remains significant at the 5% level under Driscoll–Kraay standard errors for all three lag lengths. The Driscoll–Kraay standard errors on the interaction are 0.142, 0.138, and 0.140 ($t = -2.19, -2.25, -2.23$), compared with 0.151 ($t = -2.06$) under two-way clustering. Table IA.10 shows the interaction remains negative and significant at the 10% level with trade-date fixed effects.

⁹ This insignificance does not prove a zero effect, so we do not conclude that the entire relationship is driven by announcement trades.

¹⁰ We further check the robustness of the performance analyses around earnings news. Aggregating trades into ZIP-day portfolios also yields a negative announcement-trade interaction (Table IA.11; -0.271 , $t = -1.75$).

¹¹ Our probability of trading test does not establish whether trades that were not made would have been profitable. The performance test evaluates executed trades. We therefore do not attempt to draw a welfare conclusion from the combination of effects that we document.

4.2 Other Potential Channels

In this section, we explore a number of other potential channels by which sleep disruption can hurt performance. We report the main findings of each of the tests and include the details in the internet appendix.

4.2.1 Overtrading

In general, individual investors trade too actively, in the sense that on average they lose money on their trades (Barber and Odean 2000). To examine whether overtrading could explain the performance result, we look at portfolio turnover and holding periods and their relation with sunset time. Table IA.12 shows the result for portfolio turnover. The coefficient on local sunset time is negative but statistically insignificant. We find no significant relationship between local sunset time and household portfolio turnover.

To examine holding periods in addition to turnover, we proxy for the holding period using the length of time a household holds a stock before selling it. For each sale, we calculate the number of days since the household's earliest observed preceding purchase of that stock. Table IA.13 shows the result. We find no statistically significant relationship between local sunset time and the number of days until a stock is sold. These results distinguish investor response to news from general trading activity. The hazard test in Section 4.1 concerns trades in already-held stocks in a narrow window around news, whereas turnover aggregates all trades over a month, including new purchases, and is dominated by the most active households. Sleep disruption reduces reactions to news in held positions without detectably changing overall trading volume.

4.2.2 Overreaction to Stock Characteristics with Low Expected Returns

We explore whether later-sunset-time investors exhibit a stronger tilt toward stock characteristics with lower expected returns, including lottery stocks, attention grabbing stocks, and high past return stocks.

Retail investors exhibit a strong preference for lottery stocks (stocks with positively skewed returns), which can lead to high valuations and low subsequent returns (e.g., Kumar 2009). The attraction to lottery stocks is related to theories on skewness-related preferences (Barberis and Huang 2008) and preferences over beliefs (Brunnermeier and Parker 2005), and transmission bias in social interactions (Han, Hirshleifer, and Walden 2022).

We test whether sleep disruption may strengthen the attraction to lottery stocks. Following Kumar (2009), we classify stocks as lottery-type based on their price, idiosyncratic volatility, and idiosyncratic skewness.¹² *Lottery* equals one if the traded stock's previous month-end price is at or below the price median and its idiosyncratic volatility and skewness are both at or above their respective medians, and zero otherwise. Each trade receives its stock's classification for the trade month. The results are presented in Table IA.14. Overall, the effect of sleep disruption on the attraction to lottery stocks is insignificant.

We further test whether sleep-disrupted investors are more reactive to salient but less informative stimuli. Following Barber and Odean (2008), we measure the attention-grabbing characteristics of stocks using abnormal trading volume and one-day stock returns. Table IA.15 reports the results. Consistent with Barber and Odean (2008), households in our sample are net buyers of attention-grabbing stocks relative to non-attention-grabbing stocks. However, we do not find evidence that households tilt more toward attention-grabbing stocks as local sunset time becomes later.

Finally, we test whether return extrapolation could be a source of sleep-induced performance effects. Specifically, we examine whether late-sunset investors are greater net buyers of momentum stocks. We sort stocks into deciles based on the stocks' past 21-day returns or past 12-month returns. A stock is a momentum stock if its return falls in the top decile of the past return distribution. As indicated in Table IA.16, households in our sample are net sellers of momentum stocks relative to non-momentum stocks when we define the momentum stock using past 21-day returns. However, we do not find evidence that households tilt more toward momentum stocks as local sunset time becomes later in either measure.¹³

Taken together, the tests in this subsection indicate that there is no strong evidence that sleep disruption affects investors' tilt toward stock characteristics that earn low expected returns. This null finding is informative for the mechanism. In our data, sleep disruption reduces effective

¹² For each stock-month, we estimate idiosyncratic volatility and idiosyncratic skewness using daily stock excess returns over the preceding six calendar months. Idiosyncratic volatility is measured as the residual variance from a regression on the three Fama–French factors and the momentum factor. Idiosyncratic skewness is measured as the standardized third moment of residuals from a regression on the excess market return and its square. We independently calculate monthly medians across stocks in the available return-data universe with valid estimates and a valid previous month-end price.

¹³ Defining a *sample initial buy* as a purchase that is the household's first observed transaction, buy or sell, in the stock, we again do not find that households tilt more toward high past-return stocks relative to other stocks as they experience later local sunset time. The results are in Table IA.17.

responsiveness to informative signals without increasing responsiveness to salient but uninformative ones. This points to a reduction in attentional capacity rather than a shift toward more salient cues.

4.2.3 The Disposition Effect

We also explore whether the sleep disruption effect on trading is related to the disposition effect, which is a robust and widespread trading anomaly (e.g., Shefrin and Statman 1985; Odean 1998; Grinblatt and Keloharju 2000; Kumar 2009). The disposition effect refers to the tendency of investors to be more likely to sell an asset at a gain than an asset at a loss (Shefrin and Statman 1985). The disposition effect is associated with trading losses. Although the exact cause of the disposition effect is still under debate, research indicates that it can be related to behavioral bias, such as loss aversion from prospect theory (Barberis and Xiong 2009), mental accounting (Shefrin and Statman 1985), overconfidence (Barber and Odean 2001), and confirmation bias (Gervais and Odean 2001; Feng and Seasholes 2005).

Given the potential for external factors to influence behavior patterns, we hypothesize that sleep disruption may exacerbate the disposition effect. To test this hypothesis, we adopt the individual-level disposition effect measure proposed by Dhar and Zhu (2006). This approach allows us to capture cross-sectional variation in the disposition effect across different investors. Odean (1998) quantifies the disposition effect among retail investors using the difference between the proportion of gains realized (PGR) and the proportion of losses realized (PLR). The main finding is that retail investors significantly prefer selling winners and holding losers, except in December due to tax-motivated selling, ultimately to the detriment of their wealth. For each investor and sale date, we calculate PGR, PLR, and DE.

Table IA.18 presents the results of this analysis. We find no statistically significant association between local sunset time and the disposition effect. Thus, the sleep disruption effect is not likely driven by the disposition effect.

5. Robustness Checks

We next describe additional tests to address possible concerns about the robustness of the sleep disruption effect. We test whether the result is robust to excess return. The Fama-French three-factor model for abnormal returns accounts for the inclination of retail investors to favor

small-cap stocks (Barber and Odean 2000). However, the Fama-French three-factor model may not align with the evaluation methods commonly used by most retail investors, who may focus primarily on the excess returns of stocks over the risk-free rate.

To bridge this gap, we also assess trading performance using excess returns above the risk-free rate, rather than the Fama-French three-factor abnormal returns. We also rerun each of the four identification strategies under equal weighting of households.¹⁴

Table IA.19 and Table IA.20 (excess return and equal-weighted performance measures, respectively) in the Internet Appendix show negative estimates throughout, with varying statistical reliability. The RDD and North–South excess return estimates are significant at the 5% level, in Table 4, Column (1), and Table IA.19, Panel B, respectively. Their equal-household-weighted counterparts are significant at the 10% level ($p = 0.092$ and $t = -1.92$ in Table IA.20, Panel B and C, respectively). The panel estimate is significant at the 5% level under equal-household weighting (in Table IA.20, Panel A) and at the 10% level with excess returns (in Table IA.19, Panel A). The spring DST DiD estimates are more sensitive, with p -values of 0.053 when using excess return, and 0.445 under equal-household weighting, respectively. With only six spring events and a small control portfolio, its statistical precision is limited. The DST design control portfolio contains only 43.4 households per event on average. The equal-weighted DiD estimate is smaller and not significant (-0.879 , $p = 0.445$).

We also form separate portfolios from purchases and sales. In Table IA.21, the border discontinuity is concentrated in the sell-transaction portfolio, whereas the spring DST difference-in-differences estimate is stronger for the buy-transaction portfolio; the side-specific ZIP-month and North–South estimates are imprecise. None of the reported buy–sell equality tests rejects at the 5% level, although the matched-cell RDD comparison provides marginal evidence of a larger adverse sell-side effect (robust $p = 0.059$; Table IA.21). The spring DST DiD estimate is significant on the buy side, but its difference from the sell-side estimate is not (wild-bootstrap $p = 0.577$), so the evidence does not establish a consistent predominance of either side. Our DST DiD test compares all households in the country that observe daylight saving against a small set of non-

¹⁴ In equal-weighted trade performance, we equal-weight households within each portfolio level used in the main tests. For example, in RDD, we first calculate each household’s raw trade-portfolio return for each month. We then calculate the portfolio-month raw return as the equal-weighted average of all non-missing household-month returns within each geographic cell-distance portfolio. We subsequently use these portfolio-month returns in the same estimation procedure described in the main RDD specification.

observing households, residing mainly in Arizona. This could raise concern that geographic differences in economic conditions could explain the difference in the change in trading performance around daylight saving time. To mitigate the concern, we conduct a subsample analysis focusing on two adjacent states: California and Arizona, following the existing literature (e.g., Janakiraman et al. 2024). The two states are geographically close but one observes daylight saving while the other does not. The geographical closeness renders similar economic conditions between the two states. Table IA.22 reports the result. Consistent with our hypothesis, the coefficient estimates for the interaction term of *AfterDST* and *ObservingDST* are negative, indicating worse trading performance following sleep disruption caused by the time changes.

6. Conclusion

This study investigates the relationship between mental alertness, as influenced by sleep, and the trading performance of retail investors. Building upon previous research, we use late or shifting sunset times as our proxy for sleep disruption.

The tests provide support for the hypothesis that sleep disruption impairs the information processing and trading performance of retail investors. The hypothesis links financial decisions to the timing of daylight and social schedules. Later evening light can delay sleep while morning obligations remain fixed, and clock changes can disrupt sleep timing. We use geographic and seasonal differences in sunset time, together with daylight saving time transitions, to examine whether these exposures are associated with poorer trading decisions and subsequent returns.

Later sunset is associated with lower abnormal returns on investor trades. In monthly comparisons, performance deteriorates when sunset within a residential ZIP code becomes later relative to contemporaneous changes elsewhere. Across time-zone boundaries, investors on the eastern side, where sunset occurs about an hour later by the clock, have lower trading performance. This difference is significantly larger in summer, consistent with evening light being more disruptive when sunset occurs closer to bedtime. A comparison across latitudes yields a related prediction. Northern locations have relatively later sunsets in summer and earlier sunsets in winter. Correspondingly, the performance of northern investors relative to southern investors deteriorates from winter to summer.

The spring switch to daylight saving time provides evidence around an abrupt clock adjustment. When clocks move forward, the overnight interval is shortened for someone

maintaining the same clock-based sleep schedule. Trading performance declines after this transition in observing locations relative to locations that do not change their clocks. The estimate is more sensitive to portfolio weighting than the monthly results, but its direction is consistent with the predicted consequences of disrupted sleep.

The earnings-news tests connect these performance patterns to specific decisions. Later sunset is associated with a lower probability of trading a held stock during its earnings-announcement window relative to surrounding days. On days when investors trade, later sunset also shifts net purchases away from stocks reporting earnings above expectations and toward stocks reporting earnings below expectations. Subsequent average returns tend to continue in the direction of the earnings surprise, so this is in general an inefficient way to allocate trades across surprises. Consistent with less effective use of earnings information, the negative relationship between sunset time and subsequent performance is more pronounced for announcement-stock trades than for other trades.

Together, the findings indicate that the capacity of investors to process information varies with time and conditions, and affects trading performance. Several alternative possible behavioral and rational explanations for our findings are ruled out. Less trading alone need not be harmful. However, the allocation and performance evidence suggests that loss of mental alertness impairs investment decision quality.

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Appendix A. Definitions of Variables

Variable name	Definition	Unit
Trade performance and market controls		
<i>TradePerform</i>	Trading-performance measure for the net-trade portfolio formed at the level of the relevant design. The main performance tests use factor-adjusted returns; the holding period and portfolio aggregation follow the cited table.	%
<i>21-day TradePerform</i>	Compounded trading performance over the 21 trading days following the portfolio-formation day, for the designated household, ZIP, or DST-group portfolio.	%
<i>Equal-weighted TradePerform</i>	Variant formed by averaging nonmissing household portfolio returns with equal weight within each portfolio and period before calculating the design-specific performance measure.	%
<i>Excess Return</i>	Portfolio return in excess of the risk-free rate.	%
<i>CAPM Alpha</i>	Intercept from a time-series regression of distance-group portfolio returns, adjusted as in Table 4, on the market excess return.	%
<i>FF3 Alpha</i>	Intercept from a time-series regression of distance-group portfolio returns, adjusted as in Table 4, on the Fama–French market, size, and value factors.	%
<i>Market Return (fwd 21)</i>	Compounded CRSP value-weighted market return over the following 21 trading days. Standardized in specifications that state so.	%
<i>Market Volatility (fwd 21)</i>	Annualized volatility of daily CRSP value-weighted market returns over the following 21 trading days. Standardized in specifications that state so.	decimal
<i>Market Return (past 21)</i>	Compounded CRSP value-weighted market return over the preceding 21 trading days. Standardized in specifications that state so.	%
<i>Market Volatility (past 21)</i>	Annualized volatility of daily CRSP value-weighted market returns over the preceding 21 trading days. Standardized in specifications that state so.	decimal
<i>Sunset</i>	Local sunset time at the household’s residential ZIP code on the evening preceding a trade or sale. For ZIP-month and household-month tests, the relevant daily values are averaged within the month; some regressions demean Sunset at its sample mean.	hours
<i>SAD</i>	Nighttime hours minus 12 during fall and winter, and zero during spring and summer; measured for the ZIP code and relevant date and averaged within the observation period where applicable.	hours
<i>Fall</i>	Fall-period indicator used as a seasonal control in the indicated trading-behavior specifications at daily level.	0/1
<i>SignedDistance</i>	Distance from the nearest time-zone border; negative west of the border and positive east of it.	km
<i>LateSide</i>	Indicator for a distance group east of the actual or corresponding pseudo-time-zone border.	0/1

Variable name	Definition	Unit
<i>North</i>	Indicator for a monthly portfolio of households in the top quartile of the latitude distribution; the bottom quartile is South.	0/1
<i>Summer</i>	Indicator for April–September; October–March is winter.	0/1
<i>AfterDST</i>	Indicator for trading days after a spring or fall daylight saving time transition within the event window; days before the transition are zero.	0/1
<i>ObservingDST</i>	Indicator for the DST-observing portfolio. In Table IA.22, California is one and Arizona is zero.	0/1
Earnings announcements and trading responses		
<i>EWindow</i>	Indicator for an eligible stock's earnings-announcement day and the following two trading days, within the window from five trading days before through five days after the announcement.	0/1
<i>TradeRate</i>	Natural logarithm of the household's trading frequency during its observed time in the brokerage sample.	log frequency
<i>High / Low Disagreement</i>	Classification above / below the median analyst forecast dispersion among announcements in the same year.	group
<i>EAttrade</i>	Indicator for a trade portfolio consisting of stocks whose earnings were announced during trading days $[t - 2, t]$; other trades are zero.	0/1
<i>SUE</i>	Standardized unexpected earnings, ranked into deciles separately within each quarter. Positive-SUE stocks are in deciles 7–10 and negative-SUE stocks in deciles 1–4.	decile
<i>NetPurchase</i>	Dollar value purchased minus dollar value sold for a stock, or for a specified positive- or negative-SUE trade portfolio, on the household trading day.	\$ thousands
<i>Diff(NetPurchase)</i>	<i>NetPurchase</i> for positive-SUE stocks minus <i>NetPurchase</i> for negative-SUE stocks on the same household trading day.	\$ thousands
<i>Net Sales of positive SUE</i>	Dollar value sold minus dollar value purchased for recently announcing positive-SUE stocks on the household trading day.	\$ thousands
<i>Net Purchases of negative SUE</i>	Dollar value purchased minus dollar value sold for recently announcing negative-SUE stocks on the household trading day.	\$ thousands
Household characteristics and trading preferences		
<i>ln(Ntrade)</i>	Natural logarithm of a household's total number of common-stock transactions during the sample period.	log transactions
<i>Age</i>	Household age measure used in the predetermined-characteristic balance tests.	years
<i>Gender</i>	Indicator equal to one for female and zero for male.	0/1
<i>Nstock Traded</i>	Number of different stocks traded by the household during the sample period.	stocks
<i>Avg. Position Equity</i>	Household's average total equity value in its monthly stock portfolio during the sample period.	\$

Variable name	Definition	Unit
<i>Avg. Position Nstock</i>	Household's average number of stocks held in its monthly stock portfolio during the sample period.	stocks
<i>Income</i>	Category-based proxy for estimated household income.	\$ thousands
<i>Turnover</i>	Total monthly value of stock purchases plus sales, divided by twice the value of the household's portfolio at the beginning of the month, multiplied by 100.	%
<i>Elapsed Days Since Earliest Observed Purchase</i>	For a household-stock sale, the number of days between the sale and the household's earliest observed preceding purchase of that stock.	days
<i>Lottery</i>	Indicator for a stock whose previous month-end price is at or below the monthly median and whose idiosyncratic volatility and skewness are both at or above their monthly medians.	0/1
<i>Previous month-end price</i>	Stock price at the end of the preceding calendar month, used to classify lottery-type stocks.	\$ per share
<i>Idiosyncratic volatility</i>	Residual variance from a regression of daily stock excess returns over the preceding six calendar months on the Fama–French three factors and the momentum factor.	return squared
<i>Idiosyncratic skewness</i>	Standardized third moment of residuals from a regression of daily stock excess returns over the preceding six calendar months on the market excess return and its square.	unitless
<i>AGstock</i>	Indicator for a stock in the top daily decile of abnormal trading volume or, in the alternative specification, the top daily decile of one-day stock returns.	0/1
<i>Abnormal trading volume</i>	Dollar trading volume of a stock on day divided by its average daily dollar trading volume over the preceding 252 trading days. Stocks are sorted into deciles on this ratio each trading day.	ratio
<i>One-day stock return</i>	Stock return on the trading day, used for the daily cross-sectional decile sort defining the other version of <i>AGstock</i> .	%
<i>Momentum</i>	Indicator for a stock in the top daily decile of past 21-day returns (Panel A) or past 12-month returns (Panel B).	0/1
<i>Sample initial buy</i>	Purchase for which the household's first observed transaction in that stock, whether a purchase or sale, is this buy; earlier transactions outside the sample are unobserved.	0/1
<i>RG / RL</i>	Counts of realized gains / realized losses among a household's sales on a given date, determined by comparing sale prices with average purchase prices adjusted for dividends and splits.	count
<i>PG / PL</i>	Counts of paper gains / paper losses among other stocks held but not sold on that date. A paper gain requires the daily low to exceed the adjusted purchase-price reference; a paper loss requires the daily high to fall below it.	count
<i>PGR</i>	Proportion of gains realized: $RG / (RG + PG)$, defined when the denominator is positive.	decimal
<i>PLR</i>	Proportion of losses realized: $RL / (RL + PL)$, defined when the denominator is positive.	decimal
<i>DE</i>	Disposition effect at the household-sale-date level: $PGR - PLR$; positive values indicate a greater propensity to realize gains than losses.	decimal

Figure 1: Distribution of Households and Time-Zone Borders

The figure presents the geographic distribution of 41,131 households in the contiguous United States based on ZIP-code information from the large discount brokerage data. The sample spans four time zones, Pacific, Mountain, Central, and Eastern, and the three time-zone borders separating them.

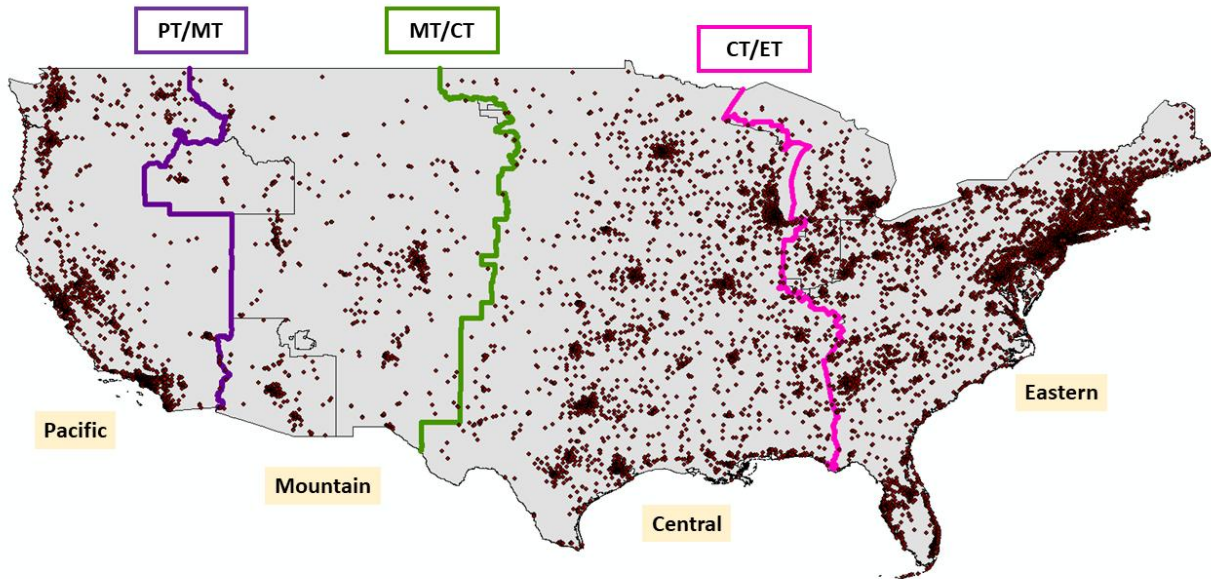


Figure 2: Local Sunset Discontinuity at Time-Zone Borders

The figure presents discontinuity in local sunset time at time-zone borders. Local sunset time is calculated using the latitude and longitude of ZIP-code centroids in the sample. $SignedDistance_h$ measures the distance of household h from the nearest time-zone border, with negative values indicating locations west of the border and positive values indicating locations east of the border. The time-zone border, at $SignedDistance_h = 0$, is indicated by the dashed vertical line. We construct 20 bins on each side of the border, equalizing the number of observations across bins as closely as possible. Circles represent the average local sunset time within each $SignedDistance_h$ bin and are plotted at the mean $SignedDistance_h$ of the bin. The two solid lines depict the fitted relationship between average local sunset time and $SignedDistance_h$ on either side of the time-zone border, estimated using the following equation:

$$AverageLocalSunsetTime_h = \alpha + \beta_1 LateSide_h + \beta_2 SignedDistance_h + \varepsilon_h$$

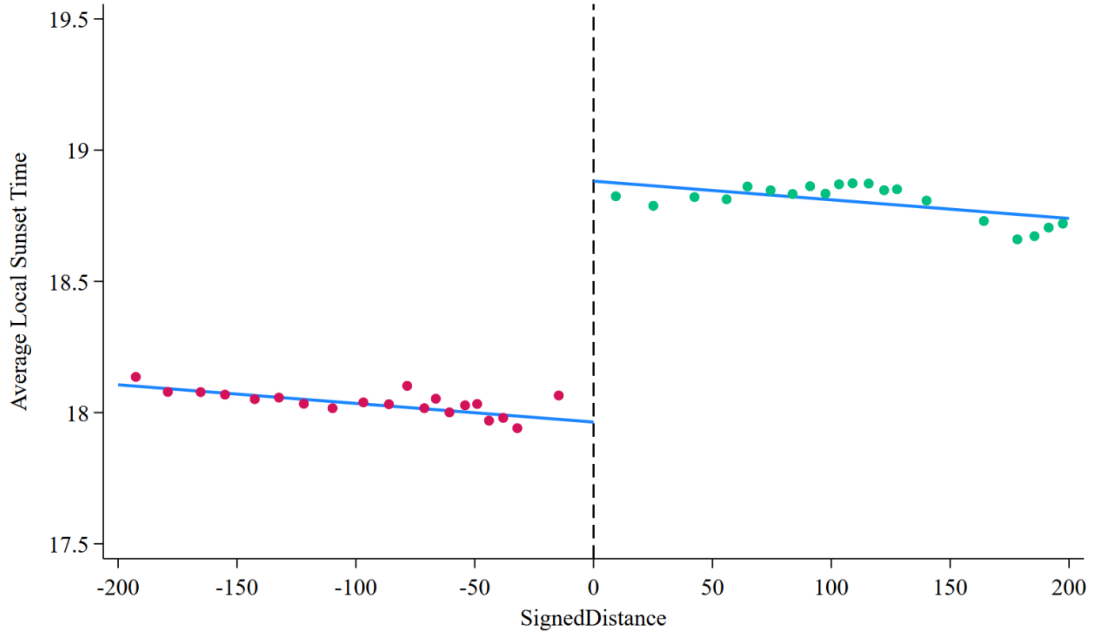


Figure 3: Trade Performance Discontinuity at Time-Zone Borders

The figure presents the discontinuity in trade performance at time-zone borders. For the RDD analysis, households are grouped by ZIP code within nine geographic cells defined by the interaction of the three time-zone borders and three latitude terciles. For each distance group h , we aggregate households' net trades into a monthly calendar-time portfolio and estimate its Fama-French three-factor (FF3) alpha, which we denote by $TradePerform_h$, as described in Section 3.2. $SignedDistance_h$ measures the distance of group h from the nearest time-zone border, with negative values indicating locations west of the border and positive values indicating locations east of the border. The time-zone border, at $SignedDistance_h = 0$, is indicated by the dashed vertical line. $LateSide_h$ equals one for distance groups east of the time-zone border. Circles represent binned averages of the FF3 alpha and are plotted at the mean $SignedDistance_h$ within each bin. The figure displays observations within 250 km of the time-zone border. The two solid lines depict local linear fits estimated separately on either side of the border using the bandwidths reported in Table 4, Column (3). The fitted relationships are estimated using the following specification:

$$TradePerform_h = \alpha + \beta LateSide_h + \theta SignedDistance_h + \delta(LateSide_h \times SignedDistance_h) + \varepsilon_h$$

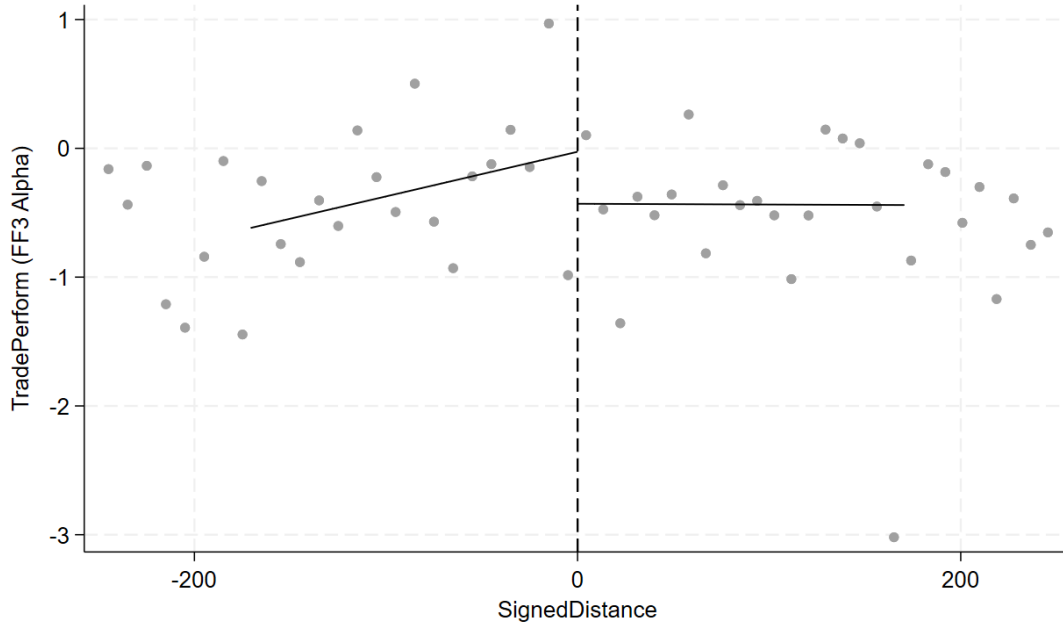


Figure 4: Sunset Variation across Latitudes: Northern versus Southern Regions

The figure illustrates variation in local sunset time over the course of 1994 for Miami, Florida, and Buffalo, New York. The centers of Miami and Buffalo are located at coordinates (25.7617, -80.1918) and (42.8864, -78.8784), respectively. The dashed line represents Buffalo, and the solid line represents Miami. The two cities have similar longitudes but substantially different latitudes, with Buffalo located farther north than Miami. As a result of this latitude difference, their local sunset times cross twice during the year, around March and September.

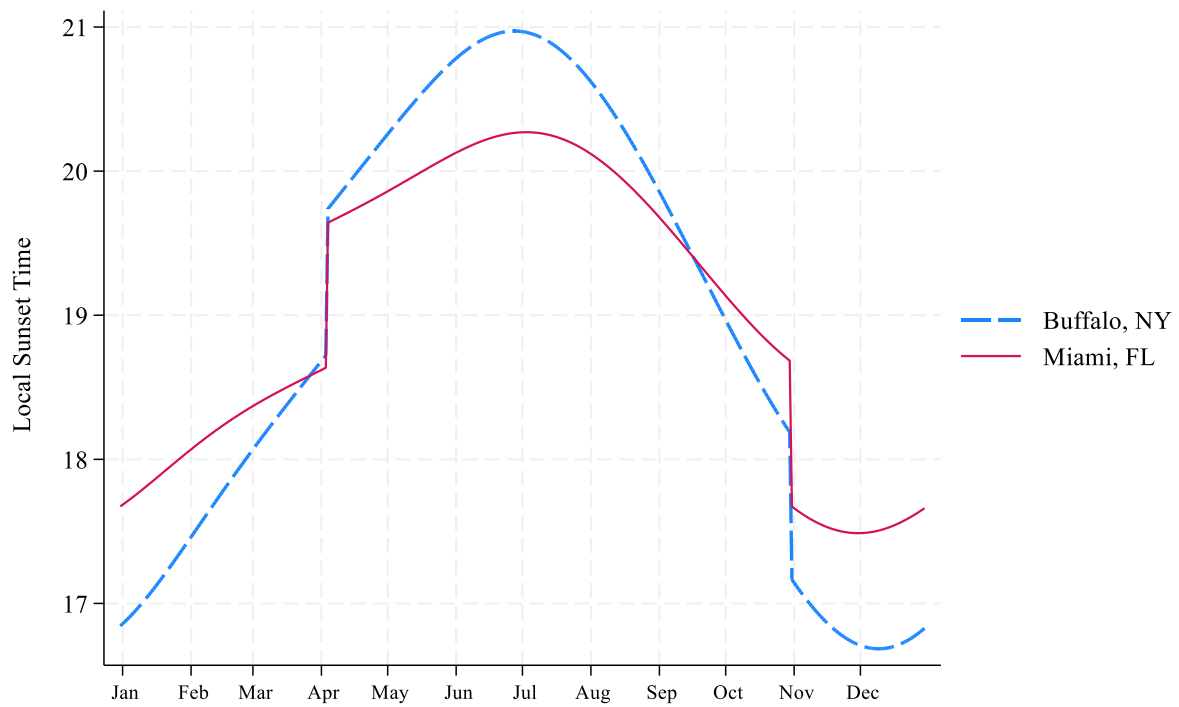


Table 1: Sample Distribution by Time Zone

This table describes households in the discount brokerage sample by time zone. Households are assigned to time zones using their residential ZIP codes. Panel A reports the number and share of households and the numbers of states, counties, and ZIP codes. Panel B reports household trading days, transactions, buys, sells, and distinct common stocks traded, along with trade size and trade price statistics per household. Trade size is measured in shares and trade price in dollars; their means and standard deviations are reported separately.

	(1)	(2)	(3)	(4)	(5)
	Pacific	Mountain	Central	Eastern	Entire
Panel A. Geographic distribution of households					
Households	13,619	2,369	8,821	16,322	41,131
Share of households (%)	33.11	5.76	21.45	39.68	100.00
State	6	11	21	24	49
County	142	162	766	787	1,853
ZIP Code	1,798	651	2,712	5,086	10,247
Panel B. Trading activity					
Household trading days	279,042	44,866	178,432	339,967	842,307
Transactions	381,323	60,009	246,625	465,403	1,153,360
Buys	207,371	32,699	136,082	256,983	633,135
Sells	173,952	27,310	110,543	208,420	520,225
Distinct common stocks traded	7,560	4,650	7,417	8,542	9,722
Trade size (shares), mean	695.74	639.91	610.74	621.58	644.73
Trade size (shares), SD	1648.11	1874.37	1473.31	1682.37	1639.64
Trade price (\$), mean	30.41	30.59	29.64	30.27	30.20
Trade price (\$), SD	93.78	85.03	92.76	95.72	93.92

Table 2: Summary Statistics of Main Variables

This table reports the number of observations (N), mean, standard deviation, 25th percentile, median, and 75th percentile for variables used in Tables 3, 4, 7, and 11. Panel A uses ZIP-month portfolios formed by aggregating household trades within each ZIP code. *TradePerform* is the portfolio trading-performance measure defined in Equation (2). *Sunset* is local sunset time on the day before each trade, averaged within the ZIP-month. *SAD* is nighttime hours minus 12 during fall and winter and zero during spring and summer, averaged within the ZIP-month. Panel B describes the time-zone-border design. *SignedDistance* is the distance from the nearest time-zone border, measured in kilometers; negative values indicate locations west of the border and positive values indicate locations east of it. *Excess Return* is the time-series average of monthly benchmark-adjusted distance-group portfolio returns. *CAPM Alpha* and *FF3 Alpha* are the intercepts from regressions of those returns on the market excess return and, for FF3 Alpha, the size and value factors. Panel C uses household-day-portfolio observations. *21-day TradePerform* is compounded trading performance over the following 21 trading days for the portfolios defined in Table 11. *Sunset* is local sunset time at the household's ZIP code on the preceding evening. *SAD* is measured at the same ZIP code and date. *Market Return (fwd 21)* is the compounded return on the CRSP value-weighted market index over the following 21 trading days; *Market Volatility (fwd 21)* is the annualized volatility of its daily returns over the same period. Panel D uses monthly portfolios formed separately by latitude group, time zone, and longitude bin. North and South comprise households in the top and bottom quartiles of latitude, respectively. Summer covers April-September and winter covers October-March. The panel reports the mean and standard deviation of portfolio *TradePerform* and local sunset time for each latitude-group-by-season combination; N counts portfolio-month observations.

Variable	N	Mean	SD	P25	Median	P75
Panel A. ZIP-Month level variables (Table 3)						
TradePerform (%)	260,378	-0.249	9.543	-4.833	-0.162	4.385
Sunset (hours)	260,378	18.756	1.371	17.536	18.774	20.036
SAD	260,378	0.735	0.954	0.000	0.006	1.504
Panel B. SignedDistance level variables (Table 4)						
SignedDistance (km)	10,219	300.442	574.529	-237.902	309.957	764.264
TradePerform (Excess Return, %)	6,698	-0.349	2.172	-1.584	-0.347	0.880
TradePerform (CAPM Alpha, %)	6,698	-0.356	2.253	-1.605	-0.322	0.921
TradePerform (FF3 Alpha, %)	6,698	-0.401	2.464	-1.746	-0.373	0.937
Panel C. Household-Day-Portfolio level variables (Table 11)						
21-day TradePerform (%)	783,414	-0.189	12.071	-6.212	-0.121	5.822
Sunset (hours)	783,414	18.718	1.371	17.477	18.671	20.009
SAD	783,414	0.748	0.956	0.000	0.000	1.547
Market Return (fwd 21) (%)	783,414	1.397	2.914	-0.370	1.323	3.041
Market Volatility (fwd 21)	783,414	0.093	0.028	0.071	0.090	0.108
Panel D. Latitude-group-Month level (Table 7)						
	N	Mean TradePerform (%)	SD TradePerform (%)	Mean Sunset (hours)	SD Sunset (hours)	
South, Winter	583	-0.485	3.918	17.847	0.601	
North, Winter	608	-0.130	4.369	17.516	0.766	
South, Summer	610	-0.279	3.709	19.821	0.539	
North, Summer	614	-0.560	4.277	20.312	0.644	

Table 3: Trading Performance and Local Sunset Time: ZIP-Month Panel Regressions

This table reports estimates from ZIP-month panel regressions of trading performance on local sunset time. The unit of observation is a ZIP-month portfolio constructed by aggregating the trades of households within each ZIP code. *TradePerform* is measured as the Fama–French three-factor abnormal return defined in Equation (2). *Sunset* is the average local sunset time at the ZIP code’s location on the day before each trade, averaged within the ZIP-month. *SAD* is the corresponding average of nighttime hours minus 12 during fall and winter and zero during spring and summer, following Kamstra, Kramer, and Levi (2003). All specifications include ZIP-code and year-month fixed effects. Standard errors are clustered by year-month (71 clusters) in Columns (1) and (2) and two-way clustered by ZIP-code and year-month (71 year-month clusters) in Column (3). *t*-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

$$TradePerform_{h,m \rightarrow m+1} = \alpha + \beta_1 Sunset_{h,m} + \beta_2 SAD_{h,m} + \gamma_h + \gamma_m + \varepsilon_{h,m}$$

	(1)	(2)	(3)
Sunset	-0.182** (-2.29)	-0.293** (-2.48)	-0.293** (-2.50)
SAD		-0.176 (-1.16)	-0.176 (-1.18)
ZIP Code FE	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes
Adjusted R^2	0.0092	0.0092	0.0092
Observations	260,378	260,378	260,378

Table 4: Trading Performance at Time-Zone Borders: Regression Discontinuity Design

This table reports regression discontinuity estimates of trading performance at U.S. time-zone borders. The unit of observation is a distance-group h . Households are grouped by ZIP code within nine geographic cells defined by the interaction of the three time-zone borders and three latitude terciles, and their net trades are aggregated into monthly distance-group portfolios. $SignedDistance_h$ measures the distance from the nearest time-zone border, with negative values indicating locations west of the border and positive values indicating locations east of the border. $LateSide_h$ equals one for groups located east of the border. $TradePerform_h$ is measured following Equation (4). Columns (1)–(3) use Excess Return, CAPM Alpha, and FF3 Alpha, respectively, as measures of $TradePerform$. For each distance group, we construct a time series of monthly portfolio returns from the group’s net trades. We demean each monthly raw portfolio return by the average raw portfolio return across distance groups in the same geographic cell and month and subtract the risk-free rate. Excess Return is the time-series average of the resulting monthly returns. CAPM Alpha and FF3 Alpha are the intercepts from time-series regressions of these returns on the market excess return and on the Fama–French three factors, respectively. Each measure produces a single $TradePerform$ observation for each distance group. The RDD is estimated using local linear regressions with separate slopes on either side of the cutoff and a triangular kernel. Bandwidths are selected using the MSE-optimal RD bandwidth selector and are reported at the bottom of the table. Reported p -values are robust bias-corrected p -values, with standard errors clustered by 10-km distance bin. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

$$TradePerform_h = \alpha + \beta LateSide_h + \theta SignedDistance_h + \delta(LateSide_h \times SignedDistance_h) + \varepsilon_h$$

Dependent Variable	(1) Excess Return	(2) CAPM Alpha	(3) FF3 Alpha
LateSide	-0.531***	-0.448**	-0.404*
Robust p -value	< 0.001	0.027	0.074
Observations Left	2,486	2,486	2,486
Observations Right	4,212	4,212	4,212
Bandwidth	229.4	213.6	170.6

Table 5: Household Characteristics and Trading Measures at Time-Zone Borders

This table examines household characteristics and trading measures around time-zone boundaries using the RDD specification in Table 4. Trading and average-holdings measures are observed during the sample and are not all predetermined relative to time-zone exposure. Household characteristics are first aggregated to the distance-group level and then de-measured within each geographic-cell-by-month group. The dependent variable, $Characteristics_h$, includes: (1) $\ln(Ntrade)$, the natural logarithm of the household's total number of common-stock transactions over the sample period; (2) Age , the household age measure in years; (3) $Gender$, an indicator equal to one for female and zero for male; (4) $Nstock\ Traded$, the number of stocks traded by the household over the sample period; (5) $Avg.\ Position\ Equity$, the household's average total equity in its monthly portfolio over the sample period; (6) $Avg.\ Position\ Nstock$, the household's average number of stocks held in its monthly portfolio over the sample period; and (7) $Income$, a category-based proxy for estimated household income, expressed in thousands of dollars. The table reports the estimated discontinuity at the time-zone border for each characteristic. Reported p -values are robust bias-corrected p -values, with standard errors clustered by 10-km distance bin. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

$$Characteristics_h = \alpha + \beta LateSide_h + \theta SignedDistance_h + \delta(LateSide_h \times SignedDistance_h) + \varepsilon_h$$

Dependent Var.	(1) $\ln(Ntrade)$	(2) Age	(3) $Gender$	(4) $Nstock\ Traded$	(5) $Avg.Position\ Equity$	(6) $Avg.Position\ Nstock$	(7) $Income$
LateSide	-0.0185	0.577	0.0152	-1.463	-1905.9	-0.00418	6.035***
Robust p -value	0.679	0.254	0.759	0.162	0.669	0.816	< 0.001
Obs. Left	3,693	3,241	3,337	3,693	3,639	3,639	3,355
Obs. Right	6,504	5,682	5,489	6,504	6,424	6,424	5,502
Bandwidth	180.9	252.4	138.1	169.3	194.1	142.4	224.6

Table 6: RDD Estimates by Season

This table examines seasonal heterogeneity in the regression discontinuity design (RDD). The RDD specification is the same as in Table 4. For each distance-group portfolio, we separately estimate FF3 alpha as *TradePerform* using returns from summer months (April–September) and winter months (October–March). Column (1), Summer (April–September), uses as the dependent variable the FF3 alpha estimated using only April–September returns. Column (2), Winter (October–March), uses the FF3 alpha estimated using only October–March returns. Column (3), Summer – Winter, uses the difference between the summer FF3 alpha and winter FF3 alpha for each distance-group portfolio as the dependent variable and estimates a single RDD on this difference. This specification directly estimates the difference in discontinuities while avoiding bandwidth differences across separately estimated seasonal RDDs. The table reports the estimated discontinuity at the time-zone boundary. Reported *p*-values are robust bias-corrected *p*-values, with standard errors clustered by 10-km distance bin. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

$$TradePerform_h = \alpha + \beta LateSide_h + \theta SignedDistance_h + \delta(LateSide_h \times SignedDistance_h) + \varepsilon_h$$

	(1)	(2)	(3)
Subsample	Summer (April-September)	Winter (October-March)	Summer - Winter
Dependent Variable		FF3 Alpha	
LateSide	-1.108***	-0.373	-0.939**
Robust <i>p</i> -value	< 0.001	0.457	0.049
Observations Left	1,767	1,859	1,656
Observations Right	2,815	2,977	2,594
Bandwidth	193.1	115.5	169.1

Table 7: North-South Differences in Trade Performance across Seasons

This table examines whether the difference in trade performance between northern and southern households varies across seasons. Households in the bottom 25% of the latitude distribution are classified as South, and households in the top 25% are classified as North. Within these latitude groups, we form monthly portfolios separately by time zone and longitude bin, using 3-degree-longitude bins within each time zone. This construction compares northern and southern households located in the same time zone and at similar east–west positions, so that differences in local sunset timing are more directly attributable to latitude. For each latitude-group $\times$ time-zone $\times$ longitude-bin $\times$ month cell, we construct *TradePerform* following Equation (2). *North* is an indicator equal to one for portfolios in the northern group and zero for those in the southern group. *Summer* is an indicator equal to one for April through September and zero for October through March. *North* $\times$ *Summer* measures whether the difference in trade performance between northern and southern portfolios is different in summer than in winter. During summer (April–September), northern locations experience relatively later sunsets than southern locations. Column (1) reports the baseline specification. Column (2) adds time-zone $\times$ longitude-bin fixed effects. Column (3) additionally includes year-month fixed effects. Standard errors are clustered by year-month (71 clusters). *t*-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

$$TradePerform_{h,m \rightarrow m+1} = \alpha + \beta_1 North_h + \beta_2 Summer_m + \beta_3 North_h \times Summer_m + \gamma_{TZ \times LongitudeBin} + \gamma_m + \varepsilon_{h,m+1}$$

	(1)	(2)	(3)
North	0.356* (1.79)	0.356* (1.71)	0.353* (1.69)
Summer	0.207 (0.87)	0.206 (0.87)	—
North $\times$ Summer	-0.637** (-2.35)	-0.646** (-2.37)	-0.644** (-2.36)
Time-zone $\times$ Longitude bin FEs	No	Yes	Yes
Year-Month FE	No	No	Yes
Adjusted R^2	0.0005	0.0036	0.0213
Observations	2,415	2,415	2,415

Table 8: DST Transitions and Portfolio Trade Performance: Difference-in-Differences

This table examines changes in trading performance around daylight saving time (DST) transitions using a difference-in-differences design. For each DST transition, we classify investors into DST-observing and non-DST-observing groups. The non-DST-observing group consists of households in Arizona and in Indiana counties that did not observe DST during the sample period. For each group, we construct the 21-day *TradePerform* measure for each trading day within the $[-5, +5]$ trading-day window around the DST transition following Equation (7). *AfterDST* is an indicator equal to one for trading days after the DST transition and zero for trading days before the transition. *ObservingDST* is an indicator equal to one for the DST-observing group and zero for the non-DST-observing group. Columns (1)-(3) report estimates around the spring DST transition, and Columns (4)-(6) report estimates around the fall DST transition. Columns (1) and (4) report the baseline specification. Columns (2) and (5) control for forward 21-day market return and market volatility, while Columns (3) and (6) control for past 21-day market return and market volatility. All specifications include DST-event and day-of-week fixed effects. All market control variables are standardized. Standard errors are clustered by DST event (6 clusters). Given the small number of event clusters, we report wild cluster bootstrap *p*-values in brackets, using Webb six-point weights with 9,999 replications and clustering by DST event. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively, based on the wild cluster bootstrap *p*-values.

$$\begin{aligned} TradePerform_{h,t \rightarrow t+21} \\ = \alpha + \beta_1 AfterDST_t + \beta_2 ObservingDST_h + \beta_3 AfterDST_t \times ObservingDST_h + X'_t \delta \\ + \gamma_{DSTevent} + \gamma_{DayofWeek} + \varepsilon_{ht} \end{aligned}$$

	(1)	(2)	(3)	(4)	(5)	(6)
	Spring DST transition			Fall DST transition		
AfterDST	2.977*	3.397	3.487*	3.474	4.209**	4.691**
	[0.075]	[0.133]	[0.058]	[0.153]	[0.045]	[0.045]
ObservingDST	1.793*	1.793*	1.793*	0.702	0.702	0.702
	[0.070]	[0.070]	[0.070]	[0.679]	[0.679]	[0.679]
AfterDST×ObservingDST	-3.973**	-3.973**	-3.973**	-3.094	-3.094	-3.094
	[0.021]	[0.021]	[0.021]	[0.190]	[0.190]	[0.190]
Market Return (fwd 21)		0.412			-0.992	
		[0.295]			[0.410]	
Market Volatility (fwd 21)		1.144			2.738*	
		[0.445]			[0.100]	
Market Return (past 21)			0.214			-1.274
			[0.480]			[0.299]
Market Volatility (past 21)			-1.068			-4.297
			[0.294]			[0.151]
DST Event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Week FEs	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.046	0.039	0.061	0.065	0.062	0.077
Observations	120	120	120	120	120	120

Table 9: Trading Probability Around Earnings Announcements

This table examines whether local sunset time affects the probability that a household trades a stock around its earnings announcement. We construct a household-stock-day panel in which each observation corresponds to a trading day on which household h holds stock i . The sample is restricted to trading days within five trading days before or after an earnings announcement of the held stock. $EAwindow$ equals one on the announcement day and the two subsequent trading days and zero otherwise. $Sunset$ is the demeaned local sunset time, measured in hours, at the household's ZIP code on the preceding evening. SAD is the seasonal affective disorder measure at the same ZIP-day. $TradeRate$ is the household's log trading frequency over its time in the sample. Columns (1) and (2) estimate stratified Cox proportional-hazard models in which the outcome is whether the household trades the held stock on a given day. The models are stratified by stock-day, allowing the baseline hazard to vary freely across stock-days and thereby absorbing factors common to all households holding the same stock on the same day. Reported coefficients in Columns (1) and (2) are on the log-hazard scale; $\exp(\beta)$ is the corresponding hazard ratio. Column (3) estimates a Poisson regression with household and stock-day fixed effects, providing an alternative specification that additionally absorbs time-invariant differences in households' trading propensities. Column (3) coefficients are on the log conditional trading-probability scale, so $\exp(\beta)$ is the corresponding ratio of trading probabilities. The main effect of $EAwindow$ is absorbed by the stock-day strata in Columns (1) and (2) and by the stock-day fixed effects in Column (3) and is therefore omitted from the table. Standard errors are clustered by ZIP code, and z-statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

$$\lambda_{hit} = \lambda_{0,it} \exp(\beta_1 Sunset_{h,t-1} + \beta_2 EAwindow_{it} + \beta_3 Sunset_{h,t-1} \times EAwindow_{it} + \beta_4 SAD_{ht} + \beta_5 TradeRate_h)$$

	(1)	(2)	(3)
	Cox Hazard Model, Stock-Day Strata		Poisson, Household & Stock-Day FEs
Sunset	-0.0452 (-1.06)	-0.0384 (-0.90)	-0.0187 (-0.60)
EAwindow $\times$ Sunset	-0.0432** (-2.42)	-0.0630*** (-3.08)	-0.0611*** (-3.01)
SAD	-0.0485 (-1.29)	-0.0293 (-0.75)	0.0361 (0.96)
EAwindow $\times$ SAD		-0.0549* (-1.82)	-0.0534* (-1.78)
TradeRate	0.4779*** (35.08)	0.4779*** (35.07)	
Observations	31,607,126	31,607,126	13,549,449

Table 10: Sunset and Directional Trading Responses to Earnings Surprises

This table examines how local sunset time is associated with households' trading responses to recent earnings announcements. For each household h on each trading day t , we construct a positive-SUE portfolio consisting of stocks traded by the household that announced earnings during $[t - 2, t]$ and are in SUE deciles 7-10, and a negative-SUE portfolio consisting of stocks traded by the household that announced earnings during the same window and are in SUE deciles 1-4. SUE deciles are formed within each quarter. A household-day enters the sample if the household trades any stock on that day. For each household-day, we calculate *NetPurchase* separately for the positive- and negative-SUE portfolios as the dollar value purchased minus the dollar value sold. We set *NetPurchase* to be zero for a SUE portfolio in which the household does not trade. *Diff(NetPurchase)* is the net purchase of positive-SUE stocks minus the net purchase of negative-SUE stocks, so a more positive value indicates greater net buying of positive-SUE stocks relative to negative-SUE stocks. Columns (1) and (2) use *Diff(NetPurchase)* as the dependent variable, with Column (2) adding controls. Column (3) uses net sales of positive-SUE stocks, defined as the dollar value sold minus the dollar value purchased, and Column (4) uses net purchases of negative-SUE stocks. Thus, Columns (3) and (4) decompose the relative trading response examined in Columns (1) and (2). All dependent variables are measured in thousands of dollars. *Sunset* is local sunset time, in hours, at the household's ZIP code on the preceding day. Controls include *SAD*, past 21-day market return, and past 21-day market volatility. All specifications include household, year-quarter, and day-of-week fixed effects. Standard errors are two-way clustered by ZIP code and trading day. t -statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

$$Diff(NetPurchase_{h,t}) = \alpha + \beta Sunset_{h,t-1} + X'_{ht}\delta_1 + \gamma_h + \gamma_{yearquarter} + \gamma_{dayofweek} + \varepsilon_{ht}$$

Dependent Variable	(1) Diff(NetPurchase)	(2) Diff(NetPurchase)	(3) Net Sales of positive SUE	(4) Net Purchases of negative SUE
Sunset	-0.075*** (-3.65)	-0.132*** (-4.43)	0.042** (2.27)	0.090*** (4.38)
Controls	No	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes
within- R^2	0.0000	0.0000	0.0000	0.0000
Observations	805,175	805,175	805,175	805,175

Table 11: Trading Performance of Earnings-Announcement and Other Trades

This table examines whether the relation between local sunset time and trading performance differs for trades in stocks with recent earnings announcements. For each household h on trading day t , we separate trades into two portfolios: an earnings-announcement portfolio, consisting of stocks that announced earnings during $[t - 2, t]$, and an other-trades portfolio, consisting of all other stocks traded by the household on that day. For each portfolio, we construct the compounded 21-day *TradePerform* measure following Equation (7). *EAttrade* is an indicator equal to one for the earnings-announcement portfolio and zero for the other-trades portfolio. *Sunset* is local sunset time, in hours, at the household's ZIP code on the preceding evening and is demeaned at the sample mean. The analysis is conducted at the household-day-portfolio level. Column (1) reports the baseline specification, Column (2) adds *Sunset* and controls, and Column (3) includes *EAttrade* $\times$ *Sunset* and interactions between *EAttrade* and each control variable. Controls include *SAD*, future 21-day market return, and future 21-day market volatility. All specifications include household, year-quarter, and day-of-week fixed effects. Standard errors are two-way clustered by ZIP code and year-month (71 year-month clusters). t -statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

$$\begin{aligned} TradePerform_{h,EAttrade,t \rightarrow t+21} \\ = \alpha + \beta_1 EAttrade_{ht} + \beta_2 Sunset_{h,t-1} + \beta_3 (EAttrade_{ht} \times Sunset_{h,t-1}) + X'_{ht} \delta_1 \\ + EAttrade_{ht} (X'_{ht} \delta_2) + \gamma_h + \gamma_{yearquarter} + \gamma_{dayofweek} + \varepsilon_{ht}, \end{aligned}$$

	(1)	(2)	(3)
EAttrade	-0.143 (-1.21)	-0.140 (-1.23)	-0.108 (-1.01)
Sunset		0.039 (0.39)	0.051 (0.52)
EAttrade $\times$ Sunset			-0.311** (-2.06)
Controls	No	Yes	Yes
Household FE	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes
Adjusted R^2	0.0010	0.0011	0.0011
Observations	783,414	783,414	783,414

Internet Appendix for “Sleep Disruption and Retail Investor Trading”

September 2026

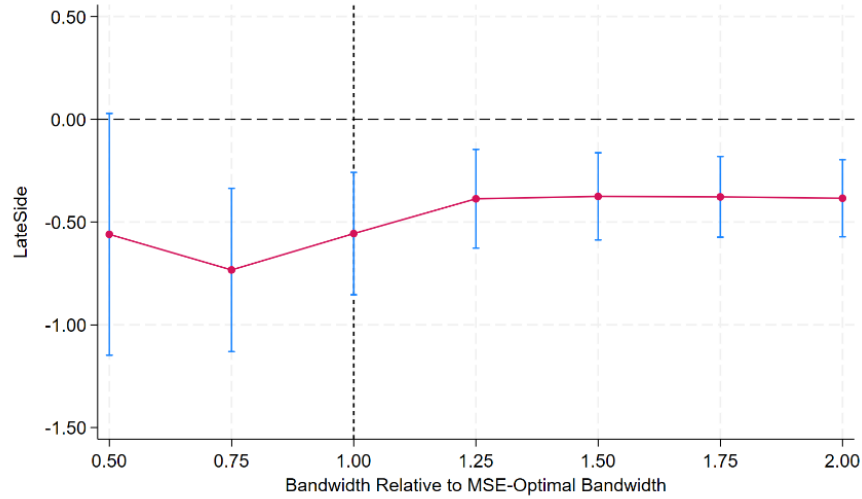
This appendix includes additional figures and tables used in the paper:

- Figure IA.1: Sensitivity of RDD Estimates to Bandwidth Choice
- Figure IA.2: Trade Performance Discontinuity at Time-Zone Borders: Alternative Measures
- Figure IA.3: Pseudo-Time-Zone Borders
- Table IA.1: RDD Estimates by Income Group
- Table IA.2: Density Test of SignedDistance at Time-Zone Borders
- Table IA.3: RDD Placebo Cutoffs
- Table IA.4: Donut RDD: Excluding Observations within 30 km of the Time-Zone Borders
- Table IA.5: RDD within States Spanning Two Time Zones
- Table IA.6: RDD at Pseudo-Time-Zone Borders
- Table IA.7: Time-Series Estimates around DST Transitions for DST-Observing Investors
- Table IA.8: Trading around Earnings Announcements without Holding Restriction
- Table IA.9: Trading Probability around Earnings Announcements by Analyst Disagreement
- Table IA.10: Trading Performance of Earnings-Announcement and Other Trades with Trade-Date Fixed Effects
- Table IA.11: Trading Performance of Earnings-Announcement and Other Trades at the ZIP-Day Level
- Table IA.12: Household Monthly Portfolio Turnover and Local Sunset Time
- Table IA.13: Elapsed Days Since Earliest Observed Purchase Among Sales
- Table IA.14: Lottery-Type Stock Preference and Local Sunset Time
- Table IA.15: Net Purchases of Attention-Grabbing Stocks and Local Sunset Time
- Table IA.16: Net Purchases of Stocks with High Past Returns and Local Sunset Time
- Table IA.17: Past-Return Tilt in Sample Initial Buy
- Table IA.18: The Disposition Effect and Local Sunset Time
- Table IA.19: Robustness to Measuring Trade Performance with Excess Returns
- Table IA.20: Robustness to Equal Weighting Households in Trade-Performance Portfolios
- Table IA.21: Buy- and Sell-Transaction Portfolio Trade Performance
- Table IA.22: DST Transitions and Portfolio Trade Performance: California versus Arizona

Figure IA.1: Sensitivity of RDD Estimates to Bandwidth Choice

The figure presents the sensitivity of the RDD estimates to alternative bandwidth choices. Panels A, B, and C use Excess Return, CAPM Alpha, and FF3 Alpha, respectively, as measures of *TradePerform*, as described in Section 3.2. For each outcome, the baseline bandwidth is the MSE-optimal bandwidth used in Table 4. The horizontal axis reports the bandwidth as a multiple of this baseline bandwidth, ranging from 0.5 to 2.0. At each multiplier, we estimate a local linear RDD using a triangular kernel and scale both the estimation and bias-correction bandwidths by the same multiplier. Points represent robust bias-corrected RDD estimates, and vertical bars represent the corresponding 95% robust confidence intervals. Standard errors are clustered by 10-km distance bins. The vertical dashed line at 1 indicates the MSE-optimal bandwidth, and the horizontal dashed line indicates an RDD estimate of zero.

Panel A. Excess Return



Panel B. CAPM Alpha

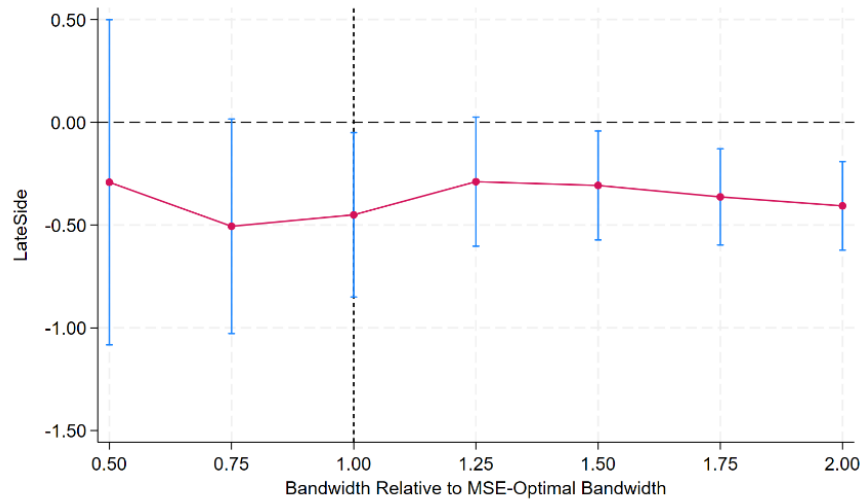


Figure IA.1 (continued)

Panel C. FF3 Alpha

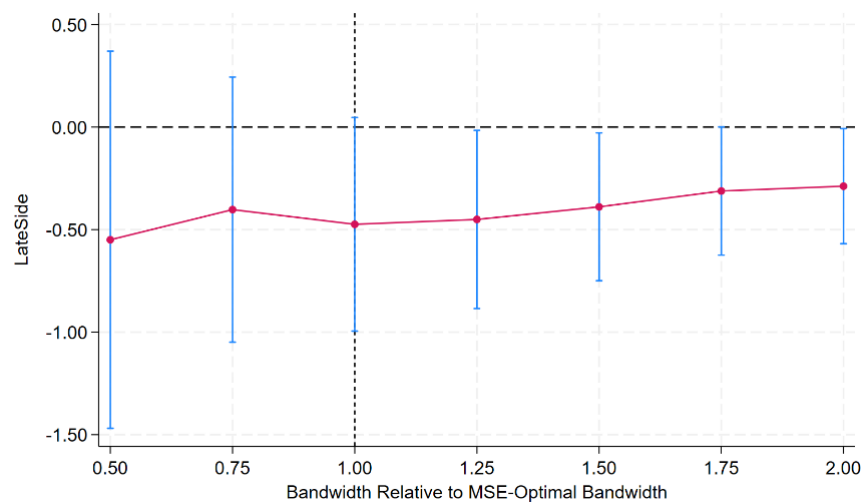
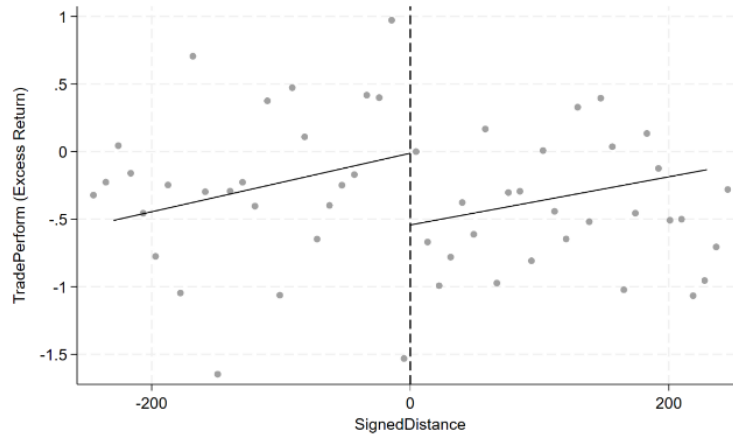


**Figure IA.2: Trade Performance Discontinuity at Time-Zone Borders:
Alternative Measures**

The figure presents the discontinuity in trade performance at time-zone borders using alternative measures of *TradePerform*. For the RDD analysis, households are grouped by ZIP code within nine geographic cells defined by the interaction of the three time-zone borders and three latitude terciles. For each distance group, we aggregate households' net trades into a monthly calendar-time portfolio. As in Table 4, we demean each monthly raw portfolio return by the average raw return across distance groups in the same geographic cell and month, then subtract the risk-free rate. Panel A uses Excess Return, the time-series average of these adjusted monthly returns. Panel B uses CAPM Alpha, the intercept from a time-series regression of these returns on the market excess return. *SignedDistance* measures the group's distance from the nearest time-zone border, with negative values west of the border and positive values east of it. The dashed vertical line marks the border at *SignedDistance* = 0. Circles show binned averages of the corresponding performance measure, plotted at each bin's mean *SignedDistance*. The figure displays groups within 250 km of the border. The solid lines show local linear fits on either side, using the bandwidths reported in Table 4, Columns (1) and (2), for Panels A and B, respectively.

Panel A. Excess Return



Panel B. CAPM Alpha

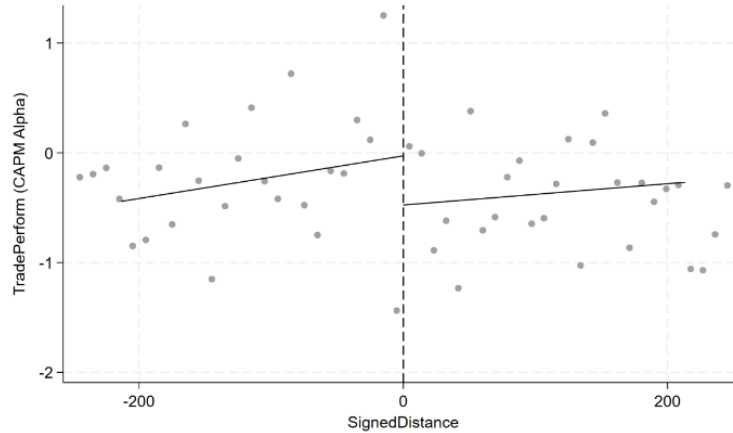


Figure IA.3: Pseudo-Time-Zone Borders

The figure illustrates the three pseudo-time-zone borders used in the placebo analysis. The pseudo-borders are constructed to follow state boundaries exactly and are shown by the green, blue, and red lines. The lighter and darker shaded areas indicate the original time zones.



Table IA.1: RDD Estimates by Income Group

This table reports RDD estimates by household income. Households are divided into low- and high-income groups at the median of the income distribution in the estimation sample used in Table 4. The dependent variable is *TradePerform*, measured using FF3 Alpha, as described in Section 3.2. Panel A estimates the RDD separately for the low- and high-income groups using the MSE-optimal bandwidth selected for each subsample. Panel B re-estimates the income-group specifications using the MSE-optimal bandwidth from the pooled-sample specification in Table 4, Column (3), thereby holding the bandwidth constant across samples. Column (1) of Panel B reports the pooled-sample estimate controlling for household income, while Columns (2) and (3) report estimates for the low- and high-income groups, respectively. All specifications use local linear regressions, and standard errors are clustered by 10-km distance bins, as in Table 4. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Income Subgroups with Subsample-Specific Bandwidths

	(1)	(2)
Subsample	Low Income	High Income
LateSide	-0.644*	-1.342**
Robust <i>p</i> -value	0.058	0.010
Observations Left	1,031	1,323
Observations Right	1,646	2,083
Bandwidth	238.3	107.4

Panel B. Income Subgroups with a Common Bandwidth

	(1)	(2)	(3)
Sample	Pooled + Income Control	Low Income	High Income
LateSide	-0.394*	-0.630	-0.151
Robust <i>p</i> -value	0.088	0.105	0.503
Observations Left	2,354	1,031	1,323
Observations Right	3,729	1,646	2,083
Bandwidth	170.6	170.6	170.6

Table IA.2: Density Test of SignedDistance at Time-Zone Borders

This table reports density tests for a discontinuity in the distribution of *SignedDistance* at the time-zone borders, where *SignedDistance* = 0. *SignedDistance* measures the distance from the nearest time-zone border, with negative values indicating locations west of the borders and positive values indicating locations east of the borders. Columns (1)-(2) report estimates using local polynomial orders $p = 1$ and $p = 2$, respectively. The density test statistic is the robust test statistic reported by *rddensity* for the null hypothesis that the density of *SignedDistance* is continuous at the cutoff, and the corresponding robust p -value is reported below it. The table also reports the number of observations on each side of the cutoff and the selected bandwidths on the left and right sides of the cutoff. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Polynomial order	p(1)	p(2)
Density Test statistic	1.681*	-2.769***
Robust p -value	0.093	0.006
Observations Left	3,702	3,702
Observations Right	6,517	6,517
Bandwidth left	29.93	165.51
Bandwidth right	45.18	197.42

Table IA.3: RDD Placebo Cutoffs

This table reports RDD estimates at placebo cutoffs located 150 km and 300 km from the actual time-zone borders. The dependent variable is *TradePerform*, measured using FF3 Alpha, as described in Section 3.2. Following Cattaneo, Idrobo, and Titiunik (2020), placebo cutoffs west of the actual time-zone borders are estimated using only observations west of the borders, while placebo cutoffs east of the borders are estimated using only observations east of the borders, thereby avoiding contamination from the treatment discontinuity at the actual borders. Columns (1) and (2) use placebo cutoffs at -300 km and -150 km, respectively, and Columns (3) and (4) use placebo cutoffs at 150 km and 300 km, respectively. All specifications use local linear regressions, MSE-optimal bandwidths, and standard errors clustered by 10-km distance bins, as in Table 4. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Placebo Cutoff	300km West	150km West	150km East	300km East
LateSide	0.025	-0.015	-0.816	0.168
Robust <i>p</i> -value	0.943	0.435	0.356	0.494
Observations Left	1,398	2,001	351	815
Observations Right	1,088	485	3,861	3,397
Bandwidth	61.9	50.5	36.0	119.6

Table IA.4: Donut RDD: Excluding Observations within 30 km of the Time-Zone Borders

This table reports donut RDD estimates that exclude observations within 30 km of the time-zone borders, that is, observations with $|SignedDistance| < 30$ km. Columns (1)-(3) use Excess Return, CAPM Alpha, and FF3 Alpha, respectively, as measures of *TradePerform*, as described in Section 3.2. All other specifications follow Table 4, including local linear regressions, MSE-optimal bandwidth selection, and standard errors clustered by 10-km distance bins. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	(1) Excess Return	(2) CAPM Alpha	(3) FF3 Alpha
LateSide	-0.521***	-0.590***	-0.374**
Robust <i>p</i> -value	0.000	0.001	0.049
Observations Left	2,455	2,455	2,455
Observations Right	4,171	4,171	4,171
Bandwidth	236.3	224.4	125.5

Table IA.5: RDD within States Spanning Two Time Zones

This table reports RDD estimates using only households located in the ten states that span two time zones; Florida, Idaho, Indiana, Kentucky, Michigan, Nebraska, North Dakota, South Dakota, Tennessee, Texas. Restricting the sample to households on opposite sides of a time-zone border within the same state helps hold constant state-level institutional and regulatory differences. Columns (1)-(3) use Excess Return, CAPM Alpha, and FF3 Alpha, respectively, as measures of *TradePerform*, as described in Section 3.2. For each outcome, we use the corresponding MSE-optimal bandwidth from the full-sample specification in Table 4 rather than reselecting the bandwidth within the same-state subsample. All other specifications follow Table 4, including local linear regressions and standard errors clustered by 10-km distance bins. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	(1) Excess Return	(2) CAPM Alpha	(3) FF3 Alpha
LateSide	-1.201***	-0.788**	-0.566
Robust <i>p</i> -value	0.000	0.011	0.177
Observations Left	145	145	145
Observations Right	1,205	1,205	1,205
Bandwidth	229.4	213.6	170.6

Table IA.6: RDD at Pseudo-Time-Zone Borders

This table reports RDD estimates at the pseudo-time-zone borders illustrated in Figure IA.3. The pseudo-borders are constructed to follow state boundaries and do not correspond to actual time-zone borders. Columns (1)-(3) use Excess Return, CAPM Alpha, and FF3 Alpha, respectively, as measures of *TradePerform*, as described in Section 3.2. The running variable measures distance from the corresponding pseudo-time-zone border, with the cutoff at zero. All specifications use local linear regressions and MSE-optimal bandwidths, as in Table 4. Reported *p*-values are robust bias-corrected *p*-values, with standard errors clustered by 10-km distance bins. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	(1) Excess Return	(2) CAPM Alpha	(3) FF3 Alpha
LateSide	0.0692	0.165	0.229
Robust <i>p</i> -value	0.881	0.541	0.338
Observations Left	1,861	1,861	1,861
Observations Right	2,554	2,554	2,554
Bandwidth	187.3	184.2	190.7

Table IA.7: Time-Series Estimates around DST Transitions for DST-Observing Investors

This table examines changes in trading performance around daylight saving time (DST) transitions using only the DST-observing group. For each DST transition, we construct the 21-day *TradePerform* measure for each trading day within the $[-5, +5]$ trading-day window around the transition, following Equation (7), as in Table 8. *AfterDST* is an indicator equal to one for trading days after the DST transition and zero for trading days before the transition. Columns (1)–(3) report estimates around the spring DST transition, and Columns (4)–(6) report estimates around the fall DST transition. Columns (1) and (4) report the baseline specification. Columns (2) and (5) control for forward 21-day market return and market volatility, while Columns (3) and (6) control for past 21-day market return and market volatility. All market control variables are standardized. All specifications include DST-event and day-of-week fixed effects. Standard errors are clustered by DST event (6 clusters). Given the small number of event clusters, we report wild cluster bootstrap p -values in brackets, using Webb six-point weights with 9,999 replications and clustering by DST event. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively, based on the wild cluster bootstrap p -values.

	(1)	(2)	(3)	(4)	(5)	(6)
	Spring DST transition			Fall DST transition		
AfterDST	-1.003 [0.108]	-0.420 [0.468]	-0.513 [0.165]	0.380 [0.551]	0.453 [0.317]	0.796 [0.379]
Market Return (fwd 21)		-0.784 [0.217]			1.678 [0.181]	
Market Volatility (fwd 21)		0.364 [0.695]			2.088* [0.100]	
Market Return (past 21)			1.225** [0.013]			-0.373 [0.622]
Market Volatility (past 21)			0.507 [0.394]			-1.499 [0.331]
DST Event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Week FEs	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.020	0.041	0.153	0.057	0.079	0.036
Observations	60	60	60	60	60	60

Table IA.8: Trading around Earnings Announcements without Holding Restriction

This table repeats the analysis of trading probability around earnings announcements in Table 9, estimating Poisson regressions after removing the requirement that a household previously held or traded a stock. We begin with all stocks with observed earnings announcements and retain the trading days from five days before through five days after each announcement, as in Table 9. On each eligible stock-day, every household enters the risk set, including households with no prior position or trade in that stock. *EWindow* equals one on the announcement day and the two subsequent trading days and zero on the other eligible days. For computational tractability, we aggregate these household–stock–day observations to the household–trading-day–*EWindow* level. The outcome counts distinct eligible stocks the household traded in that cell; multiple trades in the same stock count once. The number of eligible stocks in the cell serves as exposure, so the model estimates the trading rate per eligible stock. *Sunset* is demeaned local sunset time at the household’s ZIP code on the preceding evening, and *SAD* is measured at the same ZIP code and date. Column (1) excludes *SAD*; Column (2) includes *SAD* and *EWindow* $\times$ *SAD*. Both specifications include household and trading-date $\times$ *EWindow* fixed effects. Coefficients are on the log trading-rate-per-eligible-stock scale, so $\exp(\beta)$ is the corresponding trading-rate ratio. Standard errors are clustered by ZIP code, and *z*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Sunset	-0.0405** (-2.06)	-0.0115 (-0.34)
EWindow $\times$ Sunset	-0.0503*** (-2.73)	-0.0669*** (-3.19)
SAD		0.0485 (1.19)
EWindow $\times$ SAD		-0.0481 (-1.55)
Household FE	Yes	Yes
Date $\times$ EWindow FE	Yes	Yes
Observations	68,920,714	68,920,714

**Table IA.9: Trading Probability around Earnings Announcements
by Analyst Disagreement**

This table examines whether the relation between local sunset time and trading around earnings announcements differs with analyst disagreement. Following Gargano and Rossi (2018), we measure disagreement as the standard deviation of analysts' earnings-per-share forecasts. We classify announcements as High or Low Disagreement relative to the median forecast dispersion across announcements in the same year. Using the household-stock-day sample and stratified Cox proportional-hazard specification from Table 9, Columns (1) and (2), we estimate the model separately for the two groups. *EWindow* equals one on the announcement day and the two subsequent trading days. *Sunset*, *SAD*, and *TradeRate* are defined as in Table 9. Columns (1) and (2) omit $EWindow \times SAD$, while Columns (3) and (4) include it. The models are stratified by stock-day. Coefficients are on the log-hazard scale; $\exp(\beta)$ is the corresponding hazard ratio. Standard errors are clustered by ZIP code, and z-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	High	Low	High	Low
Sunset	-0.0235 (-0.49)	-0.0568 (-1.29)	-0.0222 (-0.46)	-0.0462 (-1.05)
EWindow $\times$ Sunset	-0.0533** (-2.06)	-0.0360 (-1.41)	-0.0572* (-1.90)	-0.0662** (-2.19)
SAD	-0.0402 (-0.97)	-0.0580 (-1.31)	-0.0367 (-0.83)	-0.0274 (-0.59)
EWindow $\times$ SAD			-0.0103 (-0.24)	-0.0860* (-1.90)
TradeRate	0.5023*** (35.36)	0.4567*** (32.94)	0.5023*** (32.93)	0.4567*** (32.93)
Observations	15,780,477	13,488,085	15,780,477	13,488,085

**Table IA.10: Trading Performance of Earnings-Announcement and Other Trades
with Trade-Date Fixed Effects**

This table repeats the analysis in Table 11 with trade-date fixed effects. For each household h on trading day t , we separate trades into two portfolios: an earnings-announcement portfolio, consisting of stocks that announced earnings during $[t - 2, t]$, and an other-trades portfolio, consisting of all other stocks traded by the household on that day. For each portfolio, we construct the compounded 21-day *TradePerform* measure following Equation (7). *EAttrade* equals one for the earnings-announcement portfolio and zero for the other-trades portfolio. *Sunset* is local sunset time, in hours, at the household's ZIP code on the preceding evening and is demeaned at the sample mean. The unit of observation is a household-day-portfolio. Column (1) reports the baseline specification, Column (2) adds *Sunset* and controls, and Column (3) includes *EAttrade* $\times$ *Sunset* and interactions between *EAttrade* and each control variable. Controls include *SAD*, future 21-day market return, and future 21-day market volatility. All specifications include household and trade-date fixed effects. Standard errors are two-way clustered by ZIP code and year-month (71 clusters). t -statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
EAttrade	-0.210* (-1.81)	-0.210* (-1.81)	-0.185* (-1.68)
Sunset		-0.090 (-0.71)	-0.075 (-0.59)
EAttrade $\times$ Sunset			-0.255* (-1.69)
Controls	No	Yes	Yes
Household FE	Yes	Yes	Yes
Trade-Date FE	Yes	Yes	Yes
Adjusted R^2	0.0037	0.0037	0.0037
Observations	783,414	783,414	783,414

**Table IA.11: Trading Performance of Earnings-Announcement and Other Trades
at the ZIP-Day Level**

This table repeats the analysis in Table 11 after aggregating trades to the ZIP-day-portfolio level. For each ZIP code on trading day t , we form an earnings-announcement portfolio from trades in stocks that announced earnings during $[t - 2, t]$ and an other-trades portfolio from all remaining trades. For each portfolio, we construct the compounded 21-day *TradePerform* measure following Equation (7). *EAttrade* equals one for the earnings-announcement portfolio and zero for the other-trades portfolio. *Sunset* is local sunset time, in hours, at the ZIP code on the preceding evening and is demeaned at the sample mean. Column (1) reports the baseline specification, Column (2) adds *Sunset* and controls, and Column (3) includes $EAttrade \times Sunset$ and interactions between *EAttrade* and each control variable. Controls include *SAD*, future 21-day market return, and future 21-day market volatility. All specifications include ZIP-code, year-quarter, and day-of-week fixed effects. Standard errors are two-way clustered by ZIP code and year-month (71 year-month clusters). t -statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
EAttrade	-0.131 (-1.09)	-0.130 (-1.11)	-0.099 (-0.90)
Sunset		0.050 (0.49)	0.060 (0.61)
EAttrade $\times$ Sunset			-0.271* (-1.75)
Controls	No	Yes	Yes
ZIP Code FE	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes
Adjusted R^2	0.0009	0.0010	0.0010
Observations	749,634	749,634	749,634

Table IA.12: Household Monthly Portfolio Turnover and Local Sunset Time

This table examines the relation between local sunset time and household trading intensity. For each household-month, *Turnover* equals the total value of stock purchases plus the total value of stock sales, divided by twice the portfolio value at the beginning of the month. *Turnover* is expressed as a percentage of beginning-of-month portfolio value. *Sunset* is local sunset time, in hours, at the household's ZIP code on the day before each trade, averaged within the household-month. *SAD* is the corresponding household-month average of nighttime hours minus 12 during fall and winter and zero during spring and summer, following Kamstra, Kramer, and Levi (2003). Column (1) regresses *Turnover* on *Sunset*; Column (2) additionally controls for *SAD*. Both specifications include household and year-month fixed effects. The unit of observation is a household-month. Standard errors are clustered by year-month (71 clusters), and *t*-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Sunset	-0.151 (-0.65)	-0.329 (-0.87)
SAD		-0.293 (-0.75)
Household FE	Yes	Yes
Year-Month FE	Yes	Yes
Adjusted R^2	0.3595	0.3595
Observations	318,748	318,748

Table IA.13: Elapsed Days Since Earliest Observed Purchase Among Sales

This table examines whether local sunset time is associated with the time elapsed between a household's earliest observed preceding purchase of a stock and a subsequent sale of that stock. For each sale of stock j by household h on trading day t , the dependent variable is the number of days since the household's earliest observed preceding purchase of that stock. *Sunset* is local sunset time, in hours, at the household's ZIP code on the evening before the sale; *SAD* is measured at the same ZIP code and date. Column (1) regresses elapsed days on *Sunset*, and Column (2) additionally controls for *SAD*. Both specifications include household, stock, and trading-date fixed effects. The unit of observation is a household–stock–sale date. Standard errors are clustered by trading date, and t -statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Sunset	1.849 (0.56)	-9.482 (-1.64)
SAD		-15.108** (-2.45)
Household FE	Yes	Yes
Stock FE	Yes	Yes
Date FE	Yes	Yes
Adjusted R^2	0.4447	0.4448
Observations	392,993	392,993

Table IA.14: Lottery-Type Stock Preference and Local Sunset Time

This table examines whether local sunset time is associated with households' preferences for lottery-type stocks. The unit of observation is a household–stock–trading day. *NetPurchase* is the dollar value of stock *j* purchased by household *h* on day *t* minus the dollar value sold, measured in thousands of dollars. Following Kumar (2009), we classify stocks monthly using their previous month-end price and idiosyncratic volatility and skewness estimated from daily excess returns over the preceding six calendar months. Volatility is the residual variance from a four-factor regression, and skewness is the standardized third moment of residuals from a regression on the market excess return and its square. *Lottery* equals one when price is at or below its monthly median and both volatility and skewness are at or above their monthly medians, calculated across stocks with valid data. Each trade receives its stock's classification for that month. *Sunset* is local sunset time at the household's ZIP code on the preceding evening, and *SAD* is measured at the same ZIP code and date. *Sunset* and *SAD* are each demeaned by their respective sample means. Column (1) estimates the association between *Lottery* and *NetPurchase*. Column (2) adds *Lottery* $\times$ *Sunset* and *Lottery* $\times$ *SAD* to test whether that association varies with sunset time. Column (3) additionally includes stock fixed effects. All columns include household and trading-date fixed effects. Standard errors are clustered by trading date, and *t*-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
Lottery	0.536*** (6.77)	0.536*** (6.73)	-0.159 (-1.58)
Sunset	0.204 (0.64)	0.200 (0.63)	0.222 (0.70)
SAD	0.319 (0.93)	0.310 (0.90)	0.359 (1.03)
Lottery $\times$ Sunset		0.038 (0.27)	0.029 (0.20)
Lottery $\times$ SAD		0.079 (0.45)	0.083 (0.45)
Household FE	Yes	Yes	Yes
Date FE	Yes	Yes	Yes
Stock FE	No	No	Yes
Adjusted R^2	-0.0003	-0.0003	-0.0053
Observations	1,079,887	1,079,887	1,079,205

Table IA.15: Net Purchases of Attention-Grabbing Stocks and Local Sunset Time

This table examines whether local sunset time is associated with households' net purchases of attention-grabbing stocks. The unit of observation is a household–stock–trading day. *NetPurchase* is the dollar value of stock *j* purchased by household *h* on day *t* minus the dollar value sold, measured in thousands of dollars. Following Barber and Odean (2008), we sort stocks into deciles each trading day by abnormal trading volume and, separately, by one-day return. *AGstock* equals one for stocks in the top decile of abnormal trading volume in Columns (1) and (2), or the top decile of one-day returns in Columns (3) and (4). *Sunset* is local sunset time at the household's ZIP code on the preceding evening, measured in hours. Columns (1) and (3) estimate the association between *AGstock* and *NetPurchase*; Columns (2) and (4) add *AGstock* $\times$ *Sunset*. Columns (2) and (4) also include interactions between *AGstock* and each control variable. Controls include past 21-day market return and volatility, *SAD*, and *Fall*. *Sunset* and all control variables are demeaned by their respective sample means. All specifications include household, stock, year, and day-of-week fixed effects. Standard errors are two-way clustered by stock and trading day, and *t*-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) Top Abnormal Trading Volume	(2) Top Abnormal Trading Volume	(3) Top One Day Return	(4) Top One Day Return
AGstock	1.148*** (5.31)	1.143*** (4.65)	0.382** (2.56)	0.350** (2.35)
Sunset	-0.084 (-0.79)	-0.097 (-0.91)	-0.082 (-0.77)	-0.092 (-0.84)
AGstock $\times$ Sunset		0.092 (0.28)		0.054 (0.32)
Controls	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes
Stock FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes	Yes
Adjusted R^2	-0.0073	-0.0073	-0.0074	-0.0073
Observations	1,094,487	1,094,487	1,094,487	1,094,487

Table IA.16: Net Purchases of Stocks with High Past Returns and Local Sunset Time

This table examines whether local sunset time is associated with households' net purchases of stocks with high past returns. The unit of observation is a household–stock–trading day. *NetPurchase* is the dollar value of stock j purchased by household h on day t minus the dollar value sold, measured in thousands of dollars. On each trading day, we sort stocks into deciles by past return. *Momentum* equals one for stocks in the top decile and zero otherwise. Panel A uses past 21-day returns to form the deciles; Panel B uses past 12-month returns. *Sunset* is local sunset time at the household's ZIP code on the preceding evening, measured in hours. In each panel, Column (1) estimates the association between *Momentum* and *NetPurchase*, while Column (2) adds *Momentum* $\times$ *Sunset*. Column (2) also includes interactions between *Momentum* and each control variable. Controls include past 21-day market return and volatility, *SAD*, and *Fall*. *Sunset* and all control variables are demeaned by their respective sample means; *Momentum* is not demeaned. All specifications include household, stock, year, and day-of-week fixed effects. Standard errors are two-way clustered by stock and trading day, and t -statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Momentum Based on Past 21-Day Returns

	(1)	(2)
Momentum	-1.639*** (-11.18)	-1.737*** (-11.67)
Sunset	-0.086 (-0.82)	-0.067 (-0.60)
Momentum $\times$ Sunset		-0.213 (-1.11)
Controls	Yes	Yes
Household FE	Yes	Yes
Stock FE	Yes	Yes
Year FE	Yes	Yes
Day-of-Week FE	Yes	Yes
Adjusted R^2	-0.0071	-0.0070
Observations	1,094,487	1,094,487

Panel B. Momentum Based on Past 12-Month Returns

	(1)	(2)
Momentum	0.045 (0.18)	-0.021 (-0.09)
Sunset	-0.082 (-0.78)	-0.135 (-1.22)
Momentum $\times$ Sunset		0.314 (1.52)
Controls	Yes	Yes
Household FE	Yes	Yes
Stock FE	Yes	Yes
Year FE	Yes	Yes
Day-of-Week FE	Yes	Yes
Adjusted R^2	-0.0074	-0.0073
Observations	1,094,487	1,094,487

Table IA.17: Past-Return Tilt in Sample Initial Buys

This table repeats the analysis in Table IA.16 using sample initial buys. A purchase is a *sample initial buy* if it is the household's first observed transaction, whether a buy or a sell, in that stock. Because transactions and holdings before 1991 are not observed, a sample initial buy need not be the household's first-ever purchase of the stock. The unit of observation is a household–stock–trading day, and *NetPurchase* is the purchase value in thousands of dollars. Because the sample contains only initial purchases, the estimates describe purchase amounts conditional on an observed sample initial buy. On each trading day, we sort stocks into deciles by past return. *Momentum* equals one for stocks in the top decile and zero otherwise. Panel A uses past 21-day returns; Panel B uses past 12-month returns. In each panel, Column (1) estimates the association between *Momentum* and *NetPurchase*, while Column (2) adds *Momentum* $\times$ *Sunset*. *Sunset* is local sunset time at the household's ZIP code on the preceding evening, measured in hours. Column (2) also includes interactions between *Momentum* and each control variable. Controls include past 21-day market return and volatility, *SAD*, and *Fall*. *Sunset* and all control variables are demeaned by their respective sample means; *Momentum* is not demeaned. All specifications include household, stock, year, and day-of-week fixed effects. Standard errors are two-way clustered by stock and trading day, and *t*-statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Sample Initial Buy and Past 21-Day Stock Returns

	(1)	(2)
Momentum	0.369*** (5.79)	0.370*** (5.94)
Sunset	0.045 (0.94)	0.039 (0.75)
Momentum $\times$ Sunset		0.039 (0.45)
Controls	Yes	Yes
Household FE	Yes	Yes
Stock FE	Yes	Yes
Year FE	Yes	Yes
Day-of-Week FE	Yes	Yes
Adjusted R^2	0.4766	0.4766
Observations	357,735	357,735

Panel B. Sample Initial Buy and Past 12-Month Stock Returns

	(1)	(2)
Momentum	1.168*** (9.34)	1.191*** (9.38)
Sunset	0.045 (0.94)	0.026 (0.54)
Momentum $\times$ Sunset		0.111 (0.94)
Controls	Yes	Yes
Household FE	Yes	Yes
Stock FE	Yes	Yes
Year FE	Yes	Yes
Day-of-Week FE	Yes	Yes
Adjusted R^2	0.4770	0.4770
Observations	357,735	357,735

Table IA.18: The Disposition Effect and Local Sunset Time

This table examines whether local sunset time is associated with households' propensity to realize gains rather than losses. Following Odean (1998) and Dhar and Zhu (2006), we construct disposition-effect measures at the household–sale-date level. We classify a sale as a realized gain (RG) or realized loss (RL) by comparing its sale price with the household's average purchase price, adjusted for dividends and stock splits. For stocks held but not sold on that date, we record a paper gain (PG) if the daily low exceeds the adjusted purchase-price reference and a paper loss (PL) if the daily high falls below it; otherwise, we record neither. For household h on sale date t , $PGR = RG/(RG+PG)$, $PLR = RL/(RL+PL)$, and $DE = PGR - PLR$. A positive DE indicates a greater propensity to realize gains than losses. We restrict all three regressions to household–sale-dates for which PGR , PLR , and DE are all nonmissing, so the columns use a common estimation sample. Columns (1)–(3) use PGR , PLR , and DE as the dependent variables, respectively. *Sunset* is local sunset time at the household's ZIP code on the evening before the sale. We exclude December sale decisions and unresolved sale dates, restrict measurement to holdings with identifiable purchase bases, and define DE only when both the PGR and PLR denominators are positive. Controls include *SAD*, *Fall*, and standardized past 21-day market return and market volatility. All columns include household, year, and day-of-week fixed effects. Standard errors are two-way clustered by ZIP code and trading date, and t -statistics are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variables	(1) PGR	(2) PLR	(3) DE
Sunset	0.001 (0.69)	0.003 (1.60)	-0.001 (-0.42)
Controls	Yes	Yes	Yes
Household FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Day-of-Week FE	Yes	Yes	Yes
Adjusted R^2	0.3478	0.2542	0.2744
Observations	112,922	112,922	112,922

Table IA.19: Robustness to Measuring Trade Performance with Excess Returns

This table repeats the trade-performance tests using portfolio returns in excess of the risk-free rate in place of Fama-French three-factor alphas. Panel A repeats the ZIP-month panel regressions in Table 3, and Panel B repeats the North–South seasonal comparisons in Table 7. Panels C and D repeat, respectively, the DST-observing time-series tests in Table IA.7 and the DST difference-in-differences tests in Table 8, using excess returns over the subsequent 21 trading days. The portfolio construction, explanatory variables, controls, fixed effects, and estimation procedures follow the corresponding main tables. Table 4 already reports the RDD estimate using Excess Return in Column (1), so the RDD is not repeated here. Panels A and B report t -statistics in parentheses; Panels C and D report wild cluster bootstrap p -values in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively, using the inference procedures described in the corresponding main table captions.

Panel A. ZIP-Month Panel Design: Excess Return TradePerform

	(1)	(2)	(3)
Sunset	-0.199** (-2.14)	-0.204* (-1.74)	-0.204* (-1.74)
SAD		-0.007 (-0.04)	-0.007 (-0.04)
ZIP Code FE	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes
Adjusted R^2	0.0144	0.0144	0.0144
Observations	260,622	260,622	260,622

Panel B. North-South Differences in Trade Performance across Seasons: Excess Return TradePerform

	(1)	(2)	(3)
North	0.518** (2.20)	0.547** (2.25)	0.538** (2.21)
Summer	0.254 (1.02)	0.257 (1.03)	—
North $\times$ Summer	-0.720** (-2.30)	-0.726** (-2.31)	-0.723** (-2.30)
Time-zone $\times$ Longitude bin FEs	No	Yes	Yes
Year-Month FE	No	No	Yes
Adjusted R^2	0.0011	0.0032	0.0348
Observations	2,422	2,422	2,422

Panel C. DST Transitions and Time-series: Excess Return TradePerform

	(1)	(2)	(3)	(4)	(5)	(6)
	Spring DST transition			Fall DST transition		
AfterDST	-1.207* [0.093]	-1.110** [0.046]	-0.956 [0.102]	1.384 [0.197]	1.561 [0.117]	1.853 [0.149]
Market Return (fwd 21)		-0.303 [0.707]			0.991 [0.257]	
Market Volatility (fwd 21)		-0.096 [0.926]			1.916** [0.015]	
Market Return (past 21)			0.864** [0.014]			-0.314 [0.664]
Market Volatility (past 21)			0.615 [0.389]			-1.749 [0.340]
DST Event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Week FEs	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.089	0.056	0.146	0.064	0.052	0.049
Observations	60	60	60	60	60	60

Table IA.19 (continued)

Panel D. DST Transitions DiD: Excess Return TradePerform

	(1)	(2)	(3)	(4)	(5)	(6)
	Spring DST transition			Fall DST transition		
AfterDST	3.014 [0.157]	3.119 [0.174]	3.147 [0.122]	5.794** [0.017]	6.755** [0.014]	6.792** [0.017]
ObservingDST	2.335 [0.145]	2.335 [0.145]	2.335 [0.145]	0.526 [0.782]	0.526 [0.782]	0.526 [0.782]
AfterDST×ObservingDST	-4.188* [0.053]	-4.188* [0.053]	-4.188* [0.053]	-4.410* [0.080]	-4.410* [0.080]	-4.410* [0.080]
Market Return (fwd 21)		1.010 [0.107]			-0.973 [0.554]	
Market Volatility (fwd 21)		1.105 [0.474]			3.907* [0.092]	
Market Return (past 21)			-0.707** [0.035]			-0.371 [0.782]
Market Volatility (past 21)			-1.427 [0.390]			-3.877 [0.238]
DST Event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Week FEs	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.042	0.038	0.061	0.107	0.102	0.103
Observations	120	120	120	120	120	120

Table IA.20: Robustness to Equal Weighting Households in Trade-Performance Portfolios

This table repeats the trade-performance tests after equal weighting households within each portfolio used in the corresponding main specification. We first calculate each household's trade-portfolio return and then average the nonmissing household returns within the relevant portfolio and period. The resulting portfolio returns are used to construct *TradePerform* following the procedures in the corresponding main tables. For example, in the RDD, we average household-month raw returns within each geographic cell-distance portfolio and month, then use that portfolio-month return series in the estimation procedure described in Table 4. Panel A repeats the ZIP-month panel regressions in Table 3; Panel B repeats the RDD in Table 4; and Panel C repeats the North–South seasonal comparisons in Table 7. Panels D and E repeat, respectively, the DST-observing time-series tests in Table IA.7 and the DST difference-in-differences tests in Table 8. All other explanatory variables, controls, fixed effects, and estimation procedures follow the corresponding main tables. Panels A and C report *t*-statistics in parentheses; Panel B reports robust bias-corrected *p*-values; and Panels D and E report wild cluster bootstrap *p*-values in brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively, using the inference procedures described in the corresponding main table captions.

Panel A. ZIP-Month Panel Design: Equal-weighted TradePerform

	(1)	(2)	(3)
Sunset	-0.149* (-1.85)	-0.249** (-2.05)	-0.249** (-2.07)
SAD		-0.159 (-1.05)	-0.159 (-1.06)
ZIP Code FE	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes
Adjusted R^2	0.0091	0.0091	0.0091
Observations	260,385	260,385	260,385

Panel B. Regression Discontinuity Design: Equal-weighted TradePerform

	(1) Excess Return	(2) CAPM Alpha	(3) FF3 Alpha
LateSide	-0.341**	-0.358*	-0.375*
Robust <i>p</i> -value	0.011	0.088	0.092
Observations Left	2478	2478	2478
Observations Right	4197	4197	4197
Bandwidth	262.0	220.2	172.0

Panel C. North-South Differences in Trade Performance across Seasons: Equal-weighted TradePerform

	(1)	(2)	(3)
North	0.293* (1.71)	0.291 (1.58)	0.289 (1.56)
Summer	0.276 (1.17)	0.276 (1.17)	—
North × Summer	-0.424* (-1.90)	-0.433* (-1.92)	-0.438* (-1.94)
Time-zone×Longitude bin FEs	No	Yes	Yes
Year-Month FE	No	No	Yes
Adjusted R^2	-0.0002	0.0072	0.0421
Observations	2,415	2,415	2,415

Table IA.20 (continued)

Panel D. DST Transitions and Time-series: Equal-weighted TradePerform

	(1)	(2)	(3)	(4)	(5)	(6)
		Spring DST transition			Fall DST transition	
AfterDST	-0.344 [0.192]	-0.081 [0.936]	-0.234 [0.181]	0.082 [0.805]	0.154 [0.723]	0.281 [0.643]
Market Return (fwd 21)		-0.440** [0.027]			0.737 [0.158]	
Market Volatility (fwd 21)		0.086 [0.836]			1.120 [0.188]	
Market Return (past 21)			0.326** [0.030]			-0.344 [0.335]
Market Volatility (past 21)			0.191 [0.334]			-0.630 [0.542]
DST Event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Week FEs	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.334	0.363	0.353	0.270	0.302	0.265
Observations	60	60	60	60	60	60

Panel E. DST Transitions DiD: Equal-weighted TradePerform

	(1)	(2)	(3)	(4)	(5)	(6)
		Spring DST transition			Fall DST transition	
AfterDST	0.559 [0.636]	0.394 [0.815]	0.676 [0.598]	1.108 [0.350]	1.735** [0.026]	1.769 [0.102]
ObservingDST	0.937 [0.498]	0.937 [0.498]	0.937 [0.498]	0.425 [0.148]	0.425 [0.148]	0.425 [0.148]
AfterDST×ObservingDST	-0.879 [0.445]	-0.879 [0.445]	-0.879 [0.445]	-1.026 [0.490]	-1.026 [0.490]	-1.026 [0.490]
Market Return (fwd 21)		0.131 [0.521]			-1.495 [0.102]	
Market Volatility (fwd 21)		-0.186 [0.843]			1.667* [0.060]	
Market Return (past 21)			-0.032 [0.754]			-0.647 [0.500]
Market Volatility (past 21)			-0.367 [0.172]			-2.357 [0.251]
DST Event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Week FEs	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	-0.021	-0.039	-0.031	0.069	0.105	0.084
Observations	120	120	120	120	120	120

Table IA.21: Buy- and Sell-Transaction Portfolio Trade Performance

This table repeats the trade-performance analyses after forming separate portfolios from buy and sell transactions. Column (1) uses buy-transaction portfolios, and Column (2) uses sell-transaction portfolios. For each panel, portfolio performance and the estimation procedure follow the corresponding main specification. Sell-side performance is sign-adjusted by reversing the sign of subsequent returns on stocks sold, so higher values indicate better selling decisions, and a negative coefficient indicates poorer sell-side performance. Panel A repeats the ZIP-month sunset regression in Table 3, with ZIP-code and year-month fixed effects. Panel B repeats the time-zone-border regression discontinuity design in Table 4, using Fama-French three-factor alpha; *LateSide* indicates distance groups east of the border. Panel C repeats the North–South seasonal comparison in Table 7, with time-zone $\times$ longitude-bin and year-month fixed effects. Panels D and E examine only the spring daylight saving time (DST) transition. Panel D repeats the time-series analysis for DST-observing investors in Table IA.7, and Panel E repeats the difference-in-differences analysis in Table 8. Both DST panels use forward 21-day market return and volatility controls, standardized to have mean zero and unit variance, and include DST-event and day-of-week fixed effects. Panels A and C report *t*-statistics in parentheses, with standard errors clustered by year-month (71 clusters). Panel B reports robust bias-corrected *p*-values, with standard errors clustered by 10-km distance bins. Panels D and E report wild cluster bootstrap *p*-values in brackets, using Webb six-point weights with 9,999 replications and clustering by DST event. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively, based on the inference procedure for each panel.

Panel A. ZIP-Month Panel

	(1) Buy Transaction Portfolio	(2) Sell Transaction Portfolio
Sunset	-0.129 (-0.79)	-0.042 (-0.28)
SAD	0.126 (0.56)	-0.146 (-0.96)
ZIP Code FE	Yes	Yes
Year-Month FE	Yes	Yes
Adjusted R^2	0.0285	0.0233
Observations	197,914	179,191

Panel B. Time-Zone-Border RDD

	(1) Buy Transaction Portfolio	(2) Sell Transaction Portfolio
LateSide	0.174	-0.515**
Robust <i>p</i> -value	0.590	0.018
Observations Left	2,085	1,925
Observations Right	3,432	3,127
Bandwidth	177.5	157.9

Panel C. North–South Seasonal Comparison

	(1) Buy Transaction Portfolio	(2) Sell Transaction Portfolio
North	0.347 (1.20)	0.116 (0.43)
Summer	—	—
North $\times$ Summer	-0.590 (-1.58)	-0.314 (-0.83)
Timezone $\times$ Longitude bin FEs	Yes	Yes
Year-Month FE	Yes	Yes
Adjusted R^2	0.0805	0.0361
Observations	2,338	2,319

Table IA.21 (continued)

Panel D. Spring DST Time-Series

	(1) Buy Transaction Portfolio	(2) Sell Transaction Portfolio
AfterDST	-0.861 [0.314]	0.022 [0.991]
Market Return (fwd 21)	-0.332 [0.425]	-0.903 [0.304]
Market Volatility (fwd 21)	0.707 [0.350]	-0.388 [0.745]
DST Event FEs	Yes	Yes
Day-of-Week FEs	Yes	Yes
Adjusted R^2	0.486	0.258
Observations	60	60

Panel E. Spring DST Difference-in-Differences

	(1) Buy Transaction Portfolio	(2) Sell Transaction Portfolio
AfterDST	3.728* [0.081]	2.734 [0.148]
ObservingDST	2.567* [0.068]	0.352 [0.816]
AfterDST×ObservingDST	-4.220*** [0.009]	-3.013 [0.238]
Market Return (fwd 21)	-1.361*** [0.010]	2.488** [0.011]
Market Volatility (fwd 21)	0.436 [0.734]	2.054** [0.028]
DST Event FEs	Yes	Yes
Day-of-Week FEs	Yes	Yes
Adjusted R^2	0.100	0.005
Observations	120	120

Table IA.22: DST Transitions and Portfolio Trade Performance: California versus Arizona

This table examines changes in trading performance around daylight saving time (DST) transitions using a difference-in-differences design restricted to households in California and Arizona. California households form the DST-observing group, and Arizona households form the non-DST-observing group. For each group, we construct the 21-day *TradePerform* measure for each trading day within the $[-5, +5]$ trading-day window around the DST transition, following Equation (7). *AfterDST* is an indicator equal to one for trading days after the DST transition and zero for trading days before the transition. *ObservingDST* is an indicator equal to one for California households and zero for Arizona households. Columns (1)–(3) report estimates around the spring DST transition, and Columns (4)–(6) report estimates around the fall DST transition. Columns (1) and (4) report the baseline specification. Columns (2) and (5) control for forward 21-day market return and market volatility, while Columns (3) and (6) control for past 21-day market return and market volatility. All specifications include DST-event and day-of-week fixed effects. All market control variables are standardized. Standard errors are clustered by DST event (6 clusters). Given the small number of event clusters, we report wild cluster bootstrap p -values in brackets, using Webb six-point weights with 9,999 replications and clustering by DST event. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively, based on the wild cluster bootstrap p -values.

	(1)	(2)	(3)	(4)	(5)	(6)
	Spring DST transition			Fall DST transition		
AfterDST	2.611 [0.179]	2.264 [0.266]	2.985 [0.187]	6.061* [0.090]	6.799 [0.102]	7.407* [0.074]
ObservingDST	2.382* [0.093]	2.382* [0.093]	2.382* [0.093]	0.904 [0.543]	0.904 [0.543]	0.904 [0.543]
AfterDST×ObservingDST	-4.949** [0.038]	-4.940** [0.038]	-4.959** [0.038]	-4.963** [0.048]	-4.963** [0.048]	-4.963** [0.048]
Market Return (fwd 21)		0.193 [0.510]			-1.980 [0.488]	
Market Volatility (fwd 21)		-0.448 [0.679]			1.739 [0.685]	
Market Return (past 21)			0.186 [0.852]			-1.705 [0.463]
Market Volatility (past 21)			-0.711 [0.436]			-4.592 [0.187]
DST Event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Day-of-Week FEs	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.024	0.007	0.015	0.057	0.051	0.060
Observations	119	119	119	120	120	120