Myopic loss aversion and market experience

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A B S T R A C T

We probe the boundaries of myopic loss aversion (MLA) theory through market treatments designed to reduce the MLA effect. Our market settings separate investment commitment from information frequency, display a running average asset value and explore the influence of participant experience. The market-based results suggest MLA persists with inexperienced participants despite efforts to mitigate MLA. Prices in markets with returning participants do not display an MLA effect. However, the same experienced participants individually succumb to MLA in an allocation setting immediately following the market. Overall, our results suggest that, while market experience mitigates the MLA effect, participants do not transfer these results to other settings.

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1. Introduction

This paper examines the ability of markets to overcome myopic loss aversion (MLA). This ability of markets to overcome the MLA effect has received little attention in prior MLA research. The notable exception is Gneezy et al. (2003), which provides initial evidence that markets do not dispel individual MLA bias. The inability of Gneezy et al. (2003) markets to overcome MLA is curious given evidence that markets can overcome individual biases (Forsythe et al., 1992), and that market structure can drive biased traders to equilibrium (Jamal and Sunder, 1996). We employ a series of modifications to Gneezy et al. (2003) market design to further explore when market conditions influence the MLA effect.

We make four key modifications to Gneezy et al. (2003). First, we hold trading commitment constant across information frequency treatments by allowing trading every period, while comparing trading results with information frequency every period versus every fourth period. This design better reflects a typical market setting and allows us to focus on whether information frequency alone can drive MLA in a market setting. Second, we expand the number of trading periods to offer the market mechanism more time to overcome the MLA bias and move toward equilibrium. Third, we incorporate a treatment prominently displaying the average asset value, along with the periodically reported asset value. The average value aggregates and summarizes previous information and frames the information in a manner that should reduce the participants’ heterogeneous beliefs about asset value and decrease the participants’ cognitive costs to estimate the assets expected value. Finally, we explore the role of experience on MLA’s effect by inviting a set of participants to return for a second set of experiments. These experienced subjects enable us to test whether market experience reduces bias in a manner that parallels research showing that trading experience can reduce behavioral biases (List, 2003).

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We find inexperienced participants succumb to MLA in our markets. Average trade values are lower when we provide information every period rather than summed and provided every four periods. This basic finding parallels Gneezy et al. (2003). The fact that participants still succumb to MLA with trading commitment and information frequency separated suggests that information frequency is sufficient to create the MLA effect. Furthermore, the treatment displaying average asset value does not mitigate the MLA effect. In contrast, we find that markets with experienced participants who return for a second market session do not display an MLA effect – mean prices are not significantly different across information frequencies. However, these same experienced participants do not overcome MLA in an allocation session immediately following the market session. The convergence in prices appears to be due to the power of the double auction to converge prices to equilibrium and not necessarily due to participants learning to overcome MLA bias. Our results suggest market experience alone does not transfer to other settings. Further research is needed to explore the conditions under which market experience reduces the MLA’s impact.

This paper proceeds as follows. First, we provide background on MLA and theory to explore both the general and our specific market conditions that potentially mitigate individual MLA biases. Section 2 describes our market setting and the subsequent allocation treatments. Section 3 describes our results, and in Section 4 we conclude. Our conclusion includes potential limitations and opportunities for further research.

2. Background

2.1. Myopic loss aversion

Benartzi and Thaler (1995) present myopic loss aversion (MLA) as a way to explain the equity premium puzzle identified by Mehra and Prescott (1985). They argue MLA results from myopic portfolio evaluation and prospect theory. The result is a tendency to over invest in low-variance securities such as bonds, creating a much higher risk-return premium demanded for equities than otherwise predicted.1

On an individual basis, MLA combines investors’ propensity to utilize mental accounting in evaluating investment outcomes on an interim, rather than longer-term basis, with the prospect theory value function that weights losses more heavily than gains (Thaler et al., 1997). When information is provided more frequently, individuals evaluate it more frequently. Investors evaluate these more frequent chunks of data as if they are concerned about short-term changes in wealth. However, the actual investor wealth does not differ over the periods of more frequent reporting unless they make different buy and sell decisions as a result of the more frequent information.2 More frequent information results in investors reacting with more loss aversion than they would based upon long-term returns and produces an underinvestment in high-variance, high-return assets relative to less frequent reporting.

Thaler et al. (1997) provide a simple MLA example based on the following utility function:

\[ U(x) = \begin{cases} x & x \geq 0 \\ 2.5x & x < 0 \end{cases} \]

where \( x \) is a change in wealth relative to the status quo. This loss aversion function weighs losses more heavily than gains consistent with prospect theory. Thaler et al. (1997) describe an investor who chooses between investing in an asset with mean return of 7 percent and a safe asset that pays a sure 1 percent per period. Further, they assume the risky asset pays 27 percent half of the time and –13 percent the other half.3 The decision maker’s choice will depend heavily on the frequency with which \( s/he \) evaluates the performance of the asset and experiences his/her loss aversion to holding the asset. In fact, the likelihood of seeing losses with more frequent observations works with prospect theory to increase the likelihood of disappointing returns (Gneezy et al., 2003). For example, based on the above loss aversion function, an investor who evaluates the return every period has an expected utility of –2.75 for the 7 percent asset compared to 1.0 for the safe asset in each individual period. When evaluated every period, the investor prefers the safe asset. However, if that same investor evaluates the 7 percent asset every two periods, his/her expected utility increases to 4.25 and \( s/he \) prefers the 7 percent asset over the sure 1 percent return. The expected change in wealth is 14 percent over two periods regardless of whether it is evaluated every period or not. This example emphasizes the role of myopia in this phenomenon. It is not just loss aversion that drives the result, but also the investor’s myopic evaluation of the returns.

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1 See, for example, Benartzi and Thaler (1995), Thaler et al. (1997), Gneezy and Potters (1997), Gneezy et al. (2003), and Barberis et al. (2001) for additional discussion.

2 This point is sometimes overlooked in the literature. If an investor holds an investment from time \( t \) to time \( t+4 \), that investor’s wealth does not differ at time \( t+4 \) across settings where in one case she only gets new information at time \( t+4 \) and the other case receives updated information at times \( t+1 \), \( t+2 \) and \( t+3 \). From a wealth utility standpoint, she should not value an asset differently between \( t \) and \( t+4 \) from one setting to the other.

3 Haigh and List (2005) appear to make a similar assumption in their Footnote 3.
2.2. Market factors

The traditional view is that markets discipline individual asset price biases out of the market in favor of the efficient asset price. Camerer (1992) outlines four basic ways markets reach rational pricing. First, random individual biases cancel each other out in the market. MLA is not a random bias as it systematically leads to lower trading prices. Accordingly, it is unlikely this cancelation hypothesis will eliminate the impact of MLA on market prices. Second, market trading reflects unbiased marginal traders’ efficient asset pricing. For example, Forsythe et al. (1992) provide evidence that despite the presence of biased traders, marginal traders who were free of such biases set the prices in their experimental market. This marginal trader view applies in the MLA setting when a sufficient number of traders, free of the MLA bias, become the marginal traders. However, if MLA is pervasive among traders, the marginal trader hypothesis will not be effective. Third, biased traders are forced out of the market when their biased trades prove unprofitable. This evolutionary hypothesis depends heavily on unsuccessful traders exiting the market whether by choice or bankruptcy. While over a long horizon this is possible, it is unlikely that the MLA bias is so severe that it drives traders out of the market. Review of our markets suggests that an MLA bias likely leaves money on the table rather than suffering significant losses. The loss aversion component potentially reflects an evolutionary response to the risk of complete failure and reduces the likelihood that the evolutionary process will force out the MLA bias. Moreover, the evolutionary effects likely take a large number of periods to be driven out of the markets. Finally, markets provide opportunity for traders to learn from each other in a so-called “learning hypothesis”. To the extent traders differ in the magnitude of their MLA bias within our markets, it is possible that more biased traders will learn from less biased traders and thereby reduce the overall bias in the market. The learning hypothesis offers a potential mechanism to overcome MLA, but will clearly take time within the market for participants to learn. Accordingly, our longer markets and the return of experienced participants give this mechanism a chance to overcome MLA bias. The market mechanism in a double auction is so powerful that over time even the most basic assumptions of rationality, markets should reach equilibrium. In fact, simulated market research suggests that even zero intelligence traders can effectively reach equilibrium in powerful double auctions (Code and Sunder, 1993).

In spite of the market setting, initial evidence from Gneezy et al. (2003) suggests that markets do not overcome the MLA effect. Our research builds on Gneezy et al. (2003) to further explore conditions under which market attributes mitigate MLA. The remainder of this section discusses the design choices we employ to identify market conditions that overcome MLA.

2.2.1. Information/investing commitment

Recent nonmarket studies separate investment commitment from information feedback to explore whether the MLA results are driven by either one singularly. Results to date are inconclusive. Langer and Weber, 2008 find that, with high-frequency feedback, the longer participants are required to commit to their decisions, the more likely they are to invest in a relatively risky lottery. In contrast, Bellemare et al. (2005) find that, with longer commitment, the frequency of information matters. However, Fellner and Sutter, 2009, find that both factors generally affect MLA equally. Pitre (2012) finds that increased reporting frequency leads to less accurate predictions and increased variance in the predictions of one-quarter forward earnings. The only prior study to explore these different information and investing commitment dimensions in a market setting is an archival study that found results indicating that the impact of changing trading commitment, while holding information constant, has an MLA effect (Kliger and Levit, 2009).

By allowing trading every period, our setting maximizes market-based feedback, while providing the ability to manipulate information feedback. The market setting provides investors the learning opportunity to overcome their biases through regular feedback via prices and trades. Consider an MLA biased participant consistently trading below expected value. A marginal trader, who is less affected by MLA, will continually outbid the biased MLA trader. Through this process, the MLA biased trader learns from the winning bids in the market that his/her value for the asset is too low. This feedback from others is not available in traditional allocation settings where the only learning is from observing the actual lottery payoffs. In addition, this learning opportunity occurs every period through the market trading activity (i.e. prices, bids and asks) even though the exogenous information received by the participants varies between the full and aggregate information treatments. As a result, learning in the market setting potentially drives participants to converge toward the rational asset value, regardless of information frequency differences.

2.2.2. Running average

MLA is based on the premise that individuals myopically evaluate financial information. Accordingly, we attempt to mitigate the MLA effect by shifting participants’ focus from short-term asset value fluctuations to longer-term average returns.

In our design, we base participant earnings on the current period asset value times the total shares held at the end of each period. While each period’s asset value can fluctuate, over the long run, each share’s expected value is simply the asset

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4 We mimic the effects of an evolutionary hypothesis by excluding the bottom 10 percent of traders from returning for our experienced session.

5 Prior research has also documented the MLA effect with individuals (Gneezy and Potters, 1997; Thaler et al., 1997), with managers (Bhojraj and Libby, 2005), and with teams (Matthias, 2007).
value average across all the periods. In the Running Average sessions, we provide the preceding periods’ average asset value prominently on the screen along with the most recent asset value and asset value history. We embed the Running Average treatment in half the FULL and AGGR markets.

The Running Average addresses two potential factors that lead to MLA. First, while participants receive a history table of prior asset values, participants must expend cognitive effort to calculate an expectation of share value in the base treatment. Providing the participants with a Running Average on the active screen, we eliminate the cognitive effort necessary to derive expected share value. In both the AGGR and FULL treatments, the Running Average value approximates share price expected value, and, therefore, should also be the average trade value in each period. In essence, this ensures that the participants in both conditions have access to the same average expected asset value. Second, the Running Average provides information for participants to focus on rather than just the periodic asset value. This information provides the participants a common focal point for the asset value to draw attention away from the period-by-period asset values. Prior research has documented the effect of heterogeneous belief on deviations in market price from fundamental asset values (Smith et al., 1988, 2000). The running average should reduce the participants’ heterogeneous beliefs about asset value by focusing participants on a common average asset value, thereby reducing the MLA effect. Therefore, under both the cognitive cost and reduced heterogeneity arguments, the Running Average decreases the marginal trader’s bias, and increases the chance of unbiased market prices.

2.2.3. Experience

Prior market research generally finds that experience increases market performance. In fact, Camerer (1987) notes, “if traders had enough experience, the apparent biases might disappear entirely”. The learning hypothesis best explains the mechanism by which market experience leads to a reduction in MLA bias. List provides evidence in a series of studies on the endowment effect that trading experienced participants are less likely to display the effect (List, 2003). To the extent experience can reduce behavioral biases, we question whether 15 periods (in Gneezy et al. (2003)) is enough time for the market to mitigate MLA. To explore the impact of experience, we utilize 24 trading periods in our settings, which increases the opportunity for experience compared with Gneezy et al. (2003). We observe that the differences between the FULL and AGGR inexperienced sessions prices narrow from the earlier periods to the later periods (see Fig. 1) but does not ultimately go away. This trend suggests that experience begins to erode the MLA effect. Therefore, to explore the trend further, we invite previous participants back for a second session to explore whether experience mitigates MLA.

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6 We provide participants 24 periods of asset value history at the beginning of trading. This is provided so participants have a basis to make estimates of share prices for trading purposes. At any point, participants can estimate share value by dividing the number of periods into the summed periodic asset values.

7 For additional discussion on market experience, see Nicolosi et al. (2009), List (2003), Chiang et al. (2011), Camerer (1992), Smith (1982), among others.

8 In fact, in the aggregated condition of Gneezy et al. (2003), the participants only traded a total of five times.
3. Experimental design

3.1. Participants

Participants include 103 undergraduate students and 22 graduate students at a large Midwestern university (50 females and 75 males). The mean participant age is 21.67 years. Ninety-seven participants are business majors and 28 are non-business majors. On average, participants have 14.38 months of work experience and have held 1.22 internships, and 23.20 percent have prior trading experience.

3.2. Design overview

Our basic study uses a between participant two-by-two design: FULL or AGGR vs. inclusion or exclusion of Running Average. We randomly assign participants to either the FULL or the AGGR financial information conditions. Participants take part in both a market and allocation setting. We incorporate the Running Average into the markets in sessions 5–8 of each FULL and AGGR conditions.

For the experienced sessions, we recruit experienced participants from among those who had previously participated. We run two additional pairings of matched FULL and AGGR treatments – four additional sessions in total. To maintain consistency, we assign participants to either a FULL or AGGR treatment matching their original participation. We make no changes to the basic design except we create two new distributions based on the same attributes as the earlier data distributions, to ensure that no participant sees the same data distributions from the inexperienced sessions. We explain to all participants that they are participating in the same experiment with a different data set based on the same attributes. In addition, we inform them that they all previously participated and that they all are receiving the same information frequency in the current session. In both the FULL and AGGR conditions, one session includes eight participants, and one session includes seven participants, for 30 participants in total.

3.3. Dependent measures

The average price per period for all completed trades is the dependent variable in the market setting. We only use prices from periods during which the participants receive the same information in both the FULL and AGGR sessions. Every fourth period following a period where aggregate asset value is provided, participants in both the FULL and AGGR conditions have the same total information (see Table 1).

Similar to the design in Thaler et al. (1997), we generate asset returns using a distribution instead of a lottery. We do not provide participants with the underlying distribution. Our distributional data provides participants not only an uncertain outcome, but also an underlying earnings distribution that they must learn. We randomly generate a normal distribution, with a variance of 49, whereby the draw is from Ecent (EC) zero average in 75 percent of the periods and an average income of EC 170 in the remaining periods. These parameters produce an overall expected net income of EC 42.50 per period. Given the results in Thaler et al. (1997), we do not anticipate the non-lottery distribution will mitigate the MLA effect, however, this design more closely captures the natural ecology faced by investors.

We use each of the FULL data sets to create corresponding AGGR data sets by aggregating the data every four periods. This approach creates four sets of matched FULL and AGGR data. Each set of data is used twice – once in sessions 1–4 and again in sessions 5–8. The Running Average treatment is included during sessions 5–8. To create the same number of trading periods, in the aggregate treatments, participants see a question mark on the screen instead of net income during the three intervening trading periods when no new information is provided. This design provides participants the same aggregated net income information every fourth period in both the FULL and AGGR conditions.

3.4. Market procedures

We run the experiment over a computer network using a program developed using z-Tree. The program enables participants to buy and sell shares with other participants in a market. It also allows us to manipulate the asset value information provided each period to implement the FULL and AGGR conditions.

The experiment consists of five parts: instructions, practice, market trading, allocation session and wrap-up payment, and post experiment questions. Before starting the experiment, participants have 10 min to read instructions outlining the market setting and how to use the software. Then participants engage in an eight-period practice session to become familiar

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9 It is possible that professional traders will exhibit different results under MLA. However, Haigh and List (2005) and Eriksen and Kvaløy (2010) find that MLA affects professional participants to a greater degree than student participants.

10 We did not invite participants who performed in the lowest 10 percent of the initial sessions. We did this to capture the idea of the evolutionary hypothesis that low-performing traders would not likely continue trading. We perform additional analysis on these dropped traders later in the paper to assess to what extent MLA may have led to their poor performance.

11 Screen shots for the FULL and AGGR market treatments are shown in Appendix A.

12 z-Tree designed and created by Fischbacher (2007).
Table 1
Market session averages.

<table>
<thead>
<tr>
<th></th>
<th>All periods</th>
<th>Matched view periods</th>
</tr>
</thead>
<tbody>
<tr>
<td>AGGR condition</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Session 1</td>
<td>47.10</td>
<td>48.40</td>
</tr>
<tr>
<td>Session 2</td>
<td>64.92</td>
<td>67.84</td>
</tr>
<tr>
<td>Session 3</td>
<td>75.30</td>
<td>69.81</td>
</tr>
<tr>
<td>Session 4</td>
<td>14.07</td>
<td>13.92</td>
</tr>
<tr>
<td>Session 5 visual average</td>
<td>40.55</td>
<td>40.28</td>
</tr>
<tr>
<td>Session 6 visual average</td>
<td>44.71</td>
<td>44.29</td>
</tr>
<tr>
<td>Session 7 visual average</td>
<td>25.08</td>
<td>28.26</td>
</tr>
<tr>
<td>Session 8 visual average</td>
<td>7.41</td>
<td>9.59</td>
</tr>
<tr>
<td>Overall</td>
<td>39.89</td>
<td>40.30</td>
</tr>
</tbody>
</table>

| FULL condition         |             |                      |
| Session 1              | 22.32       | 26.91                |
| Session 2              | 31.48       | 33.04                |
| Session 3              | 24.37       | 23.67                |
| Session 4              | 32.90       | 32.24                |
| Session 5 visual average | 30.36      | 30.00                |
| Session 6 visual average | 21.33      | 24.54                |
| Session 7 visual average | 23.26      | 26.41                |
| Session 8 visual average | 24.30      | 23.44                |
| Overall                | 26.42       | 27.53                |

Note: FULL is the high-frequency treatment, in which the current period asset value is provided at the end of each trading period. AGGR is the low-frequency treatment, in which the aggregated four-period asset value is provided at the end of every fourth trading period. We base matched view periods on those periods where participants receive new information in the AAGR treatment such that they have the same aggregate information as those in the FULL sessions. We utilize the data from six time points: periods 1, 5, 9, 13, 17, and 21 in the market setting. Session 7 has a single transaction for EC 599 from period 24 removed from AGGR condition.

with both the experimental design and market trading.\footnote{13}{The practice session is identical visually to the main session except that we do not provide any variability in the asset values to ensure the ability to learn the asset value distributions in the main sessions. Therefore, we show a net income value of EC 50 each period (aggregated four period value of EC 200 in AGGR condition). After four practice periods, participants must correctly answer a set of comprehension questions designed to confirm their understanding of experimental instructions before returning to complete the final four practice rounds.\footnote{14}{We formally restart z-Tree after the practice rounds, so participants observe that their performance in the practice session does not affect the main session.}} The practice session is identical visually to the main session except that we do not provide any variability in the asset values to ensure the ability to learn the asset value distributions in the main sessions. Therefore, we show a net income value of EC 50 each period (aggregated four period value of EC 200 in AGGR condition). After four practice periods, participants must correctly answer a set of comprehension questions designed to confirm their understanding of experimental instructions before returning to complete the final four practice rounds.\footnote{15}{We formally restart z-Tree after the practice rounds, so participants observe that their performance in the practice session does not affect the main session.} We formally restart z-Tree after the practice rounds, so participants observe that their performance in the practice session does not affect the main session.

Prior to the start of active trading, participants view 24 periods of historical asset value, displayed in a manner consistent with their assigned FULL or AGGR condition. Participants are informed they will trade for 24 periods. In total, they observe 48 periods of asset value information (24 historical + 24 active periods). A historical asset value table is included in all screens with a running summed total of asset value included at the bottom of the table. Historical asset value amounts are updated every (fourth) period in the FULL (AGGR) condition. In the Running Average condition, we also include average asset value. Market trading each period proceeds as follows:

1. Participants make an asset value prediction in the FULL (AGGR) condition for the current (average of the next four) period(s) prior to the start of trading. We then ask participants to predict the average per-share prices in the current (next four) period(s). The participant whose predicted asset value amount is closest to the actual asset value amount receives EC 100. To avoid inadvertently providing information to the market during trading, we do not inform the most accurate participants until after the final trading period. We provide no compensation for estimating the average trading price.\footnote{15}{They are allowed to retry the questions until the successfully get them all correct before moving on.}

2. After all predictions are entered, trading begins. In the first period, participants receive 10 shares of stock and EC 8000. In subsequent periods, the stock is reset to 10 shares, but wealth accumulates from period to period without a new endowment.
3. We assign a small tax on shares held at the end of the period that varies across participants to create an incentive to trade. The per-share tax is randomly based on the set {−2,0} with equal probability. Participants learn their tax status at the start of trading each period. The system randomly resets each participant’s tax status before the start of the next period.\footnote{The tax creates potential gains from trading by making shares worth more to participants who do not face a tax than those who do face a tax within a period. At the same time, it has minimal wealth effects.}

4. Participants trade their shares in a double auction market where they can both bid to buy and offer to sell shares. Participants can have both a bid to buy and an offer to sell open at the same time, but they must be rational in that they cannot offer to buy a share for more than they are willing to sell a share. Transactions require participants to actively accept offers or bids. The market mechanism does not automatically implement crossed bids and offers. Participants trade for 120 s each period for the first four periods to allow them time to become familiar with trading and 75 s per period afterward.

5. Participant accounts are updated at period end. A participant’s account increases by payments received for shares sold and decreases by payments made for shares purchased. In the FULL (AGGR) condition, asset value per share is reported every (fourth) period, and earnings based on shares held times asset value earned is likewise updated every (fourth) period.

On the active trading screen, participants can view historical asset value amounts, the sum of all prior asset value amounts, their earnings, current bids and asks, average values for the trades in the period, buttons to buy or sell shares to other participants at currently available bid and ask values, and a button to open a calculator. After trading ends in period 24, participants answer a series of demographic questions and then the system automatically calculates final EC earned in the session, converting this balance to United States dollars (US$) at a predetermined exchange rate.

3.5. Allocation setting

In addition to the market-based setting, we create an allocation setting that participants complete immediately after the market setting. The setting mirrors prior work, which finds more frequent information and investment allocation results in lower investment allocations to risky assets (Gneezy and Potters, 1997).\footnote{As our primary interest is in the market condition, we chose not to use a balanced design. While running the allocation session immediately following the market session may lead to a potential order effect, we felt that any such effect would be a reduction in MLA in the allocation setting based on potential learning in the market condition. As we report later, this did not occur; thus, mitigating potential order effects would only strengthen our allocation results.}

In the allocation setting, participants allocate investments between a risky and a risk-free asset. As in the market setting, participants in the AGGR setting, view their earnings every fourth period. We use the same data sets in the allocation setting as in the market setting across the FULL and AGGR data patterns for the risky asset. However, to ensure that participants do not see the same data set twice for the risky asset, we utilize different data sets between market and allocation sessions. In addition, we create an essentially risk-free asset value distribution for the low risk allocation option. This low risk asset pays based on a normal distribution with mean of EC 25.25 and a variance of one. Further, we maintain the separation of information presentation and allocation timing. As in the market setting, participants allocate every period, even though they only view aggregated information every fourth period.

Participants do not interact with each other, resulting in independent allocation setting observations across participants. In other words, the sample includes 63 FULL participants and 62 AGGR participants repeating their allocation choices. The amount allocated to the risky asset serves as the dependent measure for the allocation sessions. Higher allocations to the risk asset reflect less MLA. Consistent with the market treatment, we analyze data from the six periods during which the participants receive the same information in both the FULL and AGGR sessions.\footnote{In the allocation setting, the timing of income information and allocations differs by one period, so we use the data from periods 0, 4, 8, 12, 16, and 20 in our analysis.}

4. Results and discussion

4.1. Information/investing commitment

We first establish the presence of MLA by comparing whether average trade prices are lower when participants are presented with financial information every period as opposed to aggregated every fourth period, while holding investment commitment constant by allowing trading every period. Fig. 1 shows that the prices in the FULL condition are consistently lower, on average, than in the AGGR condition across all periods. Table 1 shows average prices for each session. Average prices show participants trade close to the expected value of EC 42.50 in the AGGR sessions (mean EC 40.30) and less in the FULL sessions (mean EC 27.53), consistent with MLA.\footnote{Using averages for all 24 trading periods, rather than six matched periods, provides consistent results. We utilize the matched periods as participants receive equivalent aggregate information in both conditions at the start of trading in these periods.} In fact, none of the eight FULL markets trade close to the average asset expected value price of EC 42.50. The FULL markets deviation from expected value is consistent with MLA. We utilize a repeated-measure analysis of variance (ANOVA) in Table 2, as trading activities across periods are not independent. We code the repeated ANOVA such that Report Frequency (FULL vs. AGGR) is between participants. We then code Periods as our repeated factor. In other words, our sample includes 8 FULL markets by 8 AGGR markets for a total of 16 markets repeated

\footnote{In the allocation setting, the timing of income information and allocations differs by one period, so we use the data from periods 0, 4, 8, 12, 16, and 20 in our analysis.}
across six matched periods of data. Our formal test shows that prices in the AGGR treatment are significantly higher than those in the FULL treatment (p = 0.067).  

4.2 Running average

We further explore whether displaying a Running Average mitigates MLA. We add the Running Average to session pairs 5–8. An example of the wording and average are included in Appendix A as the “Average period Net Income through last period”. We use the same distributions as in the earlier sessions. To examine the impact of adding this Running Average, we include it as a new variable in our previous ANOVA models. Table 3 reveals that there is no interaction between the Running Average variable and the Report Frequency variable (p = 0.152 one-tailed). This suggests that including the Running Average does not reduce MLA effects. Participants are still susceptible to MLA, even when they are provided with the current average asset value.

4.3 Experience

Finally, we explore whether experience mitigates MLA. Fig. 2 graphs prices in the experienced sessions. Tables 4 and 5 show that contrary to the expectations of MLA, participants traded at significantly higher average prices in the FULL compared to AGGR conditions (EC 46.93 vs. EC 38.74). In fact, the two experienced FULL sessions produce the highest average prices of the 10 overall FULL sessions. This result contradicts MLA predictions and provides evidence that market experience can reduce MLA.

We use the data we collected from participants on predicted net income and predicted prices to explore this reduction in the MLA effect. Heterogeneity in trader expectations has been cited in prior research to explain deviations in prices from equilibrium values (Smith et al., 1988). Accordingly, we focus on whether experience reduces the heterogeneity in asset value expectations across subjects such that marginal traders are more likely to set unbiased prices. The MLA effect is observed primarily in the inexperienced FULL sessions, so we compare asset value expectations reported in the inexperienced to experienced FULL sessions. The participants’ predicted net income does not show any change in average net income expectations.

20 As we expect FULL and AGGR results to be directionally consistent with relation to each other, we utilize one-tailed results throughout our analysis.
Table 4
Average asset prices – experienced sessions.

<table>
<thead>
<tr>
<th>Average prices</th>
<th>All periods</th>
<th>Matched view periods</th>
</tr>
</thead>
<tbody>
<tr>
<td>AGGR condition</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Session 1</td>
<td>40.94</td>
<td>40.19</td>
</tr>
<tr>
<td>Session 2</td>
<td>36.51</td>
<td>37.30</td>
</tr>
<tr>
<td>Overall</td>
<td>38.72</td>
<td>38.74</td>
</tr>
<tr>
<td>FULL condition</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Session 1</td>
<td>42.96</td>
<td>46.41</td>
</tr>
<tr>
<td>Session 2</td>
<td>47.93</td>
<td>47.46</td>
</tr>
<tr>
<td>Overall</td>
<td>45.45</td>
<td>46.93</td>
</tr>
</tbody>
</table>

Note: FULL is the high-frequency treatment, in which the current period asset value is provided at the end of each trading period. AGGR is the low-frequency treatment, in which the aggregated four-period asset value is provided at the end of every fourth trading period. We base matched view periods on those periods where participants receive new information in the AAGR treatment such that they have the same aggregate information as those in the FULL sessions. Two transactions equal to or greater than EC 1,000 were removed from the FULL condition.

Table 5
Repeated measure ANOVA on market prices in experienced sessions.

<table>
<thead>
<tr>
<th>Within-participants effects</th>
<th>Sum of squares</th>
<th>Degree of freedom</th>
<th>Mean square</th>
<th>f value</th>
<th>One-tailed p value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Periods</td>
<td>78</td>
<td>5</td>
<td>16</td>
<td>1.102</td>
<td>0.204</td>
</tr>
<tr>
<td>Periods × report frequency</td>
<td>91</td>
<td>5</td>
<td>18</td>
<td>1.281</td>
<td>0.188</td>
</tr>
<tr>
<td>Error (periods)</td>
<td>141</td>
<td>10</td>
<td>14</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Between-participants effects</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Intercept (grand mean)</td>
<td>44,043</td>
<td>1</td>
<td>44,043</td>
<td>3119.981</td>
<td>0</td>
</tr>
<tr>
<td>Report frequency</td>
<td>402</td>
<td>1</td>
<td>402</td>
<td>28.510</td>
<td>0.017</td>
</tr>
<tr>
<td>Error</td>
<td>28</td>
<td>2</td>
<td>14</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: N = 4 sessions × 6 matched periods.
Inclusion of an outlier from period 13 of FULL condition of session 2, does not change inference (p = 0.033).
between inexperienced and experienced FULL sessions ($p = 0.668$, untabulated), while predicted asset prices (traded values) increase in the experienced FULL sessions compared to inexperienced sessions consistent with the actual observed price difference ($p < 0.001$, untabulated). This result is a little perplexing in that the traders’ beliefs about fundamental value do not differ between experienced and inexperienced treatments. At the same time, prices change between the two conditions. The change in predicted prices appears to be in response to the increased trading prices in the experienced session and not the precursor to the higher trading prices. Price expectations in period 1 are not different between inexperienced and experienced sessions, but are different by period 21.21 To get at the expectation heterogeneity specifically, we look at the standard deviation in predicted net income and predicted asset prices. The standard deviation of predicted net income and predicted prices across participants does not appear to differ between the inexperienced and experienced sessions.22 This indicates that the heterogeneity across participants in expected net income and expected prices is consistent between inexperienced and experienced participants and, therefore, is not driving the impact of experience on the reduced MLA effect.

4.3.1. Allocation data

Overall allocation results indicate that, on an individual basis, participants allocate a statistically lower ($p < 0.005$) proportion to the risky shares when financial information is presented more frequently. Table 6 shows the AGGR participants allocate EC 62.16 in the matched periods compared to 43.18 in the FULL condition (Table 6).23 Further, experienced participants continue to allocate significantly less to risky shares under the FULL condition (EC 72.46 vs. 51.27, $p = 0.056$), consistent with MLA. Therefore, on an individual basis, participants continue to be impacted by MLA regardless of experience. However, when the same experienced subjects are included in a market setting, the MLA effect is mitigated. This implies that

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21 Insights from predicted net income should be used with caution. Compensation for predictions was based on the participant closest to the actual value. While the overall expected value was 42.5, this was based on distributions generally around zero and 170. Therefore, participants rationally predicted values at both extremes to a greater extent than at the average value of 42.5.

22 We calculated the standard deviation in predicted net income and prices by session and then compared experienced and inexperienced sessions. These results are not tabulated.

23 We also find main effects for demographic measures of gender and accounting major. Men allocate more to the risky asset ($p = 0.005$), as do accounting majors ($p = 0.043$). However, there are no interaction effects between these demographic measures and report frequency.
experience in a market setting mitigates MLA, even when it does not mitigate the bias on an individual basis. Thus, our research provides insights that experience can interact with the market setting to mitigate MLA, and suggests that further exploration is warranted on the role of experience and markets in reducing MLA.  

4.3.2. Dropping the lowest 10 percent of traders

In exploring the data further, we consider whether the removal of the lowest 10 percent of the subjects possibly impacts the overall experienced results. We removed the lowest performing 10 percent of the subjects across both conditions. This approach removed 13 subjects: nine from the FULL condition, and four from the AGGR condition. In comparing the trading of these participants, we note that these participants are more aggressive in their trading; especially selling shares. This active selling could be a sign of MLA, so we explore whether these participants are more affected by MLA than the other traders. We use the participants’ average allocation to the risky asset in the allocation task as a measure of MLA; the higher the allocation to risky assets, the lower the participants’ MLA bias. We find no evidence of a statistical difference between the dropped 10 percent and the other 90 percent we invite back to participate. There does not appear to be a difference in MLA bias on an individual basis of those participants we dropped. Therefore, individual results are not likely the cause of the experience treatment.

We also use the allocation data to explore why prices in the AGGR sessions vary from 7.41 to 75.30, while in the FULL setting prices range from 21.33 to 33.90. While the overall average of the AGGR sessions is significantly higher than the FULL sessions, consistent with MLA, there appears to be evidence that MLA impacts some of the AGGR sessions. To explore this further, we use the average allocation to the risky asset in the allocation task as a measure of individual propensity to succumb to MLA. Higher amounts allocated to the risk asset suggest less MLA bias. We then compare the average risky asset allocation to the average prices in matched market session by session. This data suggests that the variation in the average market prices across the AGGR sessions appears to be correlated with the average allocations to the risky assets across these same sessions ($r = 0.670$, $p$-value = 0.069). That is, markets consisting of participants, who subsequently allocate more to the risky asset in the allocation setting, trade at higher values than those in which participants demonstrate more MLA by allocating less to the risky asset. We see similar effects when the AGGR and FULL data are combined ($r = 0.485$, $p = 0.057$); however, the associations are insignificant in the FULL setting alone. Thus, it appears that both individual MLA differences as well as market forces impact the results of the AGGR sessions. However, in the FULL sessions, individual MLA differences do not appear to impact the results.

5. Conclusion

This paper makes several contributions. First, holding investment commitment consistent by allowing participants to trade in each period across FULL and AGGR conditions does not mitigate MLA. This suggests that information frequency alone can produce MLA in a market, which is particularly concerning in corporate settings where investment commitment is often independent of information frequency. Second, we find no evidence that lowering the participants’ costs to calculate the expected value of the underlying asset, nor that prominently framing the expected value mitigates MLA. This finding suggests that even providing year-to-date information in more frequent financial reports is unlikely to mitigate the impact of MLA on market prices. Third, we find intriguing evidence that experienced participants overcome the effects of MLA in a market setting. Nonetheless, we are unable to fully assess whether the convergence in prices is due to the power of the double auction to move prices toward equilibrium or whether participants learn to overcome MLA on an individual basis. That said, there is some indication that the market does not teach individual participants to overcome MLA. In the allocation setting, experienced participants continue to allocate less to risky shares under the FULL treatment, consistent with MLA. Thus, experience seems to mitigate the pricing impact of MLA, but the inability of the same participants to overcome MLA in the allocation setting suggests that market experience alone does not teach individuals to undo MLA’s effects.

Our research comes with a few caveats. First, we use single-period assets and re-endow participants every period in the market and allocation designs. This approach is consistent with prior research. However, the ecology we are interested in informing with our research consists, in many cases, of multiple-period assets that are not re-endowed. We made this design choice to minimize the susceptibility found in prior experimental economics research to trading bubbles with multi-period assets (Smith et al., 1988). We leave future research to explore the impact of MLA on more frequent reporting with multi-period assets. Second, we do not provide the underlying distributional detail to participants but instead have them infer them from a history of draws. This approach increases the challenge to subjects in that they must infer the underlying value of assets. Some have argued that without disclosing the distribution from which the earnings are drawn to participants the information in the FULL treatment is fundamentally different than the AGGR treatment. We use our approach to better capture natural trading ecologies where aggregated data would not include any information about the period-by-period results. We believe the results from this more realistic setting would not differ significantly if distributional data were provided. In fact, Gneezy et al. (2003) provide the underlying distribution to participants in their market study and find

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24 In supplementary analysis, we look at trading volume. We expect higher trading volume in the FULL versus AGGR conditions under MLA. We find that this is the case in both the inexperienced and experienced sessions (untabulated). This suggests that while markets mitigate the price effect of MLA, it can still impact volume in market settings. We leave this insight to future research to explore further.
results similar to ours. Future research could explore this possibility further. Finally, we provide evidence that experience impacts the MLA effect in a market setting but are unable to fully explain its cause and effect in the market. We believe future research is needed to explore both the extent to which experienced traders can reduce MLA effects and the conditions under which such experience reduces the MLA bias in prices.

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Appendix A. Market screen views

Panel A: FULL market screen view

Panel B: AGGR market screen view

Note: FULL is the high-frequency treatment, in which the current period asset value is provided at the end of each trading period. AGGR is the low-frequency treatment, in which the aggregated four-period asset value is provided at the end of every fourth trading period.
Appendix B. Experimental instructions

Experimental instructions associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.jebo.2013.10.007.

References