Essays on Expectations

by

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For Kanua, as promised

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1 Introduction

Many severe and protracted recessions begin just after the occurrence of large and rapid rises and crashes in asset prices, commonly known as asset-price "bubbles." In particular, the U.S. financial crisis of 2008 and the deep recession that followed were immediately preceded by a large rise and crash in U.S. housing prices. This begs the question: Do the asset price movements actually *cause* the slumps in real output? That possibility makes the "bubble" phenomenon an important target for research.

Disagreement exists as to whether these rises and crashes are rational responses to information about asset fundamentals, or whether they represent large-scale mispricings. In fact, some economists use the term "bubble" to refer only to episodes in which prices exceed fundamentals. Thus, the literature contains (at least) two alternative definitions for bubbles, one phenomenological and the other theoretical:

1. Definition 1 (phenomenological): A "bubble" is an "upward [asset] price movement over an extended range that then implodes" (Kindleberger 1978).

2. Definition 2 (theoretical): A "bubble" is a sustained episode in which assets trade at prices substantially different from fundamental values.

It is difficult to determine empirically whether bubbles in the sense of Definition 1 are also bubbles in the sense of Definition 2, since real-world fundamentals are rarely known. Laboratory experiments are thus an attractive technique for studying bubbles, since the experimenter knows fundamental values with certainty. Importantly, this represents a rare instance in which lab experiments may have direct relevance for understanding macroeconomic phenomena.

The classic "bubble experiments" of Smith, Suchanek, and Williams (1988) found dramatic mispricings that resembled real-world bubbles. However, the validity of these "lab bubbles" has been questioned by subsequent research. Many researchers suggest that the mispricing in these experiments is purely due to subject confusion about fundamental values - when subjects correctly understand fundamentals, these researchers say, "lab bubbles" invariably disappear. This critique is closely related to a common theoretical argument against the existence of bubbles, i.e. the idea that well-informed traders will "pop" bubbles by selling when prices are above fundamentals (Abreu and Brunnermeier 2003). The question of whether large, sustained asset mispricings can exist, in the lab or in the real world, hinges on the question of *whether traders with good information about fundamentals tend to "join" bubbles*.

Theories exist as to why they might. One idea is that well-informed traders may engage in *speculation*, ignoring their understanding of fundamentals in order to bet on price movements and reap a capital gain[1]. A second idea is *herd behavior*, in which traders choose to ignore their own information about fundamentals in order to "follow the market". Herding might apply to sophisticated traders directly, or could provide a coordination mechanism for irrational "noise" traders whose collective action overwhelms the ability of sophisticated traders to arbitrage against them.[2] In order to determine which phenomena (if any) causes sophisticated traders to ride bubbles, economists must *identify the factors that drive*

[1] Indeed, some economists consider speculation to be so central an explanation of bubbles that they define a "bubble" only as a mispricing caused by speculation; see, for example, Brunnermeier (2007).

[2] In general, this can happen when traders either are risk-averse or have constraints on short selling.

the asset demands of individual traders during bubble periods.

The present study thus addresses two important unanswered questions in the bubble experiment literature:

1. Are subjects who understand fundamentals in asset market experiments willing to pay prices in excess of those fundamentals?

2. What are the factors driving subjects' asset demands during these bubbles?

To answer these questions, I study individual investor behavior in a new type of experimental asset market that has not previously been used in this literature[3]. Using prices and dividends taken from a previous laboratory market in the style of Smith, Suchanek, and Williams (1988), I confront subjects with these time series and give them the opportunity to trade the same asset at the given market prices; I call this a *partial-equilibrium* setup. This unique setup confers at least two advantages in answering the two questions listed above. First, because the subjects in the present experiment do not interact with one another, the decisions of subjects who correctly understand asset fundamentals can be analyzed independently from the decisions of subjects who may be confused or mistaken about fundamentals. Thus, the setup can determine whether sophisticated traders in a laboratory setting regularly "buy into" bubbles. Second, the partial-equilibrium setup also allows me to study the factors influencing individual traders' asset demands. This is in contrast to the group market setup normally used in the literature, in which feedback effects exist between prices and expectations. These advantages make the partial-equilibrium setup an important addition to the toolkit of experimental economists who study bubbles.

Using this setup, I find that:

1. "Smart traders" do buy into bubbles. Subjects who demonstrate clear understanding of the risky asset's fundamental value nevertheless tend to buy the asset at prices

[3] At least, to the author's knowledge.

that they know to be well in excess of that value, thus passing up an opportunity for certain gains. This buying is concentrated just after the peak of the price bubble.

2. "Smart traders" engage in speculation. There is a significant positive correlation between a subject's trading decisions and her predictions of short-term price movements. This correlation is stronger for subjects who demonstrate understanding of fundamentals than for those who do not.

3. There is a large and significant positive correlation between lagged average asset prices and subjects' decisions to buy the asset, independently of their price predictions.

These results imply that the "bubble" result common to many asset-pricing experiments need not be purely due to subject confusion about fundamentals, and probably has more external validity than many now suppose. they also show that speculation by relatively sophisticated subjects is present in laboratory asset bubbles. The third result indicates the presence of a second source of asset mispricing, potentially related to herd behavior, that merits further study.

Section 2 describes related literature and discusses how the present study relates to that literature. Section 3 details the experimental setup and methodology. Section 4 presents and discusses the results of the experiment. Section 5 concludes.

2 Related Literature

2.1 Bubbles in experimental asset markets

In a laboratory market, fundamental values are known to the experimenter, so mispricings can be identified with certainty. The best-known instance in which this has occurred is in the classic "bubble experiments" by Smith, Suchanek, and Williams (1988) (henceforth SSW). In that study, small groups of subjects traded a single short-lived risky asset against cash using a continuous double auction market. The asset paid a dividend after every trading period, and the i.i.d. stochastic process governing the per-period dividend was told to all traders before the experiment. The outcome was a large bubble, in which the price of the asset diverged strongly from the fundamental value and then crashed at the end of the market. The effect disappeared when subjects repeated the market several times. In the next two decades, this fascinating bubble result was replicated by many other studies, and proved robust to many changes in market institutions and asset fundamentals.[4]

However, many have questioned the external validity of this result. Although fundamentals in these studies are known to the experimenter, and are *told* to subjects, subjects may be confused and fail to understand the dividend process. Laboratory "bubbles" may therefore simply reflect subjects' mistakes, rather than some more interesting phenomenon like speculation. This was proposed by Fisher (1998), who wrote: "[Experimental asset market] bubbles arise for two reasons. First, subjects take time to learn about the dividends, not trusting initially the experiment's instructions. Second, agents have heterogeneous prior beliefs." Hirota and Sunder (2007) go even further, calling the SSW result "a laboratory artifact."

In the 2000s, a number of studies emerged that appeared to support this conclusion. Lei, Noussair, and Plott (2001) showed that SSW-type bubbles can occur even when resale of the asset (and, hence, speculation) is not allowed. That implies that subjects simply fail to understand the experimental parameters. Lei and Vesely (2009) find that when subjects are allowed to experience the dividend process before the experiment, they generate no bubbles. Dufwenberg, Lindqvist, and Moore (2005) find that even when only two out of a group of six subjects have recently participated in a similar experimental asset market (and thus presumably understand the dividend process), bubbles do not occur. And Kirchler, Huber, and Stockl (2010) find that simply giving the asset a different name dramatically

[4]See, for example, King, et. al. 1993; Van Boening, Williams, and LaMaster 1993; Porter and Smith 1994 and 1995; Ackert & Church 1998; Caginalp, Porter, and Smith 1998; Smith, van Boening, and Wellford 2000; Noussair, Robin, and Ruffieux 2001; Ackert et. al. 2006a and 2006b; Oechssler, Schmidt, and Schnedler 2007; and Noussair and Tucker 2008.

reduces bubbles.[5]

These results[6] suggest an emerging consensus that laboratory bubbles only occur when most or all of a subject group fails to understand asset fundamentals. If true, that would make the classic SSW result fairly uninteresting for the study of real financial markets, since most professional investors presumably at least understand the basics of valuation of the asset classes in which they invest.

However, this emerging consensus has not yet been tested conclusively. This is because the type of asset market used in all of the aforementioned studies has inherent limitations that make it difficult to systematically characterize the behavior of traders with different levels of understanding. Because most markets involve a mix of subjects who understand asset fundamentals and those who do not, the decisions of "smart" traders can both influence and be influenced by the decisions of "confused" traders. It is therefore difficult to tell if bubbles in laboratory asset markets are occurring *in spite* of the actions of "smart" traders, or *because* of them.

This is why the partial-equilibrium setup used in the present study has the potential to shed new light on the by-now somewhat byzantine bubble experiment literature. By independently observing the behavior of "smart" and "confused" traders, the setup can test the notion that bubbles are merely the (uninteresting) result of confusion about fundamentals.

2.2 Theories of bubbles

Theories exist in which bubbles can form without the participation of traders who understand fundamentals. "Heterogeneous belief" models obtain the result that if short selling

[5]The authors hypothesize that experimental subjects expect "stocks" to rise in value over time. It is easy to show that any asset with a fixed lifetime and strictly positive dividends must fall in value over time. Hence, the authors relabeled the asset "stock in a depletable gold mine," and found that bubbles were dramatically reduced in size.

[6]A few studies, however, hint that the SSW bubble result may not simply be an artifact. Haruvy, Lahav, and Noussair (2007) elicit predictions of future prices from traders in a bubble experiment. They find that after having once observed a price runup and crash, subjects predict price peaks and attempt (unsuccessfully) to sell their holdings before the predicted peak.

is constrained, prices are determined by the valuations of the most optimistic investors (Harrison and Kreps 1978, Morris 1996). These models occupy a middle ground between the view that bubbles reflect the best available forecast of fundamentals and the view that bubbles are large-scale mispricings (Barsky 2009). However, bubbles in these models are not particularly robust to the presence of substantial percentages of traders who know true fundamentals, or to the relaxation of constraints on short-selling.

Other models have been developed to show how well-informed traders may choose to "buy into" bubbles rather than trade against what they know to be mispricings. These include the "noise trader" models of DeLong, et. al. (1990a and 1990b), and Abreu and Brunnermeier (2003). In noise trader models, rational arbitrageurs interact with fundamentally irrational "noise traders"; when the coordinated actions of the latter produce bubbles, arbitrageurs may only be able to trade against the bubbles by accepting large amounts of risk, including the risk inherent in the asset itself (fundamental risk), the risk that noise trader demand for the asset will increase even further (noise trader risk), and the risk that other arbitrageurs will choose to "ride" the bubble instead (coordination risk).

A third class of models exists. In models of "herd behavior," rational investors ignore their private information about fundamentals, because market imperfections force them to over-rely on the actions of other traders as a source of information about fundamentals (Avery and Zemsky 1998). Because of the loss of information due to incomplete markets, prices in these models do not reflect the best possible forecast of fundamentals given the total information present in the market. Models of herd behavior are typically highly stylized and sensitive to market institutions; hence, they have received comparatively little attention as an explanation for bubbles.

Each of these theories relies on a different factor that governs asset demands: beliefs about fundamentals in the case of heterogeneous-prior models, price predictions in the case of speculation models, and market prices and trades in the case of herding models. The existing bubble experiment literature therefore has difficulty testing these theories, since the factors listed above will in general be both endogenous to trading decisions and subject to interaction effects between traders. Thus, identifying the factors governing asset demand is another task at which the partial-equilibrium approach is a useful addition to what has been used in the past.

3 Experimental Setup

The asset market in this study differs substantially from that found in most asset-market experiments. The main difference is that in this experiment, subjects trade in a partial-equilibrium market; i.e., prices are not affected by the actions of subjects. Because of this, subjects do not interact, asset supply is not fixed, and markets do not clear. This setup is similar to that used by Schmalensee (1976) and Dwyer (1993) to study expectation formation. It has the advantage of being able to isolate the effects of prices on beliefs and behavior, since prices are pre-determined. It also has the advantage of being able to differentiate between the behavior of different types of subjects. Finally the effects of experimental treatments on individual-level variables can be measured by cross-sectional statistical analysis, without the presence of between-subject interaction effects. However, these advantages come at a cost; because markets do not clear, equilibrium outcomes cannot be observed. Thus, the partial-equilibrium setup is a complement to the typical setup, not a substitute.

In addition, the setup in this experiment introduces a new way to measure understanding of fundamentals. Understanding is verified dynamically by asking subjects to predict, *each period*, the future dividend income stream associated with one share of the asset. This approach has the advantage that it measures understanding of fundamentals at the time that trading occurs, so that subjects who seem to understand fundamentals at the experiment's outset but who later question their understanding will not be mis-classified as understanding fundamentals. Also, asking about dividend income as merely one prediction among many reduces the possibility of leading subjects by emphasizing dividends as the sole measure of fundamental value.

3.1 Source of the parameters

In order to maximize this study's relevance to the existing literature, I use prices and dividends that correspond as closely as possible to what is seen in a typical "bubble experiment." Before the experiment, I obtained a series of prices and dividends from a previous asset market experiment, conducted on June 6, 2011 at the University of Michigan. That experiment was a "group bubble experiment," extremely similar in nature to the original SSW setup. For the details of that previous study, see Smith (2011).[7] The market from which the present study's prices were taken will henceforth be referred to as the "Source Market." The Source Market prices and dividends (each corresponding to one share of the asset) can be seen in Figure I.1, along with the fundamental value of the asset. It is clear that Market 1 produced a bubble, both in the sense of a rise and fall in prices, and also in the sense of a mispricing. Market 2 produced what might or might not be called a bubble; prices are flat, but are above fundamentals for most of the market.[8] Both price series follow the classic patterns for the first and second repetitions[9] of a SSW-type asset market.

[7]Briefly, groups of 6 subjects traded a single risky asset with a lifetime of 10 periods, in a continuous double auction market. Each share of the asset paid an i.i.d dividend each period, with a 50% chance of 10 cents and a 50% chance of 0 cents (dividends were identical across all shares in a period). The prices used for the current experiment are the average prices from the control group of that previous experiment. The control group was chosen because the setup was closest to the original SSW setup.

[8]The subjects who produced the prices for Market 2 were the same group as for Market 1. Thus, the experience level of the subjects in the source experiment roughly matched the experience level of the subjects in the present experiment.

[9]That is, the first and second times within the experiment that the subjects participate in an asset market of this type.

3.2 Subjects and compensation

The experiment was conducted on five days, between July 15 and July 31, 2011, at Aoyama Gakuin University in Tokyo, Japan. There were 83 experimental subjects, all Japanese.[10] The majority (73) of these were university students from Aoyama Gakuin University, Keio University, and Sophia University. The rest were graduate students and staff at the same universities, except for one subject who was a business owner. The mean age of participants was just under 22 years. Five subjects had participated in economics experiments before, though none had participated in an asset market experiment. Seven had experience trading assets in a real financial market. Thirty-eight, or slightly less than half, had taken a finance class of some kind. The average total payment to each subject was 4323 yen, or about $56 at the then-prevailing exchange rate. Of this, 2500 yen (~$33) was paid as a "show-up fee," and the rest, averaging 1823 yen (~$24.30) per subject, as experimental incentives.

3.3 Experimental procedures

There were five experimental sessions, each two hours long, including the time to read and explain the instructions (approximately 40 minutes). The number of subjects in each session is listed in Table I.1. The experiment was carried out using the z-Tree software package (Fishbacher 2007). An English translation of the experimental instructions is provided in Appendix I.A.

Subjects participated in two experimental "markets," each of which lasted 10 periods.[11] In each market, a subject was given an initial endowment of cash totaling 450 yen, and an initial endowment of five shares of a risky asset (called simply "the asset"). Each period, the subject was given the opportunity to buy or sell shares of the asset at a fixed "market

[10] One possible advantage of using Japanese subjects is that, at the time the experiment was conducted, most asset prices in Japan had been trending down for over two decades. The possibility that American or European subjects might have a tacit belief that "asset prices always go up in the long run" is therefore not present here.

[11] The term "market" is a bit of a misnomer, since the subjects did not interact with one another, but only with a computer.

price." After observing the period's price, as well as the "high" and "low" (see below), the subject submitted an order to either buy or sell shares at that price. Subjects were not allowed to sell more shares than they owned (no short selling), nor to buy more shares than they could afford at the market price, given the cash in their account (no margin buying). After the subject submitted his or her order, his or her account (amount of cash and number of shares) was adjusted accordingly. Each trading period lasted 90 seconds, except for the first two periods of Market 1, which were each 180 seconds.[12]

This partial-equilibrium setup is intended to simulate the situation of a small individual investor in a large, liquid market. Small investors' offers and trades do not affect market prices. Also, in general, small investors do not see the flow of individual orders. In real-world markets, even large investors' actions may not substantially affect the movements of entire stock indices like the S&P; thus, this setup may also shed light on the behavior of institutional investors deciding what positions to take in a broad asset class such as U.S. stocks.

After trading in each period, a subject received a dividend payment for each share of the asset that he or she held in his or her account. The dividends were stochastic, and each period's dividend was determined according to an i.i.d distribution. Each period, the dividend had a chance of being 10 yen per share, and a 50% chance of being 0 yen per share.[13] The dividends differed from period to period, but were the same for each share and each subject within a period. The asset had no buyout value; that is, at the end of the tenth and final period, after the tenth dividend was collected, all shares of the asset vanished. Therefore the asset's risk-neutral fundamental value per share in period t, assuming zero

[12]This was done despite the fact that a demonstration of the interface was conducted before the experiment. As it turned out, nearly no subjects used the entire 180 seconds in the first or second trading periods of Market 1, nor all of the 90 seconds in any subsequent trading period.

[13]These dividend values and probabilities, and indeed all parameters of the current experiment, are nearly identical to the prior experiment from which the data was obtained. The single difference is that the values for the current experiment are in yen, and the values for the previous experiment were in cents. Because the Japanese after-tax median income in yen is very similar to the American median income in cents, the per-dividend levels of risk and reward involved in the two experiments are extremely similar.

discounting, is given by:

$$FV_t = 55 - 5t \tag{1}$$

Subjects were repeatedly told that the prices and dividends they were observing were taken from a previous group experiment at the University of Michigan, as described above.

In addition to the market price, while making his or her trading decision each subject had the following additional information: A) a "high" and "low" price for the period, at which the subjects were *not* allowed to trade, B) a history of the market price, high, and low in past period (empty in the first period), and C) a graph displaying the market price in each past period (empty in the first period). It was explained that the "high" and "low" prices were the highest and lowest prices for which the asset had traded in the corresponding period of the experiment from which the prices and dividends were obtained.

Therefore, in addition to the experimental parameters, a trader's information set while trading in Period n included:

- $\{P_1, P_2, ..., P_n\}$, where P_j is the market price of one share of the asset in Period j,
- $\{H_1, H_2, ..., H_n\}$, where H_j is the "high" in Period j,
- $\{L_1, L_2, ..., L_n\}$, where L_j is the "high" in Period j, and
- $\{D_1, D_2, ..., D_{n-1}\}$, where D_j is the dividend per share paid to holders of the asset in Period j.

Before each trading period, each subject was given a 90-second period in which to make three predictions (180 seconds for each of the first two periods of Market 1). Each was asked to predict:

1. P1: the price of the asset in the upcoming trading period,
2. P2: the price of the asset in the final trading period, and

3. P3: the total amount of dividend income that someone would receive from 1 share of the asset, if (s)he were to buy that share in the upcoming period and hold onto it until the end of the experiment.

P3 is the expected value of one share of the asset, i.e. the fundamental value.

An explanation of the flow of the experiment can be seen in Figure I.2.

Subjects were incentivized to make accurate predictions about price and fundamentals. For each of the three predictions made in each period, subjects were paid according to the following formula:

$$\text{Payment} = 25¥ - 2¥ \times |\text{prediction - realized value}|$$

The total maximum prediction incentive per period was slightly more than that paid in Haruvy, Lahav, and Noussair (2007). In that paper, the authors pay for the *percentage* accuracy of predictions, while I pay for absolute accuracy. I do this because natural cognitive error makes more precise predictions more difficult.[14] The theoretical maximum prediction incentive received a subject who made perfect predictions throughout the experiment was therefore equal to 1500 yen, or 100 yen more than the combined value of the subject's initial endowments in the two market repetitions. Thus, the incentive for predicting well was roughly the same as the incentive for trading well.

3.4 Market 2

Subjects in this experiment participated in a repetition of the asset market. This was done in order to ascertain what behavioral changes, if any, would be caused by "design experience."[15] After a subject finished the first market (Market 1), she was allowed to begin

[14] A truly "fair" prediction payment scheme, in which ex post payments were roughly equal across predictions, would probably be some combination of the two payment schemes. However, such a hybrid scheme would be difficult to explain to subjects. Even the payment scheme used in this experiment had to be explained multiple times before subjects understood.

[15] "Design experience" simply means "experience with a particular market setup."

Market 2 immediately[16]. Market 2 was identical to Market 1 in all respects, except that the prices and dividends were different (subjects were told this). Subjects were not told that the traders in the Source Market for Market 2 were the same individuals as the traders in the Source Market for Market 1 (in fact, they were the same). Henceforth, I sometimes refer to Market 2 traders as "design-experienced" traders, and Market 1 traders as "design-inexperienced" traders.

3.5 Experimental Treatments

The above description completely describes the experimental setup for Treatment 1, the "Basic" treatment. 44 of the subjects participated in this treatment. In addition, there were two other experimental treatment conditions, the "Uncertain" treatment and the "Pictures" treatment, which encompassed 20 and 19 subjects, respectively.

Treatment 2 is called the "Uncertain" treatment. In this treatment, the probabilities of the dividend values were withheld from the subjects. Subjects were told that the dividends were i.i.d., that 10 yen and 0 yen were the only possible values, and that the probabilities were constant every period and between both markets; however, they did not know the values of the probabilities. This treatment was included in order to test for the existence of a particular form of herding: namely, whether or not subjects update their beliefs about fundamental values to reflect observed market prices. This type of herding, however, was not observed.

Treatment 3 is called the "Pictures" treatment. In this treatment, subjects were shown images of price and dividend series from several other markets in the same experiment as the Source Market.[17] The idea was to give these subjects a general idea of what kind of

[16] This somewhat ameliorates the "active participation effect" proposed by Lei, Noussair, and Plott (2001). Those authors suggested that sophisticated traders join bubbles in these experiments because otherwise they would have nothing to do during the experiment and would hence become bored. In the present study, subjects were able to rapidly click through to the end of a market and collect dividends rapidly without trading, if they so chose.

[17] Actually, these series were not perfectly representative of what the traders would encounter in the actual market. The pictures shown to the traders came from markets in which traders received private forecasts of

price movements to expect in these markets, coupled with a (slightly) better understanding of the dividend process; in general, it was thought that seeing the "pictures" would provide these 19 subjects with a sort of market experience. The pictures shown to the subjects can be seen in Appendix I.B. Three out of the four price series in these pictures included mispricings, two included a rise-and-crash pattern, and one was close to rational pricing; the pictures were thus fairly representative of the typical mix of results in a bubble experiment. One hypothesis was that subjects in this treatment, knowing that prices often tended to rise and then fall, would speculate more than subjects in other treatments; this would be consistent with the results of Haruvy, Lahav, and Noussair (2007). An alternative hypothesis was that seeing examples of dividends might make these subjects understand fundamentals better, and that they would therefore avoid buying during bubbles, consistent with the result in Lei and Vesely (2009). In fact, both of these hypotheses are formally rejected by the data.

4 Results

In this section I first describe some general features of the experimental data, and then present the paper's three key results:

Result 1: ***Even subjects who understand fundamentals well tend to buy at prices that are well above fundamental value.***

Result 2: ***Subjects who understand fundamentals well tend to trade based on their predictions of short-term price movements.***

Result 3: ***Lagged average prices are a significant predictor of trading decisions.***

Throughout the rest of the paper, I use the terms "traders" and "subjects" interchangeably. I use the term "smart traders" to refer to subjects who understand fundamentals, and

dividend value, which was part of the experimental procedure of Smith (2011). In addition, one of the pictures came from a market whose participants had already participated in one experimental market. However, subjects in the current experiment were not told either of these things, as the idea behind the Pictures treatment was merely to give these subjects a general idea of what they might encounter in the market.

the term "confused traders" to refer to subjects who do not demonstrate understanding of fundamentals[18].

Standard errors in all regressions are clustered at the individual (subject) level.

4.1 Description of the Data

Four kinds of data were collected in this experiment: predictions about asset fundamentals, predictions about next-period prices, predictions of final-period prices, and trading decisions. The following subsections describe some general features and interesting properties of these variables.

4.1.1 Beliefs about fundamentals

Each period, subjects predict the expected future dividend income per share (fundamental value). This makes it possible to identify subjects who understand the fundamental value well. To quantify how well a subject understands fundamentals, I define subject *i*'s "mistake" about fundamentals at time *t* as the difference between her prediction of the fundamental in period *t* and the correct value:

$$M_{it} \equiv E_{it}\left[FV_t\right] - FV_t \tag{2}$$

Since one primary purpose of this study is to analyze the behavior of subjects who understand fundamentals "well," it is necessary to specify what size and frequency of mistakes disqualify a subject from this category. First, I introduce three categorizations of the size of M_{it}, in increasing order of stringency:

1. "Small" mistake: $abs(M_{it}) \leq FV_t$

[18]This terminology is not meant to indicate that traders who understand fundamentals necessarily trade in a "smart" way. The term "smart traders" is meant to indicate that subjects who understand the experimental parameters are "smart" relative to this experiment, while not necessarily being smarter in some more general sense (higher I.Q., etc.).

2. "Smaller" mistake: $abs(M_{it}) \leq \sigma FV_t$, where σ is an arbitrary parameter between 0 and 1, set to 0.5 for simplicity[19]

3. "Smallest" mistake: $abs(M_{it}) \leq 5$

A "small" mistake indicates that the subject predicted a value for the fundamental that was possible at time t (since the maximum possible value of the per-share dividend is just twice the expected value, and the minimum is zero). A "smaller" mistake indicates that the subject's prediction was reasonably close to the correct expected value; this makes allowances for Bayesian updating of dividend probabilities.[20] A "smallest" mistake indicates that the subject's prediction precisely equaled the correct expected value, with allowance for convex preferences over prediction accuracy (i.e., a desire to guess the "exact right" dividend value; in odd-numbered periods, the mathematical expected value of the total remaining dividend is not actually a possible realization).

I then introduce two specifications for whether a subject "understands fundamentals or not" at time t:

1. A subject is "correct" at time t if her mistake about fundamentals at time t is smaller than a certain size, and "incorrect" otherwise.

2. A subject is "smart" at time t if her mistakes about fundamentals are smaller than a certain size for all times earlier than or equal to t, and "confused" otherwise.

In other words, "correct"/"incorrect" measures whether a subject is *currently* mistaken about fundamentals, and "smart"/"confused" measures whether a subject has *ever* been mistaken about fundamentals during the current market. "Smart"/"confused" is therefore a much more stringent criterion for understanding.

[19] All results in this paper continue to hold if the parameter σ is set at 0.2 instead of 0.5. Thus, setting $\sigma = 0.5$ is done only for simplicity, not because the results for "smart traders" depend on using a lax criterion for understanding of fundamentals.

[20] In the Uncertain treatment, Bayesian updating is appropriate. In the other treatments, it is not, but I consider it to be only a small departure from rationality.

Crossing the three mistake size categories by the two specifications of mistaken-ness yields six dummy variables for understanding of fundamentals. These six dummies, which I will henceforth call "smartness dummies," are listed in Table I.2, along with the number of traders who meet each criterion in each period. For the rest of the analysis in this paper, I generally use the smartness dummies $D\_MORECORRECT_{it}$ and $D\_SMARTER_{it}$ when analyzing the data:

$$D\_MORECORRECT_{it} \equiv \begin{cases} 0 & |M_t| > 0.5(55-5t) \\ 1 & |M_t| \leq 0.5(55-5t) \end{cases} \tag{3}$$

$$D\_SMARTER_{it} \equiv \begin{cases} 0 & |M_\tau| > 0.5(55-5\tau) \\ 1 & |M\tau| \leq 0.5(55-5\tau) \end{cases} \quad \forall \tau \leq t \tag{4}$$

These dummies, which use the middle category of mistake size, represent a compromise between strictness and statistical power.

Although this study is focused on the behavior of subjects who understand fundamentals well, it is also interesting to ask whether or not subjects who are confused about the experimental parameters tend to become less confused over time. Figure I.3 plots average mistakes vs. time in Market 1 for subjects for whom $D\_SMARTER_{i,10} = 0$ (that is, subjects who get the fundamentals wrong at least once). The average mistake falls over time. A fixed-effects regression of mistakes on time for this subgroup confirms that mistakes trend downward over the course of the market. This indicates that subjects learn about fundamentals over time.[21]

[21] An alternate interpretation is that some of the "confused" subjects are simply reporting the expected total dividend income for their portfolio, rather than the per-share dividend income. Such subjects, while confused about the question being asked of them, would actually be less confused about the experimental parameters than their predictions would suggest.

4.1.2 Beliefs about prices

Denote a subject's period-t predictions of prices in period t and prices in period 10 by:

$$E\_PNEXT_{it} \equiv E_{i,t+1}[P_{t+1}] \tag{5}$$

$$E\_PFINAL_{it} \equiv E_{i,t+1}[P_{10}] \tag{6}$$

Here, P_t denotes the price at time t, and the expectation operator indicates a subject's stated prediction.

Note that the expectations at time t are defined using predictions given at the beginning of period $t+1$. Because predictions are made before trading, but market prices are observed only during trading, there exists the possibility that a trader updates his/her price predictions *after* seeing the market price but *before* making his/her trading decision. In other words, subjects' true beliefs about future prices at the time they trade are not observed. It is therefore necessary to proxy for beliefs during the trading stage of period t with the predictions made at the beginning of period $t+1$. In the time between when trading occurs and when this proxy is observed, the period t dividend is realized; also, subjects may revise their beliefs for unobservable reasons. This will result in an errors-in-variables problem for regression estimates using these price predictions as explanatory variables:

$$E_{i,t+1}[P_{t+1}] = E\_PNEXT^*_{it} + f(D_t) + \mu_{it} \tag{7}$$

$$E_{i,t+1}[P_{10}] = E\_PFINAL^*_{it} + g(D_t) + \nu_{it} \tag{8}$$

Here, $E\_PNEXT^*_{it}$ is the "true" prediction of one-period-ahead price appreciation at the time that period t trading decisions are made, $f(D_t)$ is some function of the realized period-t dividend D_t, and μ_{it} is a mean-zero error term; the terms in Equation 8 are defined analogously. The presence of errors in variables will result in attenuation bias of the estimates of the beta coefficients on E_PNEXT_{it} and E_PFINAL_{it} when these variables are used as regressors; the true values of the coefficients will be larger than the estimated values. However, there is reason to believe that the systematic error from the observation of period-t dividends is negligible, since regressions of $E_{i,t+1}[P_{t+1}]$ and $E_{i,t+1}[P_{10}]$ on D_t do not find effects of dividends on subsequent price predictions that are significantly different from zero.

How do expectations evolve? Figure I.4 shows plots average values of E_PNEXT_{it} and E_PFINAL_{it} over time for Market 1, along with lagged prices (i.e. the most recently observed price when the prediction was made). The figure suggests that next-period price predictions are adaptive, i.e. that they track recently observed prices. That is the finding of Haruvy, Lahav, and Noussair (2007). Those authors also find that price predictions are extrapolative, i.e. that they incorporate the expectation that recent price movements will be continued. To test for adaptive and extrapolative expectations, I regress upcoming period price predictions on lagged prices and lagged price momentum, interacting momentum with a dummy for whether subjects are "smart" about fundamentals:

$$\begin{aligned} E\_PNEXT_{it} &= \alpha_i + \beta_P P_{t-1} + \beta_M (P_{t-1} - P_{t-2}) \\ &\quad + \beta_S D\_SMARTER_{it} \\ &\quad + \beta_{SM} D\_SMARTER_{it} (P_{t-1} - P_{t-2}) + \varepsilon_{it} \end{aligned} \tag{9}$$

The results of this regression can be seen in Table I.3. The coefficient on the lagged price is highly significant (p-value 0.000) and has a value of around 0.7, indicating that price expectations have an adaptive component. The coefficient on lagged momentum is not significantly different from zero, but the coefficient on the interaction term is positive and significant at the 5% level, indicating that traders who understand fundamentals tend to form extrapolative price expectations, but that traders who misunderstand fundamentals do not. These results are highly robust to alternative regression specifications, and to the use of an Arellano-Bond panel GMM estimator. These findings broadly confirm the findings of Haruvy, Lahav, and Noussair (2007).

However, there is substantial cross-sectional variation in price predictions. To illustrate this fact, Table I.4 shows the cross-sectional standard deviation of E_PNEXT_{it} divided by the price P_t for each period in Market 1.

4.1.3 Trading Decisions

I use three measures of subjects' trading decisions. The first, $NETBUY_{it}$, is simply the number of shares bought or sold by trader i in period t. The second measure, $ACTION$, is a dummy variable that measures whether a trader bought, sold, or held in period t:

$$ACTION_{it} \equiv \begin{cases} -1 & NETBUY_{it} < 0 \\ 0 & NETBUY_{it} = 0 \\ 1 & NETBUY_{it} > 0 \end{cases} \tag{10}$$

Since $NETBUY$ is just demand for the asset at the given price, it measures whether a trader's actions would tend to inflate or deflate a bubble in a given period. However, $NETBUY$ is not a perfect measure of a subject's desire to hold the asset, because it depends on past trading decisions in a complex way. A subject who already has a large number of shares, but who perceives an increase in risk, may choose to hold or even to sell a few shares even if she believes the asset's return is greater than its price. Thus, it is desirable to have a measure of asset holdings that is not constrained by past decisions. Define $ASSETSHARE$ as:

$$ASSETSHARE_{it} \equiv \frac{SHARES_{it} * P_{it}}{SHARES_{it} * P_{it} + CASH_{it}} \tag{11}$$

$SHARES_{it}$ and $CASH_{it}$ are defined after a subject's trading decision has been made, but before dividend income is received. Hence, $ASSETSHARE_{it}$ is the fraction of a subject's beginning-of-period wealth that she chooses to hold in the risky asset. Although psychological status quo bias may be present,[22] subjects are formally free to choose any $ASSETSHARE$ they like in each period. I also define $\widetilde{ASSETSHARE}_{it}$ as the deviation of a subject's asset share at time t from her average asset share in the entire market:

$$\widetilde{ASSETSHARE}_{it} \equiv ASSETSHARE_{it} - \frac{1}{10}\sum_{\tau=1}^{10} ASSETSHARE_{i\tau} \tag{12}$$

Figure I.5 plots the average values of $NETBUY$ and $ASSETSHARE$ across subjects for each period in Market 1, along with the asset price. Net asset demand is positive at the start of the bubble, near zero at the bubble peak, then strongly positive again directly after the peak, before turning negative in the final period; average asset holdings follow a similar path.

One interesting question is whether the price path imposed on the subjects in this experiment would represent a market equilibrium if the subjects were allowed to trade with one another. If net asset demand is zero (and assuming that asset demand doesn't depend on the trading process itself), then the given price would be an equilibrium[23]. For each period in Market 1, I test the null hypothesis that net asset demand across all subjects is equal to zero, using Wilcoxon signed-rank tests. The results of these tests can be seen in Table I.5. For periods 3, 4, 5, 6, and 9, the null of zero net asset demand cannot be rejected at the 10% level. For all periods when the null is rejected, asset demand was positive, except in period 10, in which it was negative. Thus, the decisions of subjects in this experiment are consistent with the formation of a bubble of approximately the size of the bubble that emerged in the market from which the prices were taken.

[22] Indeed, a rational risk-neutral trader will always set $ASSETSHARE$ equal to 0 or 1, depending on whether she predicts a negative or positive return for the risky asset. In actuality, subjects in this experiment often choose values of $ASSETSHARE$ between 0 and 1, which may reflect status quo bias, non risk-neutrality over small gambles, or both.

[23] This is, of course, not sufficient to guarantee that a bubble would emerge if these subjects were allowed to trade with each other. But if total asset demand among these subjects had been significantly negative for this subject pool over the entire bubble, it would have indicated that the subjects in the present study were less bubble-prone than the subjects in the Source Market.

4.1.4 Similarities and differences between subject types

From Figure I.5, it is apparent that the aggregate behavior of smart traders and confused traders is broadly similar. In Figure I.6, I confirm the similarity, by plotting the average values of $NETBUY$ and $ASSETSHARE$ in Market 1 for two very different groups of traders: the "smartest" traders (who got the fundamental within 5 of the correct value in every period) and the "most confused" traders" (who never predicted a feasible value for the fundamental). The broad similarity is still evident. This is itself an intriguing result. It suggests that much of traders' behavior in this type of market represents a reaction to factors that are common across subjects - prices, dividends, and time - rather than to individual beliefs. That is not a result that would emerge from a "heterogeneous prior" model, in which heterogeneity of actions is driven by heterogeneity of beliefs. A partial explanation for the similarity will be given in Section 4.4.

Actually, the behavior of the two groups is measurably different. For any definition of "smart" and "confused," a test will reject the null that smart traders and confused traders buy the same amount (or hold the same amount) of the asset over the course of the market. This is due primarily to the difference in behavior at the peak of the bubble, during periods 3-6. In these periods, smart traders sell on average, while confused traders on average hold or buy. Because of this difference in buying behavior, a gap opens up between the two groups' $ASSETSHARE$ that persists until the end of the market (see Figures I.5 and I.6). In fact, during period 6, at the peak of the bubble, the "smartest" trader group actually sells out completely (Figure I.6b). The difference indicates that fundamental-based buying does indeed exist; smart traders arbitrage against the bubble while the bubble is inflating. After the peak, however, the arbtrage no longer persists, as will be shown in the following section.

I now turn to the key results of the experiment.

4.2 Bubble buying by smart traders

Result 1: ***Even subjects who understand fundamentals well tend to buy at prices that are well above fundamental value.***

The first question asked by this study is whether traders will buy into bubbles even when they understand fundamentals well. In the present experiment, it is known which subjects understand fundamentals at the time they make their trading decisions. Since the asset's price exceeds its fundamental value in all periods in Market 1, any nonzero holdings of the risky asset by "smart" traders can technically be viewed as buying into the bubble. The null hypothesis that subjects who understand fundamentals in every period do not buy into the bubble can therefore be formally stated as: $D\_SMARTER_{it} = 1$ $\forall t \Rightarrow SHARES_{it} = 0$ $\forall t$. This hypothesis is easily rejected (Wilcoxon signed-rank test, p-value=0.000). A far less restrictive version of the null is that subjects who understand fundamentals in all periods never hold more than 1 share of the risky asset: $D\_SMARTER_{it} = 1$ $\forall t \Rightarrow SHARES_{it} = 1$ $\forall t$. This hypothesis is also easily rejected (two-sided Wilcoxon signed-rank test, p-value=0.000).[24] Smart traders are indeed willing to hold the asset at prices that they know exceed fundamentals.

However, this null hypothesis may be viewed as too restrictive. A more dramatic demonstration of the significant degree to which "smart" traders buy into the bubble is provided by examining these traders' behavior directly after the bubble peak in Market 1, in periods 7 and 8. As Figures I.5 and I.6 show, smart traders' asset demands appear to explode upward in these periods. Table I.6 shows the percentages of smart and confused traders who bought, sold, and held in periods 7 and 8; the after-peak buying appears to persist across all levels of smartness.

To formally test that this is the case, I define two different versions of the null hypothesis

[24]The result is the same if smart traders are allowed a few periods to verify that their understanding of fundamentals is indeed correct. The hypothesis that $D\_SMARTER_{it} = 1$ $\forall t \Rightarrow SHARES_{it} = 1$ $\forall t > t_0$ is rejected for any t_0 less than 8; in other words, only in the last two periods are smart traders unwilling to hold more than one share of the risky asset.

that asset demand by smart traders does not increase in a given period:

1. $H0_1$: $NETBUY_{it} = 0$

2. $H0_2$: $ACTION_{it} = 0$

I then test these hypotheses for periods 7 and 8, for two different groups of "smart" traders: A) "more correct" traders ($D\_MORECORRECT_{it} = 1$), and B) "smarter" traders ($D\_SMARTER_{it} = 1$). All tests are two-sided Wilcoxon signed-rank tests. The results of these tests can be seen in Table I.7. In period 7, the null $H0_1$ of no net buying is rejected both for "more correct" traders (p-value=0.000) and for "smarter" traders (p-value=0.021), while the null $H0_2$ of no net buyers is rejected both for "more correct" traders (p-value=0.001) and for "smarter" traders (p-value=0.007). In period 8, neither null can be rejected at the 5% level for either group; however, a third null hypothesis, that $NETBUY_{i7} + NETBUY_{i8} = 0$, is rejected for both groups (p-value=0.003 and p-value=0.046 for "more correct" and "smarter" traders, respectively).

Why focus on periods 7 and 8? There are four reasons. The first is that Result 1 is an *existence* result; if traders who understand fundamentals *ever* buy at prices in excess of fundamentals, it means that arbitrage by smart traders is not guaranteed to prevent lab bubbles, even if smart traders are present - and, in fact, that smart traders can be a source of positive net asset demand during these bubbles. Second, in these periods the price is not only above the fundamental value, but also above the *maximum* possible value of future dividends, meaning that smart traders' decision to buy represents a decision to *forego certain gains*. Third, although periods 7 and 8 are not the only periods in which smart traders buy into the bubble - periods 1 and 2 yield similar results - periods 7 and 8 come late in Market 1. This means that, by period 7, smart traders have had time to A) rebalance their portfolios from their initial allocations, B) observe several periods of market prices, C) observe dividend series and verify that they are, in fact, smart, and D) familiarize themselves completely with the trading system. This makes it very difficult to interpret the result as a laboratory artifact. Finally, periods 7 and 8 come just after the price has begun to fall from its peak; the fact that this is when both smart and confused traders tend to go on a buying spree is itself a phenomenon worthy of investigation.

Buying in periods 7 and 8 was generally a poor decision in Market 1. The asset's realized return was negative in all subsequent periods. Nevertheless, the decision was consistent with smart traders' predictions. Define the one-period-ahead and final-period total asset returns predicted by a smart trader[25] as:

$$E\_RETURN\_1_{it} \equiv E\_PNEXT_{it} + 5 - P_t \tag{13}$$

$$E\_RETURN\_10_{it} \equiv E\_PFINAL_{it} + E_t\left[FV_t\right] - 5 - P_t \tag{14}$$

Almost all smart traders (all but one or two subjects) predicted positive values for both of these asset returns in periods 7 and 8.

The result that smart traders buy into bubbles contradicts the conclusion of Lei and Vesely (2009), Kirchler, Huber, and Stockl (2010), Fisher (1998), and others who argue that laboratory asset bubbles are purely the result of subjects' misunderstanding of fundamentals. It means that we must look elsewhere for an explanation for these bubbles. The next result offers one such explanation.

4.3 Speculation by smart traders

Result 2: ***Subjects who understand fundamentals well tend to trade based on their predictions of short-term price movements.***

"Speculation" can be defined in a number of ways. For the purposes of this study, I

[25]Note that this is only an approximation of the true predicted return, which is unobserved. These values assume that the subject predicts a dividend of 5 in the upcoming period and 5 in the final period. But because these quantities are defined only for smart traders, these approximations are reasonable.

define it as "buying an asset at a price exceeding what one believes to be the asset's fundamental value, or selling at a price below what one believes to be the asset's fundamental value, in the hopes of reaping a capital gain." If subjects in an experimental asset market are speculating, then their predictions of future price movements should be a significant predictor of their trading behavior, even when their beliefs about fundamental values are taken into account.

To determine if this is the case, I regress trading behavior on various factors. As measures of trading behavior, I use $NETBUY$ and $\widetilde{ASSETSHARE}$.[26] I will refer to these measures as "buying behavior" and "asset holding behavior," respectively. As explanatory variables, I use three measures of the income that traders expect to be able to receive from the risky asset:

$$E\_BUYANDHOLD_{it} = E_{it}\left[FV_t\right] - P_{it} \tag{15}$$

$$E\_APPRECIATION\_1_{it} = E\_PNEXT_{it} - P_t \tag{16}$$

$$E\_APPRECIATION\_F_{it} = E\_PFINAL_{it} - E\_PNEXT_{it} \tag{17}$$

The first of these is the expected buy-and-hold income, which is the difference between the expected remaining dividend income per share and the current price, i.e. the income a trader expects to be able to receive from buying a share of the asset in period t and holding it through the end of the market. The second factor is the expected one-period appreciation, i.e. the amount of capital gain a trader expects to be able to receive from buying a share of the asset in period t and selling it in the following period. The third factor is the expected

[26] I use the deviation from individual mean asset shares, $\widetilde{ASSETSHARE}$, rather than the raw asset share, $ASSETSHARE$, in order to allow for individual fixed effects in subjects' desired levels of risk. However, I do not account for fixed effects in net buying, because net buying is a flow measure rather than a stock measure, and individual fixed effects in $NETBUY$ do not have a clear interpretation. As a side note, all results obtained in the regressions using $NETBUY$ as the dependent variable also hold when fixed effects are taken into account.

further appreciation, i.e. the additional expected capital gain a trader expects to be able to receive from holding a share of the asset in period $t+1$ and selling it in period 10.[27]

With more price prediction data, a finer partition of predicted income variables would be possible; subjects could, for example, expect to buy in period 4 and sell in period 8. However, with only two price predictions and one fundamental prediction made in each period, the above partition of expected potential income uses all the available information.

The variable of interest is $E\_APPRECIATION\_1$. If this variable predicts the trading behavior of smart traders, it means that smart traders speculate. Thus, I regress trading behavior on expected one-period price appreciation, interacted with a smartness dummy. For each of the two measures of trading behavior (buying behavior and asset holding behavior), I first perform a simple baseline regression without covariates, followed by a regression with covariates included.

The equations of the regressions explaining buying behavior are:

$$\begin{aligned} NETBUY_{it} &= \alpha + \beta E\_APPRECIATION\_1_{it} \\ &+ \rho D\_SMARTER_{it} \\ &+ \gamma (E\_APPRECIATION\_1_{it} \times D\_SMARTER_{it}) \\ &+ \varepsilon_{it} \end{aligned} \tag{18}$$

$$\begin{aligned} NETBUY_{it} &= \alpha + \beta E\_APPRECIATION\_1_{it} \\ &+ \rho D\_SMARTER_{it} \\ &+ \gamma (E\_APPRECIATION\_1_{it} \times D\_SMARTER_{it}) \\ &+ \delta Z_{it} + \varepsilon_{it} \end{aligned} \tag{19}$$

[27] An alternative to using the expected further appreciation would be to simply use the expected capital gain from buying now and selling in the final period. However, in practice this turns out to be highly collinear with the expected one-period capital gain.

Where Z_{it} is a vector of covariates that includes:

- $CASH_{i,t-1}$, the cash in the subject's account at the beginning of period t,
- $SHARES_{it}$, the subject's shares of the risky asset at the beginning of period t,
- $E\_BUYANDHOLD_{it}$, the predicted profit from buying in period t and holding through the end of the market,
- $E\_APPRECIATION\_F_{it}$, the predicted further price appreciation (i.e., the predicted capital gain from holding in period $t+1$ and selling in the final period,
- $E\_BUYANDHOLD_{it} \times D\_SMARTER_{it}$, the smartness dummy interacted with the predicted buy-and-hold value,
- $E\_APPRECIATION\_F_{it} \times D\_SMARTER_{it}$, the smartness dummy interacted with the predicted further price appreciation,
- $\overline{P}_{t-1}$, the lagged average of past asset prices,
- D_{t-1}, the one-period lagged dividend, and
- t, a linear time trend.

I also run analogous regressions using $D\_MORECORRECT$ as the smartness dummy instead of $D\_SMARTER$.

Analogously, the equations of the regressions explaining asset holding behavior are:

$$\begin{aligned} \widetilde{ASSETSHARE}_{it} &= \alpha + \beta \widetilde{E\_APPRECIATION\_1}_{it} \\ &+ \rho D\_SMARTER_{it} \\ &+ \gamma(\widetilde{E\_APPRECIATION\_1}_{it} \times D\_SMARTER_{it}) \\ &+ \varepsilon_{it} \end{aligned} \tag{20}$$

$$\begin{aligned} \widetilde{ASSETSHARE}_{it} &= \alpha + \beta \widetilde{E\_APPRECIATION\_1}_{it} \\ &+ \rho D\_SMARTER_{it} \\ &+ \gamma(\widetilde{E\_APPRECIATION\_1}_{it} \times D\_SMARTER_{it}) \\ &+ \delta Z_{it} + \varepsilon_{it} \end{aligned} \tag{21}$$

Where Z_{it} is a vector of covariates that includes:

- $\widetilde{ASSETSHARE}_{i,t-1}$
- $\widetilde{ASSETSHARE}_{i,t-2}$
- $\widetilde{E\_BUYANDHOLD}_{it}$
- $\widetilde{E\_APPRECIATION\_F}_{it}$
- $\widetilde{E\_BUYANDHOLD}_{it} \times D\_SMARTER_{it}$, the smartness dummy interacted with the predicted buy-and-hold value,
- $\widetilde{E\_APPRECIATION\_F}_{it} \times D\_SMARTER_{it}$, the smartness dummy interacted with the predicted further price appreciation,
- $\overline{P}_{t-1}$,
- D_{t-1}, and

- t.

Again I run analogous regressions using $D\_MORECORRECT$ as the smartness dummy in place of $D\_SMARTER$.

The coefficients that measure short-term price speculation are β and γ. A positive value for the sum of these coefficients indicates speculation by smart traders.

Table I.8 shows the results for Equations 18 and 19, and Table I.9 shows the results for Equations 20 and 21. The coefficients β and γ are positive and significant in all specifications[28]. γ, the interaction-term coefficient, is about one order of magnitude larger than β in all specifications. The economic interpretation of the coefficients is as follows: a trader who understood fundamentals well in all previous periods (including the current period), if she predicted the price to go up by 20 more yen in the next period, bought about 1-4 more shares of the asset, or held another 10-25% of her wealth in the risky asset. In contrast, a trader who did not understand fundamentals bought approximately 0.1-0.4 more shares, or held another 1-2% of her wealth in the risky asset, for a similar increase in predicted one-period appreciation.[29] Not only does a speculative motive exist, it is substantially *stronger* for traders who understand fundamentals than for those who don't. This may be because smart traders have more confidence in their price predictions.

The result for β and γ is robust to a large set of alternative specifications, including the omission of either or both of the other predicted income regressors, the use of lagged or leading values of smartness dummies, more lags of dividends, lagged values of net buying, quadratic time trends, and the use of an Arellano-Bond panel GMM estimator instead of OLS.

[28]The coefficients β and γ are larger when the covariates are included. This is partly because $E\_APPRECIATION\_F_{it}$, the expected further appreciation, is negatively correlated with $E\_APPRECIATION\_1_{it}$. If $E\_APPRECIATION\_F_{it}$ is omitted as a covariate, β and γ are still larger in the regression with covariates, but the difference is not as large.

[29]Actually, the difference is not quite as large if we take into account the larger variance of price predictions among traders who do not understand fundamentals. A predicted 20 yen appreciation is about two unconditional standard deviations of $E\_APPRECIATION\_1$ for smart traders, but only about 2/3 of a standard deviation for confused traders. Thus, the speculation motive is only about three times as strong for smart traders, not ten times.

Thus, short-term speculation is a substantial motive for traders who understand fundamentals. Like Result 1, this is an *existence* result; the coefficients β and γ are not meant to measure the total speculation motive, but to provide a lower bound (subjects may also speculate on their predictions of more long-term price appreciation). The result does not contradict the finding in Lei, Noussair, and Plott (2001) that laboratory bubbles do not *require* speculation, but it shows that speculation nevertheless exists and is significant. Importantly, speculation will tend to prevent smart traders from exerting deflating pressure on bubbles.

4.4 Effect of past prices on trading behavior

Result 3: ***Lagged average prices are a significant predictor of trading decisions.***

Although subjects in this market do speculate, their predictions of future asset returns do not explain all of their trading behavior. In the regression results in Tables I.8 and I.9, lagged average prices emerge as a significant regressor with a coefficient several times as large as the coefficient on expected one-period price appreciation. This result persists in all of the alternative regression specifications listed above. This result is consistent with the broad features of observed trading behavior. Subjects tend to hold or sell the asset in periods 3-6, when the price is higher than its recent values. They then tend to buy in periods 7 and 8, after prices have begun to decline.

An interesting question is whether smart traders differ from other traders in their use of lagged prices. To assess whether they do, I run the regression in Equation 19 for subsamples of smart traders only, using several definitions of "smart". The results can be seen in Table I.10. For the stricter definitions of "smartness" used in most of this paper ($D\_MORECORRECT_{it} = 1$ and $D\_SMARTER_{it} = 1$), significance cannot be established; however, for looser definitions of "smartness" ($D\_MORECORRECT_{it} = 1$ and $D\_SMARTER_{it} = 1$), lagged prices emerge as significant. This is perhaps not surprising;

because prices do not vary across subjects, the relatively small numbers of subjects who meet the stricter "smartness" criteria result in this regression having low power. But if smart traders do indeed trade based on lagged prices, it would explain much of the result in Section 4.1.4; a broad similarity between trader types would be exactly what one would observe if the price history itself was a significant factor influencing the asset demands of all subjects. So although the data are insufficient to conclude that the very smartest traders trade on lagged prices, they indicate that there is a strong possibility that this is happening.

However, even if smart traders do not trade based on lagged prices, the phenomenon remains interesting and important, because it provides another mechanism by which *price-to-price feedback* can cause bubble-shaped asset mispricings. If traders who observe higher prices in the past are willing to accept higher prices in the future, temporary overpricings due to mistakes about fundamentals will grow larger over time. Instead of being corrected by a learning process, bubbles will be self-sustaining.

Why should average past asset prices predict traders' decision to buy? Because this experiment used the same price path for all subjects, it is only possible to conjecture. One possible explanation might seem to be *loss aversion*. If prices fall, loss-averse subjects whose wealth has declined may become risk-loving, buying in the hope that prices will rise and they will recoup their losses. However, given that most smart traders (as well as most traders in general) expect *positive* asset returns in periods 7 and 8, loss aversion seems like an incomplete explanation. Additionally, loss aversion is unable to explain the substantial nonzero asset holdings of many smart traders even at the bubble peak.

A second possible explanation is that a trader's predictions of future prices may depend on the prices she has observed in the past. Subjects may thus be engaging in a simple form of *technical analysis*, in which they try to gauge "support levels." If the market has demonstrated that it is willing to pay a price of 60 for the asset, and the price falls to 55, the asset may look "cheap." In fact, lagged average prices are positively correlated with predictions of future prices, as one would expect if subjects were forming their beliefs in this way. However, this explanation too seems incomplete; even when expectations of final-period prices are included as regressors, lagged average prices still emerge as significant. If subjects believed in "support levels," this belief should reflect itself in their final-period price predictions. Nor does error in the measurement of price predictions seem likely to explain all of the result, since the coefficient on lagged prices is an order of magnitude larger than the coefficients on price predictions.

There is a third possible reason for the "lagged price effect": *herd behavior*. Avery and Zemsky (1998) define herd behavior in financial markets as occurring when a trader reverses her trading decision (e.g. buys instead of sells) based on her observation of the trading decisions of others[30]. In the present experiment, subjects do not directly observe the actions of the others, only prices. However, note that observing a market price of 60 yen conveys the information that there existed subjects in the Source Market who purchased the asset for 60 or more yen in the given period[31]. If this observation affects the behavior of traders in the present study, then herd behavior is present.

Why would traders "imitate" the traders in the Source Market? This would happen if subjects *do not equate the asset's fundamental value with the dividend income that it provides*. Subjects may feel instinctively that if traders in the Source Market were willing to pay 60 yen for the asset, the asset must be worth somewhere in the neighborhood of 60 yen, while not explicitly forming beliefs about *why* it would have this value. "Smart" traders may understand all of the experimental parameters, yet fail to intuitively grasp that dividend income and resale value are the asset's only sources of value (alternatively, they may understand that dividends equal fundamentals, but not have complete confidence in this knowledge). While this type of herding would not fit the typical description of rational information-based herding (see Hirshleifer and Teoh 2001), it would constitute "behav-

[30]This is similar to the definition of herding given in Bikhchandani, Hirshleifer, and Welch (1992). It is unrelated to the notion of "managerial herding" in Scharfstein and Stein (1990). Note that the Avery and Zemsky (1998) also includes the kind of "technical analysis" postulated above; however, since in Avery and Zemsky's (1998) highly stylized model, traders are not allowed to resell the asset, I consider "support levels" and "herding" to be separate notions.

[31]And, additionally, that the traders who bought at 60 were in some sense "average".

ioral" or "instinctual" herding.

Such behavior is irrational, but it closely matches a very plausible strategy for real-world investing. A trader first assumes that the market is basically efficient, and that market prices are close to "right." She then looks for information that indicates small differences between the current price and the asset's true fundamental value or future resale value. When she is reasonably confident that such a difference exists, she makes a trade. This type of behavior is entirely consistent with the actions of the individual investors in the present experiment. However, though this strategy may sound reasonable, it represents herd behavior. If market prices deviate from fundamentals due to heterogeneous beliefs or mistakes, lagged asset prices no longer represent a good baseline for valuing an asset.

4.5 Design-experienced traders

Figure I.7 displays average net buying and average asset holdings for Market 2, along with market prices. The pattern of net buying appears similar to that in Market 1, with strong initial asset demand, reduced demand in middle periods when prices are most different from fundamentals, and then a positive spike in demand when prices begin to fall steeply. However, unlike in Market 1, $ASSETSHARE$ falls steeply as prices fall; this happens because the fall in prices overwhelms subjects' after-peak buying.

Repeating the regressions from Equations 18-21 yields results that differ from the results in Market 1. Beliefs about future prices no longer emerge as a significant predictor of trader behavior (see Table I.11). The impact of lagged prices in this market is difficult to assess, since prices in Market 2 are highly collinear with time. When time is omitted from the regressions, lagged prices become an important explanatory variable (Table I.11);[32] however, it is impossible to tell whether this is due to an effect like that seen in Market 1, or due to a time trend in asset demand.

These results indicate that design-experienced subjects rely *less* on their price predic-

[32] This is true even when the sample is restricted to smart traders.

tions than do design-inexperienced subjects; speculation is not obviously in evidence. This is consistent with the smaller incidence of bubbles seen in market repetitions in most asset pricing experiments.

4.6 Effects of experimental treatments

Table I.12 shows that the number of smart traders ($D\_SMARTER_{i,10} = 1$) was significantly higher (p-value=0.004) in the Pictures treatment, and insignificantly lower (p-value=0.134) in the Uncertain treatment (Wilcoxon rank-sum tests).[33] Showing subjects examples of feasible price and dividend series does increase their understanding of fundamentals, a result similar to that found by Lei and Vesely (2009).

However, $NETBUY$ and $ASSETSHARE$ are not significantly different across the three treatment groups in period 7 or period 8 of Market 1 (see Table I.13). The greater understanding of fundamentals induced by the Pictures treatment does not stop traders from buying into the bubble after the peak. When the sample is restricted to only smart traders ($D\_SMARTER_{it} = 1$), smart traders in Treatment 3 are found to buy significantly fewer shares in period 7 (p-value=0.045). Thus, there is some evidence that the Pictures treatment prevented smart traders from joining the bubble.

To ascertain whether subjects in the different treatments speculated to different degrees,

[33] This result also holds for $D\_SMART_{i,10}$. A similar result obtains for the null hypothesis that cumulative mistakes $\sum_t M_{it}$ are the same across treatment groups; mistakes are insignificantly higher in the Uncertain treatment and significantly lower in the Pictures treatment.

I use the following regressions:

$$\begin{aligned} NETBUY_{it} &= \alpha + \beta \times E\_APPRECIATION\_1_{it} \\ &+ \rho D\_PICTURES_i \\ &+ \gamma \times (E\_APPRECIATION\_1_{it} \times D\_PICTURES_i) \\ &+ Z_{it} + \varepsilon_{it} \end{aligned} \tag{22}$$

$$\begin{aligned} \widetilde{ASSETSHARE}_{it} &= \alpha + \beta \times \widetilde{E\_APPRECIATION}\_1_{it} \\ &+ \rho D\_PICTURES_i \\ &+ \gamma \times (\widetilde{E\_APPRECIATION}\_1_{it} \times D\_PICTURES_i) \\ &+ Z_{it} + \varepsilon_{it} \end{aligned} \tag{23}$$

Here, $D\_PICTURES_i$ is a dummy for the Pictures treatment, and Z_{it} is a vector of covariates[34]. Analogous regressions were run with $D\_UNCERTAIN_i$, a dummy for the Uncertain treatment, in its place. The Uncertain treatment is found to have no measurable effect on the degree of speculation. The results of the estimations for the Pictures treatment appear in Table I.14.. Subjects in the Pictures treatment were found to trade significantly less in accordance with their predictions of one-period price appreciation, and significantly more in accordance with their predictions of further price appreciation.

This result is perhaps surprising, since subjects who had seen examples of market price paths had more information with which to speculate. In particular, they could see that prices tended to display significant momentum, and that prices in bubble markets tended to peak and decline in the middle of the market. However, these traders chose to speculate less (at least in the short term) than traders who had no such information. This is contrary to the result of Haruvy, Lahav, and Noussair (2007), who find that traders who have witnessed

[34] The covariates are the same as in Equations (*****) and (*****), except with treatment dummies instead of smartness dummies.

bubbles try to "time the market," buying into a bubble and attempting to sell out just before the peak.

I conjecture that the reason for the decreased speculation in the Pictures treatment is greater attention to risk. Seeing a large array of *different* price paths informs subjects of the potentially large variance of prices, and discourages speculative behavior. This could also explain the decreased degree of speculation exhibited by all subjects in Market 2; after having witnessed a rise and crash in prices, and after having traded on their own incorrect price predictions, traders are more hesitant to trust their own judgment. This explanation has the flavor of the "overconfidence" literature (e.g. Barber and Odean 2000, Scheinkman and Xiong 2003), in which individual investors overestimate the precision of their own forecasts.

5 Conclusion

What can bubble experiments tell us about real-world bubbles? If bubble-shaped mispricings in real-world asset markets are caused primarily by feedback effects between non-financial sectors and asset markets, then the answer is "little." But if bubbles are caused by "price-to-price feedback," in which price movements themselves cause later price movements, then it may be possible to increase our understanding of the bubble phenomenon by studying simple, isolated financial markets like the one presented in this paper. The focus of "bubble experiments" should therefore be on identifying the behavioral and institutional sources of price-to-price feedback.

The first main contribution of this paper is to show that price-to-price feedback in simple experimental asset markets is not purely a function of subject confusion. The tendency of traders who understand fundamentals to buy just after the peak of a bubble shows that the movement of prices alone is sufficient to make these "smart" traders knowingly overpay for a risky asset - behavior that, in equilibrium, will have an effect on the price. This is the first study to show conclusively that this is the case.

This paper's second main contribution is to identify one source of price-to-price feedback, and to present evidence that a second source exists. "Smart" traders tend to trade based on their predictions of short-term price appreciation, irrespective of their understanding of fundamentals. This form of speculation may not be necessary for bubbles to appear (as in Lei, Noussair, & Plott 2001), but it clearly has the potential to contribute to bubbles. If smart traders predict that an already overpriced asset will continue to increase in price (as they do in Market 1 of this experiment), short-term speculation will tend to reduce the degree to which these smart traders pop bubbles through arbitrage.

The second source of price-to-price feedback in this experiment is unknown. In this experiment, subjects were more likely to demand a risky asset at a given price when past prices had been higher. This indicates the possibility that a form of herd behavior is at work, in which subjects believe that the market price is "close to right." If further research shows this to be the case, it will have profound implications for our understanding of how asset markets function. Generally, we think of markets as aggregating information. But if price bubbles - initiated, perhaps, due to heterogeneous beliefs or a burst of overconfident speculation - cause traders to believe that the market price is the "right" price, it means that financial markets have the capacity to aggregate *mis*information. That is clearly a phenomenon deserving of more study.

The third contribution of this paper is to identify a phenomenon of "after-peak buying." In this experiment, a group of traders whose net demand was approximately zero at the peak of a bubble began to buy strongly when the price began to fall. In a market equilibrium, this positive net demand would cause the price to rise, and thus sustain or reinflate the bubble. Although many studies focus on the *initial causes* of mispricings, another interesting question is why those mispricings are *sustained*. This question, too, is a goal for future research.

Finally, this paper makes a methodological contribution to the asset-pricing experiment literature. Whereas most asset market experiments use small markets as their unit of analysis, this study uses non-interacting individual traders facing market prices that they are too "small" to affect. Although this partial-equilibrium approach is not as useful as the typical setup for studying the effects of institutions on market outcomes, it is potentially more useful for studying the determinants of individual behavior, since it removes price endogeneity, group fixed effects, and coordination effects from the analysis. In one regard, this approach actually has more external validity than the traditional small-market general-equilibrium setup, since real-world investors are often individually too "small" to affect prices. Another way of saying this is that in a traditional asset-pricing experiment individual behavior is aggregated by a small, illiquid market, while a partial-equilibrium experiment provides insight into the individual behavior patterns that will be aggregated by large, liquid markets in the real world.

There are three fairly obvious extensions of this experiment that should be pursued in order to better understand and extend the results. The first is to vary the price path, using price series from a number of experimental "source" markets. This will allow a clearer determination of the effect of past prices on expectations of future prices, as well as on trading behavior. The second extension is to assess subjects' *willingness to pay* for the asset, using a call market setup. Such a setup would allow a determination of the size of the mispricing that the most bullish traders would be willing to support for a given history of prices and dividends. It would also allow computation of the hypothetical equilibrium price that would emerge from an aggregation of individual trader demands, and allow the experimenter to compare this price with the pre-determined market price, in order to ascertain whether traders on average would tend to pop or further inflate a given bubble. The third extension is to relax constraints on short selling and margin buying, as has been done in some "group" bubble experiments.

The methodology in this study also has applicability beyond the bubble experiment literature. The partial-equilibrium approach used here represents an important complement to traditional market-clearing experimental techniques, since it has the capability to isolate the determinants of individual behavior and expectations. There are any number of observed market phenomena that might be caused by a number of different types of behavior at the individual level - excess volatility, under-saving, and large equity premia, to name just a few. Additionally, although the process of individual expectation formation was not a major focus of this paper, expectation formation under uncertainty is obviously another area where a partial-equilibrium approach can be of use. These applications should be considered not only in terms of their relevance to financial economics, but to macroeconomics in general.

Tables and Figures

Figure I.1a: Prices and Fundamentals in Market 1

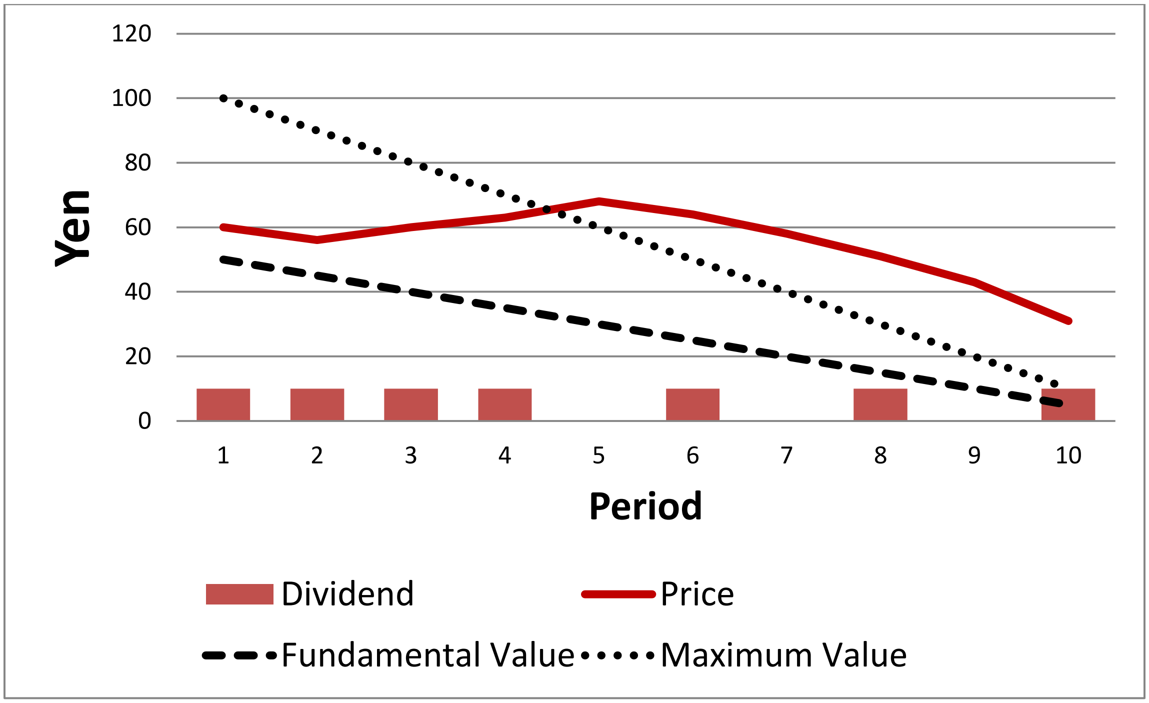


Figure I.1b: Prices and Fundamentals in Market 2

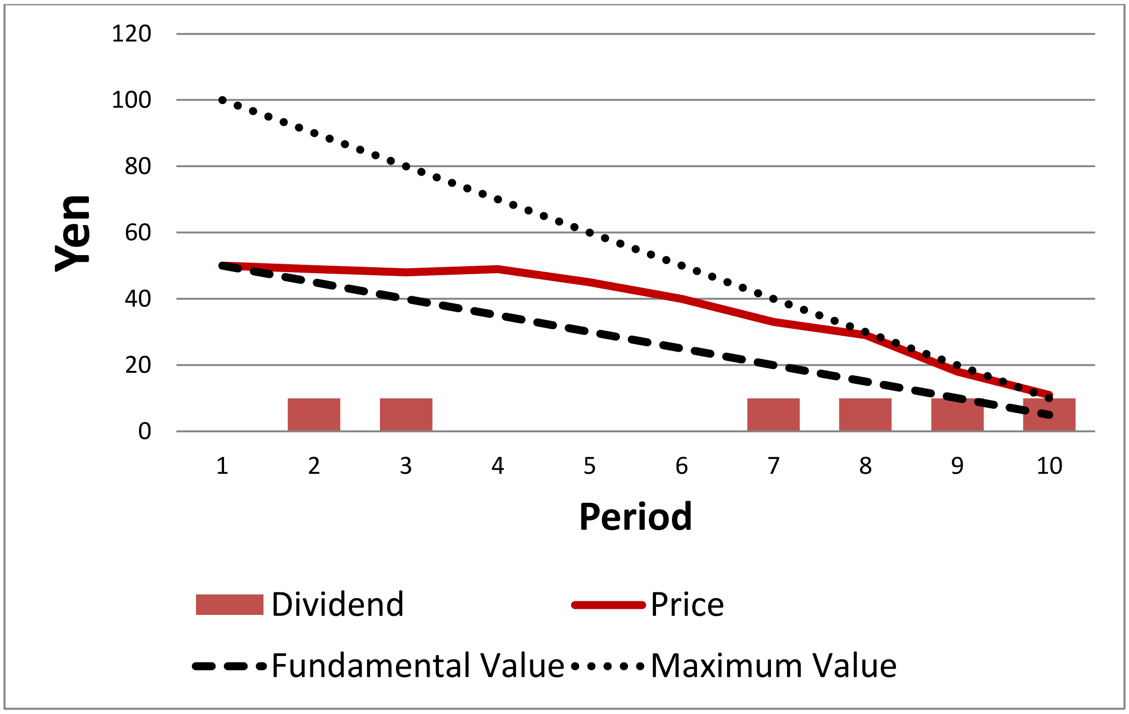


Figure I.2: Diagram of the Experimental Procedure

Figure I.3: Confused Traders' Mistakes About FV in Market 1

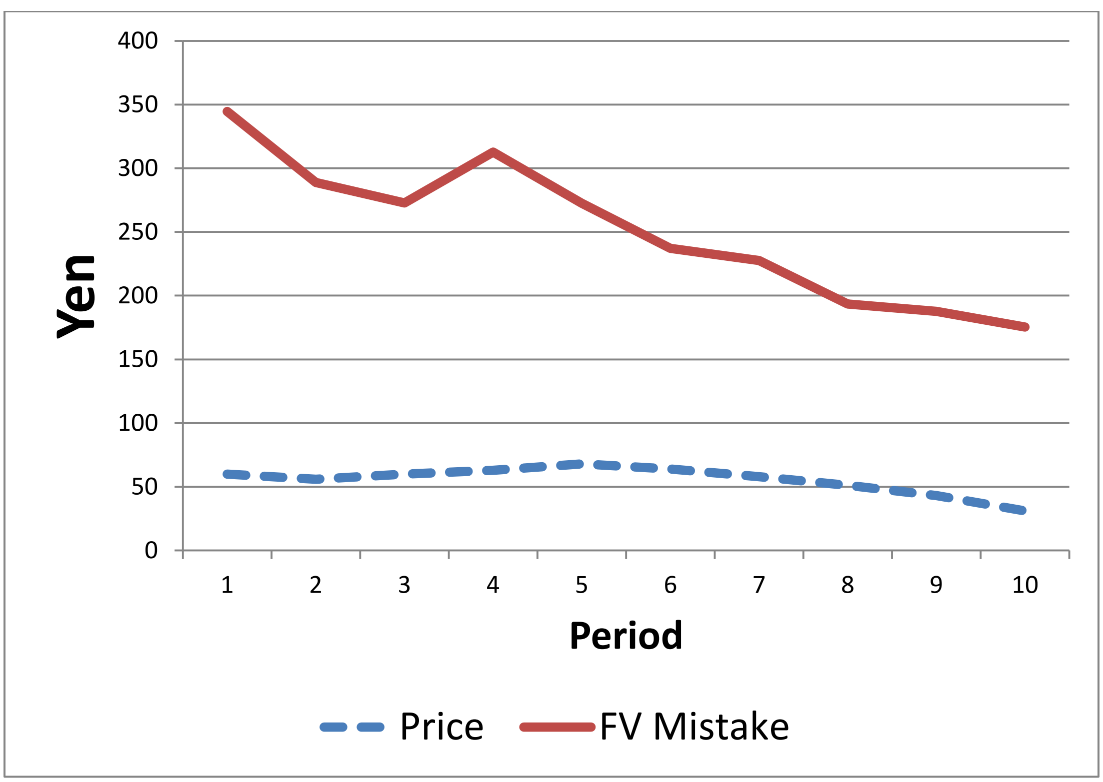


Fig. I.4a: Price Predictions vs. Lagged Prices, Market 1, All Subjects

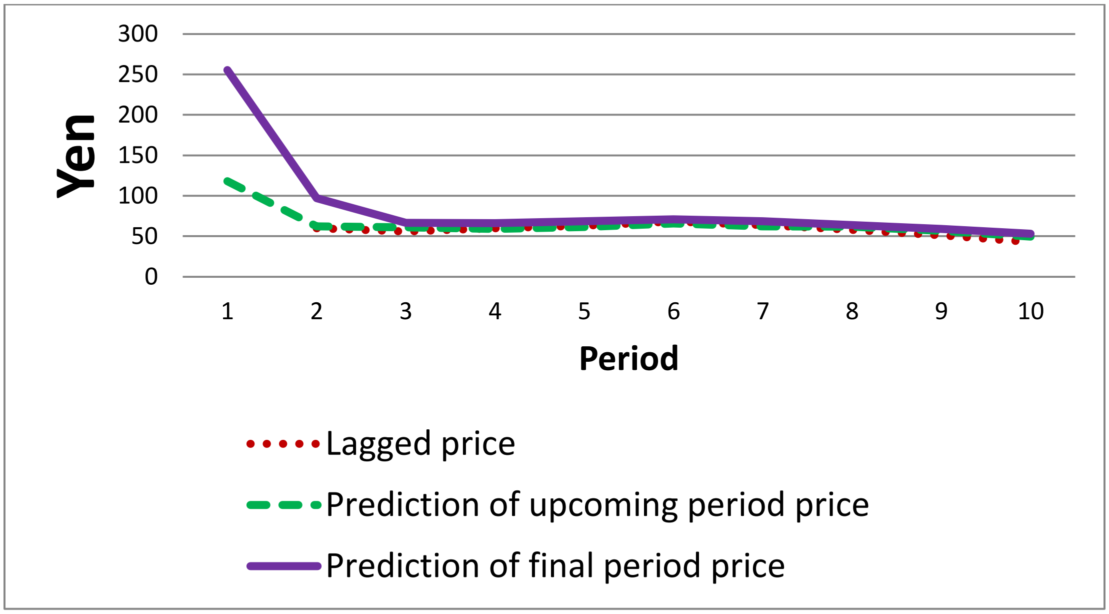


Fig. I.4b: Price Predictions vs. Lagged Prices, Market 1, Smart Traders

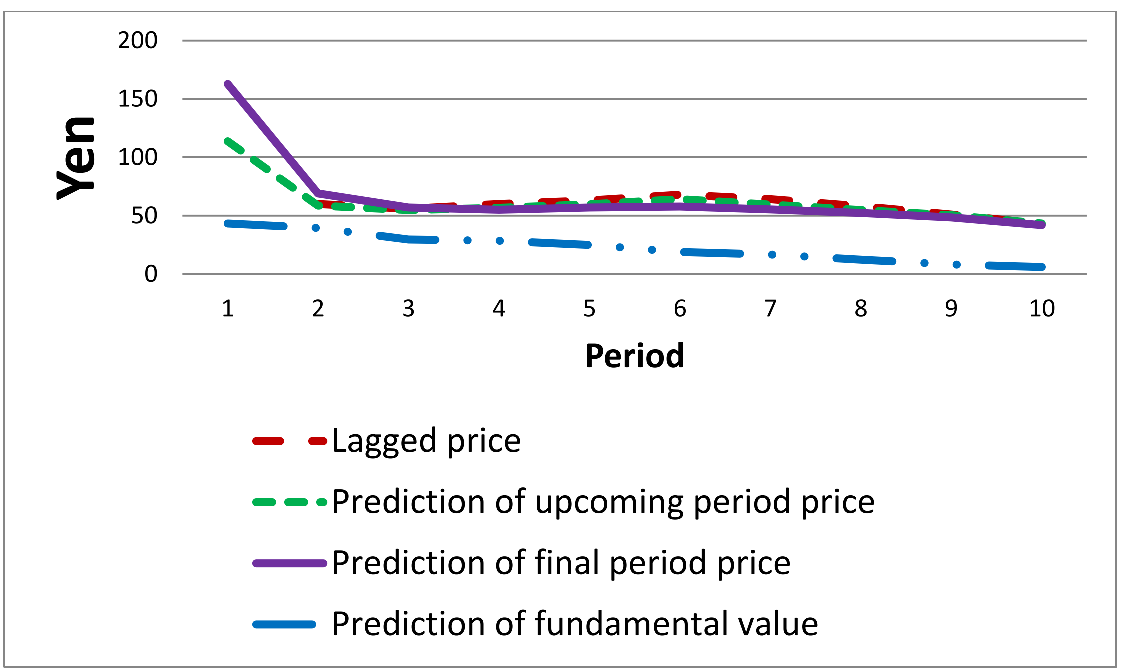


Figure I.5a: Average Net Buying in Market 1

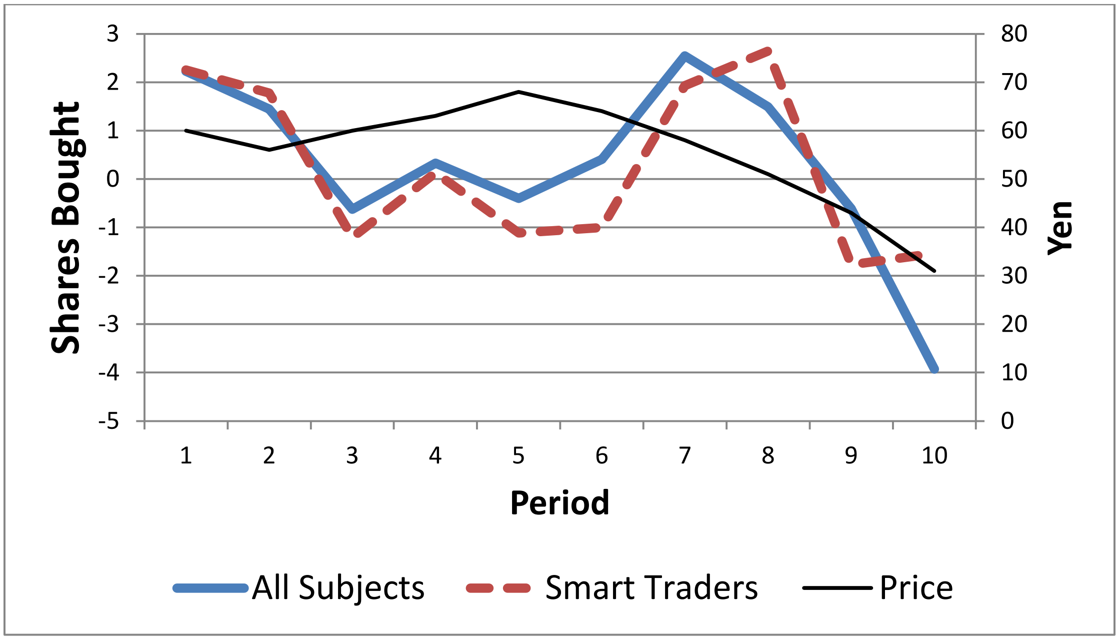


Figure I.5b: Average Asset Shares in Market 1

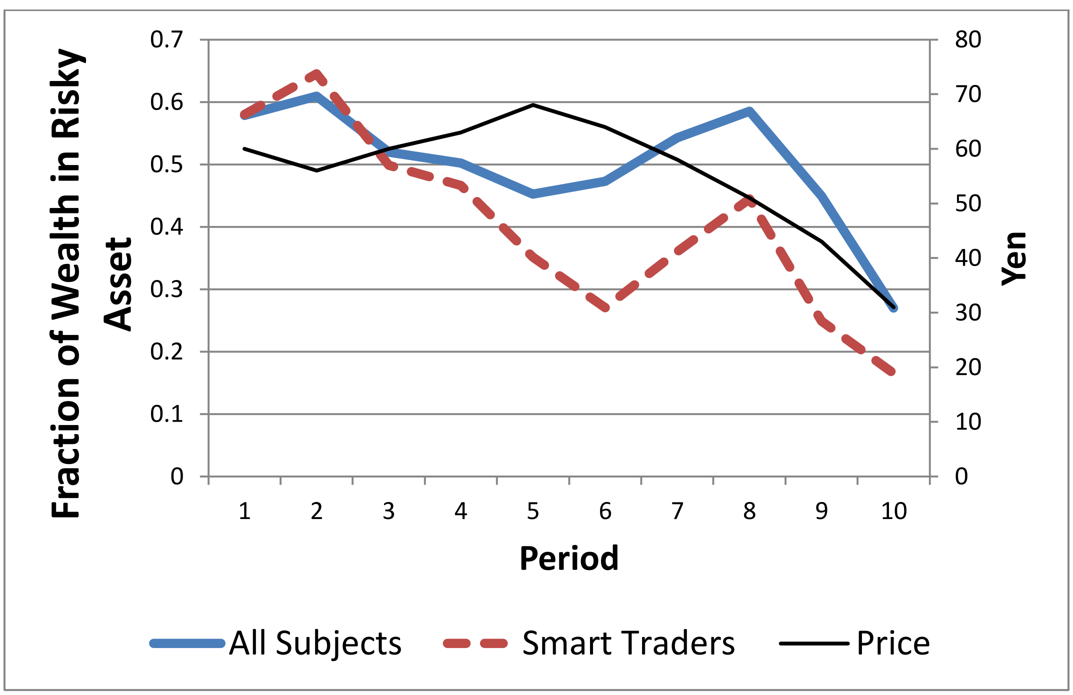


Figure I.6a: Average Net Buying in Market 1 (Extreme Groups)

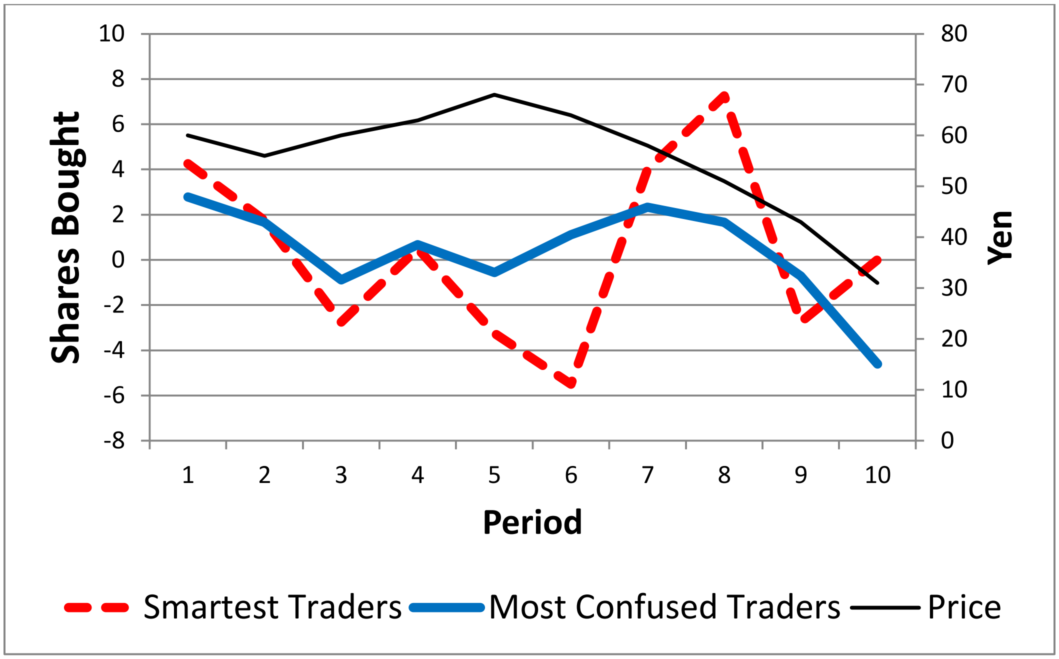


Figure I.6b: Average Asset Shares in Market 1 (Extreme Groups)

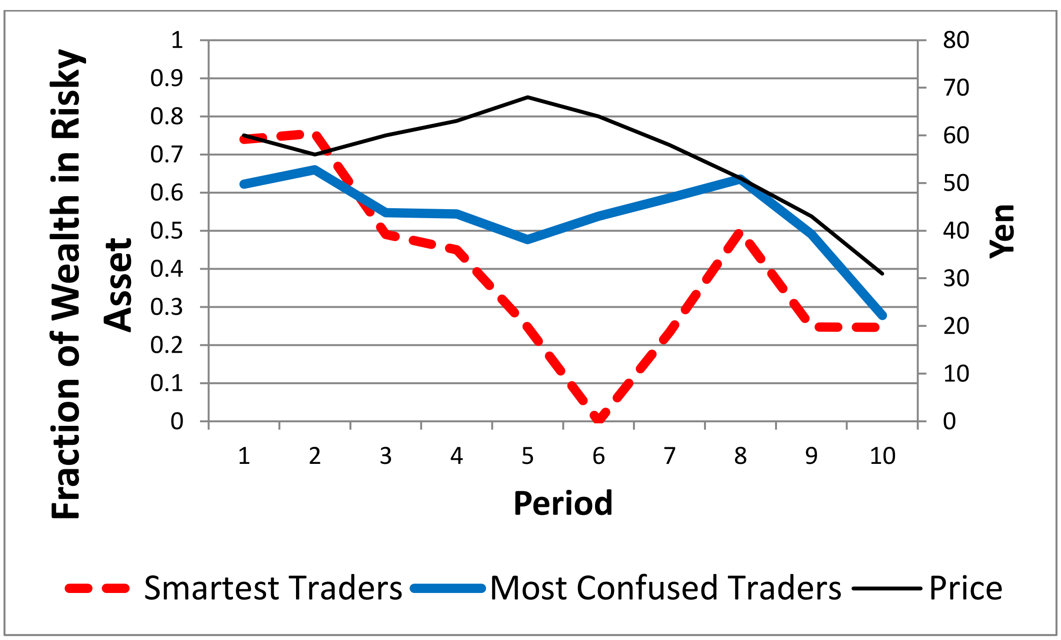


Figure I.7a: Average Net Buying in Market 2

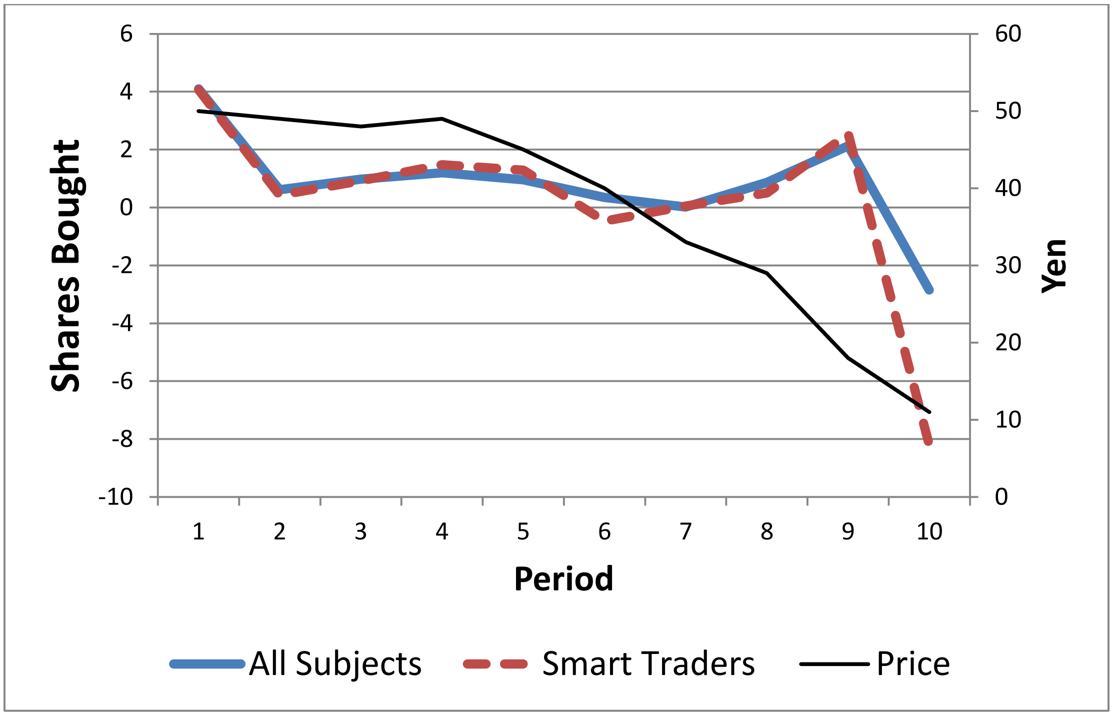


Figure I.7b: Average Asset Shares in Market 2

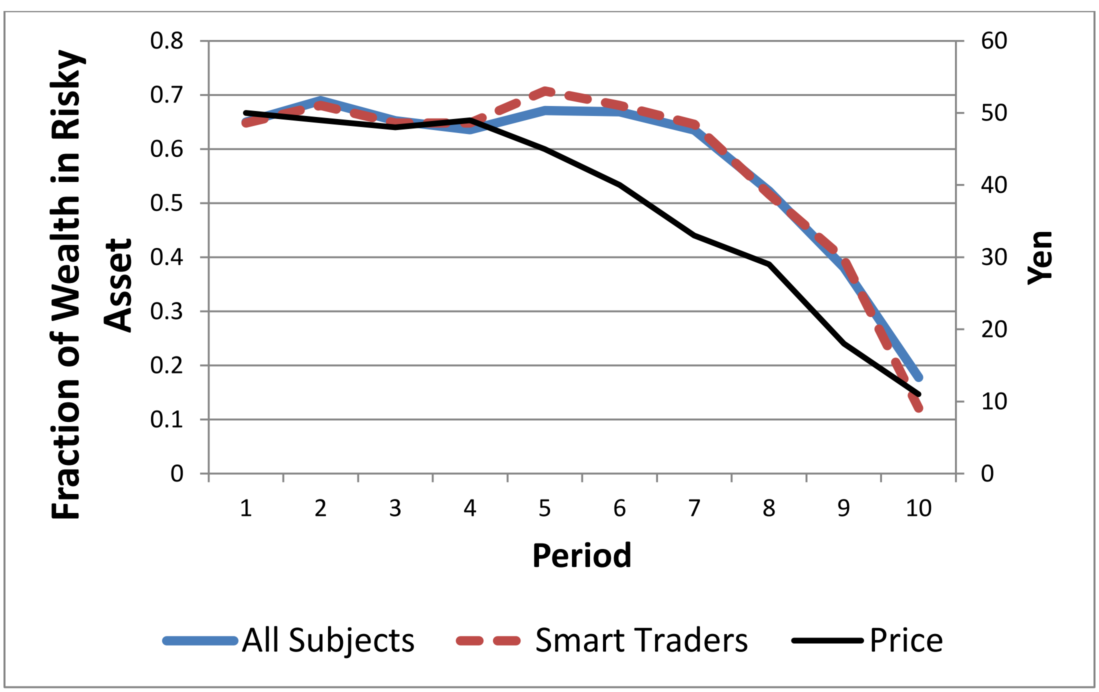


Table I.1: Treatments

Day	# of Subjects	Treatment
1	26	Basic
2	3	Basic
3	20	Uncertain
4	19	Pictures
5	15	Basic

Table I.2: Smartness Dummies

Smartness Dummy	Definition	# of Traders in Period 1	2	3	4	5	6	7	8	9	10
$D\_CORRECT$	Prediction of fundamental is feasible	57	48	45	42	41	42	36	35	36	39
$D\_MORECORRECT$	Prediction of fundamental is less than 50% from true value	36	36	33	30	25	23	27	28	26	39
$D\_MOSTCORRECT$	Prediction of fundamental is less than 5 yen from true value	18	29	15	22	20	20	17	26	26	39
D_SMART	Prediction of fundamental is feasible in all previous and current periods	57	45	41	38	35	33	31	29	29	28
$D\_SMARTEST$	Prediction of fundamental is less than 50% from true value in all previous and current periods	36	27	24	23	17	15	14	14	13	13
$D\_SMARTEST$	Prediction of fundamental is less than 5 yen from true value in all previous and current periods	18	13	8	8	6	6	4	4	4	4

Table I.3: Price Predictions

LHS: E_PNEXT_{it}

Regressor	Coefficient	p-value
P_{t-1}	0.681***	0.000
$P_{t-1} – P_{t-2}$	-0.547	0.112
$D\_MORECORRECT_{it}$	0.425	0.777
$D\_MORECORRECT_{it}$*($P_{t-1} – P_{t-2}$)	0.874*	0.059
#obs = 664	R-squared: 0.382	

LHS: E_PNEXT_{it}

Regressor	Coefficient	p-value
P_{t-1}	0.729***	0.000
$P_{t-1} – P_{t-2}$	-0.439	0.130
$D\_SMARTER_{it}$	0.914	0.665
D_SMARTERit*($P_{t-1} – P_{t-2}$)	0.758**	0.041
#obs = 664	R-squared: 0.349	

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table I.4: Variation of Price Predictions

Period	Standard deviation of upcoming-period price prediction as a percentage of market price
1	3.319
2	0.423
3	0.914
4	0.225
5	0.221
6	0.226
7	0.228
8	0.564
9	0.736
10	0.758

Table I.5: Net Asset Demand by Period

Period	Price	Price-FV	(Price-FV)/FV	Total net buying	Total net buying / shares outstanding (%)	p-value for H0: $NETBUYit = 0$
1	60	10	1.2	184.9	44.56	0.000
2	56	11	1.2	120.0	20.00	0.000
3	60	20	1.5	-52.0	-7.22	0.191
4	63	28	1.8	27.0	4.04	0.679
5	68	38	2.3	-33.0	-4.75	0.933
6	64	39	2.6	34.0	5.14	0.252
7	58	38	2.9	211.0	30.31	0.000
8	51	36	3.4	124.0	13.67	0.001
9	43	33	4.3	51.0	4.95	0.696
10	31	26	6.2	-326.0	-33.27	0.000

Table I.6: Trading Behavior in Periods 7 and 8

Period	Price - FV	(Price-FV)/FV	Trader Category	# of Traders	% who bought	% who held	Average net buying	Average asset share
7	38	1.9	All	83	71	23	2.54	0.54
7	38	1.9	$D\_MORECORRECTit = 1$	27	70	19	1.96	0.42
7	38	1.9	$D\_SMARTERit = 1$	14	71	21	1.93	0.36
8	36	2.4	All	83	48	34	1.43	0.59
8	36	2.4	$D\_MORECORRECTit = 1$	26	50	35	1.86	0.47
8	36	2.4	$D\_SMARTERit = 1$	14	50	29	2.64	0.45

Table I.7: Net Buying in Periods 7 and 8

Period	Price - FV	(Price-FV)/FV	Trader Category	# of Traders	p-value for H0: $NETBUY_{it} = 0$	p-value for H0: $ACTION_{it} = 0$
7	38	1.9	All	83	0.000	0.000
7	38	1.9	$D\_MORECORRECT_i = 1$	27	0.000	0.001
7	38	1.9	$D\_SMARTER_{it} = 1$	14	0.011	0.004
8	36	2.4	All	83	0.001	0.001
8	36	2.4	$D\_MORECORRECT_i = 1$	26	0.029	0.054
8	36	2.4	$D\_SMARTER_{it} = 1$	14	0.071	0.103
					p-value for H0: $ASSETSHARE_{it} = 0$	p-value for H0: $SHARES_{i,8} - SHARES_{i,6} = 0$
7	38	1.9	All	83	0.008	
7	38	1.9	$D\_MORECORRECT_i = 1$	27	0.360	
7	38	1.9	$D\_SMARTER_{it} = 1$	14	0.488	
8	36	2.4	All	83	0.000	0.000
8	36	2.4	$D\_MORECORRECT_i = 1$	26	0.240	0.001
8	36	2.4	$D\_SMARTER_{it} = 1$	14	0.267	0.023

Table I.8a: Determinants of Net Buying

LHS: $NETBUY_{it}$

Regressor	Simple Regression Coefficient	Simple Regression p-value	Covariates Included Coefficient	Covariates Included p-value
$E\_APPRECIATION1_{it}$	0.010***	0.008	0.010**	0.017
$D\_MORECORRECT_{it}$	-0.262	0.284	0.824	0.207
$E\_APPRECIATION1_{it}$ * $D\_MORECORRECT_{it}$	0.108***	0.005	0.099**	0.046
$E\_BUYANDHOLD_{it}$			0.001**	0.013
$E\_APPRECIATIONF_{it}$			0.003	0.516
$E\_BUYANDHOLD_{it}$ * $D\_MORECORRECT_{it}$			0.034	0.147
$E\_APPRECIATIONF_{it}$ * $D\_MORECORRECT_{it}$			0.04***	0.000
$CASH_{i,t-1}$			0.007**	0.011
D_{t-1}			0.016	0.653
$SHARES_{i,t-1}$			0.102	0.333
T			-0.375***	0.007
$P\text{-}bar_{t-1}$			0.771***	0.000
#obs: 747	R-squared: .024		R-squared: .162	

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table I.8b: Determinants of Net Buying

LHS: $NETBUY_{it}$

	Simple Regression		Covariates Included	
Regressor	Coefficient	p-value	Coefficient	p-value
$E\_APPRECIATION1_{it}$	0.010**	0.010	0.012***	0.005
$D\_SMARTER_{it}$	-0.389*	0.086	1.549	0.100
$E\_APPRECIATION1_{it} * D\_SMARTER_{it}$	0.138***	0.003	0.227**	0.010
$E\_BUYANDHOLD_{it}$			0.001**	0.014
$E\_APPRECIATIONF_{it}$			0.004	0.326
$E\_BUYANDHOLD_{it} * D\_SMARTER_{it}$			0.075**	0.024
$E\_APPRECIATIONF_{it} * D\_SMARTER_{it}$			0.034***	0.002
$CASH_{i,t-1}$			0.007***	0.005
D_{t-1}			0.004	0.900
$SHARES_{i,t-1}$			0.123	0.231
T			-0.398***	0.003
$P\text{-}bar_{t-1}$			0.722***	0.000
#obs: 747	R-squared: .030		R-squared: .282	

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table I.9a: Determinants of Asset Holding

LHS: $ASSETSHARE_{it}$

Regressor	Simple Regression: Coefficient	Simple Regression: p-value	Covariates Included: Coefficient	Covariates Included: p-value
$E\_APPRECIATION1_{it}$	0.000	0.562	0.001***	0.002
$E\_APPRECIATION1_{it}$ * $D\_MORECORRECT_{it}$	0.009***	0.000	0.007***	0.004
$E\_BUYANDHOLD_{it}$			0.000	0.943
$E\_APPRECIATIONF_{it}$			0.000	0.559
$E\_BUYANDHOLD_{it}$ * $D\_MORECORRECT_{it}$			-0.001**	0.029
$E\_APPRECIATIONF_{it}$ * $D\_MORECORRECT_{it}$			-0.001	0.52
$ASSETSHARE_{i,t-1}$			0.479***	0
$ASSETSHARE_{i,t-2}$			-0.201**	0.025
D_{t-1}			-0.002	0.165
t			-0.018***	0.005
$P\text{-bar}_{t-1}$			0.027**	0.015
#obs: 664	R-squared: .032			R-squared: .232

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table I.9b: Determinants of Asset Holding

LHS: $ASSETSHARE_{it}$

Regressor	Simple Regression: Coefficient	Simple Regression: p-value	Covariates Included: Coefficient	Covariates Included: p-value
$E\_APPRECIATION1_{it}$	0.000	0.516	0.001***	0.001
$E\_APPRECIATION1_{it}$ * $D\_SMARTER_{it}$	0.011***	0.000	0.013***	0.006
$E\_BUYANDHOLD_{it}$			0.000	0.93
$E\_APPRECIATIONF_{it}$			0.000	0.522
$E\_BUYANDHOLD_{it}$ * $D\_SMARTER_{it}$			0.009**	0.042
$E\_APPRECIATIONF_{it}$ * $D\_SMARTER_{it}$			0.000	0.959
$ASSETSHARE_{i,t-1}$			0.46***	0
$ASSETSHARE_{i,t-2}$			-0.212**	0.017
D_{t-1}			-0.003	0.112
T			-0.016**	0.012
$P\text{-}bar_{t-1}$			0.029***	0.005
#obs: 664		R-squared: .037		R-squared: .247

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table I.10: Significance of Lagged Prices in Subgroup Regressions

LHS: $NETBUY_{it}$

Subgroup	Coefficient on Lagged Prices	#obs	p-value
$D\_MORECORRECT_{it}=1$	.592	185	0.118
$D\_SMARTER_{it}=1$	.473	66	0.401
$D\_CORRECT_{it}=1$	.711**	364	0.034
$D\_SMART_{it}=1$	.686*	309	0.064

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table I.11a: Net Buying in Market 2

LHS: $NETBUY_{it}$

Regressor	Coefficient	p-value
$E\_BUYANDHOLD_{it}$	0.001*	0.088
$E\_APPRECIATION1_{it}$	0.003	0.323
$E\_APPRECIATIONF_{it}$	0.006	0.721
D_SMARTERit	0.444	0.256
$E\_BUYANDHOLD_{it} * D\_SMARTER_{it}$	0.0289	0.271
$E\_APPRECIATION1_{it} * D\_SMARTER_{it}$	0.0168	0.558
$E\_APPRECIATIONF_{it} * D\_SMARTER_{it}$	0.00339	0.923
$CASH_{i,t-1}$	0.0160***	0.000
D_{t-1}	-0.007	0.815
$SHARES_{i,t-1}$	0.518***	0.000
$P\text{-}bar_{t-1}$	0.586***	0.000
#obs: 738	R-squared: .104	

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table I.11b: Asset Holding in Market 2

LHS: $ASSETSHARE_{it}$

Regressor	Coefficient	p-value
$E\_BUYANDHOLD_{it}$	0.000	0.339
$E\_APPRECIATION1_{it}$	0.000	0.581
$E\_APPRECIATIONF_{it}$	0.001	0.350
$E\_BUYANDHOLD_{it} * D\_SMARTER_{it}$	-0.003**	0.043
$E\_APPRECIATION1_{it} * D\_SMARTER_{it}$	-0.000	0.857
$E\_APPRECIATIONF_{it} * D\_SMARTER_{it}$	-0.010***	0.002
$ASSETSHARE_{i,t-1}$	0.586***	0.000
$ASSETSHARE_{i,t-2}$	-0.160***	0.001
D_{t-1}	-0.005***	0.000
$P\text{-}bar_{t-1}$	0.027***	0.000
#obs: 656	R-squared: .491	

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table I.12: Effect of Experimental Treatments on Fundamental Mistakes

Hypothesis	z-score	p-value
$D\_SMARTER_{i,10}(UNCERTAIN) = D\_SMARTER_{i,10}(OTHER)$	1.497	0.1344
$D\_SMARTER_{i,10}(PICTURES) = D\_SMARTER_{i,10}(OTHER)$	-2.875	0.004

Table I.13a: Differences in Period 7 and 8 Trading, Uncertain Treatment

Trader Category	#obs	H0: $NETBUY(UNCERTAIN) = NETBUY(OTHER)$	H0: $ASSETSHARE(UNCERTAIN = ASSETSHARE(OTHER)$
		Period 7	
All	83	0.569	0.655
$D\_SMARTERit = 1$	14	0.706	0.533
		Period 8	
		H0: $NETBUY(UNCERTAIN) = NETBUY(OTHER)$	H0: $ASSETSHARE(UNCERTAIN = ASSETSHARE(OTHER)$
All	83	0.740	0.915
$D\_SMARTERit = 1$	14	0.705	0.535

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table I.13b: Differences in Period 7 and 8 Trading, Pictures Treatment

Trader Category	#obs	Period 7	
		H0: $NETBUY(PICTURES) = NETBUY(OTHER)$	H0: $ASSETSHARE(PICTURES) = ASSETSHARE(OTHER)$
All	83	0.208	0.324
$D\_SMARTERit = 1$	14	0.045**	0.275
		Period 8	
		H0: $NETBUY(PICTURES) = NETBUY(OTHER)$	H0: $ASSETSHARE(PICTURES) = ASSETSHARE(OTHER)$
All	83	0.678	0.172
$D\_SMARTERit = 1$	14	0.217	0.064*

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table I.14a: Net Buying, Pictures Treatment

LHS: $NETBUY_{it}$

Regressor	Coefficient	p-value
$E\_BUYANDHOLD_{it}$	0.001*	0.091
$E\_APPRECIATION1_{it}$	0.039**	0.046
$E\_APPRECIATIONF_{it}$	0.003	0.798
$D\_PICTURES_{i}$	-0.789**	0.033
$E\_BUYANDHOLD_{it} * D\_PICTURES_{i}$	0.002	0.171
$E\_APPRECIATION1_{it} * D\_PICTURES_{i}$	-0.025	0.193
$E\_APPRECIATIONF_{it} * D\_PICTURES_{i}$	0.008	0.545
#obs: 747	R-squared: .127	

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level
Note: Other covariates not shown.

Table I.14b: Asset Holding, Pictures Treatment

LHS: $ASSETSHARE_{it}$

Regressor	Coefficient	p-value
$E\_BUYANDHOLD_{it}$	-0.000	0.608
$E\_APPRECIATION1_{it}$	0.004***	0.005
$E\_APPRECIATIONF_{it}$	-0.001**	0.015
$E\_BUYANDHOLD_{it} * D\_PICTURES_{i}$	0.0004***	0.001
$E\_APPRECIATION1_{it} * D\_PICTURES_{i}$	-0.003**	0.024
$E\_APPRECIATIONF_{it} * D\_PICTURES_{i}$	0.001***	0.003
#obs: 664	R-squared: .227	

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level
Note: Other covariates not shown.

Appendix I.A: Experimental Instructions

(Note: Instructions that appear in normal type were given in all treatments. Instructions that appear in highlighted font were omitted from the Uncertain treatment. Instructions that appear in *italics* were included only in the Uncertain treatment.)

This is an experiment about decision-making in financial markets. In this experiment, you will participate in a computerized market, in which you will buy and sell a financial asset, and make predictions about the asset. The better investment decisions and predictions you make, the more money you take home, so invest wisely!

The Asset

The asset that you will be buying and selling is computer-generated. It is divided into "shares." You can buy and sell these shares individually.

The asset market is divided into 10 "periods." At the end of each period, each share of the asset pays an amount of money called a "dividend." This dividend is the amount of yen that you get for owning the share. The dividend is random, and is determined each period by a computerized random number generator. The dividends are the same for all shares, but different from period to period.

Here are the possible dividends, and the percentage chance of each:

Here are the possible dividends. The dividend probabilities are the same in all periods, but we will not reveal these probabilities:

Dividend per share	Percentage chance
0	50% *??*
10	50% *??*

So if you own 3 shares at the end of Period 6, there is a 50% chance that you will get 30 yen in dividend payments at the end of Period 6, and a 50% chance that you will get 0 yen.

Buying and Selling

You will have a computerized cash account and asset portfolio. The cash account contains your cash. You can use this cash to buy and sell shares of the asset. The asset portfolio contains your shares of the asset.

Each period, you will see the market price of the asset. You can buy as many shares as you like at this market price, as long as you have enough cash in your account. For example, if you have 1000 yen in your account and the market price is 100, you can buy up to 10 shares. Alternatively, you can sell shares at the market price. You can sell as many shares as you have in your account. Buying and selling shares does not change the price. In each period, you can buy or sell shares, but not both.

The market price comes from an earlier experiment. In that experiment, groups of 5 or 6 people (experimental subjects like yourself) traded the asset among themselves. The average price they paid for the asset in each period became the “market price” that you see.

In addition to the market price, you will see the “high” and “low”. These are the highest and lowest prices that were paid for the asset when the market price was determined. You cannot buy and sell at these prices, only at the market price. However, looking at the high and low may help you decide how much to buy or sell.

The amount of cash in your account after at the end of each period:

Your cash at the beginning of the period – (# of shares you bought) x (market price) + (# of shares you sold) x (market price) + (this period’s dividend) x (the number of shares in your portfolio)

At the end of the market, you get to take home all the cash in your account.

Predictions

Before each period, you will be asked to predict three things:

1. **The market price in the upcoming period**
This is your prediction of what you think the market price will be in the period that is about to start.

2. **The market price in the final period (period 10)**
This is your prediction of what you think the market price will be in the FINAL period of the market.

3. **The Total Dividend Yield Per Share**
The Total Dividend Yield Per Share is the TOTAL amount of dividends that you think someone would receive from ONE share of the asset, if they bought the share in the upcoming period and held it until the end of the market. So if you think that someone who bought 3 shares this period and held them until the end of the market would receive a total of 300 yen in dividends from those 3 shares, then the Total Dividend Yield Per Share is 100.

The Trading Software

The trading software you will be using is called z-Tree. We will show you how it works.

You will always see the following information at the top of the screen

Period ← This is the current trading period. If it is between periods, this is the number of the next period.

Time Remaining ← This is the time left for you to make your decision.

You will always see the following information in a column on the right-hand side of the screen:

Cash ←This shows how much cash you currently have in your account. Cash is denominated in yen.

Shares ← This shows how many shares of the asset you currently own. This is called your "portfolio".

You will also see the following information in a column on the left of the screen:

Market Price ← This is the price of the asset. You can buy and sell at this price.

High ← This is the highest price paid for a share of the asset (by the people in the market where the price was determined).

Low ← This is the lowest price paid for a share of the asset (by the people in the market where the price was determined).

On the left hand side of the screen, you will also see two charts:

Price History ← This shows the market price in each of the previous periods

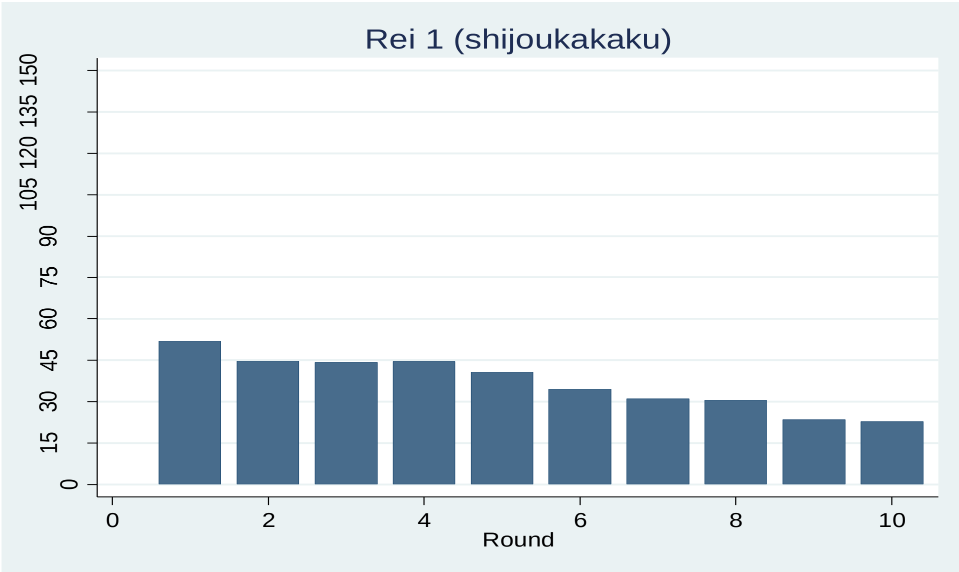
Dividend History ← This shows the dividend (per share) paid to holders of the asset in each of the previous periods

Finally, you will see **input boxes**. These are the boxes where you make your predictions and your buying/selling decisions.

Appendix I.B: Examples of Market Prices and Dividends (Pictures Treatment)

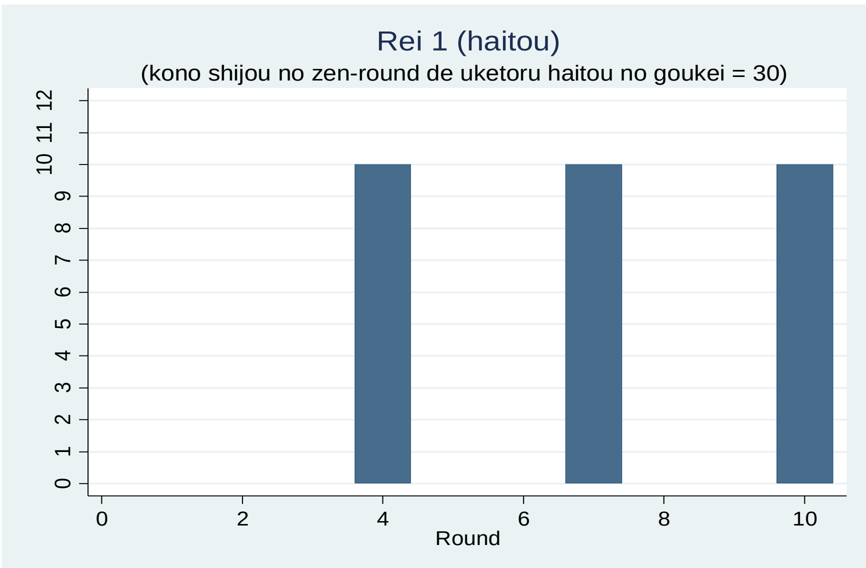
Note: Translations appear in text outside pictures.

Example 1 (market price)

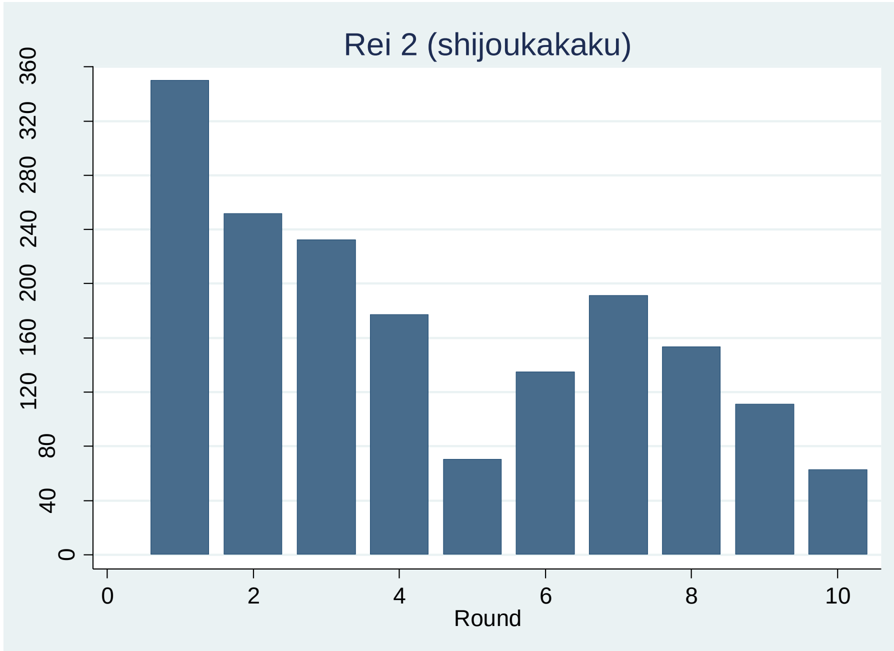


Example 1 (dividends)

(total dividend per share received in this market = 30)

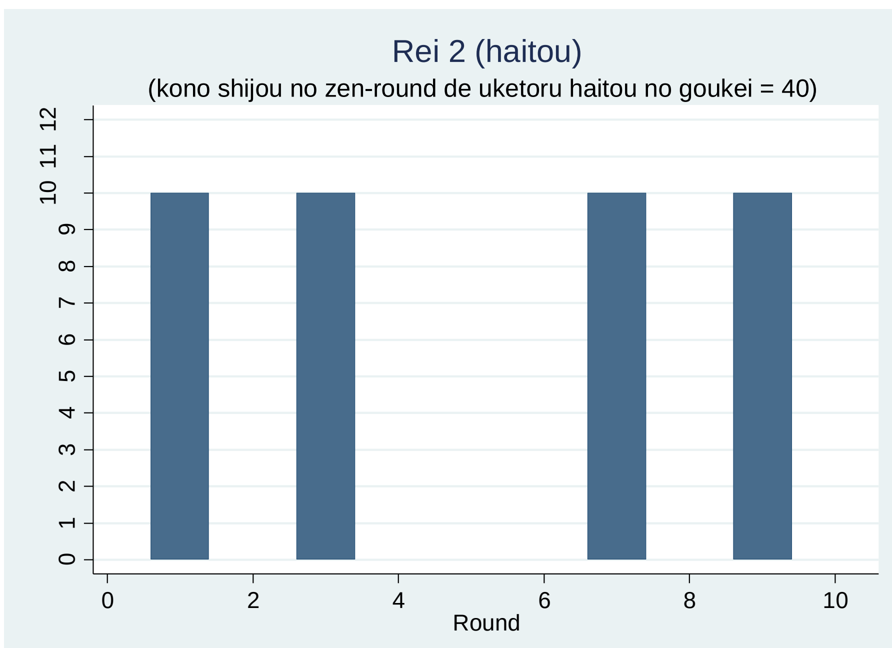


Example 2 (market price)

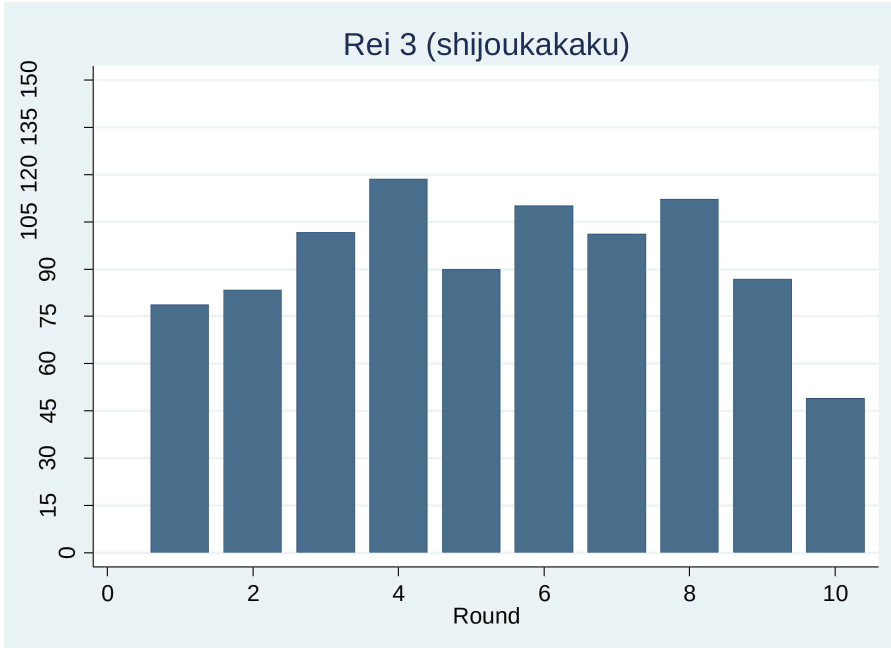


Example 2 (dividends)

(total dividend per share received in this market = 40)

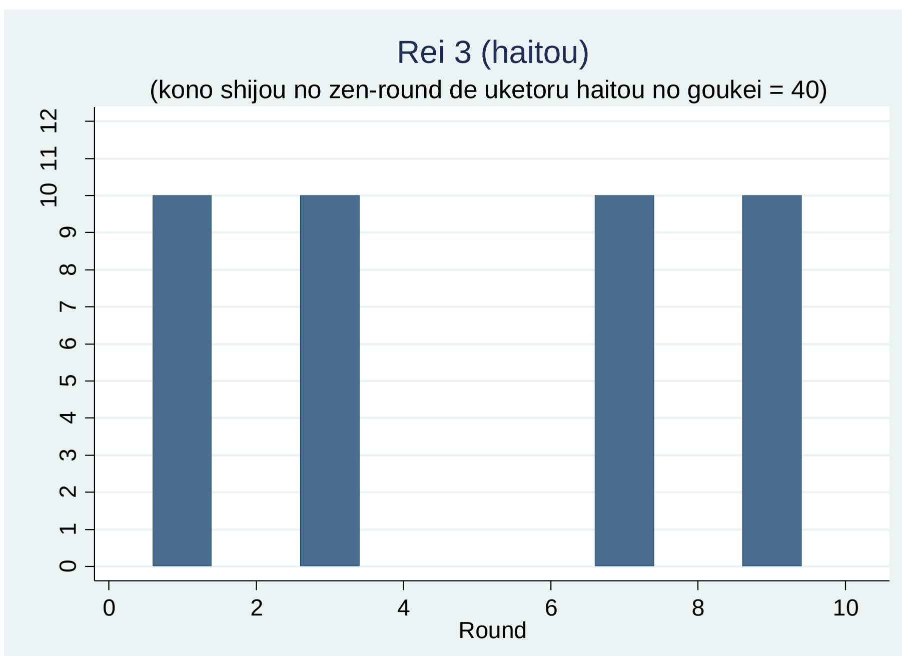


Example 3 (market price)

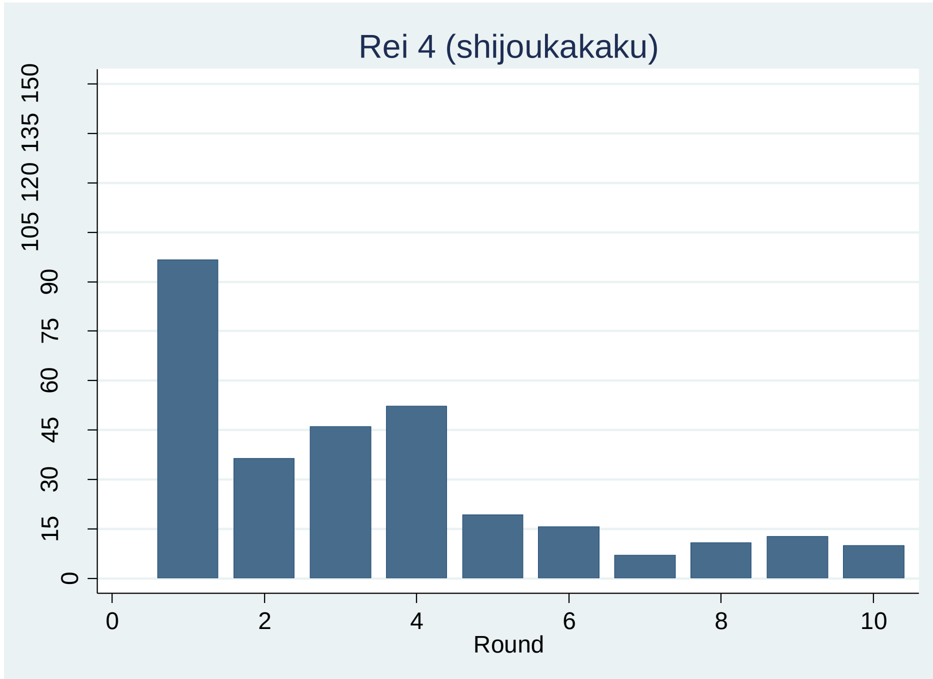


Example 3 (dividends)

(total dividend per share received in this market = 40)

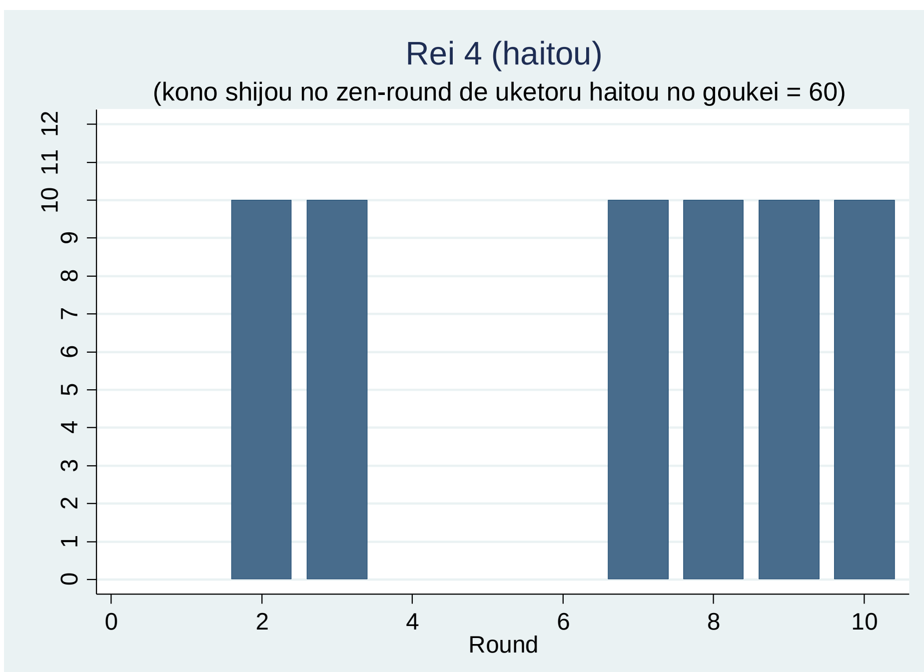


Example 4 (market price)



Example 4 (dividends)

(total dividend per share received in this market = 60)



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Chapter II
Private Information and Overconfidence in an Experimental Asset Market

1 Introduction

Trading in financial markets naturally involves an adverse selection problem. If you buy an asset at a price of x, your counterparty's willingness to sell at that price indicates that she may have private information that indicates she is getting the better end of the deal. If traders ignore this adverse selection, they will tend to engage in suboptimal trades. A growing body of empirical research supports the hypothesis that traders trade too much. Odean (1999), for example, finds that excessive trading lowers the returns that individual investors receive on their portfolios, even when controlling for liquidity and timing demands. Odean's proposed explanation for this over-trading is *overconfidence* - the tendency for investors to believe, incorrectly, that their information is of superior quality to that of their counteraparty. That sort of belief would lead traders to ignore the adverse selection problem - if you and I both believe that our information is superior to the other's, then we may trade even though we both know we disagree. For this reason, economists have constructed a number of models in which trading and asset pricing are driven by overweighting of private signals about asset fundamentals - i.e., an incorrect but dogmatic belief of each agent that her own private signals are more precise than those signals really are (and more precise than the signals of others).

This modeling assumption is typically motivated by a large psychology literature on individual overconfidence. Psychologists have shown that people tend both to overestimate the precision of their own information ("miscalibration" about signal precision) and to believe that they are above average in ability (the "better than average effect"). Both of these effects could cause the overweighting of private information in financial markets.

So behavioral theories of overtrading posit a two-step chain of causality: Psychological overconfidence –> Overweighting of private information –> Market outcomes. Experimental and empirical tests of these theories, however, have focused only on the direct link between overconfidence and market outcomes. In this paper, I use an experiment to disentangle the two steps in the causal chain; in other words, I ask two questions:

1. Do traders trade based on ex ante symmetric private signals of asset fundamentals?

2. Do more overconfident traders rely more on private signals to make trading decisions?

To answer these questions, I use a variant of the most common laboratory financial market setup (Smith, Suchanek, and Williams 1988), where groups of subjects trade a single risky asset in three successive fixed-horizon markets. In my experimental markets, traders receive noisy private signals of asset fundamentals. They also make predictions about the future path of asset prices. Before trading begins, I use a Bayesian inference task to gauge two types of individual overconfidence commonly cited in the behavioral finance literature - miscalibration about signal precision, and the "better than average effect." This task is a signal-extraction problem that is extremely similar to the one by which dividends can be inferred from forecasts during the asset market.[1] On average, subjects are overconfident

[1] The only difference between the two signal-extraction problems is the probability distribution itself. The overconfidence test has signals that are less precise than the dividend forecasts in the market. This was done so that if the similarity between the tasks gave subjects any bias regarding the precision of the dividend forecasts, that bias would be toward thinking dividend forecasts were *less* precise, not more precise, than they actually were.

according to both of these measures; also, the two measures are somewhat correlated at the individual level.

I find that traders do trade based on private information; traders who get better-than-average forecasts tend to be buyers, while traders who get worse-than-average forecasts tend to be sellers. This is true even after most trades in a period have been concluded. Also, asset buyers' inferred reservation prices depend on their signals to a greater than optimal degree. These findings are consistent with the modeling approach used in overconfidence-based models.

However, I find only mixed evidence that the overuse of private information is caused by *psychological* overconfidence. Traders who are miscalibrated, or who consider themselves "better than average" guessers of the random processes underlying their private signals, are no more likely than others to use their signals to make trading decisions. On the other hand, miscalibrated traders may set their reservation prices more in accordance with their signals than do other traders.

Since I have data on overconfidence and on market outcomes, I revisit the direct link between psychological overconfidence and market outcomes. I find that overconfident traders do not trade more, and actually may earn higher profits than other traders. When evaluating aggregate outcomes across market trading groups, my sample size is very small; however, I observe an apparent correlation between trading volume and the average "better than average effect" of a trading group. However, I find no such relationship between aggregate confidence and either asset overpricing or price volatility.

Thus, I find only mixed evidence to support the link between psychological overconfidence and the kind of market phenomena that overconfidence is generally invoked to explain. Since my overconfidence measures are highly specialized, it may be that other measures work better to explain aggregate outcomes. However, it is clear that psychological overconfidence on signal-extraction tasks does *not* explain the use of private signals that I observe in this experiment.

My findings also have an interesting implication for the "bubble experiment" literature that follows Smith, Suchanek and Williams (1988). Use of private information does not disappear, and actually strengthens, over successive repetitions of the asset market. This is in stark contrast to the behavior of asset bubbles, which typically disappear by the third repetition. By focusing on bubbles, asset pricing experiments may be ignoring a more enduring source of market inefficiency.

Section II reviews the relevant experimental literature, the literature on overconfidence-based theories of asset markets, and some psychological literature on overconfidence. Section III describes the experimental procedures for the market and for the overconfidence tests. Section IV motivates and states the hypotheses linking the experiment to existing models of overconfidence. Section V presents and discusses the results of the experiment. Section VI concludes.

2 Related Literature

2.1 Overconfidence models

Overconfidence models typically attempt to explain one or more of three phenomena: overpricing of financial assets, overtrading (or high volume during periods of overpricing), and excess price volatility.

The first theories of asset markets that could reasonably be called "overconfidence models" are models of heterogeneous beliefs. In the seminal paper of Harrison and Kreps (1978), traders have different beliefs about future asset fundamentals, which do not disappear over time; furthermore, these traders "agree to disagree," disregarding the beliefs of other traders who disagree with them, even though these differing beliefs are known to all. In this model, in which short selling is not possible, asset values exceed even the most optimistic traders' beliefs about the fundamental, since assets also have an option value; as beliefs fluctuate, assets can be sold and resold at a profit. This option value turns out to be a common result in overconfidence models.

Some later models, such as that of Morris (1996), relax the assumption that agents are dogmatic in their differences of opinion, allowing traders to learn over time. Because learning takes time, results similar to that of Harrison and Kreps (1978) occur in the interim.

Another strand of model posited the existence of "noise traders" who do not trade based on fundamentals; the classic model of this type is DeLong, Shleifer, Summers, and Waldmann (1990). These models typically leave noise traders unmodeled, but the modelers conjecture that noise traders might believe in spurious information about fundamentals.

Beginning in the late 1990s, a number of models attempted to unify the idea of heterogeneous beliefs with the psychological literature on overconfidence. These papers modeled overconfidence as a trader's incorrect, persistent belief that the precision of his own private signals about asset fundamentals is higher than it really is. One such model is Odean (1998), in which price-taking traders trade a single risky over a limited time horizon, while receiving private signals about the future payoff of the asset. Although traders in this model can infer the *average* private signal from the market price, they place excessive weight on their own signal, so that they value the asset differently than the market. This generates excessive trading relative to a rational-expectations equilibrium, and results in prices that exceed fundamentals. However, because short selling is allowed, this model does not generate overpricing via an option value; overconfidence causes prices to deviate from fundamental value, but the direction of the deviation is not determined.

A second model along these lines is Scheinkman and Xiong (2003). In this model, traders trade over an infinite horizon, receiving signals in continuous time. Although the signals are public knowledge, each trader thinks of one of the signals is his "own" signal. *All* signals are actually worthless, but each trader believes that his own signal (and no other) is correlated with innovations to the asset's payoff. This model is similar in spirit to Harrison & Kreps (1978), except that instead of stubbornly disagreeing about the process governing asset fundamentals, traders disagree only "agree to disagree" about the informativeness of the various signals. Again, with short selling impossible, option value creates a "bubble,"[2] in which prices exceed any trader's expectation of fundamentals. Also, heterogeneous beliefs lead to excess volume and excess volatility.

Other, similar "behavioral" models of overconfidence proceed along similar lines. Daniel, Hirshleifer, and Subrahmanyam (2001) obtain similar results for a portfolio of many securities. Daniel, Hirshleifer, and Subrahmanyam (1998) model overreaction and underreaction to news about fundamentals as functions of slowly changing overconfidence and self-attribution bias. Gervais and Odean (2001) also model overconfidence that changes due to self-attribution bias. Hirshleifer and Luo (2001) model how overconfident traders can survive in a market filled with traders with differing levels of confidence, without being competed out of the market.

2.2 Psychology literature on overconfidence

Several types of overconfidence have been found in psychology experiments. Two receive special attention in the behavioral economics literature. The first is *miscalibration*, in which people are found to overestimate the precision of their knowledge (Lichtenstein, Fischhoff, and Phillips 1982, Yates 1990). This is typically measured with a confidence interval test: subjects are asked a number of questions, and along with their answers they are asked to give a self-predicted confidence interval (usually a 90% confidence interval). If fewer than 90% of their answers lie within the predicted intervals, subjects are said to be miscalibrated. The second type of overconfidence is the *better-than-average effect*, which documents that most people see themselves as better than average in ability (Taylor and Brown 1988, Svenson 1981). This is usually measured by asking people if they are better than the average at some task, e.g. driving a car. If more than 50% claim to be better than average, it sig-

[2]Note that this model does not generate a runup-and-crash in asset prices; thus, it does not generate a "bubble" in the sense of the experimental literature reviewed here.

nals that overconfidence exists in the population.[3] Many studies find that overconfidence is prevalent among the population; however, on tasks that are easy or repetitive, people may be well-calibrated or even underconfident (Pulford and Colman 1997, Kahneman and Riepe 1998).

2.3 Empirical studies

Odean (1999) examines data on individual investor portfolios provided by a discount brokerage. They find that stocks sold by individual traders tend to subsequently outperform stocks bought by these traders; in other words, individual investors lose money by trading too much. Odean attempts to control for liquidity demands, tax-loss selling, rebalancing, and changes in risk aversion, and finds that once these factors are controlled for, over-trading lowers individual investor returns by an even greater amount. Barber and Odean (2001) find that this over-trading is correlated with gender; men trade more and thus perform worse than women. Since men are often found in psychology experiments to be more overconfident about their abilities than women, this may be evidence in support of the overconfidence hypothesis.

Glaser and Weber (2003) test the relationship between individual investor confidence and trading activity. Using a questionnaire distributed to a large number of investors, they measure miscalibration (using self-reported confidence intervals) and the better-than-average effect (using self-reported evaluations of investing skills). They find that miscalibration is not correlated with trading volume, but that the better-than-average effect is positively correlated with trading volume.

[3] Of course, the fact that an individual considers himself to be better than average does not mean that individual is overconfident; she may simply have good information that she is, in fact, better than average.

2.4 Asset market experiments

The classic asset market experiment is the "bubble experiment" of Smith, Suchanek, and Williams (1988) (henceforth "SSW"). In that experiment, small groups of subjects traded a single short-lived risky asset that paid a stream of dividends. Even though the dividend process was made public to all traders, the outcome was a large bubble, in which the price of the asset diverged strongly from the fundamental value and then crashed at the end of the market. This bubble was accompanied by high trading volume at the peak. When the same group of subjects repeated the market several times, the bubble progressively shrank and eventually disappeared; in other words, the outcome approached a rational expectations equilibrium as subjects became more experienced. This result has become the most widely replicated result in experimental finance; in the next two decades, bubbles were shown to be robust to many changes in market institutions and asset fundamentals (King et al. 1993). Because this market setup is so well-studied, I use it in the present experiment.

The experiment most similar to the one in this paper is the one run by Kirchler and Maciejovsky (2002). These authors use a setup similar to SSW, but with several modifications. First, traders are asked to predict the price of the risky asset in the upcoming period. Second, traders receive signals about the *other* traders' average predictions. Third, traders are asked to give *confidence intervals* about their price predictions. The authors use the confidence intervals to measure overconfidence (miscalibration). They find that traders are generally well-calibrated at the beginning of the experiment, but become more overconfident as they make successive predictions.[4] Interestingly, Kirchler and Maciejovsky find no correlation between overconfidence and trading volume, although they do find that overconfident traders earn less on average. The authors do not test the relationship between overconfidence and use of private information.

Other experiments use market setups quite different from the SSW setup. Deaves,

[4]However, using a second measure of confidence relying on traders' subjective certainty about their predictions, the authors find that subjects are often well-calibrated or even underconfident.

Luders, and Luo (2003) use twelve single-period markets, where endowments are reset at the beginning of each market, and payoffs occur at the end of the market. The authors give traders i.i.d. signals of fundamental value, but deceive their subjects by telling them that the signals' precision is correlated with subjects' performance on a general knowledge questionnaire. They measure overconfidence by asking subjects to give subjective confidence intervals on the questionnaire. The authors find that overconfidence (positive miscalibration) is positively correlated with trading volume. They also find a positive relationship between miscalibration and the "better than average" effect, as measured by subjects' self-predicted rankings of total profits.

Biais et al. (2005) use a trading setup with asymmetric information about asset fundamentals, measuring overconfidence (miscalibration) with a confidence interval task. They find no significant relationship between miscalibration and trading volume, although they find that miscalibrated subjects earn lower profits.

Finally, a very interesting experiment by Hales (2009) investigates the closely related question of whether traders "agree to disagree" about the value of an asset. In his experiment, subjects trade in pairs and receive private signals about asset value. Hales finds that whether traders are prompted to consider the adverse selection problem has a strong effect on whether trade occurs. When traders are asked to guess the difference between their own signal and the signal of the other trader, trade tends not to occur; however, without such prompting, trade does tend to occur. This result suggests that over-reliance on private information is not due to traders "agreeing to disagree," but simply to their failure to consider the fact that others have information that may disagree with their own. Hales calls this effect "myopia"; it is also sometimes called "availability bias."

2.5 Differences from previous literature

The present experiment differs from the existing literature in several ways. First, it directly tests agents' use of private information about fundamentals. Traders in this study receive private signals about asset payouts, of a form very similar to the type of signals in models like Odean (1999). Unlike in previous experiments on overconfidence, this information is both ex ante symmetric, and contains only information about fundamentals.[5]

Second, this experiment is the first to measure overconfidence using a test that is extremely similar to the signal-extraction problem faced by traders in the actual asset market. In the psychology literature, "overconfidence" is measured by many different types of tasks, and there are often large discrepancies between these measures. The present study attempts to control for these possible sources of systematic error by using the same task for the confidence test and the asset market.

Third, this experiment uses a market setup - the "bubble experiment" or SSW setup - that is known to almost always converge to a rational expectations equilibrium by the third market repetition.

Fourth, this study measures confidence using a task that is very unlikely to be subject to overconfidence. The signal-extraction task used here is repetitive and simple, and feedback is swift - all characteristics that are found to be less conducive to overconfidence (Kirchler and Maciejovsky 2002). Thus, any observed overconfidence on this task probably represents a lower bound on the degree to which a subject overestimates her own predictive accuracy and relative performance.

3 Experimental Setup

3.1 Subjects and compensation

The experiment was conducted on four days, between May 29 and June 7, 2012, at the Institute for Social Research at the University of Michigan. Subjects were de-identified, so

[5]Some financial market experiments have established the result that markets aggregate information that is distributed among different traders (Plott and Sunder 1988). However, these experiments have typically not given subjects noisy information, and have typically not examined whether subjects' individual behavior reflects an underestimation of the amount of noise in their own information relative to the noise in others' information.

no individual data is known except that all were over 18 years old. The majority of subjects were probably undergraduate students at the University of Michigan. On each day, there were 12 experimental subjects in the lab, divided into two trading groups of 6 subjects each. Data from six trading groups are excluded from the analysis due to procedural errors by the experimenters, so that there are a total of 36 subjects in the sample.[6] The entire experiment lasted approximately two and a half hours. Ex ante average compensation[7] was $38, including a $5 show-up fee. Realized average compensation over the five days was $35.79. This is in line with the typical per-hour compensation for this type of experiment. Compensation was divided among the preliminary tasks and the financial market itself, as will be explained in the following subsections. Payment for all parts of the experiment was given in cash at the end of the Asset Market portion of the experiment.

3.2 Experimental procedures

The instructions for the experiment are presented in Appendix II.A. The experiment consisted of four parts:

1. A financial literacy test
2. A time-series prediction task
3. A confidence test

[6]One of the 6 excluded groups was excluded because in Round 1, subjects in the group exhibited a strong tendency to trade *against* their forecasts; subjects with better forecasts strongly tended to sell in this round, while subjects with worse forecasts strongly tended to buy. It was thus inferred that this group was subject to an error on the part of the experimenters - either a coding error that reversed the displayed forecasts, or possibly an error by the experimenters when explaining the "bullish" vs. "bearish" terminology. This may at first appear to represent "cherry-picking" of the data on the part of the experimenter. However, inclusion of this group in the sample does not in any way alter the result that, across all groups, traders strongly tended to buy when they received better forecasts and sell when they received worse ones (Hypothesis 1); in fact, the result is strengthened for Round 3. Also, the results on overconfidence are not altered (and the positive results are actually strengthened) by the inclusion of this group. Hence, exclusion of this group on the basis of an inferred experimenter error does not represent cherry-picking of the data to support the experimental hypotheses.

[7]Since some compensation depended on the accuracy of subjects' price predictions during the financial market, the $38 estimate was to some degree a guess on the part of the experimenter, based on trial runs of the experiment.

4. The asset market itself

The purpose of the financial literacy test and time-series prediction task was twofold: A) to measure subjects' basic competence (since nothing was known about the subjects' experience or ability), and B) to induce exogenous variation in group confidence levels. For this reason, the test and the time-series prediction task were administered in two different versions, an "easy" version and a "hard" version (subjects were not told that two versions existed). The differences between these versions will be explained later. Unfortunately, however, the different versions failed to induce measurable differences in confidence, as will be explained later. The version of the preliminary tasks received by each trading group is displayed in Table II.1.

All portions of the experiment were carried out using the z-Tree software package (Fishbacher 2007).

3.3 Financial literacy test

The first portion of the experiment was a five-question financial literacy test. This test was based on the literacy test in Rooij, Lusardi, and Lessie (2011). The "easy" version of the test consisted of the five "basic literacy questions" created by Rooij et al., while the "hard" version of the test consisted of five of the eleven "advanced literacy questions." The text of these questions is given in Appendix II.C. Subjects were given one minute to answer each question, and were told the correct answer after answering each question.

Each subject received $2 for taking the financial literacy test, regardless of score.

3.4 Time-series prediction task

The second portion of the experiment was a "prediction task". Subjects were asked to predict the future value of a time series (labeled "GDP") from past values of the time series, using no other information other than the series' past values. The time series was generated before the experiment by an AR(1) process with Gaussian white noise innovations and a linear time trend. At the beginning of the task, subjects were shown a pre-existing ten-period "history" of the time series, which they used to make their first one-period-ahead prediction. After each prediction, the realized value of "GDP" for the next period was revealed, along with the subject's prediction error; subjects would then make another one-period-ahead prediction, while seeing the initial ten-period history and all subsequent realizations of the time series. Each subject made ten predictions in all. At the end of the task, the subject was told their total prediction error.

The "easy" version of the time series had higher persistence and a smaller variance of the innovation term than the "hard" version; the time trends were the same. The values of the "easy" and "hard" time series are listed in Appendix II.D.

Each subject received $2 for completing the prediction task, regardless of score.

3.5 Confidence test

The confidence test was presented to the subjects as "Prediction Task 2". In this task, subjects were asked to guess whether a pair of "balls" was drawn from "Jar A" or "Jar B". Each ball was identified by its color; these colors could take on five values, red, orange, yellow, green, and blue. Subjects were told the number of balls of each color in Jar A and Jar B, respectively. Each subject made 15 guesses; the number of balls of each color in each jar did not change between guesses, indicating that the draws were i.i.d. The number of balls of each color in each jar are listed in Table II.2.

Subjects were not told whether their guesses were right or wrong. After making 15 guesses, subjects were asked to predict how many correct guesses they had made (out of a possible 15). They were also asked to predict their rank among the 12 subjects in the lab on that day, so that a rank of 1 would indicate that a subject made as many or more correct guesses than any other subject in the lab. Even after making these guesses, subjects were not told how many correct guesses they had made, or their true rank. These two self-predictions were used to generate the two confidence measures used in the experiment. The difference between a subject's guess of his or her number of correct answers and the true number he or she got correct is defined as that subject's "miscalibration" about signal precision, while a subject's predicted ranking defines his or her "better than average effect" (or "BOA").

This task was a simple Bayesian inference task. It was essentially identical to the problem of guessing the true per-period dividend from a private forecast during the asset market (see next subsection), though the probabilities were different. This is important, because individuals may have different levels of confidence for different types of tasks. An individual's confidence on a Bayesian prediction task may be a good or a poor proxy for her confidence on, say, a market-timing task. To my knowledge, this experiment is the first to measure confidence using the same kind of signal-extraction problem that is required of traders in the asset market.

3.6 Asset market

The asset market portion of the experiment closely follows the setup of Smith, Suchanek, and Williams (1988). Each market consisted of a trading group of 6 subjects. The assets traded included one riskless asset ("cash," measured in "yen"), and one risky asset (called simply "the asset," measured in "shares").

Each group of subjects traded over three market *rounds*. The subjects in each group were the same from round to round. At the beginning of each market round, each subject was given an initial endowment of cash and shares. There were three types of endowments, each with identical ex-ante value; in each market, two traders in each group received Type 1 endowments, two received Type 2 endowments, and two received Type 3 endowments; the allocation of endowments was randomized given this constraint.

Each market round included ten two-minute trading *periods*. During a period, subjects could buy and sell the asset from each other. At the end of a period, each share of the asset paid a *dividend* to its holder; this dividend was either 0 yen or 10 yen, with a 50% probability of each. Dividends were the same across all shares within a period, but different from period to period and different across groups. Dividends were i.i.d. At the end of each period, traders were told the realized dividend for that period, and the appropriate dividend income was added to traders' cash accounts[8]. After the final dividend was realized at the end of Period 10 in each round, the asset vanished (no buyout value).

At the end of a market round, subjects received the promise of an amount of cash equal to the amount of cash in their account, to be paid after the end of the experiment. The exchange rate was 1 "yen" = 1¢. Thus, there was no "carry-over" between rounds; whatever a subject earned or did during Round 1 could not affect his or her expected payoff in Round 2 or Round 3.

Trading was done via continuous double auction with an open order book. Each subject was able to enter bids or asks, or to accept the best available bid or ask. In addition, to facilitate rapid trading, any subject could press a button that submitted a bid (ask) that was 1 yen higher (lower) than the highest (lowest) available bid (ask). Shares could only be bought and sold one at a time. During trading, subjects were told the "buy-and-hold value" of their current portfolio of risky asset shares; this was equal to 5 yen multiplied by the number of periods remaining, multiplied by a subject's number of shares. In other words, this "buy-and-hold value" was equal to the ex ante risk-neutral fundamental value of the asset. However, this value did *not* include the information in subject's forecasts (see following subsection).

3.6.1 Forecasts

At the beginning of each period (before the start of trading), each subject received a forecast (noisy signal) of that period's dividend value. There were five possible forecasts: "extremely bullish," "moderately bullish," "neutral," "moderately bearish," and "extremely

[8]Thus, the cash/asset value ratio of the market as a whole increased each period.

bearish." Forecasts were i.i.d. across subjects. The probabilities of receiving the different forecasts were different depending on the different upcoming (but not yet observed) dividend realization; for example, if the dividend was to be 10 yen, the probability of a subject receiving an "extremely bullish" forecast was 35%. Subjects were given a hard copy of a chart telling them these conditional probabilities; guessing the dividend from the forecast was thus a matter of Bayesian inference, identical in form to the guessing task in the earlier Confidence Test. The forecast probability chart can be seen in Appendix II.B.

Henceforth, to avoid confusion, the term "forecasts" will be reserved exclusively for these dividend forecasts, while traders' own predictions of future asset prices (described in the next subsection) will be referred to only as "predictions."

3.6.2 Price predictions

Before trading in each period, subjects were asked to predict the future average trading price of the risky asset. Specifically, they were asked to predict:

1. the price in the upcoming trading period, and
2. the price in the final period of the round (Period 10).

Subjects were paid for making accurate predictions, according to an absolute deviation formula with a minimum of zero. Specifically, for each of the two price predictions in each period, a subject received a payment equal to $\max(10\, yen - |predicted\ value - actual\ value|, 0)$. Thus, perfect predictions in all periods in all three rounds would yield a total payment of 600 yen, or \$6.[9]

As mentioned above, the term "predictions" will be reserved for these price predictions, while the term "forecasts" will be reserved exclusively for the dividend forecasts.

[9]It is theoretically possible for subjects to intentionally make predictions that are not unbiased functions of their actual beliefs, even given this symmetric loss function. This would occur if subjects attempt to make market-timing bets, e.g. betting that the price will go up, but predict a lower price for the purpose of hedging. However, previous studies, such as Haruvy, Lahav, and Noussair (2007) indicate that such behavior is negligible in these experiments.

3.6.3 Control group

One group (Group C) was not shown any forecasts. This group will be referred to as the "control group."

4 Theory and Hypotheses

4.1 Overconfidence measures

In the behavioral finance literature on overconfidence, two psychological phenomena - miscalibration, and the better-than-average effect - are commonly used to motivate the agents' overweighting of private signals. However, it is not clear that either one of these phenomena, *by itself*, is a necessary or sufficient condition for overweighting of private signals. To see why, let's examine the model of Odean (1998), which is most similar to the setup used in the present experiment. In Odean's model, each agent is aware of the existence of multiple pieces of noisy information, including the trader's own private signal of asset payouts, and other traders' private signals. The agent's belief about the precision of each of these signals differs from the true value. The behavioral multiple that an agent assigns to his belief about the precision of his own signal is called κ and is assumed to be greater than or equal to 1, meaning that agents believe that their own signals are more precise than they really are. The multiple that agents assign to the signals of others is called γ, and is both assumed to be less than or equal to 1, meaning that agents believe that other agents' signals are *less* precise than they really are.

However, in this experiment, it cannot be assumed a priori that these sign restrictions hold. Suppose a subject demonstrates miscalibration on a test. It may be that this person overbelieves in the precision not just of her *own* signals, but of *all* signals; in other words, that $\kappa = \gamma > 1$. In the Odean (1998) model, this would cause traders to have common beliefs, which, given Odean's other assumptions about preferences would preclude trade after the first period[10] and reverse many of the results of the model. Similarly, suppose a subject demonstrates that he or she believes him or herself to be better than average. It may be that this person *under*believes in the precision of her own signals, and simply underbelieves even more in the precision of other signals; in other words, $\gamma < \kappa < 1$. This is a general problem with these overconfidence measures, and not specific to the Odean (1998) model, which is used simply to illustrate the point. It is *possible* that either or both of these observable confidence measures is a good proxy for the overweighting of private signals, but this is not *guaranteed*.

Therefore, when testing hypotheses about the effects of overconfidence, I use three measures. The first are miscalibration and the better-than-average (henceforth "BTA") effect, as measured by the two parts of the confidence test described above:

$$MISC_i \equiv \text{self-predicted \# of correct guesses - \# of correct guesses} \tag{1}$$

$$BTA_i \equiv \text{6.5 - self-predicted rank} \tag{2}$$

The third is a dummy variable, D_CONF_i, equal to 1 if a subject both is positively miscalibrated and predicts that she is better than average (and zero otherwise). In other words, this dummy is 1 when a subject is overconfident according to both common psychological measures:

$$D\_CONF_i \equiv \begin{cases} 1 & MISC_i > 0, BTA_i > 0 \\ 0 & otherwise \end{cases} \tag{3}$$

Note that if $MISC_i$ and BTA_i are increasing functions of $\kappa - 1$ and $\kappa - \gamma$ in the Odean (1998) model, respectively, then $D\_CONF_i = 1$ is sufficient to preserve all relevant results of that model (proof omitted); again, this is for illustrative purposes only, since the ex-

[10] Odean (1998) allows for traders to have different endowments; after a point of Pareto optimality has been reached, trade only occurs due to overweighting of the private signals that subsequently arrive.

periment is not an exact test of this or any other existing model. In most of the analysis, I examine only miscalibration and BTA; however, in one or two cases, D_CONF_i yields slightly different results.

4.2 Use of private signals

The first question this paper seeks to answer is: Do traders overweight private signals when they make their trading decisions? This is the mechanism by which models of heterogeneous beliefs generate market inefficiencies. It is also the mechanism by which behavioral models like Odean (1998) and Scheinkman and Xiong (2003) assume psychological overconfidence to operate.

A necessary condition for the interaction of individual overconfidence with private signals is that traders make use of private signals at all. In other words, the dividend forecasts traders receive in the asset market must cause trades that would not otherwise have occurred. Within a trading group within a period, the distribution of forecasts is i.i.d. Thus, if traders do not use their forecasts to decide whether to buy or to sell, the probability of the buyer getting a forecast of 2 (the best possible) and the seller getting a forecast of -2 (the worst) is the same as the probability of the buyer getting a forecast of -2 and the seller getting a forecast of 2.

Hypothesis 1: The average spread between the dividend forecasts of the buyer and seller in concluded transactions will be positive.

Use of private signals to make trading decisions does not necessarily imply that traders *overweight* these private signals. Because traders may have different risk preferences, adverse selection does not necessarily preclude conditioning trading decisions on private signals. However, note that if public information in the market - the price, the history of trades, and the history of orders - reveals the distribution of private forecasts, then trading decisions should not be conditioned on forecasts if rationality is common knowledge. Many experiments, for example Plott and Sunder (1988), have found that experimental markets with a small number of participants are capable of aggregating private signals about asset value. Therefore, the finding that subjects in the present experiment condition their actions on their forecasts will provide stronger support for overweighting of private signals if the finding holds even after a number of trades have taken place. For this reason, I re-test Hypothesis 1 for various restricted samples of "late" trades, as described in Section 5.2.

4.3 Private signals and overconfidence

The second question this paper seeks to answer is whether psychological overconfidence is associated with greater overuse of private information. There are two observables that represent trading decisions: Quantity and price. Therefore I examine how overconfidence and private signals interact to affect buying/selling decisions and the prices at which assets are exchanged.

4.3.1 Trading decisions

Overconfidence theories predict that overconfident traders will tend to buy when they receive higher forecasts of asset payouts, and sell when they receive worse forecasts. Therefore, my second hypothesis is:

Hypothesis 2: Overconfident traders will tend to buy more of the risky asset when their forecasts are better than the forecasts of the other traders in the market.

4.3.2 Pricing

Behavioral theory also predicts that overconfidence leads to greater use of private signals when determining a buyer's *reservation price*. In this experiment, there is no short-selling; hence, each contract price theoretically represents the reservation price of the buyer, who is the most optimistic trader in the market (Scheinkman & Xiong 2003).[11] In this market,

[11]More realistically, some error on the part of the experimental subjects is probably involved; still, it is reasonable to assume that the contract price is a random variable with the upper limit of support at the buyer's reservation price, and is hence is correlated with the unobservable reservation price.

dividends and forecasts are i.i.d.; thus, in any period, the option value of the asset should be the same across traders, and buyers' reservation prices should reflect only differences in preferences or differences of opinion about fundamentals. Thus, theory predicts that forecasts should have a greater effect on the prices of concluded contracts when the buyers of the contracts are more overconfident.

Hypothesis 3: Overconifdent buyers will tend to pay prices than other buyers in the market when their forecasts are better than the forecasts of the other traders in the market.

As with Hypothesis 1, I also test this hypothesis on a restricted sample representing "late" trades. After a number of trades have been concluded, a market price for the asset has been established. If the traders with the better forecasts in a group-period continue to pay higher prices even late in a period, and if this tendency is stronger for overconfident traders, it means that overconfident traders are less sensitive to the market's aggregation of information. Such insensitivity is exactly what behavioral theory predicts.

4.4 Overconfidence and market outcomes

In addition to studying the role of private information and its interaction with overconfidence, this paper also revisits the predictions of behavioral theory about the relationship of overconfidence and market outcomes at the individual and aggregate level.

Behavioral theories predict that overconfident traders trade more frequently than other traders.

Hypothesis 4: The number of trades concluded by a trader over the course of the experiment will be positively correlated with that trader's measured overconfidence.

A number of experimental and empirical studies have found that some measures of overconfidence are correlated with lower earnings in financial markets. This is predicted by some theories, although others predict that overconfident traders earn equal or higher returns and lower expected utility. The hypothesis I test is:

Hypothesis 5: Overconfidence is negatively related to a subject's earnings in the financial market portion of the experiment.

At the aggregate level, overconfidence-based theories generally predict that markets in which a large number of traders are overconfident will exhibit one or more of the following outcomes: higher volume (due to over-trading), increased price volatility (due to fluctuating heterogeneous beliefs), and prices above fundamentals (due to the option value associated with fluctuating heterogeneous beliefs). Thus, using only the small sample of trading groups available, I attempt to test the following hypotheses:

Hypothesis 6: Trading groups that are more overconfident on average will exhibit higher trading volumes.

Hypothesis 7: Trading groups that are more overconfident on average will exhibit greater overpricing of the asset relative to fundamentals.

Hypothesis 8: Trading groups that are more overconfident on average will exhibit greater within-period price volatility.

The experimental setup used in this experiment is believed to converge as close as possible to a rational expectations equilibrium by the third market round. Therefore, when possible, I test these hypotheses both for the market as a whole and for the third market round only.

5 Results and Discussion

5.1 Description of the data

5.1.1 Overconfidence

On the type of Bayesian inference task used for the confidence test, answers are either correct or incorrect; thus, subjective confidence intervals cannot be given for each answer. However, an equivalent measure of miscalibration is a subject's own estimation of how many guesses he or she got right. If the subject's subjective confidence intervals for his or her guesses are too small on average, she will tend to predict that she got more questions correct than she actually did.[12]

On average, subjects are miscalibrated. Define $CORRECT_i$ as the number of times (out of a possible 15) that subject i guessed the correct "jar" in the confidence test. Define $SELFPRED_i$ as that subject's guess for his or her total number of correct guesses. Then define $MISC_i$ as the difference of the two:

$$MISC_i \equiv SELFPRED_i - CORRECT_i \tag{4}$$

The average value of $MISC_i$ is 1.72, with a standard deviation of 2.68. A total of 26 out of 36 subjects are positively miscalibrated by this measure, while 6 are negatively miscalibrated and the remaining 4 are qwell-calibrated. A Wilcoxon sign-rank test rejects the null of $MISC_i = 0$ with a p-value of 0.0003. Hence, on average, subjects are positively miscalibrated.

On average, subjects also predict that they are better than average guessers. Define BTA_i ("better than average") as the difference between 6.5, the correct ex ante average rank of all subjects (1=best, 12=worst), and the rank that a subject predicts for his or her own guessing accuracy (out of all subjects present in the lab). The average value of BTA_i is 1.39 (standard deviation of 1.74). A Wilcoxon sign-rank test rejects the null of $BTA_i = 0$ with a p-value of 0.0001.[13] Thus, on average, subjects suffer from the "better than average effect."

There is some evidence that the two measures of overconfidence are correlated across individuals. An OLS regression of $MISC_i$ on BTA_i with a sample size of 36 fails to reject the null of no relationship (p-value 0.184). However, there are an additional 6 subjects

[12] An alternative measure of miscalibration would be to compare a subject's predicted number of correct answers with the ex ante expected number of correct guesses made by a guesser who made the proper inference every time. However, this would not take into account the subject's knowledge of his or her own ability to make correct inferences.

[13] It is possible that subjects incorrectly imagined the ex ante average rank as 6 rather than 6.5. Define BTA_i' as 6 minus a subject's predicted rank. A test of the null of $BTA_i' = 0$ rejects the null with a p-value of 0.002.

who successfully completed the confidence test even though their asset market data was excluded due to procedural error. When these additional subjects are included in the sample, a regression of $MISC_i$ on BTA_i yields a p-value of 0.085, showing that the two measures are probably weakly correlated (the correlation coefficient is 0.269).

22 of the 36 subjects are overconfident by both measures ($D\_CONF_i = 1$), and 24 out of 42 in the expanded sample. A two-sided test cannot reject the null that $D\_CONF_i = 0.5$ (p-value 0.182); in other words, there is not sufficient evidence to conclude that subjects, on average, suffer from *both* miscalibration and the better-than-average effect.

Because this task is repetitive and simple, the psychology literature predicts that subjects will be less overconfident on this task than on other tasks. Hence, it is reasonable to assume that subjects will tend to be even *more* overconfident on other tasks - for example, market timing, assessing the rationality of other traders, and understanding the experimental setup itself.

5.1.2 Bubbles

The prices in these markets clearly display the classic "bubble" result. Figure II.1 displays price histories for the three market rounds, along with the risk-neutral fundamental value (unconditional on forecasts), and the maximum possible dividend value. Prices are measured as the difference between the realized contract price per share and the risk-neutral fundamental value, which in turn is defined as the expected total dividend payment per share.[14] The general pattern of decreasing overpricing over successive market rounds is clearly visible. Stockl et al. (2010) suggest a measure of overpricing called "Relative Deviation," defined by the following:

$$RD_m \equiv \frac{10\sum_t P_{m,t} - FV_{m,t}}{\sum_t FV_{m,t}} \tag{5}$$

[14]When forecasts are visible, i.e. for all groups except C, the expected dividend payment includes the true dividend value (0 or 10) for the current period rather than the expected value (5). This assumes that forecasts, in aggregate, are perfectly revealing of the upcoming dividend. In fact, this is an approximation, but only a slight one.

Here, $P_{m,t}$ is the average realized contract price in period t of market m, and $FV_{m,t}$ is the risk-neutral fundamental value. The Relative Deviation for all trading groups in this experiment decreases from Round 1 to Round 2, and again from Round 2 to Round 3. The Relative Deviations for each market round can be seen in Table II.3. Furthermore, in Round 3, the Relative Deviation is very small or negative for all groups except one, indicating that bubbles generally disappear by the third trading round. These facts are all in agreement with the rest of the bubble experiment literature.

For this reason, Round 3 of these markets probably presents the closest approximation to a Rational Expectations Equilibrium achievable in an experimental setting.

It is clear from theory that the bubbles observed in many of these markets, especially Round 1 markets, cannot possibly be explained by overconfidence about the dividend forecasts alone - or, in fact, about any signals of fundamental value that are actually present in this experiment. This is because in many of these markets, price exceeds the maximum possible dividend payout.

Proposition 1 *Overconfidence about signals of fundamental value alone cannot, as the only departure from rationality, lead to market prices exceeding the maximum dividend payout.*

Proof. Suppose that all traders believe their private information to be perfectly revealing of the current-period dividend in every period. Also, suppose each trader expects her private information to be fully revealing of dividend value in all future periods. Furthermore, suppose that in every period, each trader believes that some other trader will receive an incorrect signal of fundamental value, and will believe this signal with certainty. In the final period, this situation will lead a trader to expect to be able to complete an arbitrage trade with a value equal to the difference between the maximum and minimum possible dividend realizations - i.e., 10 yen. By backwards induction, this adds an option value of 10 yen to the asset's value to any trader in the previous period. But in the previous period, a similar arbitrage trade will also be available. Thus, the maximum that any trader is willing to pay for the asset in a given period is given by the maximum possible dividend (10 yen) plus 10 x the number of subsequent periods. This is equal to 10 x the total number of periods remaining, which is also equal to the maximum possible dividend payout. ■

Thus, the observed bubbles require the presence of some other wedge between price and fundamental value, in addition to overconfidence. This could be provided by subjects' initial misunderstanding of the parameters of the experiment, as suggested by Lei, Noussair, and Plott (2001), or by speculative behavior, as suggested by Smith, Suchanek, and Williams (1988).

5.2 Use of private signals

Define F_{it} as the forecast received by trader i in period t. Recall that there are five possible forecasts, ranging from -2 to 2. Now define ΔF_{cgt} as the difference in forecasts between buyer i and seller j in group g who conclude contract number c in trading period t, so that $\Delta F_{cgt} = F_{it} - F_{jt}$. To test whether forecasts are used in trading decisions, I use a Wilcoxon sign-rank test of the null that ΔF_{cgt} in groups in which forecasts are available. The unit of observation is the *completed contract* - i.e., the actual exchange of one share of the asset. c is the index of the contract within a period (for example, the 8th share exchanged in Market 3, period 2 within trading group A is assigned $c = 8$ and $g = 1$).

The test rejects the null in the positive direction with a p-value of 0.000. Restricting the sample only to Round 3 - where learning is expected to be complete - the test again rejects the null, with a p-value of 0.0002. The average difference between buyer and seller forecasts in Round 3 is 0.357 forecast categories, which is equal to the average ΔF_{cgt} for all rounds combined. Thus, even in the third round, traders are using the dividend forecasts to decide whether to buy or sell the asset; learning, which eliminates bubbles, does not eliminate this over-reliance on private signals. Note that this result does not show that *all* subjects use forecasts to make their decisions, only that some do.

For comparison, a test of the same hypothesis for the control group, for which forecasts were invisible[15], fails to reject the null of $\Delta F_{cgt} = 0$ (p-value of 0.509).

A small robustness check in order. First, since the trading system used in the experiment only allows traders to trade asset shares one at a time, it is possible to view multiple transactions between a buyer and a seller during a single period as one single transaction. Therefore, for this test, one observation is equal to the average of all contracts c in period t between a specific buyer i and a specific seller j (to be rigorous, the difference between forecasts should now be called ΔF_{ijt}). Testing the hypothesis $\Delta F_{ijt} = 0$ across buyer-seller pairs yields identical results to the test above; traders with better forecasts tend to buy from traders with worse forecasts.

Next, I test whether forecasts are used in "late" trades. Since there is no theory about how long it takes markets like this to aggregate private signals, I test the hypothesis for subsets of trades representing several alternative definitions of "late," including: A) the final 1/3 of trades in each period,[16] B) the final 10% of trades in each period, and 3) trades that occur after 6 or more trades have already been concluded.[17] The results of these tests can be seen in Table II.4. All of them reject the null; even in late trades, subjects with better forecasts tend to buy, and subjects with worse forecasts tend to sell. This remains true when the sample is restricted to Round 3.

This result shows, in a very stark setting, that injecting private information - "news" - into an asset market induces trade, even when the signals are ex ante symmetric. What's more, the use of private signals persists even after bubbles are gone, and even after most trading in a period is finished. The result is consistent with theories in which heterogeneous beliefs are induced by excessive belief in the precision of private signals. However, it does not necessarily support the link between psychological overconfidence traits and overestimation of private signal precision. The following subsections deal with that question.

[15] Forecasts for the control group were still *generated* by the same random process, behind the scenes; they were simply not *shown* to the traders.

[16] For periods with fewer than 3 trades, this set is empty.

[17] The number 6 is arbitrarily chosen to equal the number of private signals present in each trading group in each period.

5.3 Overconfidence and private signals

5.3.1 Trading decisions

To test Hypothesis 2, I use data on subjects' trading activity. The dependent variable is $NETBUY_{it}$, the net asset purchase of trader i in period t. The key regressor is the interaction between the buyer's confidence and the buyer's "differential forecast" $\widetilde{F_{it}}$. By "differential forecast," I mean the difference between the forecast of trader i in period t and the average of the forecasts of all other traders in the same group in the same period. To define this variable, let F_{igt} be the (raw) forecast of subject i (who happens to be in group g) at time t, and then define $\widetilde{F_{it}}$ as:

$$\widetilde{F_{it}} = F_{igt} - \frac{1}{5}\sum_{j \neq i} F_{jgt} \tag{6}$$

I use the differential forecast instead of the raw forecast for two reasons. The first is that the group-period's average forecast contains information about the dividend itself; dividends may be bunched at the beginning or end of a market round, so that using the raw forecast will introduce spurious correlations because of the presence of bubbles. The second reason is that the differential forecast essentially represents noise.[18] Thus, if overconfident traders use differential forecasts more than other traders when deciding whether to buy, sell, or hold, it demonstrates that overconfident traders are more sensitive to noise.

Here are the regression equations:

$$NETBUY_{it} = \alpha + \beta_1 MISC_i + \beta_2 \widetilde{F_{it}} + \beta_3 (MISC_i \times F_{it}) + \varepsilon_{it} \tag{7}$$

$$NETBUY_{it} = \alpha + \beta_4 BTA_i + \beta_5 \widetilde{F_{it}} + \beta_6 (BTA_i \times F_{it}) + \varepsilon_{it} \tag{8}$$

[18] This is not quite the case. Because forecasts are discretized, the average forecast is not a sufficient statistic for the expected dividend value conditional on all six forecasts in a group-period; however, it is very close.

Results for these regressions are in Table II.5. Standard errors are clustered at the group-period level.[19] Forecasts have a strongly significant effect on trading decisions; this confirms the result in the previous subsection. Again, the result holds even when the sample is restricted to Round 3. However, neither β_3 nor β_6 is significant. Although traders in general buy more when their forecasts are good and sell more when their forecasts are bad, there is no evidence that overconfident traders do this more so.

This result is quite robust to the inclusion (as control variables) of the cash and asset shares in the subject's portfolio at the beginning of the period. It is also robust to the subtraction of individual fixed-effects in net buying, and to the scaling of net buying by total trading activity within a period (i.e. using the fraction of a period's net asset purchases rather than the level of purchases). These results are not shown.

The result does not support the theory that overconfidence is responsible for over-weighting of private signals.

5.3.2 Pricing

To test Hypothesis 3, I regress the price paid by a buyer with the interaction of the buyer's confidence and her forecast. $BMISC_{cg}$ and $BBTA_{cg}$ are the miscalibration and BTA effect, respectively, of the buyer in completed contract c in trading group g in period t. The unit of observation is again the completed contract, as in Section 5.2.

For this regression I again use the differential rather than the raw forecast. Since all forecasts within a group-period are correlated with the upcoming dividend (and, hence, with each other), forecasts should affect prices even in a rational expectations equilibrium. However, if the traders within a group-period who get better forecasts pay higher prices, it indicates the presence of heterogeneous beliefs that persist even after trade.

Because prices vary from period to period depending on any number of factors (e.g. bubbles), for this regression I use the differential price of a contract $\widetilde{P}_{cgt}$. This is defined as

[19]This clustering is due to the fact that the decision of a trader to buy is correlated with the decision of another trader in the same group to sell.

the difference between the price paid for one share of the asset in contract c in group g at time t, minus the average price for all other concluded trades in the same trading group in the same period:

$$\widetilde{P}_{cgt} = P_{cgt} - \frac{1}{NC_{gt} - 1} \sum_{c' \neq c} P_{c'gt} \tag{9}$$

Here, NC_{gt} is the number of contracts in the group-period.

The explanatory variables are the confidence measures, $BMISC_{cgt}$ and $BBTA_{cgt}$, and the differential forecast of the buyer, $\widetilde{BF}_{cgt}$. To define the latter, let subject i be the buyer in completed contract c in group g at time t, and BF_{igt} be the (raw) forecast of this subject in this period. $\widetilde{BF}_{cgt}$ is defined as:

$$\widetilde{BF}_{cgt} = BF_{igt} - \frac{1}{5} \sum_{j \neq i} F_{jgt} \tag{10}$$

The two regression equations are:

$$\widetilde{P}_{cgt} = \alpha + \beta_1 BMISC_{cg} + \beta_2 \widetilde{BF}_{cgt} + \beta_3 \left(BMISC_{cgt} \times \widetilde{BF}_{cgt} \right) + \varepsilon_{ct} \tag{11}$$

$$\widetilde{P}_{cgt} = \alpha + \beta_4 BBTA_{cg} + \beta_5 \widetilde{BF}_{cgt} + \beta_6 \left(BBTA_{cgt} \times \widetilde{BF}_{cgt} \right) + \varepsilon_{ct} \tag{12}$$

Again, one contract is used as one observation.[20]

Regression results are in Table II.6. Standard errors are now clustered at the subject level. All results are robust to the inclusion of subjects' beginning-of-period portfolios as controls, to the subtraction of individual-level fixed effects, and to the averaging of contracts over buyer-periods (results not shown).

First, the results for miscalibration. The coefficient β_3 is significant at the 10% level. The point estimate is 0.317, meaning that a subject who is miscalibrated by 1 unit is ex-

[20] Again, the results are robust to the alternative specification of one observation as the average across all contracts between a specific buyer-seller pair within a group-period.

pected to pay a price premium of 0.317 yen relative to the other buyers in the group when her forecast is 1 level better (one forecast level corresponds to a difference of either 2.5 yen or 1.25 yen in expected dividend payout). Given that 1 unit of miscalibration is less than one standard deviation of miscalibration (which is 1.72 units), this is a large point estimate for β_3. Note that β_2 is not significant here; well-calibrated buyers do not tend to pay higher prices when their forecasts are higher, at least when averaged over all three market rounds.

However, when the sample is restricted to Round 3 only, β_3 becomes *negative* (p-value 0.10), and β_2 becomes significant (p-value 0.009). In the final round, when learning is assumed to have occurred, it is no longer only miscalibrated buyers who pay higher prices when they get higher forecasts than the average; it is now *all* buyers, with miscalibrated buyers actually paying somewhat *less* when they get an above-average forecast.

What this means is that well-calibrated subjects are "learning" to pay attention to forecasts when deciding how much to pay for the asset. This is a very interesting result, because it represents *anti-learning*. Recall that when forecasts are different from the group average, they are more likely than not to be *incorrect* forecasts. That well-calibrated traders make this mistake in increasing amounts as the market progresses is very interesting, because it runs exactly counter to the notion that subjects and markets become more rational as the experiment progresses. It shows that while bubbles are an ephemeral phenomenon in laboratory asset markets, over-reliance on private signals can persist and even increase over time.

The estimate for β_6 is not significant; BTA buyers do not pay more than other buyers when they get better forecasts. Oddly, β_4 is negative and significant; buyers who suffer from the BTA effect pay *lower* prices, on average, than other traders. Why? One hypothesis is that BTA subjects are more sensitive to signals of a bubble. However, when I regress price on the interaction of the buyer's BTA level and the true overpricing of the asset, the interaction is not significant, and the main effect remains. Thus, the tendency of BTA subjects to pay lower prices, regardless of dividend forecasts or bubbles, remains a puzzle.

When the sample is restricted to "late" trades, the coefficient β_3 - the interaction of overconfidence and forecasts - remains significant. When "late" trades are defined as the final third of trades in a period, the point estimate of β_3 rises to 0.607, with a p-value of 0.077. When "late" trades are defined as trades that take place after 6 or more trades have been completed, the point estimate of β_3 rises to 0.642, with a p-value of 0.032. Thus, if anything, miscalibrated buyers rely on their forecasts *more* in late trading than in early trading. This is the strongest evidence I find that overconfident traders are less sensitive than other traders to the information present in the market price, offer history, and history of trades.

To sum up, I find strong evidence that traders use private signals of fundamentals when they make their trading decisions, exactly in accordance with overconfidence-based behavioral theories like Odean (1998) and Scheinkman and Xiong (2003). However, I find only mixed evidence of a link between reliance on private signals and measured overconfidence. When their forecasts are better than the market average, overconfident subjects are no more likely than other subjects to buy; however, if they do buy, they tend to pay higher prices than better-calibrated traders in the first two iterations of the market.

I now examine whether the predictions of the behavioral literature hold true with regards to the effect of overconfidence on individual-level and market-level outcomes.

5.4 Effects of individual overconfidence

5.4.1 Trading frequency

Define $NUMTRADES_i$ as the number of trades made by subject i. To determine if overconfident subjects trade more, I regress $NUMTRADES_i$ on $MISC_i$ and BTA_i. The results are in Table II.7. There is no observable correlation. This is also the case when the sample is restricted to Round 3. In other words, there is no evidence that overconfident subjects trade more than other subjects. This is consistent with the findings of Kirchler & Maciejovsky (2002) and Biais et al. (2005), though it contradicts the findings of Deaves, Luders, and Luo

(2003). The lack of correlation between miscalibration and trading frequency agrees with the empirical study of Glaser and Weber (2003), though those authors do find a relationship between the BTA effect and trading frequency, which I do not.

5.4.2 Performance

Some behavioral theories predict (and some empirical papers argue, e.g. Barber & Odean 2001) that overconfident traders perform worse than other traders. Regressing a subject's total profit Π_i on miscalibration and BTA, I find no relationship between confidence and total profit. However, I find a *positive* relationship between D_CONF_i and profit. Looking at profit earned in Round 3 only, I find no relationship between profit and miscalibration, a positive relationship between profit and the BTA effect, and no relationship between D_CONF_i and profit. These results are in Table II.7.

Some models of overconfidence predict that overconfident traders have expected returns equal to or greater than other traders, but lower expected utility, driven by a greater variance in profit. To test this, I define $\Pi SQ_i \equiv (\Pi_i - \overline{\Pi})^2$, where $\overline{\Pi}$ is the average profit across all subjects, and regress this on confidence. However, I find no relationship; overconfident traders do not have a detectably higher variance in their earnings that would lead a risk-averse individual to have lower expected utility from trading (results not shown).

Other experiments, including Kirchler & Maciejovsky (2002) and Biais et al. (2005), found a negative correlation between miscalibration and earnings. However, those studies measured miscalibration on general tasks, while the present experiment measured miscalibration only on a very specific and simple task related to the type of private signals provided in the experiment. Hence, the lack of correlation between miscalibration and profits should not be construed as a contradiction of those studies' findings.

5.5 Effects of average overconfidence

There are only six trading groups in this experiment. This is a very small sample. Hence, the ability of this data set to evaluate the effects of overconfidence on market outcomes is very limited; we should not expect too much action at the aggregate level. However, there is one interesting pattern in the aggregate data.

5.5.1 Volume

Regressing average per-period trading volume on average overconfidence at the trading group level, I find no relationship between average miscalibration and trading volume, but I find a significant positive relationship (p-value of 0.058) between group average BTA effect and volume. The results are in Table II.8. The point estimate is large; an average BTA effect of 1 rank is associated with an increase in volume of 8.04 trades per period, equal to almost two standard deviations. The R-squared of the regression is 0.634. The relationship can be seen visually in Figure II.2. If the sample were larger, this would be clear evidence that average overconfidence increases trading volume; as it is, the sample is very small and this result should be taken with a grain of salt. Still, if true, this would provide support for the link between trading volume and psychological overconfidence as measured by the BTA effect, found by Glaser and Weber (2003).

Why would there be a relationship between overconfidence and volume at the aggregate level, but not between overconfidence and trading frequency at the individual level? The answer is not clear, but it might be a function of higher-order beliefs. Non-overconfident traders may not assume (as in most models) that other traders are more confident than themselves, and hence may be more sensitive to adverse selection when dealing with overconfident traders, stifling trade.

Alternatively, this result may be a spurious one, given the small sample. When only volume in Round 3 is considered, the relationship between volume and group BTA becomes insignificant (p-value of 0.120). Therefore, this result is merely suggestive, not conclusive, evidence in favor of overconfidence-based theories.

5.5.2 Overpricing and Volatility

I define group overpricing B_g to be the difference between contract price and the asset's risk-neutral fundamental value, conditional on perfect knowledge of the current period's dividend (as justified in the earlier subsection on bubble measurement), averaged across all contracts concluded within a trading group. I define price volatility SDP_g as the within-period standard deviation of contract prices, averaged across all contacts concluded within a group. I find no relationship between these aggregate quantities and overconfidence of either type. These results can be seen in Table II.8. Since the sample is very small, this negative result probably does not reveal much.

6 Conclusion

The results of this experiment are consistent with models of financial markets in which traders rely excessively on their private signals, relative to the information in prices and the information implicit in other traders' willingness to trade. Adding ex ante symmetric signals about fundamentals to a common asset market experiment resulted in trade, in which those who received positive signals bought more and those who received negative signals sold more. Signals also influenced the reservation prices of asset buyers in the manner predicted by behavioral theory.

Therefore, traders in this market were trading on noise. This does not *confirm* that traders over-believed in the precision of the dividend forecasts. Noise trading can occur if A) the market fails to aggregate private signals, and B) some traders are substantially risk-averse or loss-averse. It can also occur if there is not common knowledge of rationality. However, there are reasons to believe that these other reasons are not sufficient to explain the reliance on private signals. First of all, reliance on forecasts persisted even at the end of trading periods, after a number of trades and offers had been observed. It also persisted even in Round 3, in which SSW-type markets typically converge to outcomes consistent with rationality. The evidence seems to favor the explanation that traders relied on their private signals more than was perfectly rational.

However, the link between overconfidence and reliance on private signals was not clear. There was some evidence suggesting a link. Miscalibrated buyers paid prices that were more sensitive to private signals than those of their well-calibrated counterparts, even in "late" trades. Also, groups where the average trader subscribed to the "better than average effect" tended to have higher trading volume. However, the former result had relatively weak statistical significance, with hypothesis tests typically rejecting the null only at the 5% of 10% confidence level, and so should be replicated before it is regarded as established; meanwhile, the latter result needs a larger sample size of trading groups before it can be viewed as definitive.

Meanwhile, the rest of the hypotheses of overconfidence-based models were not supported. Overconfident traders did not rely more on their forecasts to decide whether to buy or sell. They did not trade more frequently than other traders. They did not earn lower profits, and may in fact have made *higher* profits.

These findings do not debunk the notion that overconfidence leads to over-trading, overpricing, or excess volatility. The first reason for this is that this experiment was very protective of the null hypothesis, since it involved a simple task on which humans are typically found to be less overconfident. Real-world markets are much more complex; hence, we should expect people to be more overconfident in the real world. Second, overconfidence can be measured in many ways, and there is no clear consensus as to what sort of psychological test best relates to financial market behavior. It very well may be that the typical findings of miscalibration and the "better than average" effect do not precisely correspond to the type of overconfidence that causes people to over-rely on private information. Third, the behavioral theories whose predictions informed this experiment may not model overconfidence correctly; for example, overconfident people may make false inferences from public information, e.g. seeing false patterns in random-walk data.

But the findings in this experiment do show is that reliance on private signals is not completely driven by overconfidence.[21] In other words, subjects in this experiment traded as if the noise in their signals represented real information, but the degrees to which they did this was not related to how good they thought they were at separating signal from noise.

There are other potential reasons for overweighting of private information. One of these is myopia or availability bias, as conjectured by Hales (2009). Overconfidence tests, such as the one used in this experiment, deal only with an agent extracting information from her *own* signals. But even if she does this perfectly, bounded rationality or higher-order uncertainty may stop her from properly extracting the information implicit in the actions of *others*. She may not know if other agents are rational, or how overconfident they are. She may find it cognitively difficult to make inferences from their actions. Or she may simply be attentionally limited, concentrating only on what she is doing and not monitoring the offers and trades being concluded by other agents. Any of these things would tend to cause the reliance on private signals observed in this experiment.

The findings of this experiment indicates that economists and psychologists should continue the search for psychological measures that predict financial market behavior. But they also indicate that modelers of financial markets need not rely too heavily on the psychology literature to motivate the idea that traders overuse private information.

The results presented here also have important implications for the asset pricing experiment literature. Experimental results like that of Plott and Sunder (1988) have found that markets aggregate information; however, the finding that traders treat noise as information means that experimental markets may aggregate *mis*information in fairly predictable ways. Also, the "bubble experiment" literature begun by Smith, Suchanek, and Williams (1988) has found that simple asset markets of the type used here tend to converge to something

[21] Or, more precisely, not driven by overconfidence on the type of task involved in extracting information from the signals.

resembling a rational expectations equilibrium after three market rounds, and some have concluded that once agents learn how the market works, they behave rationally. However, in this experiment, traders' reliance on private signals not only persisted, but increased as the experiment progressed. This indicates that noise trading may be a much more persistent source of inefficiency in experimental asset markets than the dramatic bubbles that usually characterize the beginning of these experiments.

Tables and Figures

Figure II.1a: Average Market Prices in Round 1

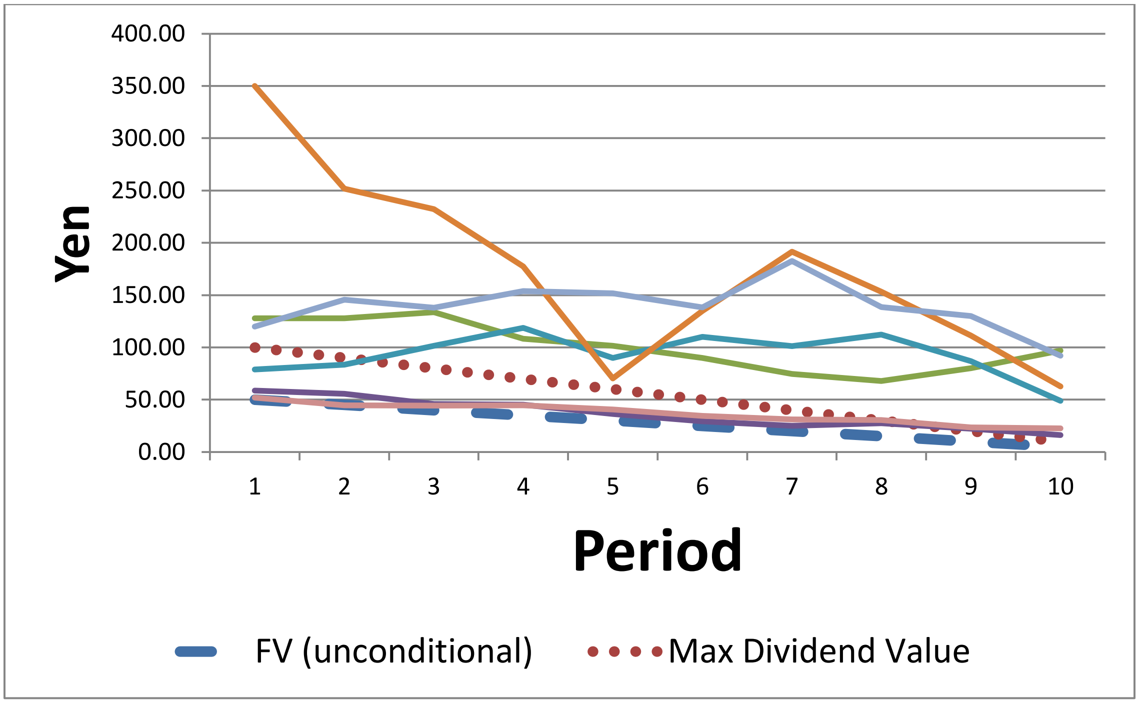


Figure II.1b: Average Market Prices in Round 2

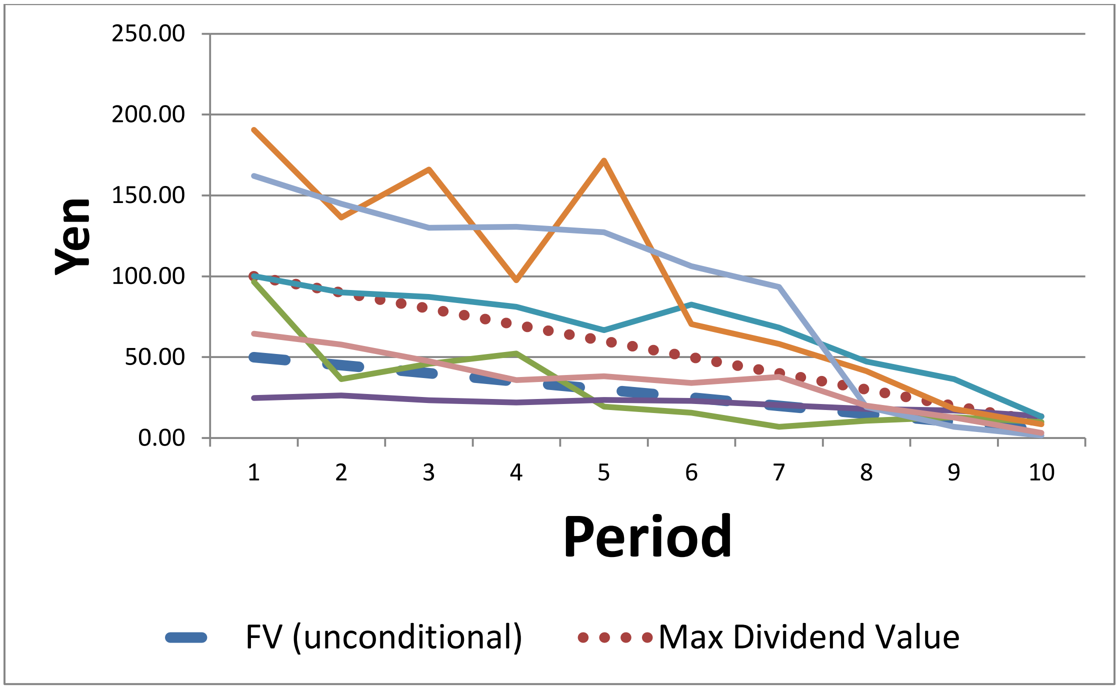


Figure II.1c: Average Market Prices in Round 3

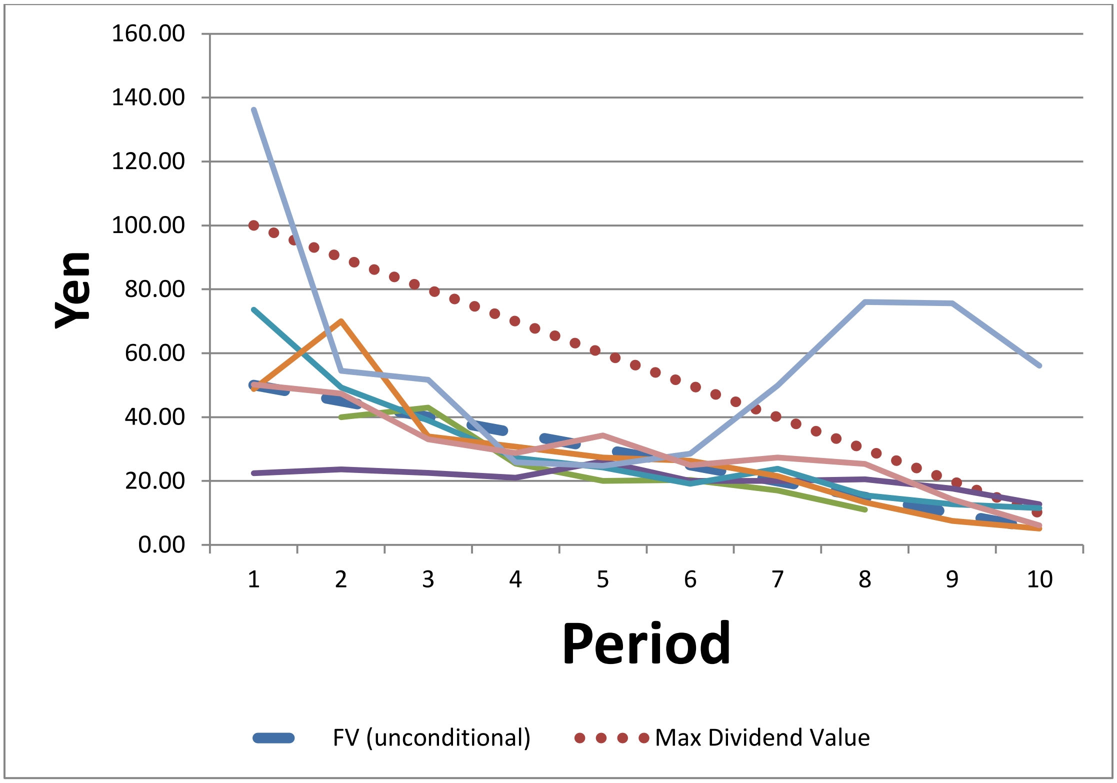


Figure II.2a: Volume vs. Better Than Average Effect

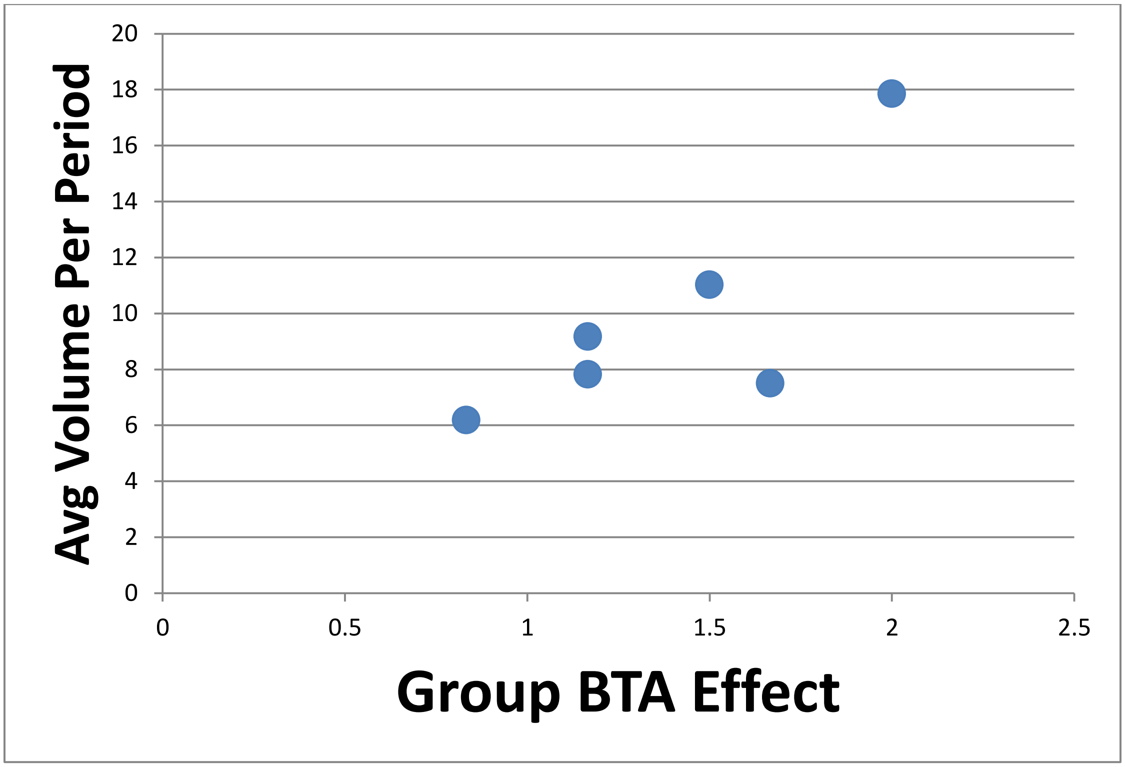


Figure II.2b: Volume vs. Miscalibration

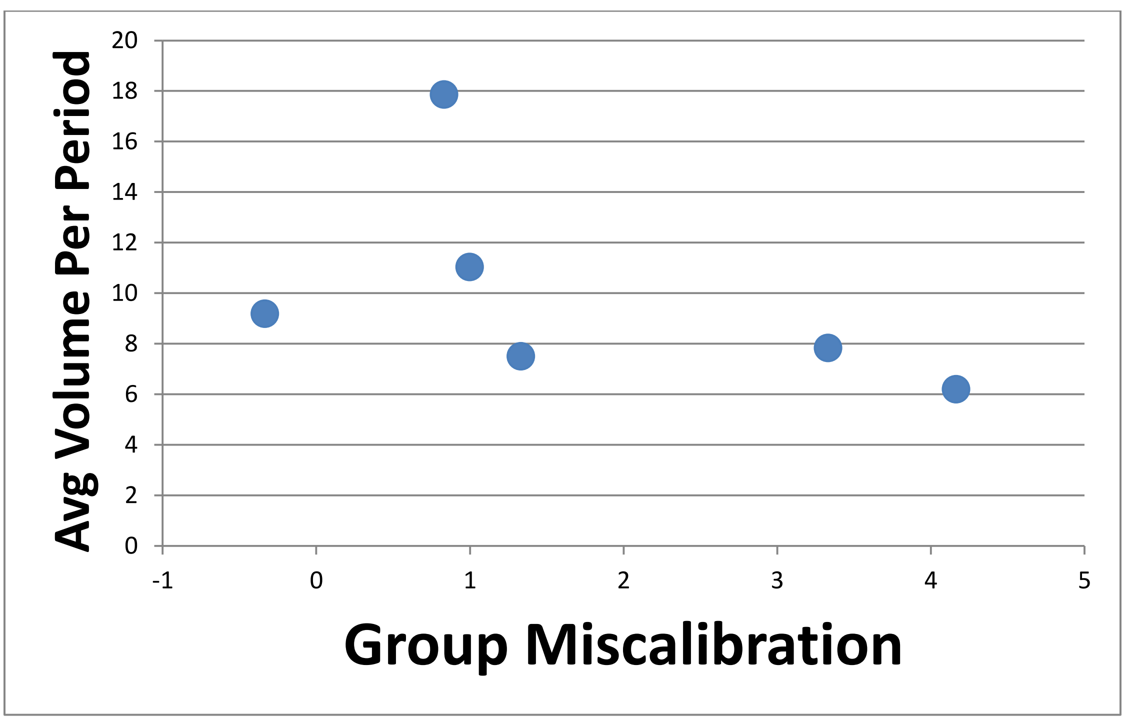


Table II.1: Groups

Group	Forecasts	Preliminary Tasks	Average Miscalibration	Average BTA Effect
C	No	hard	-0.33	1.17
P	Yes	hard	1.33	1.67
Q	Yes	easy	3.33	1.17
R	Yes	hard	4.17	0.83
S	Yes	easy	1.00	1.50
T	Yes	hard	0.83	2.00

Table II.2: Confidence Test

Jar	Color	# of Balls
A	Red	15
	Orange	18
	Yellow	20
	Green	22
	Blue	25
B	Red	25
	Orange	22
	Yellow	20
	Green	18
	Blue	15

Table II.3: Bubble Sizes

	Relative Deviation		
Group	Round 1	Round 2	Round 3
C	26.67	1.16	-1.24
P	3.2	-2.3	-2.48
Q	23.88	14.47	0.77
R	53.11	24.86	0.35
S	40.56	23.53	11.07
T	3.4	2.79	-0.12

Table II.4: Tests for Buyer-Seller Forecast Spreads

Sample	#obs	p-value for H0: $\Delta F_{cgt}=0$
All trades with forecasts available	1266	0.000
Round 3 only	398	0.000
Final 1/3 of trades	467	0.000
Final 10% of trades	197	0.003
After 6th trade	490	0.008
Round 3, final 1/3 of trades	157	0.004
Round 3, final 10% of trades	73	0.012

Table II.5: Forecasts and Trading Decisions

LHS: $NETBUY_{it}$

	All Rounds		Round 3	
Regressor	Coefficient	p-value	Coefficient	p-value
$MISC_i$	0.02	0.37	0.05	0.11
F_{it}	0.35***	0.00	0.28*	0.06
$MISC_i$ x F_{it}	-0.02	0.21	0.00	0.90
	R-squared: 0.035		R-squared: 0.034	
BTA_i	0.00	0.93	0.05	0.61
F_{it}	0.34***	0.00	0.31*	0.06
BTA_i x F_{it}	-0.02	0.51	-0.01	0.90
	R-squared: 0.031		R-squared: 0.030	
	#obs: 1080		#obs: 360	

Note: Forecasts demeaned at the group-period level.

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table II.6: Forecasts and Prices

LHS: $PRICE_{it}$								
	All Rounds		**Round 3**		**Last 1/3 of trades**		**After 6th trade**	
RHS	Coeff.	P	Coeff.	p	Coeff.	p	Coeff.	p
$BMISC_i$	-0.57	0.32	0.32	0.66	-1.61**	0.03	-0.86	0.20
F_{it}	0.32	0.52	2.58***	0.01	-0.32	0.77	-0.53	0.46
$BMISC_i$ x F_{it}	0.35*	0.07	-0.56*	0.10	0.61*	0.08	0.64**	0.03
	R^2: 0.011		R^2: 0.023		R^2: 0.045		R^2: 0.025	
$BBTA_i$	-1.38*	0.07	-1.05	0.17	-0.93	0.30	-1.61	0.13
F_{it}	0.94**	0.02	1.78**	0.03	1.06	0.13	0.01	0.98
$BBTA_i$ x F_{it}	-0.05	0.82	-0.19	0.56	-0.21	0.61	0.10	0.77
	R^2: 0.017		R^2: 0.026		R^2: 0.012		R^2: 0.026	
	#obs: 1262		#obs: 396		#obs: 463		#obs: 490	

Note: Prices and forecasts demeaned at the group-period level.
Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table II.7: Individual Outcomes

		All Rounds		Round 3	
LHS	Regressor	Coefficient	p-value	Coefficient	p-value
$NUMTRADES_i$	$MISC_i$	-0.08	0.50	0.01	0.87
	BTA_i	-0.05	0.77	-0.05	0.41
Π_i	$MISC_i$	40.67	0.53	-9.69	0.61
	BTA_i	93.29	0.34	55.93**	0.06
	D_CONF_i	608.37**	0.07	-13.34	0.90
		#obs: 36		#obs: 36	

Note: Adjusted R-squared values are negative for all regressions in this table.
Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table II.8: Group Outcomes

LHS	Regressor	Coefficient	p-value	Adj. R-squared
VOL_g	$MISC_g$	-0.13	0.31	0.07
	BTA_g	8.04**	0.06	0.54
B_g	$MISC_g$	5.22	0.53	-0.12
	BTA_g	-32.17	0.32	0.06
SDP_g	$MISC_g$	-0.09	0.98	0.25
	BTA_g	-14.07	0.15	0.30
		#obs: 6		

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Appendix II.A: Experimental Instructions

(Note: Instructions that appear in normal type were given in all treatments. Instructions that appear in highlighted font were omitted from the control group.)

General Instructions

This is an experiment about decision-making in financial markets. In this experiment, you will participate in a computerized market, in which you will buy and sell a financial asset. The better investment decisions you make, the more money you take home, so invest wisely!

Before the asset market begins, you will take three tests – a written financial literacy test, and two prediction tests.

After the experiment has begun, we ask that you do not talk to the other test subjects until the entire experiment is over. You may direct any questions to the experimenters.

Payment will be given at the end of the experiment.

Literacy Test

This is a test of your general financial literacy. There are 5 questions. You have 1 minute to answer each question.

The computer screen shows one question at a time. All questions are multiple choice. When you have chosen your answer to a question, click the "**OK**" button. You can't go back once you have gone to the next question.

At the end of the test you will be told your score. You will be paid $2 for taking this test, regardless of your score.

Prediction Test 1

In this test, you will be trying to predict the value of something called "GDP". This "GDP" has absolutely nothing to do with the financial assets that you will be trading later on. Each period's "GDP" is generated by a mathematical formula, and depends somewhat on past GDP values and somewhat on randomness (we don't show you this formula).

On the right hand side of the screen will be a graph and a table, both of which show the history of "GDP." When the test begins, you will see a history of 10 past periods. Looking at this history will help you make your predictions.

On the left of the screen is a box where you enter your prediction for the next period's GDP. When you have entered your prediction, hit "OK". You have 30 seconds to enter each prediction. Try to focus on making the best predictions you can.

After you enter each prediction, you get to see the actual GDP for the next periods, as well as your **prediction error** and a message telling you how accurate your prediction was.

After you make 10 predictions, this test ends. You will get to see your total <u>prediction error</u> – that is, the total difference between your predictions and the real GDP value in those periods.

You will be paid $2 for taking this test, regardless of how well you do.

Prediction Test 2

In this test, you will be trying to guess whether some "balls" are being drawn out of "Jar A" or "Jar B".

Jar A and Jar B each contain different numbers of balls of various colors. The five colors are red, orange, yellow, green, and blue. The numbers of each color ball in each jar are listed on the side of the screen.

Each period, we flip a (computerized) coin to choose which jar to draw balls from. Without telling you which jar we're drawing from, we draw two balls and show you the colors. Based on the color of the balls, you guess which jar (Jar A or Jar B) the two balls came from. Enter your guess and press "**Submit Guess**". There are 15 periods, so you will make 15 guesses. You will get 30 seconds to make each guess.

This time, we won't tell you if you guessed right or wrong each time. At the end of the test, you will be asked to guess how many you got right. Try to guess this as accurately as you can.

You will also be asked to guess your *rank* (out of the total number of subjects in the room) – if you think you got more right than any other subject, enter "1", if you think three people guessed more right than you, enter "4", etc. Again, try to guess as accurately as you can.

You will be paid $2 for taking this test, regardless of how well you do.

The Asset Market

What You Will Be Doing

You will participate in three “Rounds” of trading. In each Round, you will get the chance to buy and sell a simulated financial asset (called “Asset”), using cash that we (the experimenters) give you. You can think of this asset as a mortgage or a bond. Each share of the asset pays some cash each period – this cash is called a “dividend”.

After each Round you will get to keep all the cash that is in your account at the end of that Round. We will record this amount and add it to your payment at the end of the experiment.

You will trade in a market of up to six people (including yourself). We, the experimenters, will pick your market group for you. If there are more than six people in the experiment, you will be divided into two groups.

The unit of currency is called “yen”. During each Round, you will have your “yen” in an account, and the shares of the asset will be priced in “yen”. At the end of each Round, you will be able to exchange all the “yen” in your account for real dollars, at an exchange rate of 1 yen = $0.01 (1 yen = 1 cent). So remember, these “yen” are real money!

How Trading Works

At the beginning of a Round, you will be given some cash and some shares of the asset. Different people will get different starting amounts of each. For example, you might start out with 400 yen and 8 shares of the asset, or 700 yen and 4 shares of the asset.

A Round consists of 10 trading periods. Each period lasts for 2 minutes.

At the end of each period, each share of the asset pays a **dividend**. This dividend is an amount of yen that you get for owning the share. The dividend is random, and is determined each period by a computerized coin-flip. In each period, there is a 50% chance that each share will pay a dividend of 10 yen, and a 50% chance that each share will pay a dividend of 0 yen. The dividends are the same for all shares, but different from period to period.

So the cash in your account after each period is given by:

Your cash after Period T =
Your cash at the beginning of Period T
– Purchase price of all asset shares you purchased in Period T
+ Sale price of all asset shares you sold in Period T
+ Dividend paid in Period T x # of asset shares you hold at the end of Period T

The amount of money you take home from the Round will just be the amount of cash in your account at the end of the Round (that is, at the end of Period 10).

Predictions

In addition to trading during each period, you will get a chance to make two predictions before the period. You will be asked to predict the **Average Asset Price in the Upcoming Period**, and the **Average Asset Price in Period 10**. These are the average prices that you think a share of the asset will trade for in those periods. (Note: In Period 10, the final period, these two will be the same thing.)

In addition to your earnings from the market, <u>you will be paid for how accurate these predictions are</u>. For each prediction, you will get 10 yen – |your prediction – the actual value| (with a minimum of zero). So the better your predictions turn out to be, the more you get paid.

Remember, the <u>other traders will never see your predictions</u>. So just try to make the most accurate predictions you can.

Analyst Forecasts

To help you make your decisions about buying and selling the asset, before each period (but after you make Predictions), each of you will receive an **Analyst Forecast**. This is a forecast regarding the dividend that the asset will pay in the next period. The forecasts may be different for each person, and no one else will be able to see your forecast.

The Analyst Forecasts are not perfect. The forecasts can be one of five types: “extremely bullish,” “moderately bullish,” “neutral,” “moderately bearish,” or “extremely bearish”. (Note: in financial slang, “bullish” means “optimistic” and “bearish” means “pessimistic”!)

Which forecast you see depends somewhat on random chance (since each person’s forecast is different), but also depends on the actual dividend that is going to be paid in the next period. We have provided you with a sheet, labeled “Forecast Chart”, that tells you how the probabilities of each type of forecast depend on the probabilities of the next period’s dividend.

The Trading Software

The trading software you will be using is called z-Tree. We will show you how it works.

You will always see the following information at the top of the screen:

Period ← This is the current trading period. If it is between periods, this is the number of the next period.

Time Remaining ← This is the number of seconds left in the period.

On the right hand side of the screen, you will see your **Account Information**:

Cash ←This shows how much cash you currently have in your account. Cash is denominated in "yen".

Shares ← This shows how many shares of the asset you currently own. This is also called your "portfolio".

Buy-and-Hold Value ← This is the expected (average) amount of money you would get for all the shares in your portfolio, if you held onto those shares until the end of the experiment. (Note: This value does NOT take your most recent Analyst Forecast into account! It is the buy-and-hold value with ALL dividend percentages set to 50-50, including the current period's dividend.)

Below your account information, you will also see a **Price History box**, which tells you about asset prices in previous periods:

Average Price ← This shows the average price of a share of the asset in each of the previous periods in the Round
(empty in Period 1)

Below the Price History box, you will see the **Traded Price box**. This lists the sale prices of every share sold in the current period. During trading, you will have two columns: the **Sell Column** on the left, and the **Buy Column** in the center.

Sell Column

There are two ways to sell assets: 1) You can make a sell offer that other traders can choose to accept, and 2) You can accept bids (buy offers) made by other traders. In the Sell Column, you will see the following three boxes:

Make Sell Offer ← This lets you make an offer to sell one share of asset. Type in the price you are willing to accept for a share, and press "**Enter**". This price must be a whole number (no decimals!), and must be positive. When you enter a sell offer, your offered price will appear in the "Offer Price" box in the Buy Column in the lower middle of the screen, where buyers can choose to accept it.

Undersell Lowest Offer ← Pressing this button will enter a sell offer that is 1 yen lower than the lowest existing sell offer. It is a way to make an offer quickly. If there are no existing sell offers, then this button won't do anything.

Bid Price box ← This box contains a list of all outstanding bids. You can sell a share by pressing the "**SELL NOW**" button at the bottom of the box. You will automatically sell one share for the highest available bid price (as long as it's not your own bid!). Each time you sell a share, ALL of the existing bids and sell offers of both you and the buyer will disappear.

Buy Column

There are two ways to buy assets: 1) You can make a bid that other traders can choose to accept, and 2) You can accept sell offers made by other traders. In the Buy Column, you will see the following three boxes:

Make bid ← This lets you make an offer to buy one share of the asset. Type in the price you are willing to pay for a share, and press "OK". <u>This price must be a whole number (no decimals!), and must be positive</u>. When you enter a bid, your bid price will appear in the "Bid Price" box in the Sell Column on the lower left hand side of the screen, where sellers can choose to accept it.

Top Highest Bid ← Pressing this button will enter a bid that is 1 yen higher than the highest existing bid. It is a way to make a bid quickly. If there are no existing bids, then this button won't do anything.

Offer Price box ← This box contains a list of all outstanding sell offers. You can buy a share by pressing the "**BUY NOW**" button at the bottom of the box. You will automatically buy one share for the lowest available sell offer price (as long as it's not your own sell offer!). Each time you buy a share, ALL of the existing bids and sell offers of both you and the seller will disappear.

Note: If you try to buy a share for more cash than is in your account, or if you try to sell a share when you have zero in your portfolio, you will get an error message.

Appendix II.B: Forecast Chart

(Note: This chart was given to all treatment groups except for the control group.)

If the Dividend is 10 Yen

Forecast	% Chance
Extremely Bullish	35
Moderately Bullish	30
Neutral	20
Moderately Bearish	10
Extremely Bearish	5

If the Dividend is 0 Yen

Forecast	% Chance
Extremely Bullish	5
Moderately Bullish	10
Neutral	20
Moderately Bearish	30
Extremely Bearish	35

Appendix II.C: Literacy Test

Note: Correct answers in **bold.**

Easy Version

Q1. Suppose you had $100 in a savings account and the interest rate was 2% per year. After 5 years, how much do you think you would have in the account if you left the money to grow?

A) More than $102

B) Exactly $102

C) Less than $102

Q2. Suppose you had $100 in a savings account and the interest rate was 20% per year, and you never withdraw your money or interest payments. After 5 years, how much would you have in the account in total?

A) More than $200

B) Exactly $200

C) Less than $200

Q3. Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, how much would you be able to buy with the money in this account?

A) More than today

B) Exactly the same as today

C) Less than today

Q4. Assume that a friend inherits $10,000 today and his sibling inherits $10,000 3 years from now. Who is richer because of the inheritance?

A) The friend

B) His sibling

C) They are equally rich.

Q5. Suppose that in the year 2010, your income has doubled, and prices of all goods have doubled too. In 2010, how much will you be able to buy with your income?

A) More than today

B) The same as today

C) Less than today

<u>Hard Version</u>

Q1. Which of the following statements is correct? If somebody buys the stock of Firm B in the stock market,

A) He owns a part of Firm B.

B) He has lent money to Firm B.

C) He is liable for Firm B's debts.

D) None of the Above

Q2. Considering a long time period (10 or 20 years), which asset normally gives the highest return?

A) Savings accounts

B) Bonds

C) Stocks

Q3. When an investor spreads his money among many different assets, does the risk of losing money increase, decrease, or stay the same?

A) Increases

B) Decreases

C) Stays the same

Q4. If you buy a 10-year bond, it means you cannot sell it after 5 years without incurring a major penalty. True or false?

A) True

B) False

C) It depends on the bond.

Q5. If the interest rate falls, what should happen to bond prices?

A) They should rise.

B) They should fall.

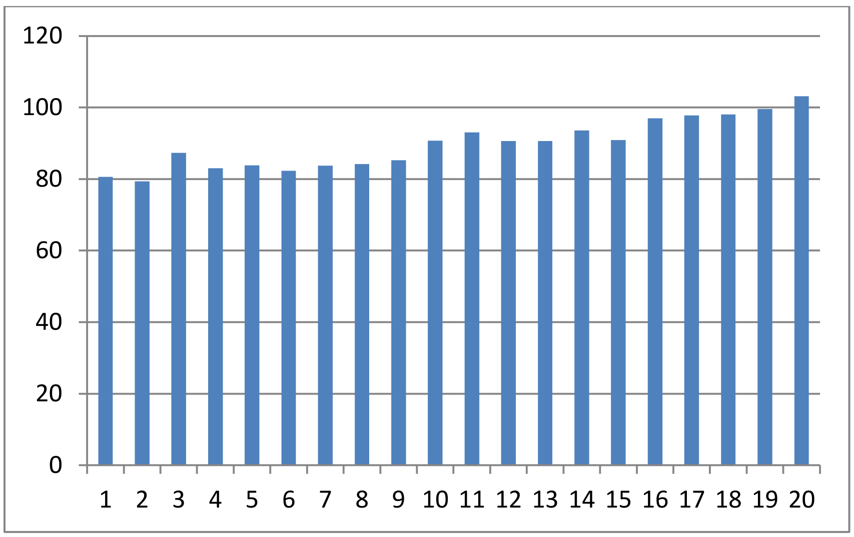
C) They should stay the same.

D) None of the Above

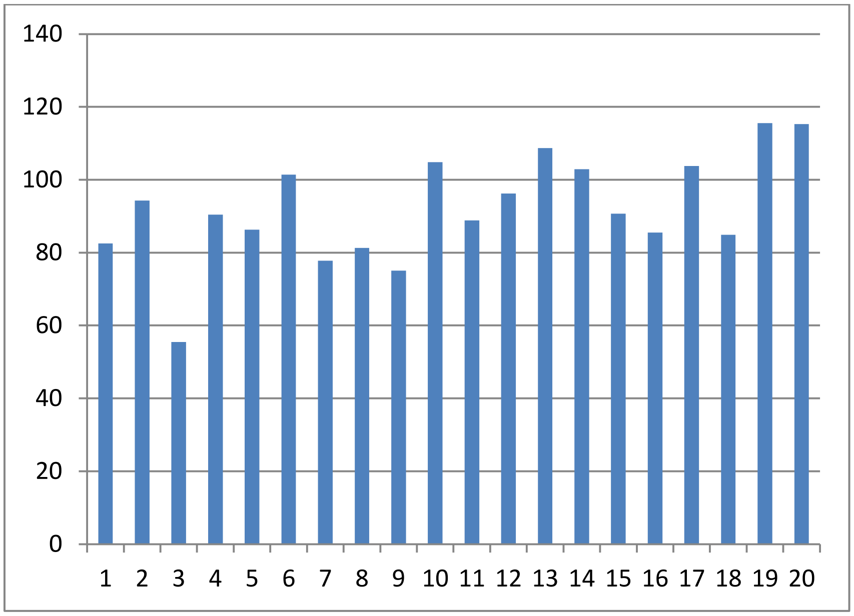
Appendix II.D: Time-Series Prediction Task

Note: Subjects were given periods 1-10 as "history" at the beginning of the task, and asked to make 10 consecutive 1-step-ahead predictions. Feedback after each prediction was divided into four categories, corresponding to prediction errors of <3, <5, <8, and <13. Feedback at the end was divided into four categories, corresponding to total prediction errors of <30, <50, <80, and <130.

<u>Easy Version</u>



Hard Version



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Chapter III
Affect and Expectations

1 Introduction

John Maynard Keynes coined the term "animal spirits" when he wrote:

> Even apart from the [economic] instability due to speculation, there is the instability due to the characteristic of human nature that a large proportion of our positive activities depend on spontaneous optimism… whether moral or hedonistic or economic. Most, probably…our decisions to do something positive … can only be taken as the result of animal spirits – a spontaneous urge to action rather than inaction, and not as the outcome of a weighted average of quantitative benefits multiplied by quantitative probabilities.

How much of this "spontaneous optimism" is "hedonistic" in nature? This is an important question. If expectations can be changed by swings in emotional affect – surges of happiness, waves of fear – then the Rational Expectations Hypothesis is false, and much of macroeconomics must involve the study of human psychology. Some prominent economists have claimed that emotion is a key determinant of both expectations and behavior. For instance, a January 2010 New York Times column by Robert Shiller was entitled, "Stuck in Neutral? Reset the Mood" (Shiller 2010). In that article, Shiller made explicit references to "post-boom pessimism," "malaise," "negative 'mood,"' "irrational exuberance," and the "hearts and minds" of consumers. These ideas echoed the arguments in Shiller and George Akerlof's book Animal Spirits: How Human Psychology Drives the Economy, and Why It Matters for Global Capitalism (Princeton University Press, 2009). We call this the "affective animal spirits" hypothesis.

The notion that emotion has large effects on expectations is consistent with a substantial psychological and decision sciences literature showing that emotional state affects judgment in a number of ways. For instance, Johnson and Tversky (1983) found that "experimental manipulations of affect induced by report of a tragic event produced a pervasive increase in [subjects'] estimates of the frequency of many risks and other undesirable events." A number of other studies and experiments have yielded a similar result (see Section II). Since estimates of probability risk play a central role in economic decision-making, this psychology literature gives us reason to suspect that "affective animal spirits" are at work in the economy.

However, despite psychologists' findings, and despite the popularity of the idea in the press, little attempt has been made by economists to study the link between emotional affect and economic expectations. This paper represents a first attempt to fill that gap. We use survey data on affect and on economy-related expectations to study the link between the two. Our data come from the Michigan Survey of Consumers, a large and widely used survey that contains a number of questions about expectations for the economy, some of which are used to construct the much-studied Index of Consumer Sentiment. Our measure of emotional affect comes from a questionnaire that we included in the survey from 2005 to 2010, asking four questions relating to positive and negative emotional states.

Because each survey respondent in our sample is contacted twice (at an interval of six months), we have panel data. This allows us to disentangle the effects of three postulated components of mood – national or average mood, an individual's fixed effect, and an individual's six-month change in self-reported mood. We are thus able to ask at least three different questions:

1. Does the national mood affect expectations about the economy?

2. Are fluctuations in affect at the 6-month frequency associated with fluctuations in expectations at that frequency?

3. Are people who are happier in the long term also more optimistic in the long term?

Our answer to these questions are: 1) Not that we can tell, 2) Not very much if at all, and 3) Yes. Because of the endogeneity of happiness and expectations, we are only able to provide upper bounds on the effect of fluctuations in happiness on fluctuations in expectations. Even so, we find no measurable relationship between national mood and national expectations. Additionally, we find only a very small, and possibly nonexistent relationship between changes in mood and changes in expectations. We do, however, find a significant correlation between individual fixed effects in mood and expectations; happier people are indeed more optimistic. We also investigate whether differences in the happiness of demographic or political groups (Democrats and Republicans, the old and the young, etc.) can account for differences in the economic expectations of these groups, but find little or no such effect.

These findings throw cold water on the idea that swings in mood, either of the nation or of individuals, cause large swings in expectations about the economy. If macroeconomic variables (consumption, investment) or financial markets are driven by the mood of the nation, the effect does not appear to operate via altered expectations as proposed by Keynes. Similarly, the well-documented effect of events like recessions and elections on happiness also does not seem to work by altering expectations. However, the finding that happiness and expectations are correlated for individuals over the long term indicates that there are individual fixed effects in expectations, and that these fixed effects are related in some way to individual emotional traits; this finding is troubling for some (strict) versions of Rational Expectations theory, since it indicates that aggregate expectations may suffer from composition effects.

The plan of the paper is as follows. Section 2 reviews the economics literature on survey measures of consumer confidence, the psychology literature on affect and expectations, and some other relevant papers about self-reported affect. Section 3 introduces the survey data, reintroducing the Survey of Consumers and explaining our added questionnaire on affect.

Section 4 presents our analysis of the data, including our national-level, individual-level, and subgroup-level analyses. In Section 5 we discuss our results. We argue that our results hold true not only for the types of affect measured in our survey (happiness and sadness), but for any emotion that is correlated with these. We also discuss the relevance of our results for the fields of macroeconomics, finance, and psychology. Section 6 concludes.

2 Literature

2.1 Consumer sentiment

The Michigan Survey of Consumer Sentiment asks people many questions about their expectations for future economic conditions and their evaluation of recent economic conditions. Five of these questions (see Tables III.1a and III.1b) are averaged to create the Index of Consumer Sentiment, which is also called "consumer confidence" (we will use the two terms interchangeably unless otherwise specified). Many studies have explored the use of consumer confidence for forecasting macroeconomic variables. Matsusaka and Sbordone (1995) find that the Index of Consumer Sentiment Granger-causes U.S. GNP, and that consumer sentiment accounts for 13 to 26 percent of the variance in U.S. output. Heim (2009) finds that consumer confidence is systematically related to future consumption of nondurable goods. Dees and Brinca (2011) find that large changes in consumer confidence give good out-of-sample predictions of changes in future consumption. Acemoglu and Scott (1994) find that an alternative measure of consumer confidence in the United Kingdom predicts future consumption, rejecting a simple version of the Permanent Income Hypothesis. For an overview of studies of the use of consumer confidence in forecasting consumer spending, see DesRoches and Gosselin (2002).

There is also a literature on using consumer confidence to forecast asset prices. Fisher and Statman (2002) find that consumer confidence predicts subsequent returns for the Nasdaq and for small-cap stock indices. Chen (2011), using a Markov switching model of “bull” and “bear” markets, finds that large drops in consumer confidence forecast an increased likelihood of a switch to a “bear” market. However, Christ & Bremmer (2003) find that stock prices Granger-cause consumer confidence but not vice versa.

Do these results imply that the Index of Consumer Sentiment can be regarded as a measure of “autonomous” expectations, i.e. optimism or pessimism that is unrelated to economic fundamentals? Not necessarily. As DesRoches and Gosselin (2002) note, some studies find that once economic fundamentals are taken into account, consumer confidence loses much of its predictive power. Barsky and Sims (2008) examine the shapes of empirical impulse responses of macroeconomic variables to innovations in consumer confidence, and compare these to the shapes of impulse responses in a New Keynesian DSGE model. They find that impulse responses for confidence shocks are similar to the impulse responses to shocks to news about future productivity in the DSGE model, but not similar to impulse responses to shocks to sentiment itself. This implies that swings in consumer confidence reflect information about economic fundamentals rather than autonomously moving “animal spirits.” In other words, this strand of research finds that consumer confidence may reflect expectations, but not expectations that move spontaneously, as Keynes, Shiller, and others postulate. As we will see, this interpretation agrees with our own findings in this paper. However, note that this literature only examines changes in consumer confidence, and does not deal with the long-term level of confidence. This, too, will bear on our results.

2.2 Affect and expectations

A number of psychology experiments have established a link between individuals’ emotional affect and their assessments of probability and risk. One study already mentioned was Johnson and Tversky (1983). Nygren et al. (1996) found that when positive affect was induced in experimental subjects, their estimates of the probabilities of favorable events increased. Lerner and Keltner (2001) found that fear and anger have substantial but opposite effects on risk perception. Wright and Bower (1992) found that happier people are more optimistic. Mayer et al. (1992) found that people are likely to perceive “mood-congruent events” – i.e., events that would tend to cause a mood similar to their current mood – were perceived to be more probable. Ambady and Gray (2002) found that negative affect (sadness) impaired certain kinds of social judgments. Perhaps most intriguingly, behavioral economists Eduardo Andrade and Dan Ariely (2009), using a series of dictator and ultimatum games, found that short-lived changes in emotional state had a persistent effect on decision-making long after the emotion had passed.

This literature strongly motivates our hypothesis that self-reported happiness and sadness affect economic expectations.

2.3 Happiness

Survey measures of affect are common in the economics literature. The most widespread survey measure of affect, and thus the closest we have to a standard measure, is self-reported happiness, which has been widely used by economists for a variety of purposes (see DiTella and MacCulloch (2006) for a survey). Our own survey measure of affect uses two questions about happiness and two about sadness, and thus fits squarely into the happiness literature. For this reason, we refer to our composite index of affect valence (positivity) as “happiness.”

There is significant evidence that happiness contains a large individual fixed effect. Psychologists who study happiness have found it to have both “trait” (individual fixed-effect) and “state” (transitory, event-dependent) properties, with the former probably explaining more of the variance in the data than the latter (Stones, et. al. 1995). In other words, it is likely that each person has her own fairly stable long-term baseline level of happiness, with day-to-day events causing fluctuations around this mean. This conclusion is supported by studies that show that genetics have strong predictive power for an individual’s happiness (Weiss, Bates, & Luciano 2008). For this reason, we conceive of happiness as being the sum of (at least) two components - "persistent" happiness that represents an individual's psychological traits, and "transitory happiness" that moves in response to news or life events.

One implication of the this two-component model is that happiness should mean-revert. In fact, this phenomenon is well-documented in the psychology literature, and is known as as "hedonic adaptation." Lyubormisky, Sheldon, and Schkade (2005) review a large number of studies showing that self-reported happiness reverts to the individual's long-term mean after both positive and negative life events. They then propose a model of happiness in which happiness is explained by three components: a genetic component, reactions to daily events, and the effects of certain daily activities and practices. Clark et. al (2007) use German panel data to establish that complete hedonic adaptation to major life events - divorce, layoffs, etc. - generally occurs within two to three years. Other studies find that hedonic adaptation happens much faster. Kimball, Ohtake and Tsutsui (in progress) find that innovations to happiness caused by personal or national news disappear in a matter of days. This result is particularly interesting for the present study, since "news" is generally believed to the main driver of expectations under the Rational Expectations Hypothesis. In the present study, we take hedonic adaptation as a maintained hypothesis - we assume that any piece of news that affects expectations of the economy, be it personal news (a layoff) or national news (the start of a recession), exerts no influence on an individual's happiness six months later.

Although few or no economists have studied the causal impact of happiness on economic outcomes, there is a literature on the effect of economic variables on happiness. This is important because happiness is often used as an “alternative” indicator of economic performance. For example, Wolfers (2003) finds that high unemployment, high macroeconomic volatility, and (to a lesser extent) high inflation lower perceived well-being. Additionally, DiTella, MacCulloch, and Oswald (2001) find that changes in self-reported happiness are strongly correlated with changes in GDP, even after accounting for other real economic variables (and for individual fixed effects). Our results in the current study are first and foremost about effects of happiness on the economy, but as we will see they also have relevance for this literature.

3 Data

Our data are survey data from the Survey of Consumer Sentiment, from August 2005 through January 2011. The Survey contains a number of questions regarding expectations regarding the future state of the economy, as well as questions regarding evaluation of the economy's present and past performance. Henceforth we refer to these simply as "expectation variables" unless other wise noted. A full list of all such questions used in this paper is available in Table III.1, and descriptive statistics for the expectation variables are given in Table III.2. In particular, five of these questions are averaged to produce the variable known as "consumer confidence." The names of these five variables, and the wording of the associated survey questions, are as follows:

- pago: "Would you say that you (and your family living there) are better off or worse off financially than a year ago?"
- pexp: "Do you think that a year from now you (and your family living there) will be better off financially, or worse off, or just about the same as now?"
- bus12: "Do you think that during the next 12 months we'll have good times financially, or bad times, or what?"
- bus5: "Which would you say is more likely – that in the country as a whole we'll have continuous good times during the next 5 years or so, or that we will have periods of widespread unemployment and depression, or what?"
- dur: "Generally speaking, do you think now is a good time or a bad time for people to buy major household items?"

Each expectation variable is normalized to a 0-100 scale, with more positive values signaling better expectations about or evaluations of the economy.

The Survey also contains a number of demographic variables, which we will use when we estimate group happiness.

Beginning in August 2005, we added to the survey four questions regarding respondents' emotional state in the previous week. These questions are listed in Table III.1c. The wording of the questions is as follows:

- "Much of the time during the past week, you were happy. Would you say yes or no?"
- "Much of the time during the past week, you felt depressed. Would you say yes or no?"
- "Much of the time during the past week, you enjoyed life. Would you say yes or no?"
- "Much of the time during the past week, you felt sad. Would you say yes or no?"

We asked questions regarding both negative and positive emotions. Responses to all questions were normalized to a 100-point scale, with "No" responses to the negative-emotion questions (e.g. "Much of the time during the past week, you felt sad. Would you say yes or no?") being given positive values. There were four possible answers for each question, so that each answer can have a value of 0, 25, 50, or 100. Survey respondents' answers to these four questions are highly correlated; these correlations are listed in Table III.3.

All four of our affect questions related to positive or negative affect. To simplify things, we average a person's four responses to produce a "happiness index" that measures the person's overall emotional valence (happiness or sadness). Henceforth we refer to this index simply as "happiness." Because this is our only measure of affect, we will use the terms "happiness" and "affect" interchangeably until we discuss other possible emotional states in Section (4.3.5).

Each survey respondent was interviewed twice ; the interval between interviews was about 6 months for all respondents. Respondents who answered a question of interest in one of the interviews but not the other were dropped from the sample.[1] Thus, we have a balanced panel. The total number of individual respondents in our data set is 11,986, meaning that the total number of contacts is double that, or 23,972. The minimum monthly number of usable survey contacts in our dataset is 194 (maximum 421, average 380).

A full explanation of the sampling techniques used to construct the Survey of Consumers is available at http://www.oecd.org/dataoecd/13/42/33650864.pdf. For the purposes of this paper, we treat the sample as perfectly representative of the American adult population.

4 Analysis

4.1 Definitions and Normalizations

For variables measuring affect, we use the symbol x. An individual i's response to affect question q on day t is therefore x_{itq}. q ranges between 1 and 4. The Happiness Index (henceforth, simply "happiness") we call x_{it}, and is defined as the average of the four questions:

$$x_{it} = x_{it1} + x_{it2} + x_{it3} + x_{it4} \tag{1}$$

Variables measuring expectations (or evaluations) of economic conditions are given the symbol y. We have many expectation variables, so we use y_{it} generically, and give results for the specific expectation variables in the tables.

We first studentize all variables (happiness and all expectations variables) at the level of the entire sample. Therefore, a happiness value of 1 means "1 unconditional standard

[1]This includes respondents for whom one contact fell outside of the months in which the affect questionnaire was included with the survey.

deviation above the unconditional mean of happiness." We think this makes our point estimates more intuitive, since it defines swings in affect and expectations in terms of how much these variables tend to swing.

4.2 Aggregate-level analysis

The "affective animal spirits" hypothesis is that swings in the national mood cause swings in expectations about the economy, which in turn causes changes in macroeconomic aggregates. Therefore, this hypothesis requires that the national mood be corre
lated with national expectations. Such a correlation is necessary, though not sufficient, to vindicate the idea of "affective animal spirits." So before we conduct the analysis at the individual level, we do a "first pass" by looking for a correlation at the aggregate level. As our measures of aggregate happiness and aggregate expectations, we use the monthly averages of the answers to our survey questions. Call these $\bar{x}_{i\tau}$ and $\bar{y}_{i\tau}$, where τ represents a month instead of a day. We assume for now that these survey averages are perfect proxies for the national averages; in other words, we ignore composition effects.

First, a casual look at the data tells us that movements in happiness do not explain much of the movement of expectations. Figure III.1 shows the movements of monthly aggregate happiness and aggregate bus12, (an expectation variable representing expectations for business conditions over the next 12 months) over our sample period. Bus12 exhibits large swings, coinciding with the financial crisis of 2008, while happiness basically does not move. Looking at Table III.2, we can see that expectations are substantially autocorrelated at one lag, while happiness is not. Table III.4 shows autocorrelations for happiness and bus12 at several lags, showing much the same result - bus12 is highly persistent even at 5 lags, while happiness is not persistent even at one lag. Expectations are slow to change, while happiness is quick to change; this makes it unlikely that big movements in happiness will be able to cause big movements in expectations in anything other than the contemporaneous month.

But a relationship of some sort may still exist. Assuming that both $\bar{x}_{i\tau}$ and $\bar{y}_{i\tau}$ are stationary (which seems reasonable), we run a 2-variable vector autoregression of $\bar{y}_{i\tau}$ on $\bar{x}_{i\tau}$. We run the VAR once with 2 lags and once with 5 lags.[2] For all but one of these expectation variables, there is no relationship between aggregate happiness and aggregate expectations at any lag. The one exception is bus12. As shown in Table III.5, lagged bus12 has a statistically significant effect on happiness at one, two, three and four lags (the reverse is not true, due to the much greater time-series variance of bus12). In other words, this VAR shows a possible relationship between happiness and lagged expectations at the aggregate level. Our later analysis, which is done at the individual instead of the aggregate level, cannot confirm or reject this result, since individuals are recontacted at a 6-month frequency instead of a 1-month frequency.

However, observe that the VAR coefficients representing the effect of bus12 on happiness *alternate signs* from one lag to the next; the coefficient for 1-month-lagged bus12 is positive, the coefficient for 2-month-lagged bus12 is negative, and so on. This "ringing" effect does not make much logical sense, and may indicate that this is an artifact of the relatively small sample, especially given the lack of significant effects for the other expectation variables. Therefore, taken together, the VAR analysis does not show much evidence of a relationship between happiness and expectations at the national level, with the caveat that the bus12-happiness relationship merits further investigation.

In this VAR, we have not controlled for other national-level variables that may affect happiness and expectations - for example, GDP growth. In general, it is possible that correlated omitted variables have biased our estimates of the VAR coefficients downward. However, it is more reasonable to assume that omitted variables only bias these coefficients *upward*. We assume in this paper that anything that makes people more optimistic about the economy - a burst of higher GDP growth, for example - also makes them happier. Thus,

[2]The Akaike Information Criterion and Schwartz Information Criterion are both maximized for five lags. However, we can't use lag 6, because this is the lag at which subjects are recontacted, and so our proxy of national aggregates would become biased.

if we find little or no correlation between happiness and expectations at the aggregate level, we can safely assume that the *causation* between the two is even smaller. We formalize this assumption in the next section.

This aggregate-level analysis is inherently limited. Our time series contain only 66 periods, and aggregate happiness does not vary much over time (compared to the variance of happiness across individuals). Therefore, these time-series regressions have low power. By looking at the relationship between expectations and happiness at the individual level, we can make use of a huge amount of additional information. Also, we can dispense with the assumption that successive monthly averages are all perfect proxies for the national average. So in the next section, we turn to our individual-level analysis.

4.3 Individual-level analysis

Our panel data are daily. Therefore, from here on out, our variables are all indexed by individual i and by time t, where t represents a day.

Happiness and expectations are correlated at the individual level. Regression estimates from an OLS regression of expectations y_{it} on happiness x_{it} are shown in Table III.6 for the five ICS variables. T-statistics range from 10.19 to 25.49. The point estimates are all around 0.1, meaning that on a given day, a person who is 1 standard deviation happier than the mean[3] is likely to be about 0.1 standard deviations more optimistic about the economy. Thus, people who are happier are more optimistic. This agrees with the findings of psychology studies like Wright & Bower (1992).

But we want to ask more specific questions about the relationship between affect and expectations. Our questions are:

1. How does the "national mood" affect individual expectations?

2. Do swings in an individual's happiness cause swings in her expectations?

[3] "Standard deviation" and "mean," remember, refer to the standard deviation and mean across all people over the entire sample period, not across all people at that specific point in time.

3. Are people who are happier in general also more optimistic in general?

Because we have panel data, we can address these questions. At this point we return to the idea that there are multiple components of happiness. Psychology tells us that happiness is determined by (at least) two underlying processes - "trait" happiness that changes only over very long periods of time, and "state" happiness that varies from day to day or month to month. We call these the "persistent" and "transient" components of happiness. Also, we postulate that the transient part of happiness is really two parts: a "national mood" that is the same for all individuals in the entire country, and "transient personal happiness" that depends only on individual factors. We assume that "persistent personal happiness" doesn't change over six months, so this is just the individual fixed effect in our sample. Algebraically, we represent happiness x_{it} as the sum of these three components:

$$x_{it} = x_t^N + x_i^P + x_{it}^T \tag{2}$$

This is the typical three-component model found in many panel studies, with one component dependent only on time, one component dependent only on the individual, and one component dependent on both time and the individual. We assume that these happiness components are mutually uncorrelated. Since happiness itself is defined to be mean zero, this means the components are mean zero as well.

We assume that each of the three happiness components affects expectations differently. We also assume that expectations can be affected by new information about relevant economic variables, or by spontaneous innovations to expectations that have no identifiable cause. As we explain in the following section, we combine all of non-happiness-related things that affect expectations, along with measurement error, into a single term ε_{it}. Note that *stylistically*, this is the reverse of the typical model of expectations, in which news variables are the regressors and irrational determinants of expectations are placed in the error term; however, our modeling approach does not differ from the typical one in *substance*, and simplifies our analysis greatly, as we explain in the subsequent sections. Our basic model of expectations is thus:

$$y_{it} = \alpha + \beta_N x_t^N + \beta_P x_i^P + \beta_T x_{it}^T + \varepsilon_{it} \tag{3}$$

4.3.1 Identifying assumptions and omitted variables

In our model of expectations in Equation 3, we include every factor other than happiness that might affect expectations in the "error" term, ε_{it}. Therefore, ε_{it} should not be construed to represent "noise" or measurement error only. ε_{it} includes publicly observed unexpected events such as innovations to GDP growth, stock returns, employment, and other factors that may cause national expectations about the economy to be revised. ε_{it} also includes personal events such as layoffs that might affect an individual's expectations about the economy. We refer to any time-dependent variable other than happiness that affects economic expectations as "news."[4] In other words, we implicitly think of ε_{it} as containing several components:

$$\varepsilon_{it} = NN_t + PN_{it} + e_{it} \tag{4}$$

Here NN_t is weighted sum of national news variables, PN_{it} is a weighted sum of personal news variables, and e_{it} represents any spontaneous innovations to expectations, as well as classical measurement error. The reason we use a single term ε_{it} instead of all of these terms separately is simple: since we make the same identifying assumptions about all of these terms, rolling them all into ε_{it} makes our algebra less cumbersome.

In general, both "news" and spontaneous innovations to expectations may affect happiness. Therefore, the model in Equation 3 is not fully identified by our dataset! The implicit

[4]The label "news" should not be construed to imply that these variables affect expectations in a *rational* manner - in other words, we leave open the possibility that these "news" variables should not affect expectations in a fully rational world, but do affect expectations in the real world.

components of ε_{it} include all correlated omitted variables. In other words:

$$Cov(\varepsilon_{it}, x_t^N) \neq 0 \tag{5}$$

$$Cov(\varepsilon_{it}, x_{it}^T) \neq 0 \tag{6}$$

Without any further assumptions, this would make it impossible for us to learn anything interesting about the β coefficients in Equation 3 using our dataset. This is not surprising; all regressions are subject to the possibility of correlated omitted variables, and even when they include many covariates, must rely on assumptions in order to be identified. Fortunately, we have two additional assumptions that allow us to learn some interesting things about our β's. The first of these is the assumption that *any piece of news that makes a person more optimistic, or any positive spontaneous innovation in the person's expectations, also makes that person happier contemporaneously*. In other words:

$$Cov(\varepsilon_{it}, x_t^N) \geq 0 \tag{7}$$

$$Cov(\varepsilon_{it}, x_{it}^T) \geq 0 \tag{8}$$

As we will see, this assumption allows us to set upper bounds on the effect of national happiness and of transitory personal happiness on expectations.

Our second assumption is that neither news nor spontaneous innovations to expectations affects persistent personal happiness x_i^P. This assumption is motivated by the idea of hedonic adaptation. Persistent personal happiness is assumed to be a fixed psychological trait. Furthermore, we assume that hedonic adaptation happens within six months.

Finally, note that we have not included an individual fixed effect in our model of expectations. We will discuss this in a later section.

4.3.2 Effect of national happiness

In the aggregate analysis, we examined the effect of national happiness on national expectations. In this section we look at the effect of national happiness on *individual* expectations. In other words, in this section we ask: “If a person wakes up on a certain day and finds that the nation as a whole is much happier than normal, is she likely to be much more optimistic about the economy than she normally is?”

The national happiness component x_t^N is assumed to be the same for all individuals. The average happiness of a number of individuals interviewed on the same day can, roughly speaking, be thought of as a noisy proxy for x_t^N. For our estimation of β_N, we find it useful to define a quantity x_{-it} that we call the "daily other-mean" of happiness - in other words, the average happiness of all individuals other than individual i who are interviewed on the same day as individual i:

$$x_{-it} \equiv \frac{1}{m_t - 1} \sum_{j \neq i} x_{jt} \tag{9}$$

Here m_t is the number of people contacted on day t. Since we assume that only national happiness (and not either of the two kinds of personal happiness) is correlated across individuals, x_{-it} is a proxy for x_t^N. We will also find it useful to define "de-national-meaned" happiness, $\widehat{x_{it}}$:

$$\widehat{x_{it}} \equiv x_{it} - x_{-it} \tag{10}$$

We estimate β_N by the method of moments. Consistent estimation of β_N is not possible, given the endogeneity discussed in Section 4.3.1. However, we use moment conditions to define an *asymptotically biased* estimate $\widetilde{\beta_N}$. $\widetilde{\beta_N}$ is a consistent estimate of $\beta_N + bias$:

$$\widetilde{\beta_N} \xrightarrow{P} \beta_N + \frac{Cov(\varepsilon_{it}, x_t^N)}{Var(x_t^N)} \tag{11}$$

See Appendix III.A.2.1 for the derivation of $\widetilde{\beta_N}$. Mechanically, the moment condition that identifies $\widetilde{\beta_N}$ is equivalent to the parameter estimate from an instrumental variables regression of expectations y_{it} on happiness x_{it}, instrumenting for x_{it} with x_{-it}.

As stated above, we assume that the bias is positive; that is, that $Cov(\varepsilon_{it}, x_t^N) \geq 0$. $\widetilde{\beta_N}$ is therefore an upper bound on the true β_N. We refer to $\widetilde{\beta_N}$ as "the estimate of β_N," keeping in mind that it is really an estimate of an upper bound of β_N and that the estimate standard errors for $\widetilde{\beta_N}$ are actually the standard errors of an upper bound on β_N. We use this sloppy terminology throughout the remainder of the paper, though we periodically remind the reader when an estimate is upwardly biased.

The estimates of β_N for our various expectation variables are in Table III.8. The estimate is not statistically significant for any of these variables except for bus12 (p-value of 0.032). For bus12, the point estimate is 0.632, which is a substantial effect. As in the aggregate analysis, there appears to be a correlation between national happiness and bus12. However, the direction of causation is not identifiable.

We also care about the explanatory power of national happiness. Even if swings in the national mood can cause swings in expectations, these swings may be only a tiny fraction of the total. We can estimate an upper bound on the fraction of the variance of y_{it} that can be explained by x_t^N (see Appendix III.A.3). We call this value R_N^2. We estimate R_N^2 to be 0.004 for bus12, and lower for other expectation variables - a tiny amount, even though it is an upper bound. The reason for this is that the national mood simply does not swing by very much; see Table III.7 for estimates of the variances the three happiness component.[5] The variance of x_t^N is two orders of magnitude lower than the variance of either of the other two components.

Individual-level analysis essentially confirms the conclusions of the aggregate analysis. Bus12 and national happiness are related, but the direction of causality is unknown and may be due any one of a large number of omitted variables. National happiness is not detectably

[5]These are in terms of number of unconditional standard deviations of total happiness.

related to any other expectation variable. Also, the power of national happiness to explain variations in expectations is negligible. Therefore, we find very little evidence to support the "affective animal spirits" hypothesis at the national level, though the correlation with bus12 is interesting and worthy of further study.

4.3.3 Effect of transitory personal happiness

In this section we look at the effect of transitory happiness fluctuations on individual expectations. In other words, we ask: "If a person wakes up on a certain day and finds that she is much happier, relative to other people, than normal, is she likely to be much more optimistic about the economy than she normally is?"

To answer this question, we estimate an upper bound on β_T. Again, we use the method of moments to find an estimate of the upper bound on β_T, which we call $\widetilde{\beta_T}$. Mechanically, $\widetilde{\beta_T}$ is the same as the parameter estimate from an IV regression of the change in happiness between an individual's first and second contacts, Δy_{it}, on the change in de-national-meaned happiness, $\Delta\widehat{x_{it}}$. See Appendix III.A.2.2 for a derivation of the moment conditions. Essentially, we are just performing an instrumental variable regression in first differences, after differencing out national happiness.

This estimate is another upper bound, this time an upper bound on the true effect of swings in personal happiness on swings in expectations. The bias exists for the same reason as in the last section; swings in happiness and optimism may cause each other. Again, we sloppily refer to this estimate as the "estimate of β_T."

Estimates of β_T for our various expectation variables are in Table III.9. A large fraction of these are significant at the 5% level. Most of the point estimates are on the order of 0.05; this is about an order of magnitude smaller than the correlation between expectations and happiness found at the beginning of this section. Most of the fact that happier people are more optimistic is explained by the individual fixed effect.

As in the previous section, we calculate an upper bound on the fraction of the variance of y_{it} that can be explained by x_{it}^T. These values are never more than 0.001; we estimate that no more than 0.1% of swings in an individual's expectations can be explained by swings in her own happiness. In reality, given the influence of personal news - layoffs and the like - on both expectations and happiness, it is likely that the true causal effect of happiness on expectations is considerably smaller. How small, or whether the effect exists at all, is a question beyond the scope of this analysis; we attempt to be as favorable as possible to the hypothesis that swings in happiness cause swings in expectations, and to show that even in the most favorable case, the effect is not substantial.

4.3.4 Effect of persistent personal happiness

If swings in happiness do not account for the correlation between happiness and expectations, then most of the relationship must be due to the individual fixed effect, or what psychologists call the "trait" of happiness. Here we confirm that intuition. We use the method of moments to derive a moment ratio that is equivalent in population to β_P (see Appendix III.A.2.3). This moment ratio is mechanically equivalent to the parameter estimate from an IV regression of expectations on happiness, instrumenting for an individual's happiness with the same individual's happiness at the other contact date, after instrumenting for happiness at the second contact date with de-national-meaned happiness at the first contact date. (See Appendix III.A.2.3). In other words, if x_{i2} is an individual's happiness at the second contact date, we perform an IV regression of y_{i2} on x_{i2}, instrumenting for x_{i2} with $\widehat{x_{i1}}$, to obtain an estimate of β_P. We assume that there is no individual fixed effect in expectations apart from the effect of persistent personal happiness; hence, unlike in previous estimators, we consider our estimates of β_P to be consistent.[6] Our estimates are in Table III.9. We see that, as predicted, our estimates of β_P are about an order of magnitude larger than our estimates of β_T, and are very similar to the overall correlation between happiness and expectations.

[6]This is motivated by the voluminous literature on hedonic adaptation.

As a robustness check, we can allow for persistent personal happiness to be correlated with slow-moving spontaneous innovations to expectations at the national level - in other words, we can relax the assumption of hedonic adaptation with respect to national news.[7] The model in Equation 3 will then become:

$$y_{it} = \alpha + \gamma_t + \beta_N x_t^N + \beta_P x_i^P + \beta_T x_{it}^T + \varepsilon_{it} \tag{12}$$

To see if this changes the analysis, we simply demean $\widehat{y_{it}}$ at the daily level - i.e., we define $\widehat{y_{it}}$ equivalently to x_{it} above - and repeat the above estimation using $\widehat{y_{it}}$ as our dependent variable. The results are virtually unchanged (results not shown).

4.3.5 Fixed effects in expectations

In our previous analysis, we assumed that there is no individual fixed effect in expectations other than the fixed effect attributable to persistent personal happiness. This may not be the case; for example, rich people may be permanently more optimistic than poor people. On one hand, any sort of individual fixed effect in expectations is interesting, since it raises the possibility of composition bias in expectations. However, if individual fixed effects in expectations are caused by differences in psychological traits such as happiness, that is particularly interesting. Thus, we repeat the analysis in Section 4.3.4, but we add covariates representing several slow-changing variables:

- age
- education level
- income
- a dummy for homeownership

[7] This would happen if, for example, shocks to national economic variables such as GDP growth were serially correlated at the 6-month horizon, but if people were not aware of this correlation, leading them to be repeatedly "surprised" by the realizations of these variables.

- wealth invested in stocks
- a dummy for white non-Hispanic race

The estimates of β_P with these covariates are included are shown in Table III.9. The estimates are only slightly changed from the estimates without covariates. Although there may be other omitted variables that account for the effect, it is clear that the effect of persistent personal happiness on expectations is very hard to explain with non-psychological variables.

4.3.6 Model with survey response error

Until now, we have worked with the happiness index x_{it}, instead of the individual answers to the four questions on our questionnaire. However, exploiting the variation between the answers to those four questions may help us get better estimates of β_P and β_T. We do that in this section.

Also, in this section we want to deal explicitly with the fact that our questions do not actually measure true happiness. It is possible that survey respondents differ more in *how they respond to questions* than in how they actually feel. So in this section we include "response errors" in our model of happiness. We consider response errors of four basic types:

- Errors that depend only on the individual (individual fixed effects)
- Errors that depend on individual and the time of the survey
- Errors that depend on the individual and the wording of the question, but are fixed in time
- Errors that depend on the individual, the wording of the question, and the time of the survey

We represent these four response errors as s_i, u_{it}, v_{iq}, and w_{itq}, respectively. For simplicity's sake we assume that all of these response errors show up in the happiness questions, though mathematically it doesn't matter if response errors in the expectation questions exist as well. So our new model of happiness is:

$$x_{itq} = x_t^N + x_i^P + x_{it}^T + s_i + u_{it} + v_{iq} + w_{itq} \tag{13}$$

This assumes that all four questions are noisy measures of the same thing (affect valence, happiness, etc.).

Our new model of expectations is:

$$y_{it} = \alpha + \beta_N x_t^N + \beta_P x_i^P + \beta_T x_{it}^T + \beta_s s_i + \beta_u u_{it} + \beta_v v_{iq} + \beta_w w_{itq} + \varepsilon_{it} \tag{14}$$

Derivations of the estimates for this model are in Appendix III.A.4. The estimate for β_N is unchanged. Two things about the estimates of β_P and β_T are different. First of all, they are no longer estimates of β_P and β_T! They are now estimates of $(1-\theta)\beta_P + \theta\beta_S$ and $(1-\xi)\beta_T + \xi\beta_T$, where θ and ξ are parameters (both between 0 and 1) representing the relative importance of the individual-specific response errors s_i and u_{it}:

$$\theta = \frac{Var(s_i)}{Var(s_i) + Var(x_i^P)} \tag{15}$$

$$\xi = \frac{Var(u_{it})}{Var(u_{it}) + Var(x_{it}^T)} \tag{16}$$

In other words, if you believe that most of the variance in survey responses is due to response error, then the estimates in this paper do not mean much; however, if you believe that true values vary about as much as response error, then the results in this paper stand.

The second change from the previous sections is that our estimates, and the associated R^2 values (again, upper bounds on the true values), are now both multiplied by correction factors that come from the variance across the four affect questions (see Appendix III.A.4.6). The correction factors are 1.38 for persistent personal happiness and 0.536 for transitory personal happiness. Results for the new estimates for β_P and β_T, and the associated R^2 values, can be seen in Table III.11. After accounting for cross-question response errors, the estimates of the effect of persistent personal happiness grow substantially while the estimates of the effect of transitory personal happiness shrink substantially. This confirms our basic story.

4.3.7 Other Emotions

Our survey only contains questions about two kinds of emotion: happiness and sadness. But if we are really going to test the "affective animal spirits" hypothesis, shouldn't we look at other emotions too? What about fear, or excitement, or anger?

Our answer to this question is that different emotional states are probably correlated with each other. A person who answers that she is "happy" is probably more likely to report that she is "excited," and a person who answers that she is "sad" is probably more likely to answer that she is "worried" or "afraid." If different emotions are very correlated, our results hold true for all emotions; only an affect measure that was nearly orthogonal to happiness would still be a candidate for affective animal spirits. Our result is thus much broader than the limited scope of our survey questions might suggest.

To demonstrate this, we do a simple numerical experiment. Suppose that self-reported happiness is equal to true happiness , and that there exists another emotion – call it "Emotion Z" – that is imperfectly correlated with happiness. For simplicity, assume that Emotion Z has only a transitory component, z_{it}. Then we can write:

$$x_{it} = \rho z_{it} + \varsigma_{it} \tag{17}$$

Now assume that affective animal spirits are in fact caused by Emotion Z:

$$y_{it} = \alpha + \beta_z z_{it} + \varepsilon_{it} \tag{18}$$

We can show that R_z^2, the percentage of the variance of expectations explained by swings in Emotion Z, is bounded above by $\frac{1}{\rho}R_T^2$. In order for swings in Emotion Z to explain an economically significant amount of the swings in expectations, ρ must be very very small. For example, if ρ is equal to 0.2, then swings in Emotion Z explain no more than 0.02% of swings in expectations.

4.3.8 Subgroup Happiness

One final interesting possibility is that different demographic subgroups have common emotions. Suppose that there is a national election in the U.S., which the Democrats win. The happiness of Democrats surges; that of Republicans plummets by an equal amount. Since the nation is evenly divided between Republicans and Democrats (suppose the happiness of Independents is unchanged), the election will register no change in "national happiness" as we have defined it up until now. But if Democrats and Republicans have different roles in the economy – for example, if Republicans are more likely to be investors – it is possible to imagine that the emotional reaction to the election could have an effect on the economy. The presence of group effects could introduce additional bias into our estimates of β_T and β_P in the previous section. Fortunately, our data includes many demographic characteristics that can be used to test this hypothesis.

We add two more components to the model of happiness – 1) a “persistent group happiness” component that represents the long-term happiness of a particular group (e.g., if Democrats are on average happier than Republicans), and 2) a “transitory group happiness” component that represents differential changes in these groups that arise in response to national events. Define γ_i as a dummy that indicates membership in some group (-1 or

1). So the model of happiness is now:

$$x_{itq} = x_t^N + x_i^P + x_{it}^T + \gamma_i x_t^G + \gamma_i \phi_G \tag{19}$$

Here, x_t^G is transitory group happiness, and ϕ_G is persistent group happiness. The model of expectations becomes:

$$y_{it} = \alpha + \beta_N x_t^N + \beta_P x_i^P + \beta_T x_{it}^T + \beta_G \gamma_i x_t^G \varepsilon_{it} \tag{20}$$

As shown in Appendices III.A.5.3 and III.A.5.4, the estimates of β_T and β_P in the previous section are now shown to be the sum of three terms instead of two. The new term in the estimate of β_P depends on the square of the parameter ϕ_G, while the new term in the estimate of β_T depends on the variance of x_t^G. To get a sense of how much our estimates of the effects of persistent and transitory personal happiness are altered by the presence of subgroup effects, we estimate ϕ_G^2 and the $Var(x_t^G)$ and compare these point estimates to the estimated variances of the happiness components given in Table III.7. In Appendix III.A.5.6 we show how we estimate these from demographic data; the results are in Table III.12. We see that the point estimate of the square of group happiness is one order of magnitude smaller than the variance of persistent personal happiness, which is consistent with the earlier finding that persistent personal happiness affects expectations even when control variables such as income, wealth, and education are included. The variance of group happiness is even smaller; in fact, that the point estimate is negative for some groups, which is of course an impossible result.[8] Thus, it is unlikely that the presence of group differences in happiness affects our results.

[8]This might indicate misspecification in the model of group happiness; more likely, it simply reflects the large confidence interval around the point estimate.

5 Discussion

Our results are bad news for the "affective animal spirits" hypothesis of business cycles. The upper bounds on the explanatory power of national and transitory personal happiness mean that emotion is not a major cause of "animal spirits"; even swings in the happiness of a key individual or small group of individuals are unlikely to move those individuals' expectations by much. And the effect on real economic variables - at least, if it acts through expectations - must be even smaller. For example, let's take the most optimistic estimate of the effect of swings in consumer confidence on GNP - 26%, the highest value reported by Matsusaka and Sbordone (1995). Using our estimates for R_T^2, we can conclude that no more than 0.026% of swings in GNP can possibly be explained by swings in happiness. Suppose that it is actually fear that drives business cycles, and set the covariance between fear and happiness equal to only -0.2. Then less than 0.005% of the swings in GNP are due to fear. Remember, also, that these are upper bounds on upper bounds; the true value is bound to be much lower.

However, our results may be interesting for the field of finance. In finance, a profitable trading opportunity does not have to have a large R-squared; it just has to exist. Some papers have found that consumer confidence predicts stock returns, but consumer confidence surveys are very infrequent. Happiness surveys, however, are easy to do. Our results show a correlation between happiness (national or personal) and one measure of expectations (bus12). If this component of consumer confidence predicts stock returns, then traders may gain useful information from measuring national happiness, or from paying attention to public data on happiness as it emerges.

The hypothesis of "affective animal spirits" is often put forth as an alternative to Rational Expectations. Barsky and Sims (2008) showed, in the context of a specific macroeconomic model, that autonomous movements in consumer confidence don't cause swings in real economic variables. The results in this paper yield a similar result. Does this mean that our results support Rational Expectations? Actually, no. Rational Expectations requires expectations to be unbiased estimates of the predictions of the best model of the economy. But if expectations depend on psychological traits, this cannot be the case. Suppose that half of the population is naturally optimistic all the time, and half is naturally pessimistic, regardless of economic conditions. A composition shock - say, if the pessimistic people all decided to take the day off - would cause a bias to appear in the expectations of the representative agent.

In Section 4.3.3, we found that persistent personal happiness has a statistically significant effect on most survey measures of economic expectations. Persistent personal happiness is a psychological trait. This means that if A) survey responses are a good measure of expectations, and if B) survey responses are a good measure of true happiness, then this finding indicates the presence of composition effects in expectations. Strict versions of Rational Expectations theory hold that composition effects in expectations can't exist, because aggregate expectations would then be vulnerable to a composition shock (for example, if a bunch of happy people suddenly moved to Zambia). Because the economy does not usually suffer large composition shocks, the anomaly is unlikely to make a big difference in economic outcomes. But it may signal the existence of a wider class of ways in which individual psychological traits cause departures from rationality.

Our results are of minor importance for the cognitive economics literature. Studies have shown that recessions make people less happy. Our negative results for national and transitory personal happiness show that this effect probably does not work through expectations. In other words, recessions are not making people unhappy because they are making them pessimistic. This means that recessions' adverse effect on happiness probably cannot be allayed by taking steps to raise expectations about the economy's long-term performance.

6 Conclusion

In this paper, we investigated the relationship between affect and economic expectations, as measured by surveys. We found mostly no correlation between average national happiness and either average expectations or individual expectations, with one possible exception (the variable bus12), which deserves further study. In fact, we found only small and short-lived movements in average national happiness, meaning that the "national mood" is unlikely to drive much of anything. At the individual level, we found that the causal effect of happiness swings on expectation swings, at the 6-month frequency, is much smaller than the cross-sectional correlation; generally happy people are generally more optimistic, but this is a long-term fixed effect. We also found that fluctuations in happiness explain only a tiny percent of the observed fluctuations in expectations. This result is robust to question-specific response errors. It is also reasonably robust to misspecification of the relevant affective state; our results imply that fear, excitement, and other emotions are also unlikely to be significant causes of business cycles. However, we did find that individual fixed effects in happiness are associated with more optimistic expectations

The negative results in this paper deal a blow to the popular notion of “animal spirits” as being an emotional phenomenon. Even if business cycles can be caused by changes in the public’s expectations, those expectation changes are almost certainly not caused by changes in the national mood, or in the mood of any special subset of the population. Although one might search for other emotions to explain expectation movements, such emotions would have to be unrelated to happiness; thus, we believe that it will be much more useful for advocates of “animal spirits” theories of the business cycle to look at expectation-formation processes rather than emotional factors. On the other hand, our results raise the possibility that survey measures of affect might be an important piece of data for financial traders. Also, the importance of slowly-changing psychological traits on expectations would be troubling for certain theories of individual rationality; further study, to confirm this effect and to establish the causality between fixed effects in expectations and fixed effects in happiness, is warranted.

Future research in this area should also focus on using a more diverse set of measures of both affect and expectations. Alternate measures of expectations might include betting on futures markets. Alternate measures of emotion might include questions on a wider range of emotions, or even neurological studies using fMRI to track emotional states.

Tables and Figures

Figure III.1: Bus12 and Happiness (Monthly Aggregates)

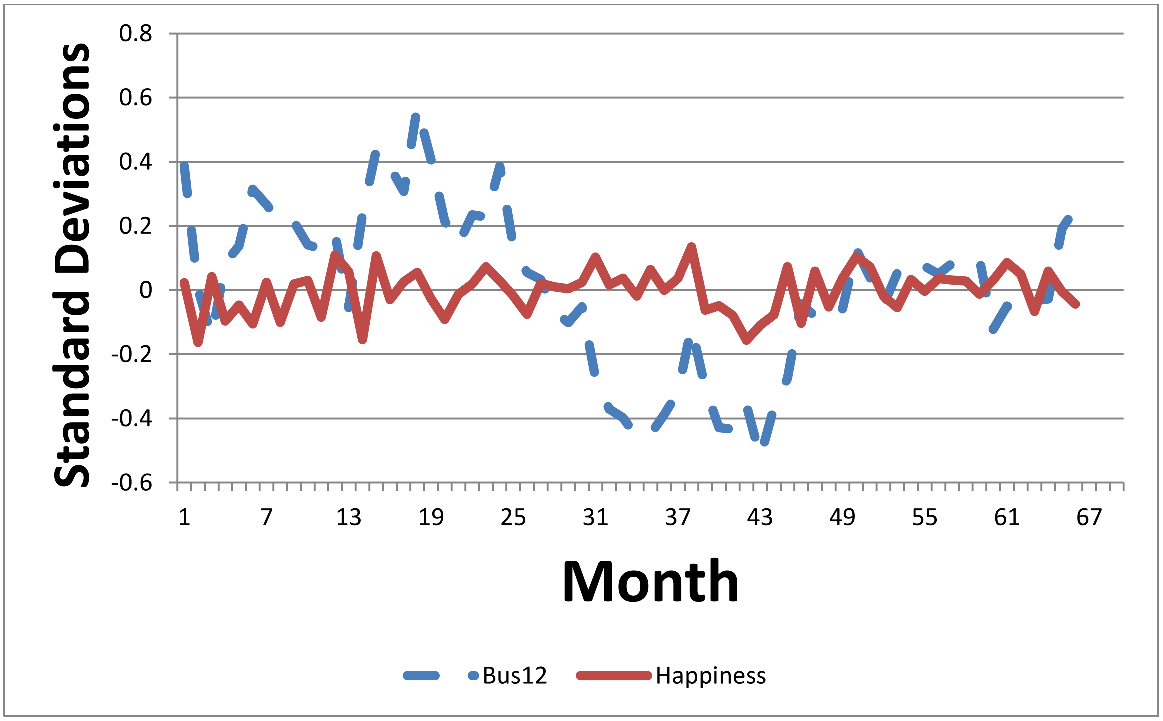

Table III.1a: Text of Consumer Confidence Questions

pago	"Would you say that you (and your family living there) are better off or worse off financially than a year ago?"
pexp	"Do you think that a year from now you (and your family living there) will be better off financially, or worse off, or just about the same as now?"
bus12	"Do you think that during the next 12 months we'll have good times financially, or bad times, or what?"
bus5	"Which would you say is more likely – that in the country as a whole we'll have continuous good times during the next 5 years or so, or that we will have periods of widespread unemployment and depression, or what?"
dur	"Generally speaking, do you think now is a good time or a bad time for people to buy major household items?"

Table III.1b: Text of Other Expectations Questions

v228	"Compared with 5 years ago, do you think the chances that you (and your husband/wife) will have a comfortable retirement have gone up, gone down, or remained about the same?"
hom	"Generally speaking, do you think now is a good time or a bad time to buy a house?"
car	"Do you think the next 12 months will be a good time or a bad time to buy a vehicle?"
homeval	"Do you think the present value of your home – I mean what it would bring you if you sold it today – has increased compared with a year ago, decreased, or stayed about the same?"
bago	"Would you say that at the present time business conditions are better or worse than they were a year ago?"
bexp	"A year from now, do you expect that in the country as a whole business conditions will be better, or worse than they are at present, or just about the same?"
govt	"As to the economic policy of the government – I mean steps taken to fight inflation or unemployment – would you say the government is doing a good job, only fair, or a poor job?"
unemp	"How about people out of work during the coming 12 months – do you think that there will be more unemployment than now, about the same, or less?"

Table III.1c: Text of Happiness Questions

h1	"Much of the time during the past week, you were happy. Would you say yes or no?"
h2	"Much of the time during the past week, you felt depressed. Would you say yes or no?"
h3	"Much of the time during the past week, you enjoyed life. Would you say yes or no?"
h4	"Much of the time during the past week, you felt sad. Would you say yes or no?"

Table III.2: Descriptive Statistics

Variable	Mean (0-100)	Std. Dev.	Monthly Autocorrelation
Pago	45.37	42.18	0.90
pexp	56.49	36.83	0.65
bus12	36.83	46.57	0.81
bus5	42.19	43.82	0.69
dur	68.79	45.64	0.86
v228	39.38	35.63	0.93
hom	71.10	45.05	0.86
car	64.70	47.30	0.68
homeval	48.74	40.50	0.96
bago	31.31	44.08	0.92
bexp	50.28	35.30	0.87
govt	39.34	34.89	0.83
unemp	65.10	33.67	0.94
happiness			-0.09

Table III.3: Happiness Question Correlations

	h1	h2	h3	h4
h1	1			
h2	0.583	1		
h3	0.572	0.464	1	
h4	0.526	0.609	0.417	1

Table III.4: Autocorrelations of Aggregate Variables

Lag (months)	Autocorrelation of Happiness	Autocorrelation of Bus12
1	-0.09	0.81
2	0.09	0.64
3	0.03	0.58
4	-0.13	0.53
5	0.14	0.50

Table III.5: VAR of Bus12 and Happiness

LHS Variable	Lag (months)	RHS Variable Bus12	Happiness
Bus12			
	1	0.912***	0.161**
	2	-0.114	-0.368***
	3	0.149	0.326***
	4	0.045	-0.177*
	5	-0.095	0.0784
Happiness			
	1	0.016	0.199
	2	-0.177	0.050
	3	-0.092	0.119
	4	-0.552**	-0.140
	5	0.279	0.044

Note: *** = significant at the 1% level; ** = significant at the 5% level; * = significant at the 10% level

Table III.6: OLS of Expectations on Happiness

LHS Variable	Effect of Happiness	Std. Error
pago	0.163	0.006
pexp	0.066	0.007
bus12	0.110	0.007
bus5	0.137	0.007
dur	0.083	0.007
v228	0.119	0.006
hom	0.086	0.006
car	0.085	0.007
homeval	0.039	0.007
bago	0.081	0.007
bexp	0.081	0.007
govt	0.101	0.007
unemp	0.072	0.008

Note: all coefficients listed are significant at the 1% level.

Table III.7: Estimated Variances of Happiness Components

Component	Estimated Variance
National Happiness	0.006
Transitory Personal Happiness	0.431
Persistent Personal Happiness	0.561

Note: Total happiness is normalized to have a variance of 1.

Table III.8: Effect of National Happiness

LHS Variable	Effect of National Happiness	Std. Error	R^2_N
pago	-0.254	0.286	0.000
pexp	0.112	0.272	0.000
bus12	0.505*	0.263	0.002
bus5	0.268	0.260	0.000
dur	0.252	0.288	0.000
v228	0.242	0.267	0.000
hom	-0.108	0.288	0.000
car	-0.464	0.317	0.001
homeval	0.277	0.259	0.000
bago	0.538*	0.304	0.002
bexp	0.432	0.296	0.001
govt	-0.114	0.283	0.001
unemp	0.495	0.288	0.000

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table III.9: Effects of Personal Happiness Components

LHS Variable	Transitory Effect	Persistent Effect	Persistent Effect (Covariates Included)	R^2_T
pago	.054***	.273***	.316***	
	(.009)	(.021)	(.040)	0.001
pexp	.027***	.099***	.317***	
	(.010)	(.021)	(.038)	0.000
bus12	.052***	.153***	.318***	
	(.010)	(.022)	(.043)	0.001
bus5	.022**	.270***	.319***	
	(.010)	(.022)	(.041)	0.000
dur	.025***	.143***	.320***	
	(.011)	(.022)	(.040)	0.000
v228	.052***	.213***	.321***	
	(.009)	(.021)	(.042)	0.001
hom	.007	.185***	.322***	
	(.010)	(.021)	(.036)	0.000
car	.013	.165***	.323***	
	(.010)	(.022)	(.039)	0.000
homeval	.007	.061***	.324***	
	(.009)	(.026)	(.043)	0.000
bago	.036	.091***	.325***	
	(.010)	(.022)	(.042)	0.001
bexp	030***	.113***	.326***	
	(.010)	(.021)	(.040)	0.000
govt		.188***	.327***	
	.035***	(.022)	(.041)	0.000
unemp	.045***	.095***	.328***	
	(.010)	(.021)	(.040)	0.001

Note: *** = significant at the 1% level, ** = significant at the 5% level, * = significant at the 10% level

Table III.10a: Covariance Matrix for Transitory Correction Factor

	$\Delta x_{it,1}$	$\Delta x_{it,2}$	$\Delta x_{it,3}$	$\Delta x_{it,4}$
$\Delta \hat{x}_{it,1}$	2.223	1.006	0.697	0.981
$\Delta \hat{x}_{it,2}$	0.999	2.136	0.516	1.125
$\Delta \hat{x}_{it,3}$	0.698	0.523	1.230	0.501
$\Delta \hat{x}_{it,4}$	0.983	1.130	0.499	2.720

Correction Factor = **0.536**

Table III.10b: Covariance Matric For Persistent Correction Factor

	$x_{i1,1}$	$x_{i1,2}$	$x_{i1,3}$	$x_{i1,4}$
$\hat{x}_{i2,1}$	0.553	0.449	0.373	0.432
$\hat{x}_{i2,2}$	0.463	0.560	0.319	0.515
$\hat{x}_{i2,3}$	0.356	0.303	0.323	0.288
$\hat{x}_{i2,4}$	0.485	0.534	0.340	0.608

Correction Factor = **1.38**

Table III.11: Personal Happiness (With Response Errors)

LHS Variable	Transitory Effect	Persistent Effect	R^2_T
pago	0.029	0.376	0.000
pexp	0.015	0.136	0.000
bus12	0.028	0.212	0.000
bus5	0.012	0.373	0.000
Dur	0.013	0.197	0.000
v228	0.028	0.294	0.000
Hom	0.004	0.255	0.000
Car	0.007	0.228	0.000
Homeval	0.004	0.084	0.000
Bago	0.019	0.125	0.000
bexp	0.016	0.156	0.000
govt	0.019	0.260	0.000
unemp	0.024	0.132	0.000

Note: Standard errors are the same as in Table 8.

Table III.12: Group Effects

Group	Square of persistent group happiness	Variance of transitory group happiness
Political	0.005	0.005
Over 44	0.000	-0.002
Income over $100k	0.013	-0.013
Homeowner	0.015	-0.017
Stockholder	0.025	-0.024
College Degree	0.011	-0.014
Race (White/Nonwhite)	0.003	0.000

Appendix III.A: Models and Derivations

A.1. Definitions

R-squared values:

$$R^2{}_N = \beta_N{}^2 Var(x_t^N)$$

$$R^2{}_T = \beta_T{}^2 \frac{Var(x_{it}^T)}{Var(\Delta y_{it})}$$

$$R^2{}_P = \beta_P{}^2 Var(x_i^P)$$

Note that x_{it_2} represents individual i 's (self-reported) happiness on the date of her second interview, and Δx_{it} represents an individual's change in happiness between her first and second interview, etc.

A.2. Basic Model

A.2.1. Effect of x_t^N

An instrumental variables regression of y_{it} on x_{it} , using x_{-it} as an instrument for x_{it} , yields the following estimator:

$$\tilde{\beta}_N = \frac{Cov(y_{it}, x_{-it})}{Cov(x_{it}, x_{-it})}$$

Expanding the numerator and denominator, we get:

$$Cov(y_{it}, x_{-it}) = Cov(\alpha + \beta_N x_t^N + \beta_P x_i^P + \beta_T x_{it}^T + \varepsilon_{it}, x_t^N + x_{-i}^P + x_{-it}^T)$$

$$Cov(x_{it}, x_{-it}) = Cov(x_t^N + x_i^P + x_{it}^T, x_t^N + x_{-i}^P + x_{-it}^T)$$

To simplify these expressions, we use the assumptions:

$$Cov(\varepsilon_{it}, x_j{}^P) = Cov(\varepsilon_{it}, x_{jt}{}^T) = 0$$

$$Cov(x_{it}^N, x_i^P) = Cov(x_{it}^T, x_i^P) = Cov(x_{it}^N, x_i^T) = 0$$

$$Cov(x_i^P, x_j^P) = Cov(x_{it}^T, x_{jt}^T) = 0 \text{ where } j \neq i$$

This leaves us with simplified expressions for numerator and denominator:

$$Cov(y_{it}, x_{-it}) = \beta_N Var(x_t^N) + Cov(\varepsilon_{it}, x_t^N)$$

$$Cov(x_{it}, x_{-it}) = Var(x_t^N)$$

Dividing these expressions and simplifying, we get:

$$\beta_N = \frac{Cov(y_{it}, x_{-it})}{Cov(x_{it}, x_{-it})} - \frac{Cov(\varepsilon_{it}, x_t^N)}{Var(x_t^N)}$$

We have assumed that the second term is positive. Thus, $\tilde{\beta}_N$ is an estimate of an upper bound on β_N.

A.2.2. Effect of x_{it}^T

An instrumental variables regression of Δy_{it} on Δx_{it}, using Δx_{it} as an instrument for Δx_{it}, yields the following estimator:

$$\tilde{\beta}_T = \frac{Cov(\Delta y_{it}, \Delta x_{it})}{Cov(\Delta x_{it}, \Delta x_{it})}$$

Expanding the numerator and denominator, we get:

$$\begin{aligned} Cov(y_{it_2} - y_{it_1}, [x_{it_2} - x_{it_1}] - [x_{-it_2} - x_{-it_1}]) = Cov(\beta_N [x_{t_2}^N - x_{t_1}^N] + \beta_T [x_{it_2}^T - x_{it_1}^T] \\ + [\varepsilon_{it_2} - \varepsilon_{it_1}], [x_{it_2}^T - x_{it_1}^T] - [x_{-it_2}^T - x_{-it_1}^T]) \end{aligned}$$

$$\begin{aligned} Cov(x_{it_2} - x_{it_1}, [x_{it_2} - x_{it_1}] - [x_{-it_2} - x_{-it_1}]) = \\ Cov([x_{t_2}^N - x_{t_1}^N] + [x_{it_2}^T - x_{it_1}^T], [x_{it_2}^T - x_{it_1}^T] - [x_{-it_2}^T - x_{-it_1}^T]) \end{aligned}$$

To simplify these expressions, we use the following additional assumptions:

$$Cov(\varepsilon_{it_1}, x_{it_2}^T) = Cov(\varepsilon_{it_2}, x_{it_1}^T) = 0$$

$$Cov(\varepsilon_{it_2}, x_{jt_2}^T) = Cov(\varepsilon_{it_1}, x_{jt_2}^T) = Cov(\varepsilon_{it_2}, x_{jt_1}^T) = Cov(\varepsilon_{it_1}, x_{jt_1}^T) = 0$$

This leaves us with simplified expressions for the numerator and denominator:

$$Cov(y_{it_2} - y_{it_1}, [x_{it_2} - x_{it_1}] - [x_{-it_2} - x_{-it_1}]) = 2\left[\beta_T Var(x_{it}^T) + Cov(\varepsilon_{it}, x_{it}^T)\right]$$

$$Cov(x_{it_2} - x_{it_1}, [x_{it_2} - x_{it_1}] - [x_{-it_2} - x_{-it_1}]) = 2Var(x_{it}^T)$$

Dividing these expressions, we get:

$$\beta_T = \frac{Cov(\Delta y_{it}, \Delta x_{it})}{Cov(\Delta x_{it}, \Delta x_{it})} - \frac{Cov(\varepsilon_{it}, \Delta x_{it})}{Cov(\Delta x_{it}, \Delta x_{it})}$$

Again, we have assumed that the second term is positive. Thus, $\tilde{\beta}_T$ is an estimate of an upper bound on β_T.

A.2.3. Effect of x_i^P

An instrumental variables regression of y_{it_2} on x_{it_2}, using x_{it_1} as an instrument for x_{it_2}, yields the following estimator:

$$\tilde{\beta}_P = \frac{Cov(y_{it_2}, x_{it_1})}{Cov(x_{it_1}, x_{it_2})}$$

Expanding the numerator and denominator, we get:

$$Cov(y_{it_2}, x_{it_1} - x_{-it_1}) = Cov(\alpha + \beta_N x_{t_2}^N + \beta_P x_i^P + \beta_T x_{it_2}^T + \varepsilon_{it_2}, x_i^P - x_{-it_1}^P + x_{it_1}^T - x_{-it_1}^T)$$

$$Cov(x_{it_2}, x_{it_1} - x_{-it_1}) = Cov(x_{t_2}^N + x_i^P + x_{it_2}^T, x_i^P - x_{-it_1}^P + x_{it_1}^T - x_{it_1}^T)$$

To simplify these expressions, we use assumptions already listed above. This leaves us with simplified expressions for numerator and denominator:

$$Cov(y_{it_2}, x_{it_1} - x_{-it_1}) = \beta_P Var(x_i^P)$$

$$Cov(x_{it_2}, x_{it_1} - x_{-it_1}) = Var(x_i^P)$$

Dividing these expressions, we get:

$$\beta_P = \frac{Cov(y_{it_2}, x_{it_1})}{Cov(x_{it_1}, x_{it_2})}$$

Thus given our assumptions, $\tilde{\beta}_P$ is an unbiased estimator of β_P.

A.3. Variances of Happiness Components

Our model of observed happiness, given in Equation 2, is:

$$x_{it} = x_t^N + x_i^P + x_{it}^T$$

As shown in the previous section the variances of the happiness components can be computed thus:

$$Var(x_t^N) = Cov(x_{it}, x_{-it})$$

$$Var(x_i^P) = Cov(x_{it_2}, x_{it_1} - x_{-it_1})$$

$$Var(x_{it}^T) = \frac{1}{2} Cov(\Delta x_{it}, \Delta x_{it} - \Delta x_{-it})$$

The estimated variances of the three happiness components are listed in Table 8.

A.4. Model with response errors

A.4.1. Happiness with response errors

Our model of happiness with response errors, given in Equation 12, is:

$$x_{itq} = x_t^N + x_i^P + x_{it}^T + s_i + u_{it} + v_{iq} + w_{itq}$$

x_t^N, x_i^P, x_{it}^T, s_i, u_{it}, v_{iq}, and w_{itq} are taken to be iid mean-zero random variables that are orthogonal to one another.

In our data set, self-reported happiness is just the average of these four components. We write:

$$x_{it} = x_t^N + x_i^P + x_{it}^T + s_i + u_{it} + v_i + w_{it}$$

Where:

$$x_{it} \equiv (x_{it1} + x_{it2} + x_{it3} + x_{it4}) / 4$$

$$v_i \equiv (v_{i1} + v_{i2} + v_{i3} + v_{i4}) / 4$$

$$w_{it} \equiv (w_{it1} + w_{it2} + w_{it3} + w_{it4}) / 4$$

Our expressions for the variances of the happiness components now represent the sum of the variances of the happiness components and (where applicable) of the response errors that are not observably different from the happiness components:

$$Cov(x_{it}, x_{-it}) = Var(x_t^N)$$

$$Cov(x_{it_2}, x_{it_1} - x_{-it_1}) = Var(x_i^P) + Var(s_i)$$

$$\frac{1}{2} Cov(\Delta x_{it}, \Delta x_{it} - \Delta x_{-it}) = Var(x_{it}^T) + Var(u_{it})$$

A.4.2. Expectations with response errors

We may safely assume that expectation questions produce similar "response errors". Allowing that expectation-question response errors may be correlated with happiness-question response errors, our model of expectations becomes:

$$y_{it} = \alpha + \beta_N x_t^N + \beta_P x_i^P + \beta_T x_{it}^T + \beta_s s_i + \beta_u u_{it} + \varepsilon_{it}$$

This model includes the effect of a respondent's mood on their expectations (β_P for the long-term components of mood, β_T for the transitory components); the relationship between a respondent's tendency to answer happiness-related questions in a particular way and his/her tendency to answer expectation-related questions in a particular way (β_s and β_u); and the potential for the social construction of expectations (β_N)[1]. Again, we assume that the error term, ε_{it}, is mean-zero and is uncorrelated with x_i^P, s_i, and u_{it}.

Our purpose is, again, to identify the coefficients β_N, β_P, and β_T. For the latter two, this will no longer be possible; we will only be able to identify expressions that contain the effects of the response styles, β_s and β_u. Obtaining economically significant results must depend on our belief that the variances of s_i and u_{it} are small.

A.4.3. Effect of x_t^N with response errors

We now define the "daily other mean" of happiness, x_{-it}, as:

[1] The question-specific response style terms from the measurement model - v_{iq} and w_{itq} - do not enter the structural model, since a respondent's answer to an expectation-related question is presumably not related to the particular set of happiness-related questions that were asked on his/her survey – especially because the latter are asked after the former.

$$x_{-it} = x_t^N + x_{-it}^P + x_{-it}^T + s_{-it} + u_{-it} + v_{-it} + w_{-it}$$

Using the same regression as in the basic model, our estimator is:

$$\tilde{\beta}_N = \frac{Cov(y_{it}, x_{-it})}{Cov(x_{it}, x_{-it})}$$

We can rewrite the numerator and denominator:

$$\begin{aligned} Cov(y_{it}, x_{-it}) = Cov(&\alpha + \beta_N x_t^N + \beta_P x_i^P + \beta_T x_{it}^T + \beta_s s_i + \beta_u u_{it} + \varepsilon_{it}, \\ &x_t^N + x_{-i}^P + x_{-it}^T + s_{-i} + u_{-it} + v_{-i} + w_{-it}) \end{aligned}$$

$$\begin{aligned} Cov(x_{it}, x_{-it}) = Cov(&x_t^N + x_i^P + x_{it}^T + s_i + u_{it} + v_i + w_{it}, \\ &x_t^N + x_{-i}^P + x_{-it}^T + s_{-i} + u_{-it} + v_{-i} + w_{-it}) \end{aligned}$$

We can simplify these expressions using the orthogonality assumptions:

$$Cov(\varepsilon_{it}, x_j{}^P) = Cov(\varepsilon_{it}, x_{jt}{}^T) = Cov(\varepsilon_{it}, s_j) =$$
$$Cov(\varepsilon_{it}, u_{jt}) = Cov(\varepsilon_{it}, v_j) = Cov(\varepsilon_{it}, w_{jt}) = 0$$

$$Cov(x_t^N, s_j) = Cov(x_t^N, u_{jt}) = Cov(x_t^N, v_j) = Cov(x_t^N, w_{jt}) = 0$$

$$Cov(x_i^P, s_j) = Cov(x_i^P, u_{jt}) = Cov(x_i^P, v_j) = Cov(x_i^P, w_{jt}) = 0$$

$$Cov(x_{it}^T, s_j) = Cov(x_{it}^T, u_{jt}) = Cov(x_{it}^T, v_j) = Cov(x_{it}^T, w_{jt}) = 0$$

$$Cov(s_i, u_{it}) = Cov(s_i, v_i) = Cov(s_i, w_{it}) = Cov(u_{it}, v_j) =$$
$$Cov(u_{it}, w_{jt}) = Cov(v_i, w_{jt}) = 0$$

$$Cov(s_i, s_j) = Cov(v_i, v_j) = Cov(w_{it}, w_{jt}) = 0$$

These let us cancel most of the terms in the numerator and denominator, leaving us with:

$$Cov(y_{it}, x_{-it}) = \beta_N Var(x_t^N) + Cov(\varepsilon_{it}, x_t^N)$$

$$Cov(x_{it}, x_{-it}) = Var(x_t^N)$$

Dividing then yields:

$$\beta_N = \frac{Cov(y_{it}, x_{-it})}{Cov(x_{it}, x_{-it})} - \frac{Cov(\varepsilon_{it}, x_t^N)}{Var(x_t^N)}$$

Thus, the presence of response errors does not change our result for the effect of x_t^N.

A.4.4. Effect of x_{it}^T with response errors

This time, our estimator is:

$$\tilde{\beta}_T = \frac{Cov(\Delta y_{it}, \Delta x_{it})}{Cov(\Delta x_{itr}, \Delta x_{itq})}$$

Expanding the numerator and denominator, we have:

$$\begin{aligned} Cov(y_{it_2} - y_{it_1}, [x_{it_2} - x_{it_1}] - [x_{-it_2} - x_{-it_1}]) = Cov(&\beta_N [x^N_{t_2} - x^N_{t_1}] + \beta_T [x^T_{it_2} - x^T_{it_2}] \\ &+ \beta_u [u_{it_2} - u_{it_1}] + [\varepsilon_{it_2} - \varepsilon_{it_1}], \\ &[x^T_{it_2} - x^T_{it_1}] + [u_{it_2} - u_{it_1}] + [w_{it_2} - w_{it_1}] \\ &-[x^T_{-it_2} - x^T_{-it_1}] - [u_{-it_2} - u_{-it_1}] - [w_{-it_2} - w_{-it_1}]) \end{aligned}$$

$$\begin{aligned} Cov(x_{it_2q} - x_{it_1q}, [x_{it_2r} - x_{it_1r}] - [x_{-it_2r} - x_{-it_1r}]) = Cov([x^N_{t_2} - x^N_{t_1}] + [x^T_{it_2} - x^T_{i1}] + \\ [u_{it_2} - u_{it_1}] + [w_{it_2q} - w_{it_1q}], \\ [x^T_{it_2} - x^T_{it_1}] + [u_{it_2} - u_{it_1}] + [w_{it_2r} - w_{it_1r}] - \\ [x^T_{-it_2} - x^T_{-it_1}] - [u_{-it_2} - u_{-it_1}] - [w_{-it_2r} - w_{-it_1r}]) \end{aligned}$$

We can simplify these with the aforementioned assumptions, plus the following:

$$Cov(\varepsilon_{i1}, x^T_{i2}) = Cov(\varepsilon_{i2}, x^T_{j2}) = 0$$

$$Cov(\varepsilon_{i1}, u_{i2}) = Cov(\varepsilon_{i1}, w_{i2}) = 0$$

Simplified, the numerator and denominator become:

$$Cov(y_{it_2} - y_{it_1}, [x_{it_2} - x_{it_1}] - [x_{-it_2} - x_{-it_1}]) = 2\left[\beta_T Var(x^T_{it}) + \beta_u Var(u_{it}) + Cov(\varepsilon_{it}, x^T_{it})\right]$$

$$Cov(x_{it_2} - x_{it_1}, [x_{it_2} - x_{it_1}] - [x_{-it_2} - x_{-it_1}]) = 2\left[Var(x^T_{it}) + Var(u_{it})\right]$$

We take the ratio of the two, to get:

$$(1-\xi)\beta_T+\xi\beta_u=\frac{Cov(y_{i2}-y_{i1},[x_{i2}-x_{i1}]-[x_{-i2}-x_{-i1}])}{Cov(x_{i2q}-x_{i1q},[x_{i2r}-x_{i1r}]-[x_{-i2r}-x_{-i1r}])}+\frac{Cov(\varepsilon_{it},x_{it}^T)}{Var(x_{it}^T)+Var(u_{it})}$$

Where:

$$\xi\equiv\frac{Var(u_{it})}{[Var(x_{it}^T)+Var(u_{it})]}$$

As before, if the variance of u_{it} is small relative to the variance of x_{it}^T, then $(1-\xi)\beta_T+\xi\beta_u$ is close to β_T.

We can decompose the moment ratio for $(1-\xi)\beta_T+\xi\beta_u$ into the product of a moment ratio and a "correction factor":

$$\begin{aligned}(1-\xi)\beta_T+\xi\beta_u&=\frac{Cov(y_{i2}-y_{i1},[x_{i2}-x_{i1}]-[x_{-i2}-x_{-i1}])}{Cov(x_{i2q}-x_{i1q},[x_{i2r}-x_{i1r}]-[x_{-i2r}-x_{-i1r}])}+\frac{Cov(\varepsilon_{it},x_{it}^T)}{Var(x_{it}^T)+Var(u_{it})}\\&=\left[\frac{Cov(x_{i2}-x_{i1},[x_{i2}-x_{i1}]-[x_{-i2}-x_{-i1}])}{Cov(x_{i2q}-x_{i1q},[x_{i2r}-x_{i1r}]-[x_{-i2r}-x_{-i1r}])}\right]\bullet\\&\quad\left[\frac{Cov(y_{i2}-y_{i1},[x_{i2}-x_{i1}]-[x_{-i2}-x_{-i1}])}{Cov(x_{i2}-x_{i1},[x_{i2}-x_{i1}]-[x_{-i2}-x_{-i1}])}\right]\\&\quad+\frac{Cov(\varepsilon_{it},x_{it}^T)}{Var(x_{it}^T)+Var(u_{it})}\end{aligned}$$

The "correction factor" $\left[\frac{Cov(x_{i2}-x_{i1},[x_{i2}-x_{i1}]-[x_{-i2}-x_{-i1}])}{Cov(x_{i2q}-x_{i1q},[x_{i2r}-x_{i1r}]-[x_{-i2r}-x_{-i1r}])}\right]$ must only be estimated once for all expectation variables. The denominator can be found by averaging the off-diagonal elements of a cross-question covariance matrix of $Cov(x_{i2q}-x_{i1q},[x_{i2r}-x_{i1r}]-[x_{-i2r}-x_{-i1r}])$ for all pairs of q and r (shown in Table 11).

Our estimate for the "correction factor" for β_T in this simple model is 1.38, and our estimates for $(1-\xi)\beta_T + \xi\beta_u$ can be found in Table 12. A quick inspection of this table reveals that most estimates of $(1-\xi)\beta_T + \xi\beta_u$ are neither large nor significant, a fact which will be important in showing that transitory movements in happiness have relatively little effect on economic expectations.

A.4.5. Effect of x_i^P with response errors

Our estimator is now:

$$\tilde{\beta}_P = \frac{Cov(y_{it_2}, x_{it_1})}{Cov(x_{it_2 q}, x_{it_1 r})}$$

Expanding the numerator and denominator, we have:

$$\begin{aligned} Cov(y_{it_2}, x_{it_1} - x_{-it_1}) = Cov(&\alpha + \beta_N x_{t_2}^N + \beta_P x_i^P + \beta_T x_{it_2}^T + \\ &\beta_s s_i + \beta_u u_{it_2} + \varepsilon_{it_2}, \\ &x_i^P - x_{-it_1}^P + x_{it_1}^T - x_{-it_1}^T + (s_i + u_{it_1} + v_i + w_{it_1}) - \\ &(s_{-it_1} + u_{-it_1} + v_{-it_1} + w_{-i1})) \end{aligned}$$

$$\begin{aligned} Cov(x_{it_2 q}, x_{it_1 r} - x_{-it_1 r}) = Cov(&x_{t_2}^N + x_i^P + x_{it_2}^T + \\ &(s_i + u_{it_2} + v_{iq} + w_{it_2 q}), \\ &x_i^P - x_{-it_1}^P + x_{it_1}^T - x_{it_1}^T + \\ &(s_i + u_{it_1} + v_{ir} + w_{it_1 r}) - \\ &(s_{-i} + u_{-it_1} + v_{-ir} + w_{-it_1 r})) \end{aligned}$$

With the orthogonality assumptions already listed above, these simplify to:

$$Cov(y_{i2}, x_{i1} - x_{-i1}) = \beta_P Var(x_i^P) + \beta_s Var(s_i)$$

$$Cov(x_{i2q}, x_{i1r} - x_{-i1r}) = Var(x_i^P) + Var(s_i)$$

Dividing, we get:

$$(1-\theta)\beta_P + \theta\beta_s = \frac{Cov(y_{i2}, x_{i1} - x_{-i1})}{Cov(x_{i2q}, x_{i1r} - x_{-i1r})}$$

Where:

$$\theta \equiv \frac{Var(s_i)}{[Var(x_i^P) + Var(s_i)]}$$

As previously stated, if the variance of s_i is small relative to the variance of x_i^P - if the response error matters less than the true emotional process driving the response - then $(1-\theta)\beta_P + \theta\beta_s$ is close to β_P.

Conceptually, this moment ratio "cleans out" the effects of the transient components of happiness by instrumenting for today's happiness with happiness at a different date[2].

As before, we can decompose the moment ratio for $(1-\theta)\beta_P + \theta\beta_s$ into the product of a moment ratio and a "correction factor":

$$(1-\theta)\beta_P + \theta\beta_s = \frac{Cov(y_{i2}, x_{i1} - x_{-i1})}{Cov(x_{i2q}, x_{i1r} - x_{-i1r})} = \frac{Cov(x_{i2}, x_{i1} - x_{-i1})}{Cov(x_{i2q}, x_{i1r} - x_{-i1r})} \frac{Cov(y_{i2}, x_{i1} - x_{-i1})}{Cov(x_{i2}, x_{i1} - x_{-i1})}$$

---

[2] As a robustness check, we also estimated $\frac{Cov(y_{i1}, x_{i2} - x_{-i2})}{Cov(x_{i1q}, x_{i2r} - x_{-i2r})}$ for each dependent variable; in theory, this should yield the same result.

In order to test that $(1-\theta)\beta_P + \theta\beta_s$ is significantly nonzero, we must only test the significance of the second term in this product, $\frac{Cov(y_{i2}, x_{i1} - x_{-i1})}{Cov(x_{i2}, x_{i1} - x_{-i1})}$ [3]. Similarly, the "correction factor" $\frac{Cov(x_{i2}, x_{i1} - x_{-i1})}{Cov(x_{i2q}, x_{i1r} - x_{-i1r})}$ must only be estimated once, which will be useful when comparing β_P and β_T.

Our estimate for the "correction factor" for β_P in this simple model is 1.07, and our estimates for $(1-\theta)\beta_P + \theta\beta_s$ can be found in Table 12. A glance at this table shows that most estimates of $(1-\theta)\beta_P + \theta\beta_s$ are significant (with the exception of estimates for price-related expectation variables), and the point estimates are generally in the 0.1-0.3 range.

A.4.6. Proof that estimated R² is an upper bound on the true value

As shown above:

$$\frac{1}{2}Cov(\Delta x_{it}, \Delta x_{it} - \Delta x_{-it}) = Var(x_{it}^T) + Var(u_{it})$$

Our R^2 measure is given by:

$$R^2{}_T = CF_T * \tilde{\beta}_T{}^2 * \frac{1}{2}\frac{Cov(\Delta x_{it}, \Delta x_{it} - \Delta x_{-it})}{Var(\Delta y_{it})} = CF_T * \tilde{\beta}_T{}^2 \left(Var(x_{it}^T) + Var(u_{it})\right)$$

Rearranging the expression for $\tilde{\beta}_T$ defined above, we have:

---

[3] Confidence intervals for $(1-\theta)\beta_P + \theta\beta_s$, though, must be estimated either using the original moment ratio, or by the delta method.

$$\tilde{\beta}_T = \frac{1}{CF_T}\left[\frac{Var(x_{it}^T)}{[Var(x_{it}^T)+Var(u_{it})]}\beta_T + \frac{Var(u_{it})}{[Var(x_{it}^T)+Var(u_{it})]}\beta_u\right] + bias$$

Where the bias term is known to be positive.

Plugging this into the previous equation gives us:

$$R^2{}_T = \beta_T{}^2 \frac{Var(x_{it}^T)}{Var(\Delta y_{it})} + \beta_u{}^2 \frac{Var(u_{it})}{Var(\Delta y_{it})} + 2\frac{\beta_T \beta_u Var(x_{it}^T) Var(u_{it})}{Var(\Delta y_{it})\left(Var(x_{it}^T)+Var(u_{it})\right)} + bias$$

Since the second and third terms are known to be positive, and using our point estimate for the correction factor, we know that:

$$R^2{}_T \geq \beta_T{}^2 \frac{Var(x_{it}^T)}{Var(\Delta y_{it})}$$

A.5. Group Effects Model

A.5.1. Happiness and expectations with group effects

With group effects, our happiness measurement model becomes:

$$x_{itq} = x_t^N + x_i^P + x_{it}^T + \gamma_i x_t^G + \gamma_i \phi_G + s_i + u_{it} + v_{iq} + w_{itq}$$

Here, γ_i is a mean-zero discrete group affiliation variable. The variable might represent political affiliation, gender, stockholders vs. nonstockholders, black vs. nonblack, age>60 vs. age<60, etc. For the purposes of exposition, let us assume that the group in question is political preference; γ_i will take integer values ranging from -2 ("strong Democrat") to 2 ("strong Republican")[4].

x_t^G indicates the size of the group happiness effect; a large value of x_t^G indicates that Republicans are relatively happy and Democrats relatively unhappy at time t, etc.

We assume that γ_i and x_t^G are both uncorrelated with all other terms in the happiness measurement model.

Our model of expectations becomes:

$$y_{it} = \alpha + \beta_N x_t^N + \beta_P x_i^P + \beta_T x_{it}^T + \beta_G \gamma_i x_t^G + \beta_\phi \gamma_i \phi_G + \beta_s s_i + \beta_u u_{it} + \varepsilon_{it}$$

The constant ϕ_G is the permanent group differential in happiness; if Republicans are generally happier than Democrats, ϕ_G will be positive.

Using this model, our method-of-moment estimators from the previous section contain some additional terms:

$$\tilde{\beta}_N \frac{Cov(y_{it}, x_{it})}{Cov(x_{it}, x_{-it})} = \beta_N + \frac{Cov(\varepsilon_{it}, x_t^N)}{Var(x_t^N)}$$

$$\tilde{\beta}_T \frac{Cov(\Delta y_{it}, \Delta x_{it})}{Cov(\Delta x_{itq}, \Delta x_{itr})} = \beta_T \left(\frac{Var(x_{it}^T)}{\xi_T} \right) + \beta_u \left(\frac{Var(u_{it})}{\xi_T} \right) + \beta_G \left(\frac{Var(x_t^G)}{\xi_T} \right) + \frac{Cov(\varepsilon_{it}, x_{it}^T)}{Var(x_{it}^T) + Var(u_{it}) + Var(x_t^G)}$$

[4] The political preference variable represents an individual's distance from the center (mean) of the Democrat-Republican political spectrum; hence, γ_i is taken to be mean zero. A second assumption is that this variable does not change over any 6-month period; though the center may move around in half a year's time, people's distance from the center is assumed to take longer to shift.

$$\xi_T \equiv Var(x_{it}^T) + Var(u_{it}) + Var(x_t^G)$$

$$\tilde{\beta}_P \frac{Cov(y_{it_2}, x_{it_1})}{Cov(x_{it_2 q}, x_{it_1 r})} = \beta_p \left(\frac{Var(x_i^P)}{\xi_P} \right) + \beta_s \left(\frac{Var(s_i)}{\xi_P} \right) + \beta_\phi \left(\frac{\phi_G{}^2}{\xi_P} \right),$$

$$\xi_P \equiv Var(x_i^P) + Var(s_i) + \phi_G{}^2$$

Proofs of these relationships follow.

A.5.2. Effect of x_t^N with group effects

Expanding the numerator and denominator of the moment ratio, we have:

$$\begin{aligned} Cov(y_{it}, x_{-it}) = Cov(&\alpha + \beta_N x_t^N + \beta_P x_i^P + \beta_T x_{it}^T + \beta_G \gamma_i x_t^G + \beta_\phi \gamma_i \phi_G + \beta_s s_i + \beta_u u_{it} + \varepsilon_{it}, \\ &x_t^N + x_{-i}^P + x_{-it}^T + \gamma_{-i} x_t^G + \gamma_{-i}\phi_G + s_{-i} + u_{-it} + v_{-i} + w_{-it}) \end{aligned}$$

$$\begin{aligned} Cov(y_{it}, x_{-it}) = Cov(&x_t^N + x_i^P + x_{it}^T + \gamma_i x_t^G + \gamma_i \phi_G + s_i + u_{it} + v_i + w_{it}, \\ &x_t^N + x_{-i}^P + x_{-it}^T + \gamma_{-i} x_t^G + \gamma_{-i}\phi_G + s_{-i} + u_{-it} + v_{-i} + w_{-it}) \end{aligned}$$

In addition to the conditions listed in the previous section, we use the orthogonality conditions:

$$Cov(\gamma_i x_t^G, x_t^N) = Cov(\gamma_i x_t^G, x_j^P) = Cov(\gamma_i x_t^G, x_{jt}^T) = 0$$

$$Cov(\gamma_i x_t^G, s_j) = Cov(\gamma_i x_t^G, u_{jt}) = Cov(\gamma_i x_t^G, v_j) = Cov(\gamma_i x_t^G, w_{jt}) = 0$$

$$Cov(\gamma_i, x_t^N) = Cov(\gamma_i, x_j^P) = Cov(\gamma_i, x_{jt}^T) = 0$$

$$Cov(\gamma_i, s_j) = Cov(\gamma_i, u_{jt}) = Cov(\gamma_i, v_j) = Cov(\gamma_i, w_{jt}) = 0$$

Also, using the law of total expectations, the independence of x_t^G and γ_i, and the assumption of iid γ_i, we can write:

$$Cov(\gamma_i x_t^G, \gamma_j x_t^G) = E\left(x_t^G\right)^2 E\left[\gamma_i \gamma_j \mid x_t^G\right] = E\left(x_t^G\right)^2 E\left[\gamma_i \gamma_j\right] = 0$$

$$Cov(\gamma_i x_t^G, \gamma_j \phi_G) = E\left(x_t^G \phi_G\right) E\left[\gamma_i \gamma_j \mid x_t^G\right] = E\left(x_t^G \phi_G\right) E\left[\gamma_i \gamma_j\right] = 0$$

This leaves us with:

$$Cov(y_{it}, x_{-it}) = \beta_N Var(x_t^N) + Cov(\varepsilon_{it}, x_t^N)$$

$$Cov(x_{it}, x_{-it}) = Var(x_t^N)$$

as before. Dividing yields the result listed above. The presence of group effects does not change our estimation for the effect of national happiness.

A.5.3. Effect of x_{it}^T with group effects

Expanding the numerator and denominator of the moment ratio in the estimator, we have:

$$
\begin{aligned}
&Cov(y_{it_2} - y_{it_1}, x_{it_2} - x_{-it_2} - x_{it_1} + x_{-it_1}) = \\
&Cov(\beta_N x_{t_2}^N + \beta_T x_{it_2}^T + \beta_G \gamma_i x_{t_2}^G + \beta_u u_{it_2} + \varepsilon_{it_2} - \\
&\beta_N x_{t_1}^N - \beta_T x_{it_1}^T - \beta_G \gamma_i x_{t_1}^G - \beta_u u_{it_1} - \varepsilon_{it_1}, \\
&x_{it_2}^T + \gamma_i x_{t_2}^G + u_{it_2} + w_{it_2} - x_{-it_2}^T - \gamma_{-i} x_{t_2}^G - u_{-it_2} - w_{-it_2} - \\
&x_{it_1}^T - \gamma_i x_{t_1}^G - u_{it_1} - w_{it_1} + x_{-it_1}^T + \gamma_{-i} x_{t_1}^G + u_{-it_1} + w_{-it_1})
\end{aligned}
$$

$$
\begin{aligned}
&Cov(x_{it_2q} - x_{it_1q}, x_{it_2r} - x_{-it_2r} - x_{it_1r} + x_{-it_1r}) = \\
&Cov(x_{it_2}^T + \gamma_i x_{t_2}^G + u_{it_2} + w_{it_2r} - x_{it_1}^T - \gamma_i x_{t_1}^G - u_{it_1} - w_{it_1r}, \\
&x_{it_2}^T + \gamma_i x_{t_2}^G + u_{it_2} + w_{it_2r} - x_{-it_2}^T - \gamma_{-i} x_{t_2}^G - u_{-it_2} - w_{-it_2r} - \\
&x_{it_1}^T - \gamma_i x_{t_1}^G - u_{it_1} - w_{it_1r} + x_{-it_1}^T + \gamma_{-i} x_{t_1}^G + u_{-it_1} + w_{-it_1r})
\end{aligned}
$$

We need the additional orthogonality conditions:

$$Cov(\gamma_i x_t^G, x_i^P) = Cov(\gamma_i x_t^G, x_{it}^T) = 0$$

$$Cov(\gamma_i x_t^G, s_i) = Cov(\gamma_i x_t^G, u_{it}) = Cov(\gamma_i x_t^G, v_i) = Cov(\gamma_i x_t^G, w_{it}) = 0$$

$$Cov(\gamma_i, x_i^P) = Cov(\gamma_i, x_{it}^T) = 0$$

$$Cov(\gamma_i, s_i) = Cov(\gamma_i, u_{it}) = Cov(\gamma_i, v_i) = Cov(\gamma_i, w_{it}) = 0$$

and the following application of the Law of Total Expectations:

$$Cov(\gamma_i x_t^G, \gamma_i x_t^G) = E\left(x_t^G\right)^2 E\left[\gamma_i^{\,2} \mid x_t^G\right] = E\left(x_t^G\right)^2 E\left[\gamma_i^{\,2}\right] = Var(x_t^G)$$

The numerator and denominator then become:

$$
\begin{aligned}
&Cov(y_{it_2} - y_{it_1}, x_{it_2} - x_{-it_2} - x_{it_1} + x_{-it_1}) = \beta_T Var(x_{it}^T) + \beta_u Var(u_{it}) + \\
&\beta_G Var(x_t^G) + Cov(\varepsilon_{it}, x_{it}^T)
\end{aligned}
$$

$$Cov(x_{it_2q}-x_{it_1q}, x_{it_2r}-x_{-it_2r}-x_{it_1r}+x_{-it_1r})=Var(x_{it}^T)+Var(u_{it})+Var(x_t^G)$$

Dividing gives us the result listed above.

A.5.4. Effect of x_i^P with group effects

Expanding the numerator and denominator of the moment ratio in the estimator, we have:

$$\begin{aligned}Cov(y_{it_2}, x_{it_1}-x_{-it_1}) = Cov(&\alpha+\beta_N x_{t_2}^N+\beta_P x_i^P+\beta_T x_{it_2}^T+\beta_G\gamma_i x_{t_2}^G+\\ &\beta_\phi\gamma_i\phi_G+\beta_s s_i+\beta_u u_{it_2}+\varepsilon_{it_2},\\ &x_i^P-x_{-i}^P+x_{it_1}^T-x_{-it_1}^T+\gamma_i x_{t_1}^G-\gamma_{-i}x_{t_1}^G+\gamma_i\phi_G-\\ &\gamma_{-i}\phi_G+(s_i+u_{it_1}+v_i+w_{it_1})-\\ &(s_{-it_1}+u_{-it_1}+v_{-it_1}+w_{-it_1}))\end{aligned}$$

$$\begin{aligned}Cov(x_{it_2q}, x_{it_1r}-x_{-it_1r}) = Cov(&x_t^N+x_i^P+x_{it_2}^T+\gamma_i x_{t_2}^G+\gamma_i\phi_G+\\ &s_i+u_{it_2}+v_{iq}+w_{it_2q},\\ &x_i^P-x_{-i}^P+x_{it_1}^T-x_{-it_1}^T+\gamma_i x_{t_1}^G-\gamma_{-i}x_{t_1}^G+\\ &\gamma_i\phi_G-\gamma_{-i}\phi_G+(s_i+u_{it_1}+v_{ir}+w_{it_1r})-\\ &(s_{-it_1}+u_{-it_1}+v_{-it_1r}+w_{-it_1r}))\end{aligned}$$

We use the following application of the Law of Total Expectations:

$$Cov(\gamma_i x_t^G, \gamma_j\phi_G)=E\left(x_t^G\phi_G\right)E\left[\gamma_i\gamma_j \mid x_t^G\right]=E\left(x_t^G\phi_G\right)E\left[\gamma_i\gamma_j\right]=0$$

$$Cov(\gamma_i x_{t_2}^G, \gamma_j x_{t_1}^G)=E\left(x_{t_2}^G x_{t_1}^G\right)E\left[\gamma_i\gamma_j \mid x_{t_2}^G, x_{t_1}^G\right]=E\left(x_{t_2}^G x_{t_1}^G\right)E\left[\gamma_i\gamma_j\right]=0$$

This leaves us with:

$$Cov(y_{it_2}, x_{it_1}-x_{-it_1})=\beta_P Var(x_i^P)+\beta_s Var(s_i)+\beta_\phi {\phi_G}^2$$

$$Cov(x_{it_2q}, x_{it_1r} - x_{-it_1r}) = Var(x_i^P) + Var(s_i) + \phi_G{}^2$$

The ratio of these two then yields the result given above.

A.5.5. Correction factor forms

Rewriting the estimators for in “correction factor form as above yields:

$$\beta_T\left(\frac{\mathrm{var}(x_{it}^T)}{\xi_T}\right) + \beta_u\left(\frac{\mathrm{var}(u_{it})}{\xi_T}\right) + \beta_G\left(\frac{\mathrm{var}(x_t^G)}{\xi_T}\right) + \frac{Cov(\varepsilon_{it}, x_{it}^T)}{Var(x_{it}^T) + Var(u_{it}) + Var(x_t^G)} =$$
$$\left[\frac{Cov(x_{i2} - x_{i1}, [x_{i2} - x_{i1}] - [x_{-i2} - x_{-i1}])}{Cov(x_{i2q} - x_{i1q}, [x_{i2r} - x_{i1r}] - [x_{-i2r} - x_{-i1r}])}\right] \bullet \left[\frac{Cov(y_{i2} - y_{i1}, [x_{i2} - x_{i1}] - [x_{-i2} - x_{-i1}])}{Cov(x_{i2} - x_{i1}, [x_{i2} - x_{i1}] - [x_{-i2} - x_{-i1}])}\right]$$

$$\beta_p\left(\frac{\mathrm{var}(x_i^P)}{\xi_P}\right) + \beta_s\left(\frac{\mathrm{var}(s_i)}{\xi_P}\right) + \beta_\phi\left(\frac{\phi_G{}^2}{\xi_P}\right) = \frac{Cov(x_{i2}, x_{i1} - x_{-i1})}{Cov(x_{i2q}, x_{i1r} - x_{-i1r})} \frac{Cov(y_{i2}, x_{i1} - x_{-i1})}{Cov(x_{i2}, x_{i1} - x_{-i1})}$$

These can be computed as in the previous section.

A.5.6. Estimation of ϕ_G and $Var(x_t^G)$

Note that while the expression for β_N is the same as in the simple model, the expressions for β_P and β_T contain additional terms. These terms can be consistently estimated from our data on self-reported happiness and political preference. To do this, we create a measurement model relating self-reported political preference, f_{it}, to the unobserved true political preference γ_i:

$$f_{it} = A\gamma_i + \mu_t + \upsilon_{it}$$

Where A is a constant, μ_t is the time-varying political "center", and υ_{it} is a classical measurement error. Correspondingly, define f_{it} and π_{it} as:

$$f_{it} \equiv f_{it} - \frac{1}{m_t - 1}\sum_{j \neq i} f_{jt} = A\left(\gamma_i - \frac{1}{m_t - 1}\sum_{j \neq i}\gamma_j\right) + \upsilon_{it} - \frac{1}{m_t - 1}\sum_{j \neq i}\upsilon_{jt}$$

$$\pi_{it} \equiv f_{it} x_{it}$$

We can then write method-of-moment expressions for $\text{var}(x_t^G)$ and $\phi_G{}^2$ in terms of this observable:

$$\phi_G = \frac{Var(f_{it})}{\sqrt{Cov(f_{it_1}, f_{it_2})}} * \frac{Cov(x_{it}, f_{it})}{Var(f_{it})}$$

$$Var(x_t^G) = \frac{Cov(\pi_{it}, \pi_{-it}) - Cov(f_{it}, f_{-it})Cov(x_{it}, x_{-it})}{Cov(f_{i1}, f_{i2})} -$$
$$\left(\frac{Cov(f_{it}, x_{it})}{Var(f_{it})} * \frac{Var(f_{it})}{\sqrt{Cov(f_{i2}, f_{i1})}}\right)^2$$

Derivation for ϕ_G

Observe that:

$$E\, f_{it} x_{it} = E\left[\begin{array}{l}\left(A\left(\gamma_i - \frac{1}{n_t - 1}\sum_{j \neq i}\gamma_j\right) + \upsilon_{it} - \frac{1}{n_t - 1}\sum_{j \neq i}\upsilon_{jt}\right) \\ *\left(x_t^N + x_i^P + x_{it}^T + \gamma_i x_t^G + \gamma_i \phi_G + s_i + u_{it} + v_i + w_{it}\right)\end{array}\right]$$

Using the orthogonality conditions on γ_i and υ_{it}, this reduces to:

$$E\, f_{it} x_{it} = AE\left[\left(\gamma_i - \frac{1}{n_t - 1}\sum_{j\neq i}\gamma_j\right)\left(\gamma_i x_t^G + \gamma_i \phi_G\right)\right]$$

Now apply the law of total expectations:

$$E[\gamma_i^{\,2} x_t^G] = E\gamma_i^{\,2} E[x_t^G \mid \gamma_i] = E\gamma_i^{\,2} E[x_t^G] = 0$$

And use the fact that:

$$E[\gamma_i \gamma_j] = 0$$

To get:

$$E\, f_{it} x_{it} = A\phi_G$$

Now observe that:

$$E\, f_{it_2} f_{it_1} = E\left[\begin{array}{l}\left(A\left(\gamma_i - \frac{1}{n_2 - 1}\sum_{j\neq i}\gamma_j\right) + \upsilon_{it} - \frac{1}{n_2 - 1}\sum_{j\neq i}\upsilon_{j1}\right)\\ *\left(A\left(\gamma_i - \frac{1}{n_1 - 1}\sum_{k\neq i}\gamma_k\right) + \upsilon_{i1} - \frac{1}{n_1 - 1}\sum_{k\neq i}\upsilon_{kt}\right)\end{array}\right]$$

Also observe that $E\left[\sum_{j\neq i}\gamma_j \sum_{k\neq i}\gamma_k\right] = 0$ iff no two individuals are interviewed on the same day. For this estimation, we restrict our dataset such that no such same-day pairs of interviews exist (this results in the throwing away of about 5% of our observations), and hence can safely set this expectation to 0. Thus, our total expression reduces to:

$$E\, f_{it_2}\, f_{it_1} = A^2$$

Combining this with the result in (), we get:

$$\phi_G = \frac{Cov(f_{it}, x_{it})}{\sqrt{Cov(f_{it_2}, f_{it_1})}} = \frac{Cov(f_{it}, x_{it})}{Var(f_{it})} * \frac{Var(f_{it})}{\sqrt{Cov(f_{it_2}, f_{it_1})}}$$

This is our desired result.

Derivation for $Var(x_t^G)$

Observe that:

$$E\left[f_{it}x_{it}f_{jt}x_{jt}\right] = E\left[\begin{matrix}\left(\begin{matrix}A^2\gamma_i\gamma_j + \mu_t^2 + \upsilon_{it}\upsilon_{jt} + A\mu_t(\gamma_i + \gamma_j) \\ +A(\gamma_i\upsilon_{jt} + \gamma_j\upsilon_{it}) + \mu_t(\upsilon_{it} + \upsilon_{jt})\end{matrix}\right) \\ *\left(x_{it}x_{jt} + \gamma_i\gamma_j(x_t^{G2} + \phi_G^2) + \gamma_i x_{jt} + \gamma_j x_{it}\right)\end{matrix}\right]$$

$$x_{it} \equiv x_t^N + x_i^P + x_{it}^T + s_i + u_{it}$$

We use the following conditions to eliminate terms from the expectation:

$$E\left[\gamma_i\gamma_j x_{it}x_{jt}\right] = E\left[\gamma_i^2\gamma_j x_{jt}\right] = 0$$

$$E\left[\mu_t^2\gamma_i\gamma_j(x_t^{G2} + \phi_G^2)\right] = E\left[\mu_t^2\gamma_i x_{jt}\right] = 0$$

$$E\left[\mu_t\gamma_i x_{it}x_{jt}\right] = E\left[\mu_t\gamma_i^2\gamma_j(x_t^{G2} + \phi_G^2)\right] = E\left[\mu_t\gamma_i^2 x_{jt}\right] = 0$$

$$E\left[\upsilon_{jt}\gamma_i x_{it}x_{jt}\right] = E\left[\upsilon_{jt}\gamma_i^2\gamma_j(x_t^{G2} + \phi_G^2)\right] = E\left[\upsilon_{jt}\gamma_i^2 x_{jt}\right] = 0$$

$$E\left[\mu_t\upsilon_{it}x_{it}x_{jt}\right]=E\left[\mu_t\upsilon_{it}\gamma_i\gamma_j(x_t^{G2}+\phi_G{}^2)\right]=E\left[\mu_t\upsilon_{it}\gamma_i x_{jt}\right]=0$$

These conditions are obtained using our orthogonality conditions, as well as the fact that:

$$E\left[\gamma_i\right]=E\left[\gamma_i\gamma_j\right]=E\left[\gamma_i{}^2\gamma_j\right]=0$$

$$E\gamma_i{}^2\gamma_j{}^2=1$$

$$E\left[x_{it}\right]=0$$

This gives us:

$$E\left[\pi_{it}\pi_{-it}\right]=E\left[A^2\gamma_i{}^2\gamma_j{}^2(x_t^{G2}+\phi_G{}^2)+\mu_t{}^2x_t^{N2}\right]=A^2\left(Var(x_t^G)+\phi_G{}^2\right)+Var(\mu_t)Var(x_t^N)$$

We now use:

$$E\left[f_{it}f_{jt}\right]=E\left[\left(A^2\gamma_i\gamma_j+\mu_t^2+\upsilon_{it}\upsilon_{jt}+A\mu_t(\gamma_i+\gamma_j)+A(\gamma_i\upsilon_{jt}+\gamma_j\upsilon_{it})+\mu_t(\upsilon_{it}+\upsilon_{jt})\right)\right]=Var(\mu_t)$$

And:

$$E\left[x_{it}x_{jt}\right]=E\left[x_{it}x_{jt}+\gamma_i\gamma_j(x_t^{G2}+\phi_G{}^2)+\gamma_i x_{jt}+\gamma_j x_{it}\right]=Var(x_t^N)$$

Rearranging our expression and substituting in for $\phi_G{}^2$, we get:

$$Var(x_t^G) = \frac{Cov(\pi_{it}, \pi_{-it}) - Cov(f_{it}, f_{-it})Cov(x_{it}, x_{-it})}{Cov(f_{it_1}, f_{it_2})}$$

$$-\left(\frac{Cov(f_{it}, x_{it})}{Var(f_{it})} * \frac{Var(f_{it})}{\sqrt{Cov(f_{it_2}, f_{it_1})}} \right)^2$$

This is the result we wanted.

With this machinery, we can obtain point estimates for the "correction terms" that must be added to the estimators for the coefficients in the simple model to account for the presence of any single group-specific effect. Note that this model cannot easily be extended to account for the simultaneous existence of multiple group effects, since membership in various groups will generally be correlated. However, our aim is to show that the "correction terms" in the estimators for the effect of happiness on expectations are small for any given group; thus, the fact that we cannot include all groups at once will not alter our basic result.

Estimates for these quantities for various groups can be found in Table 13. The correction terms are small compared to the variances of the happiness components.

A.6. "Emotion Z"

We hypothesized that another emotion, and not happiness, drives affective animal spirits. We postulated that this emotion, called "Emotion Z," is imperfectly correlated with happiness:

$$x_{it}^T = \rho z_{it} + \varsigma_{it}$$

$$y_{it} = \alpha + \beta_Z z_{it} + \varepsilon_{it}$$

The parameter ρ is assumed to be between 0 and 1.

Differencing, we have:

$$\Delta x_{it} = \rho \Delta z_{it} + \Delta \varsigma_{it}$$

$$\Delta y_{it} = \beta_Z \Delta z_{it} + \Delta \varepsilon_{it}$$

A first-difference estimation of the change in happiness on the change in expectations will give us:

$$\tilde{\beta}'_T = \frac{Cov(\Delta y_{it}, \Delta x_{it})}{Var(\Delta x_{it})}$$

Evaluating the top and bottom of this expression, and dividing, gives us:

$$Cov(\Delta y_{it}, \Delta x_{it}) = 2\beta_Z \rho Var(z_{it}) + 2\left(\rho Cov(\varepsilon_{it}, z_{it}) + Cov(\varepsilon_{it}, \varsigma_{it})\right)$$

$$Var(\Delta x_{it}) = 2\left(\rho^2 Var(z_{it}) + Var(\varsigma_{it})\right)$$

$$\tilde{\beta}'_T = \beta_Z \frac{\rho Var(z_{it})}{\rho^2 Var(z_{it}) + Var(\varsigma_{it})} + \frac{\rho Cov(\varepsilon_{it}, z_{it}) + Cov(\varepsilon_{it}, \varsigma_{it})}{\rho^2 Var(z_{it}) + Var(\varsigma_{it})}$$

Now we use:

$$R^2{}_T = \tilde{\beta}_T * \frac{1}{2} Var(\Delta x_{it})$$

Substituting, we have:

$$R^2{}_T = \beta_Z \rho Var\left(z_{it}\right) + \rho Cov\left(\varepsilon_{it}, z_{it}\right) + Cov\left(\varepsilon_{it}, \varsigma_{it}\right)$$

This is the result we wanted.

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