Money, Happiness, and Aspirations: An Experimental Study

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Abstract: The past decade has witnessed an explosion of interest in the scientific study of happiness. Economists, in particular, find that happiness increases in income but decreases in income aspirations, and this work prompts examination of how aspirations form and adapt over time. This paper presents results from the first experimental study of how multiple factors—past payments, social comparisons, and expectations—influence aspiration formation and reported satisfaction. I find that expectations and social comparisons significantly affect reported satisfaction, and that subjects care relatively more about social comparisons once they have achieved a satisfactory outcome. These findings support an aspirations-based theory of happiness.

Keywords: satisfaction, happiness, adaptation, experiment.
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1. Introduction

The past decade has witnessed an explosion of interest in the scientific study of happiness among both researchers and the general public.[1] At stake for the discipline of economics is the validity of the fundamental premise that "more is better;" at stake for the wider public is the belief that economic growth should be a primary goal of public policies.[2] In numerous studies, economists and others find that an increase in income does increase an individual's happiness, usually measured as the individual's subjective assessment of her own happiness or well-being.[3] However, these studies also conclude that an increase in income is accompanied by a rise in consumption aspirations that, over time, works to offset the initial rise in happiness.[4] Thus, although more income is better for happiness temporarily, the question becomes whether or not it is better in the long run.

This question has shifted attention to how income aspirations form and adapt, and three factors have been identified as particularly important. First, an individual's aspiration level depends positively on her past outcomes, such that higher past incomes trigger higher aspirations and lower levels of reported happiness (e.g., McBride 2001; Stutzer 2004). Second, her aspirations depend positively on the outcomes of others in her comparison group, such that an improvement in others' incomes decreases her happiness (e.g., McBride 2001; Stutzer 2004;

[1] Consider, for example, a recent issue of *Time Magazine* (2005) devoted to the "The Science of Happiness," numerous popular and academic books (e.g., Argyle 2001; Frey and Stutzer 2002a; Van Praag and Ferrer-i-Carbonell 2004; Layard 2005, Haidt 2005), numerous academic articles (see references), special issues or partial issues of academic journals (in economics, *Journal of Economic Behavior and Organization* July 2001; *Economic Journal* November 1997), professional conferences (e.g., the 2006 *Economics of Happiness Symposium* hosted by the University of Southern California and the University of Warwick), and the formation in 2000 of an academic journal devoted solely to the topic (*Journal of Happiness Studies*). See Clark, Frijters, and Sheilds (2006) for a recent review of the economics of happiness literature. McMahon (2006) traces today's interest in happiness back through time to ancient Greece.

[2] Lane (2000), for example, argues that income maximization should not be a top policy priority.

[3] Frey and Stutzer (2002b) overview the happiness concept and measurement issues. Non-income influences on happiness include health, age, and marital status, as well as sexual activity (Blanchflower and Oswald 2004).

[4] The articles are too numerous to list individually. The seminal papers are Easterlin (1974, 1995). For a general discussion of key issues related to happiness of interest to economists, see Frey and Stutzer (2002a, 2002b).

Ferrer-i-Carbonell 2005; Luttmer 2005). Third, her aspirations depend positively on her expected outcome, such that a higher expected income decreases reported happiness.[5]

Can we find evidence of these aspiration factors at work in a well-controlled laboratory environment? It is not clear that experimental subjects will form payoff aspirations in the same manner that individuals form income aspirations. Income depends on hours worked, market forces, and societal institutions, and income aspirations might depend on all of these as well as other societal factors. Subjects in an experiment that abstracts from these many influences might form aspirations in a very different manner. Nonetheless, because the debate about the role of aspirations in the income-happiness relationship continues,[6] experimental evidence of payoff aspirations would support an aspirations-based theory of happiness. Moreover, an experimenter can identify, control, and measure the three mechanisms—past outcomes, social comparisons, and expectations—thought to drive aspirations but difficult to isolate using survey data. Hence, an experiment can not only determine which factors operate but also which factors play the largest role in aspiration formation.

Recognizing the potential for experimental research to contribute to our understanding of happiness, Charness and Grosskopf (2001) conducted an experiment in which a subject controlled another's payoff, and they found that most subjects disregard making social comparisons and instead show a preference for efficient and fair outcomes. Other experiments further investigated the relationship between feelings (such as happiness) and other-regarding behavior (e.g., Bosman and van Winden 2002; Konow and Early 2003; Konow 2005). However,

[5] This mechanism has little attention in the income-happiness literature, though acknowledged by psychologists. See Kahneman (1999) and Frederick and Lowenstein (1999) for larger discussions of adaptation and expectations.

[6] For example, Veenhoven (1991) disputes the claim that happiness is relative, while Easterlin (2001) argues that adaptation makes temporary the impact of an increase of income. The empirical evidence remains mixed. Easterlin's (1995) work suggests complete adaptation in the USA since World War II, while Frijters, Haisken-DeNew, and Shields (2004) find that rising income in reunified Germany had a lasting impact on happiness for former East Germans.

these authors were not interested in aspiration formation directly but instead in how comparison effects or emotions influenced strategic behavior and vice versa. Because happiness assessments in their studies are intertwined with choices that determine equity and fairness, it remains to be seen whether their findings apply to a non-strategic setting which more closely mimics the comparisons examined in the income-happiness literature (e.g., you do not alter you neighbor's income). Moreover, the earlier studies do not consider the other factors thought to influence aspirations.

An underlying premise of this paper is that we must separate the impact of aspirations from other strategic considerations in order to study aspiration formation; otherwise, aspirations might not be accurately observed. For example, a subject might in fact care about her relative payoff, but if she greatly fears retaliation, then her observed behavior will not reflect any concern for relative payoff.[7] To avoid this possibility, I design an experiment in which a subject's choice affects her own payoff but not others' payoffs, and which also simultaneously collects data on past payments, social comparisons, and expected payoffs. Individuals play repeated rounds of the "matching pennies" game against various computer opponents whom play at announced probability distributions. After being told outcome information for a round, each subject reports her subjective satisfaction with the outcome.

This paper's findings may be summarized as follows. First, holding the payoff constant, an increase in aspirations has a negative and statistically significant impact on a subject's reported satisfaction. In other words, there is conclusive evidence that aspirations affect happiness. Second, while aspirations do influence reported satisfaction, each of the three

[7] The experiment by Lazear, Malmendier, and Weber (2006) justifies this fear. They find that other-regarding subjects opt out of playing the dictator game when given the chance, thereby illustrating that the strategic environment determines the degree to which we observe other-regarding behavior.

aspiration mechanisms influences satisfaction to different degrees. The strongest factor is a subject's expected payoff, which negatively and significantly affects satisfaction (or positively affects aspirations) as predicted. The next strongest factor is social comparisons, which also affects satisfaction negatively and significantly as predicted. The weakest effect on satisfaction is that of previous payments, which has a negligible effect on current satisfaction. A third finding is that social comparisons matter relatively more to subjects once they have attained a satisfactory outcome.

These findings support an aspirations-based theory of happiness and pecuniary rewards, and many of them match empirical patterns identified in the income-happiness literature. Payoff satisfaction depends on aspirations which change in response to environmental conditions, particularly expectations and others' outcomes. Perhaps most surprising is that social comparisons increase in importance as outcomes improve. Just as wealthy people care more about their peers' incomes than poor people (see McBride 2001), subjects who report high satisfaction with their outcomes care more about their peers' experimental payoffs than do dissatisfied subjects. The one pattern inconsistent with the income-happiness literature is that past payoffs do not prominently impact aspirations. In hindsight, this finding is not surprising because subjects are unlikely to become too accustomed to prior payoffs during the duration of a relatively short experiment. However, it also suggests that the impact of past income and consumption on happiness might be due not to a direct effect of past consumption on aspirations but instead on an indirect effect through expected consumption. Future work should further examine this issue to determine how and to what extent aspirations over small outcomes aggregate to overall life happiness. Yet, these findings establish that aspirations operate even

with respect to relatively small experiment payoffs, thus providing a foundation for a theory about happiness and large pecuniary rewards such as income.[8]

Although economists have done relatively little experimental work closely related to this study,[9] there is a large experimental literature on aspirations by psychologists. Decades ago, Helson (1964) synthesized the experimental work in psychology to present the first systematic theory of adaptation, and the notion has since been theoretically and experimentally applied more specifically to hedonic and aspirations adaptation (e.g., Brickman and Campbell 1971, Kahneman 1999; Frederick and Loewenstein 1999).[10] Three areas of the psychology research are of particular relevance for this study. The first—familiar to economists and decision theorists—is that related to Kahneman and Tversky's (1979) Prospect Theory, which posits that the subjective value of an outcome is assessed relative to a reference point (see Edwards 1996 for a review). The Prospect Theory reference point is a type of aspiration level based on the status quo, however, in my experiment and in the income-happiness literature, the reference point depends on multiple aspiration factors.[11] The second area is the study of goals and motivation (Pervin 1989), which connects to my work because goals and other subjective expectations factor into the frame of reference used when evaluating satisfaction with outcomes (e.g., Heath, Larrick, and Wu 1999). The third literature examines the role of social comparisons in self-assessments, first articulated by Festinger (1954) but then developed extensively (see Suls and

[8] For discussion and research of how various life experiences aggregate to subjective happiness, see Kahneman (1999) and Van Praag, Frijters, and Ferrer-i-Carbonell (2003).

[9] In addition to Charness and Grosskopf (2001), Bosman and van Winden (2002), Konow and Early (2003), and Early (2005), there is some work on the relationship between satisfaction and familiarity with the decision making environment (Novarese and Rizzello forthcoming), and there is also some experimental work on reference points in auctions (Ham, Kagel, and Lehrer 2005) and expectations and bargaining (Oliver, Balakrishnan, and Barry 1994).

[10] The difference between hedonic adaptation and aspirations adaptation is that the former occurs due to changes in objective circumstances and hedonic experience, while the latter does not require such changes (Khaneman 1999). It is often the case that both types of adaptation will be in effect, as in my experiments.

[11] Lim (1995), for example, shows that the Prospect Theory reference point in some settings can be the expected outcome. My experiment takes as given that the reference point also depends on other aspiration factors.

Wheeler 2000). My paper extends the experimental work in new directions. As described earlier, the design isolates comparison effects from other strategic considerations. It also generates direct measures on past experience, social comparisons, and expectations. While two of the three (past experience and social comparisons) have been simultaneously examined in a previous experiment (e.g., Smith, Diener, and Wedell 1989), to my knowledge, this is the first attempt to obtain data on all three factors simultaneously in a single experiment.

2. The Aspirations Theory of Happiness

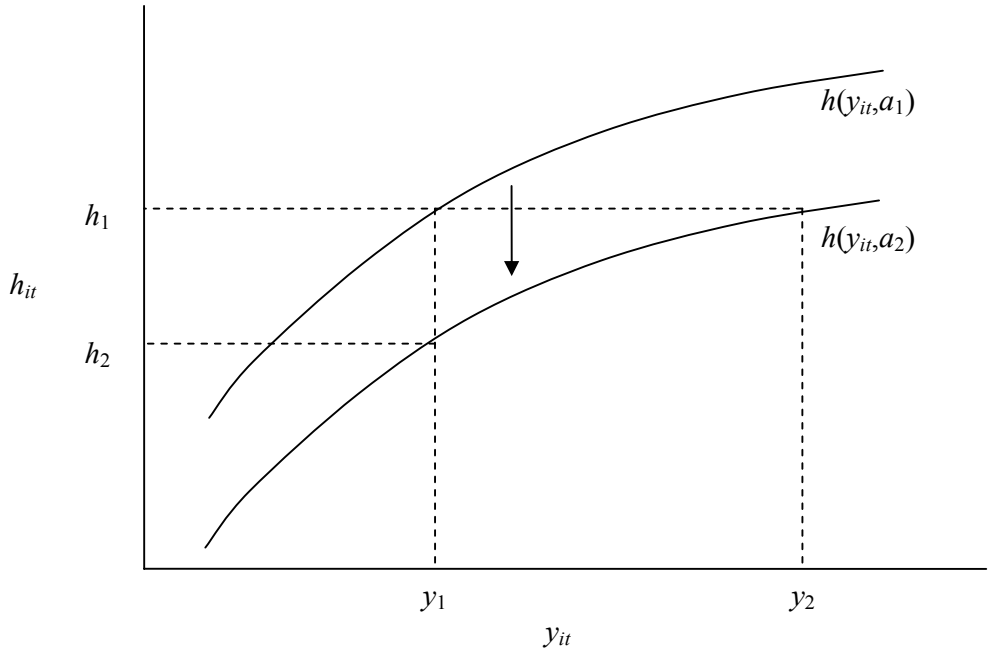
Because more extensive discussions of the theory of happiness developed in the recent literature have been provided elsewhere (e.g., Easterlin 2001; Frey and Stutzer 2002a, 2002b), I here provide a brief description. According to this newly developing theory, an individual's subjective assessment of her own happiness at time t, denoted h_{it} , depends positively on her achievement y_{it} and negatively on her aspiration level a_{it}:

$$h_{it} = h(y_{it}, a_{it}), \tag{1}$$

with $h_y > 0$ and $h_a < 0$. For our purposes, this happiness function can be thought of as an indirect utility function that depends on aspirations.

Figure 1 illustrates a simple happiness function which is concave in y_{it}. At achievement y_1 and aspiration level a_1, individual i has happiness h_1. That the function is upward sloping indicates that happiness is increasing in achievement, all else constant. However, an increase in aspiration level to a_2, holding achievement constant, results in a decrease in happiness at each achievement level depicted as a downward shift in the happiness function in Figure 1. The individual's happiness level decreases to h_2. To achieve her original happiness under the new aspiration level, the individual's achievement must increase to y_2.

Figure 1: A Shift in a Happiness Function due to Changing Aspirations



Various factors are thought to affect one's aspiration level, although three specific factors have been identified as particularly relevant for income or other pecuniary aspirations. First, an individual's aspiration depends positively on her past outcomes. An individual gets accustomed to good outcomes, so higher past outcomes trigger a higher aspiration in the current period. Second, an individual's aspiration level depends positively on the outcomes of others in her comparison group. A person prefers to perform well relative to others, so an improvement by the others decreases her own satisfaction. Third, an individual's aspiration level depends positively on her expectation. If individuals A and B each received payoff y, but A expected to receive less than y while B expected to receive more than y, then A should report a higher satisfaction because A's outcome exceeded expectations while B's fell short of expectations.

Consider the following example of an aspiration function. Let y_{it} be i's achievement in period $t \geq 1$, with C_i the set of individuals in i's comparison group, let y_{jt} be the payoff in time t

of individual j in C_i; and let $E[y_{it}]$ be i's expectation of y_{it} at the beginning of t before y_{it} is realized. Define the linear aspiration function to be

$$a_{it} = \theta_1 E[y_{it}] + \theta_2 \frac{1}{t-1} \sum_{t'=1}^{t-1} y_{it'} + \theta_3 \frac{1}{|C_i|} \sum_{j \in C_i} y_{jt} \ , \tag{2}$$

where $\theta_1 > 0$, $\theta_2 > 0$, $\theta_3 > 0$, and $\theta_1 + \theta_2 + \theta_3 = 1$. In this case, θ_1, θ_2, and θ_3 are the weights given to the expectation, past experience, and social comparison aspiration factors, and the aspiration level a_{it} is bounded by the values of the three terms in the aspiration function. Of course, there are many possible aspiration and happiness functions. The aspiration function could instead be non-linear in the aspiration factors just as happiness is nonlinear in achievement. If the aspiration level is associated with the reference point in Prospect Theory, then the happiness function will be concave in y_{it} if $y_{it} > a_{it}$ but convex in y_{it} if $y_{it} < a_{it}$.

3. Experiment Design

The core of the experiment is a version of the matching pennies game. In each round, each subject is randomly matched with one of the five following computer partner-types:

20% heads – 80% tails

35% heads – 65% tails

50% heads – 50% tails

65% heads – 35% tails

80% heads – 20% tails.

The computer then tells the subject the partner-type, which, in other words, informs the subject of the probability distribution used by the computer to select coins in that round. Each partner-type is equally likely so that a subject has a 20% chance of being matched with a 20-80 type, a

20% chance of being matched with a 35-65 type, and so on. These matches are also i.i.d. across subjects and time so that in any given round some subset of the subjects will be matched with a 20-80 type, another subset will be matched with a 35-65 type, and so on. Next, the subject chooses heads or tails for each of five coins. Then, the computer randomly and independently selects heads or tails according to partner-type distribution. If the subject's first coin and the computer's first coin match (either both are heads or both are tails), then the subject wins the coin, and so on for the other coins. Thus, a subject can win anywhere from 0 to 5 coins in any given round. Figure 2A depicts the subject's payoff matrix for a single coin choice.

After the computer partner's choices are made, the computer reports to the subject the coin choices made by the computer and the number of coins won by the subject. In Treatment A, the subject is told only her own outcomes for each round. In Treatment B, the subject is told her own outcomes and also the average coins won by the other subjects in the experiment. In Treatment C, the subject is told her own outcomes, but instead of being told the average of all other subjects, she is told the average coins won by others by partner type. That is, she is told the average of all those matched with a 20-80 partner-type, the average of all those matched with a 35-65 partner type, and so on. The purpose for using both Treatments B and C is to identify the reference group used in making social comparisons. In those treatments, the calculated averages do not include the subject's own coins won in that round.

Immediately after being told the outcome of a round (i.e., the number of coins won and, depending on the treatment, information about others' coins won), the subject is asked "How satisfied are you with the result of this round?" She then reports her satisfaction on a scale of 1 to 7, with 1 signifying "very dissatisfied," 4 signifying "satisfied," and 7 signifying "very

satisfied."[12] The form of this question matches the convention used in happiness surveys (Schwarz and Strack 1999), and although answers to these subjective questions suffer from various imperfections, a widespread conclusion is that such data meaningfully capture relevant aspects of happiness or satisfaction.[13]

After all subjects report their satisfaction levels, the next round begins. Subjects are randomly assigned a new, possibly different, partner-type. Information on past partner-types, coin choices and payments remain on the computer screen. After all rounds have ended, subjects then answer a brief questionnaire[14] on the computer. The questions ask the subject to report his or her sex, grade in school, major, number of economics classes taken, etc.

The experiment was conducted at the California Social Science Experimental Laboratory (CASSEL) located at the University of California, Los Angeles (UCLA).[15] UCLA students learn of the laboratory through word of mouth and on-campus advertising. Any UCLA student can then enter the CASSEL subject pool by registering on the CASSEL web page. The date and time of my particular experiment is sent via email to all subjects in the pool, and subjects who want to participate in my experiment then sign-up through the CASSEL web page. Students are not screened by major, sex, race, etc., yet no subject could participate in more than one treatment.

[12] This type of question has been used to gather satisfaction or subjective well-being (i.e., happiness) data in experiments (e.g., Charness and Grosskopf 2001) and surveys (e.g., Ferrer-i-Carbonell and Frijters 2004). The term satisfaction is thought to entice a more cognitive response than the term happiness, which is thought to be more emotive. The income-happiness literature uses the terms interchangeably because they appear empirically equivalent (e.g., the title of Van Praag and Ferrer-i-Carbonell's (2004) book includes both terms). I suspect that the terms would yield similar results in this experiment, yet future experiments must verify that conjecture. I use satisfied in the question in the unlikely case that satisfy does prompt a more cognitive, thoughtful response.

[13] See Diener (1984) for greater discussion on happiness and satisfaction questions.

[14] Contact the author for a copy of the questionnaire. The questionnaire was not used in the analysis presented in this paper but will be used in later analysis of the data.

[15] More information about CASSEL is available at their web site www.cassel.ucla.edu.

Figure 2: The Experiment Design

(A) The Basic Matching Pennies Game

		Computer: Heads	Computer: Tails
Subject	Heads	1	0
	Tails	0	1

(B) Summary of the Experiment Order

1. The subjects receive verbal instructions and participate in one practice
2. In each round (1 practice, 25 real)
   a) The computer randomly chooses a partner-type for each subject.
   b) The computer tells each subject her partner-type.
   c) Each subject then chooses heads or tails for each of five coins.
   The computer chooses heads or tails for five coins for each subject
   d) according to the partner-type distribution.
   The computer tells each subject her resulting coin matches and payment
   e) for that round.
      - In addition to her own payment, in Treatment B the subject is also told the average payment of all other subjects.
      - In addition to her own payment, in Treatment C the subject is also told the average payment of all other subjects by partner-type.

   f) Each subjects reports her satisfaction on a scale of 1 to 7.

   After 2(a)-(f) are repeated for each of 25 rounds, each subject answers a
3. brief questionnaire.
4. Subjects receive US dollars according to 8 coins to 1 dollar exchange rate.

After entering the lab, each subject sits at a computer terminal, receives verbal instructions (see appendix) appropriate for the treatment, participates in one practice round, participates in 25 real rounds, and then answers the questionnaire. In each round of each treatment, a subject's partner-type was chosen randomly and independently of the partner-types

of the other subjects. Coin choices by the computer are also random and independent across coins for a given subject and also across subjects. Treatments A, B, and C had 36, 32, and 36 subjects, respectively. At the experiment's end, subjects were paid actual US dollars for their coins received according to an exchange rate of 8 coins for 1 dollar. To provide an incentive for completing the questionnaire, each subject is given an additional $2 for completing the questionnaire. Figure 2B summarizes the experiment order. Each treatment lasted approximately one hour, and the average total take home amount was roughly $17.

This design has many advantages. First, the computer collects data on payments, past payments, and expected payments, and it allows me to control the subjects' information about other subjects' payments. I can thus examine how different mechanisms impact satisfaction. Second, because the subject is only ever paired with the computer and payments do not depend on other subjects' choices, there are no strategic aspects of the decision making process. Aspirations should thus be independent of attitudes toward others' cooperativeness or spite, which could generate additional and difficult to control heterogeneity in aspiration formation. Third, the optimal action is easy to deduce. The optimal action is to choose heads for all five coins when paired with an 80-20 or 65-35 type, to choose all tails when paired with a 35-65 or 20-80 type, and to choose anything when paired with a 50-50 type. These optimal actions yield expected payments for the round of 4.25 against the 80-20 and 20-80 types, 3.25 against the 65-35 and 35-65 types, and 2.5 against the 50-50 type. Having an easy to deduce optimal action should minimize the degree of learning about the correct action throughout the experiment. Fourth, because the optimal action is straightforward to calculate, the expected payment from the optimal strategy is a good proxy for the subject's self-perceived expected payment. The merit of this proxy depends how closely subjects' actual coin choices mimic choices consistent with

expected payoff maximization, and because subjects' take-home payments are increasing in coins won, the experiment provides an incentive for them to maximize the number of coins won. Fifth, because the satisfaction scale and the range of possible payments are not the same, subjects will be less inclined to associate a particular monetary payment with a "natural" satisfaction report. For example, if the satisfaction scale was 0 to 5 like the payment range, and a subject received payment 3, she might automatically associate a payment 3 with satisfaction 3. Under my design, the satisfaction report requires more of a subjective assessment. Sixth, because both the selection of partner-types and their coins are random, the experiment generates variation in both realized payments (both own and others) and expected payments. This variation is necessary for econometric identification of the impact of expectations on satisfaction. Finally, all three aspirations measures are in the same units, so I can directly compare the coefficients in my regressions and thereby make a specific statement about the relative magnitude of each factor's impact on satisfaction and aspirations.

4. Results and Comments

Summary Statistics

Table 1 Panel A pools the data from all treatments and presents the average reported satisfaction level and number of observations by payment. This average is calculated treating the satisfaction variable as if it were linear in the responses, and since the response might not be linear, the averages are only for illustrative and suggestive purposes. We observe a clear, positive relationship between average reported satisfaction and payment. Because we would expect a positive relationship if subjects reported their satisfaction levels sincerely, this finding suggests that the subjectively reported data do meaningfully capture some element of actual payoff

satisfaction even though the subjects had no pecuniary incentive to report their subjective satisfaction accurately. Panel B, which reports the mean reported satisfaction and number of observations by treatment and payment, provides further support for this conclusion. Not only does reported satisfaction depend positively on the payment in all treatments, but the treatment averages are similar to the pooled averages. This suggests consistency in reported satisfaction across treatments.

Table 1: Satisfaction Means by Payment, Treatment, and Partner Type

	Payment					
	0	1	2	3	4	5
A) Pooled	**1.09**	**1.39**	**2.22**	**3.76**	**5.58**	**6.75**
Observations	33	167	476	763	790	371
B) By Treatment						
Treatment A (36 subjects)	**1.00**	**1.40**	**2.30**	**3.72**	**5.47**	**6.57**
Observations	10	50	172	254	279	135
Treatment B (32 subjects)	**1.00**	**1.44**	**2.26**	**3.90**	**5.66**	**6.93**
Observations	11	50	145	250	243	101
Treatment C (36 subjects)	**1.25**	**1.34**	**2.11**	**3.68**	**5.62**	**6.79**
Observations	12	67	159	259	268	135
C) By Partner-type						
20-80 (expected payment 4)	**1.00**	**1.14**	**1.81**	**3.33**	**5.44**	**6.77**
Observations	2	7	47	139	203	140
35-65 (expected payment 3.25)	**1.43**	**1.21**	**2.16**	**3.69**	**5.60**	**6.75**
Observations	7	38	114	164	142	40
50-50 (expected payment 2.5)	**1.00**	**1.59**	**2.50**	**4.22**	**5.90**	**7.00**
Observations	16	69	150	174	93	19
65-35 (expected payment 3.25)	**1.00**	**1.26**	**2.23**	**3.97**	**5.80**	**6.80**
Observations	7	43	128	182	134	40
80-20 (expected payment 4)	**1.00**	**1.40**	**1.81**	**3.34**	**5.42**	**6.67**
Observations	1	10	37	104	218	132

Notes: Averge satisfaction is calcuated by treating the satisfaction level as a linear variable.

Panel C reports the average satisfaction level and number of observations by payment and partner-type. For each partner-type type except 35-65, we observe the same positive relationship between satisfaction and payment. The single exception for partner-type 35-65 is a mean satisfaction for payment 1 that is higher than the mean satisfaction for payment 2 in the 35-65. This is likely due to the small number of observations in the low payment cases, and so does not seriously challenge the initial conclusion that satisfaction in increasing in payment.

Holding the payment fixed in Panel C, however, we generally observe a negative relationship between satisfaction and the expected payment as predicted by the aspirations-based theory. For example, in the column for payment 3, the average satisfaction is highest with partner type 50-50, which has the lowest expected payment of 2.5. Average satisfaction decreases as the expected payment increases, i.e., as we change partner type from 50-50 to 65-35 to 80-20 or from 50-50 to 35-65 to 20-80. We observe the same pattern for payments 2 and 4. For payments 0, 1 and 5, we observe the same general pattern but with small deviations: type 35-65 for payment 0, type 80-20 for payment 1, and type 20-80 for payment 5. The deviations for payments 0 and 1 may be due to the small number of observations, yet the deviation for payment 5 is harder to attribute to sample size. Nonetheless, the general pattern is that higher expectations decrease happiness, holding the payment fixed.

Table 2 displays the correlation and *p*-value corresponding to a null hypothesis of zero correlation for various variable pairs by treatment. Again, for simplicity, the reported satisfaction values are treated as if they are linearly related instead of being discrete categorical responses. The strongest single correlation between reported satisfaction and another variable is that between satisfaction and payment—strong evidence that the payment is a primary factor in the determination of reported satisfaction. That satisfaction is positively correlated with other

variables predicted to affect satisfaction negatively is due to the correlation between those variables and the payment. For example, having a higher expected payment or a higher average of those with same partner-type means the subject is likely to have a high payment, which should increase her satisfaction. It is only after holding the payment fixed, that we should observe a negative relationship between expected payment and satisfaction (as seen in Table 1 Panel C. That the simple correlation is positive suggests that the payment effect is stronger. This conjecture is supported by the regression analysis below.

The "Expected Payment—Maximization" variable in Table 2 is the expected payment if the subject chose coins consistent with expected payoff maximization. As stated earlier, this entails choosing heads for all five coins when matched with an 80-20 or 65-35 partner-type, choosing tails when matched with a 20-80 or 35-65 partner-type, or choosing anything when matched with a 50-50 partner-type. As is common in laboratory experiments, the subjects do not always act to maximize their expected payoffs, so the table also displays correlations using the "Expected Payment—Choices" variable, which calculates the exact expected payment given the coin choices actually made by the subject.

That the two measures of expected payoff are highly correlated (0.79, 0.90, and 0.86 in Treatments A, B, and C, respectively) is not surprising because most of the coin choices are likely to be consistent with expected payoff maximization, as shown in Table 3. The first column in Table 3 reports the number of coin choices consistent with expected payoff maximization, which is overall and by treatment about 92%. The variation comes across partner-types, with the lowest choice consistency coming when players are matched with the 35-65 and 65-35 partner-types. The 100% consistency when matched with a 50-50 type is due to the fact that any coin choice is consistent when matched with a 50-50 partner-type. The second column

reports the number of times all the coins in a given round by a given subject were chosen consistently with expected payoff maximization. Whereas the first column treats each coin separately, the second column essentially treats each group of five coins chosen in a round by a given subject as the choice of interest, and so must be lower than the first column.

Table 2: Correlations by Treatment

A) Treatment A	Satis.	Payment	EP Max	EP Ch.	Prior Rd.
Payment	**0.82** < 0.01	--			
Expected Payment–Maximization	**0.18** < 0.01	**0.36** < 0.01	--		
Expected Payment–Choices	**0.34** < 0.01	**0.50** < 0.01	**0.79** < 0.01	--	
Prior Round Payment	**0.02** 0.60	**0.02** 0.54	**-0.02** 0.51	**0.03** 0.35	--
Avg. Payment Through Last Rd.	**0.04** 0.31	**0.02** 0.51	**0.07** 0.05	**0.05** 0.15	**0.05** 0.17

B) Treatment B	Satis.	Payment	EP Max	EP Ch.	Prior Rd.	Avg. Pay.
Payment	**0.86** < 0.01	--				
Expected Payment–Maximization	**0.26** < 0.01	**0.42** < 0.01	--			
Expected Payment–Choices	**0.31** < 0.01	**0.48** < 0.01	**0.90** < 0.01	--		
Prior Round Payment	**0.12** < 0.01	**0.09** 0.02	**< 0.01** 0.98	**0.06** 0.11	--	
Avg. Payment Through Last Rd.	**0.07** 0.06	**0.06** 0.10	**-0.02** 0.61	**-0.02** 0.64	**-0.02** 0.61	--
Average of All Other's Payments	**-0.01** 0.77	**0.03** 0.35	**0.04** 0.23	**0.02** 0.54	**0.05** 0.18	**0.29** < 0.01

C) Treatment C	Satis.	Payment	EP Max	EP Ch.	Prior Rd.	Avg.Avg.	Avg.Oth.
Payment	**0.85** < 0.01	--					
Expected Payment–Maximization	**0.30** < 0.01	**0.43** < 0.01	--				
Expected Payment–Choices	**0.38** < 0.01	**0.51** < 0.01	**0.86** < 0.01	--			
Prior Round Payment	**0.03** 0.31	**-0.00** 0.94	**-0.06** 0.07	**-0.03** 0.34	--		
Avg. Payment Through Last Rd.	**0.04** 0.24	**0.05** 0.16	**0.06** 0.06	**0.06** 0.07	**0.05** 0.17	--	
Avg. of All Partner-type Averages	**-0.07** 0.03	**-0.08** 0.02	**-0.07** 0.04	**-0.06** 0.06	**0.02** 0.65	**0.29** < 0.01	--
Avg. of Own Partner-type	**0.21** < 0.01	**0.33** < 0.01	**0.75** < 0.01	**0.66** < 0.01	**-0.02** 0.52	**0.07** 0.03	**0.16** < 0.01

Notes: The p-value corresponding to the null hypothesis of no correlation is listed under each estimated correlation. The satisfaction response is treated as a linear variable. 900 observations each for Treatments A and C, except 864 used for prior round payment. 800 observations used for Treatment B, except 768 observations used for prior round payment

Table 3: Percent of Decisions Consistent with Expected Payoff Maximization

	Percent of Coin Choices Consistent with Expected Payoff Maximization	Percent of Subject-rounds in which all Five Coin Choices Consistent with Expected Payoff Maximization	Percent of Subjects who always Chose Consistently with Expected Payoff Maximization
A) Pooled	92%	74%	33%
B) By Treatment			
Treatment A (36 subjects)	92%	74%	39%
Treatment B (32 subjects)	91%	75%	34%
Treatment C (36 subjects)	93%	73%	25%
C) By Partner-type			
20-80 (expected payment 4)	94%	77%	na
35-65 (expected payment 3.25)	86%	58%	na
50-50 (expected payment 2.5)	100%	100%	na
65-35 (expected payment 3.25)	85%	55%	na
80-20 (expected payment 4)	95%	81%	na

Notes: 900 observations used for Treatments A and C. 800 observations used for Treatment B.

As seen in the table, the percentage in the second column is 15-20% lower for each grouping. Panel C of the table shows that the drop is much larger for the 35-65 partner-types—about 30%—than for the 20-80 and 80-20 partner-types—about 15%. The third column reports the percent of subjects who always chose consistently with expected payoff maximization. We see that 33% of the (pooled) subjects always chose consistently, and there is some variation across the treatments. Overall, these results indicate that although subjects' choices are not always consistent with expected payoff maximization, a very large majority of the individual coin choices are consistent, which in turn suggests that the subjects understood the game.

Regression Analysis

Regression analysis of these data must confront serious econometric issues, two of which stand out. The first is that prior work establishes the presence of significant individual fixed effects likely due to fixed personality or genetic traits (e.g., Diener and Lewis 1999; Clark and Oswald 2002; Ferrer-i-Carbonnell and Frijters 2004; Frijters, Haisken-DeNew, and Shields 2004), and the second is that reported satisfaction is a discrete dependent variable. Unfortunately, the normal methods for analyzing fixed effects and discrete dependent variables concerns cannot be used here without some cause for concern. Fixed, individual level factors, such as personality traits, are normally captured using fixed effects OLS estimation, yet OLS in this context assumes an explicit cardinalization between the satisfaction responses. Economists dislike this explicit cardinaliztion and prefer instead to use ordered probit or logit analysis. Unfortunately, it is known that using fixed effects in ordered probit or logit analysis yields inconsistent estimates due to the incidental parameters problem.[16] Methods have been developed for fixed effects in a binary discrete dependent variable case (see Winkelmann and Winkelmann 1988, Ferrer-i-Carbonnell and Frijters 2004), yet applying those methods here involves collapsing seven categories to two, thereby losing valuable variation in the reported satisfaction variable.

Because there is no ideal econometric method, my approach here is to present a variety of regression results and look for an emergent picture in the data. I focus first on three particular regression specifications. The first is a standard ordered probit regression without fixed effects, which is the traditional approach used by economists in the happiness literature. Table 4 Panel I displays results from three such ordered probit regression, one for each treatment. In each

[16] In words, the incidental parameters problem occurs because the coefficient estimates are a function of the fixed effects estimates which, when estimated in the probit and logit setting, are not consistent (see Green 2003).

regression, an individual's payment has a positive impact on the reported satisfaction latent variable, while an individual's expected payment has a negative impact on reported satisfaction. These coefficients are highly significant and have the predicted signs. Contrary to the prediction, however, the coefficient on the prior round's payment is always positive, although it is also always very small in magnitude relative to the distance between the cut-points and is only of borderline statistical significance. The coefficient on the overall average of all others up through the last round is also positive contrary to the prediction, is always small relative to the cut-point distances, and statistically significant in Treatment A only. I also included the round number to capture a possible time effect. The round coefficient is always very small in magnitude. I note that in other regressions I used variations on the round, such as round squared, and only ever obtained coefficients of very small magnitude and usually not statistically significant. To conserve space, I do not report those regressions.

Regressions I-B and I-C include different measures of others' payoffs. Because subjects in Treatment B were told the overall average of others for that round, I include that measure as another independent variable in regression I-B. The coefficient on that variable is negative as predicted, has a non-trivial magnitude, but is not statistically significant. Subjects in Treatment C were told the average of others by partner-type. Regression I-C captures this distinction using the average of other type averages as one variable and the average of one's own type average as another variable. The coefficient on own type average is negative as predicted, has non-trivial magnitude, but has low statistical significance. The coefficient on the other type average is very small in magnitude and from statistically significant. As will be discussed below, it appears that subjects in Treatment C only compare their outcomes with the outcomes of players who faced the same partner-type.

Table 4: Regression Results

	Panel I: Ordered Probit			Panel II: FE Logit			Panel III: FE Probit OLS		
Panel-Treatment	I-A	I-B	I-C	II-A	II-B	II-C	III-A	III-B	III-C
Constant	--	--	--	--	--	--	-1.506**	-1.512**	-1.74**
							(0.120)	(0.248)	(0.364)
Payment	1.507**	1.752**	1.552**	4.717**	4.330**	5.532**	0.705**	0.752**	0.707**
	(0.055)	(0.064)	(0.056)	(0.384)	(0.380)	(0.518)	(0.015)	(0.014)	(0.014)
Exp. Payment–Max.	-0.538**	-0.471**	-0.192*	-1.388**	-0.779**	-0.941**	-0.231**	-0.190**	-0.091**
	(0.076)	(0.078)	(0.112)	(0.307)	(0.258)	(0.480)	(0.030)	(0.029)	(0.045)
Prior Round Payment	0.063*	0.057	0.051	0.338**	0.156	0.314**	0.001	0.023*	0.020
	(0.034)	(0.035)	(0.033)	(0.130)	(0.119)	(0.137)	(0.014)	(0.013)	(0.013)
Avg. Pymt. through Prior Rd.	0.094**	0.004	0.005	0.168	0.133	0.284	0.015	0.027*	0.005
	(0.037)	(0.041)	(0.037)	(0.152)	(0.136)	(0.176)	(0.015)	(0.016)	(0.016)
Overall Average of Others	--	-0.214	--	--	-0.854	--	--	-0.129*	--
		(0.193)			(0.632)			(0.070)	
Avg. of All Type Averages	--	--	-0.020	--	--	-0.476	--	--	-0.008
			(0.225)			(0.861)			(0.089)
Own Type Average	--	--	-0.138	--	--	-0.926**	--	--	-0.077**
			(0.090)			(0.358)			(0.036)
Round	0.010*	-0.003	-0.008	0.001	0.002	0.020	-0.006**	0.000	-0.003
	(0.006)	(0.007)	(0.006)	(0.021)	(0.022)	(0.025)	(0.002)	(0.002)	(0.002)
Cut-point 1/2	1.163	0.702	1.695	--	--	--	-1.191	-1.251	-1.080
	(0.296)	(0.691)	(0.926)						
Cut-point 2/3	2.003	1.797	2.556	--	--	--	-0.749	-0.763	-0.660
	(0.296)	(0.691)	(0.927)						
Cut-point 3/4	2.685	2.747	3.280	--	--	--	-0.418	-0.374	-0.334
	(0.298)	(0.694)	(0.929)						
Cut-point 4/5	3.876	3.783	4.300	--	--	--	0.160	0.075	0.122
	(0.309)	(0.698)	(0.933)						
Cut-point 5/6	4.371	4.471	4.865	--	--	--	0.421	0.385	0.393
	(0.314)	(0.701)	(0.936)						
Cut-point 6/7	5.386	5.491	5.707	--	--	--	0.977	0.887	0.820
	(0.326)	(0.706)	(0.938)						
Observations	864	768	864	864	768	864	864	768	864
R^2	0.31†	0.36†	0.33†	0.74†	0.71†	0.79†	0.68††	0.75††	0.72††

Notes: Each regression used the last 24 rounds of the experiment session. Standard errors are listed in parentheses. Treatments A and C had 36 subjects each, and Treatment B had 32 subjects. * and ** denote significance at 10% and 5% levels, respectively. Panel I cut-points are estimated by the ordered probit procedure. Panel III cut-points are derived directly from the sample distribution as described in the text. † denotes Psuedo R^2, and †† denotes Overall R^2.

Although some of the ordered probit coefficients, such as those on the payment and expected payment, match our predictions, not all do. Before rejecting those non-verified

predictions, however, we must recognize that this finding may be due to the ordered probit regressions' not accounting of individual fixed effects. Ferrer-i-Carbonnell and Frijters (2004), for example, examine various methods for accounting for individual fixed effects in an ordinal setting and find significantly different results when compared to ordered probit results. These methods take advantage of the fact that we can do fixed effects in a binary dependent variable setting. With k response categories, one such method is to recode the dependent variable to take value 1 if the actual response is equal to or greater than x, $0 < x < k$, and take value 0 otherwise. Because this recoding involves losing observations if an individual's responses never cross the threshold x, Ferrer-i-Carbonnell and Frijters (2004) suggest using an individual specific threshold. Their new binary variable takes value 1 if the response is greater than the individual's average response. No observations are lost if all individuals' responses change at least once.

Table 4 Panel II presents the results from this second FE logit regression method. Though the Panel II coefficients do not have the same statistical interpretation as the Panel I coefficients, they do tell a qualitatively similar story in both coefficient significance and relative magnitudes with one important exception. We now see the social comparison effect to be highly significant and of magnitude similar to the expectation effect in Treatment C. Other variables constant, subjects in Treatment C report lower satisfaction if they received a payment lower than the average payment of the other subjects who faced a similar partner-type. Though the social comparison effect in Treatment B is still not precisely estimated in Regression II-B, the t-value is larger in absolute value than the corresponding t-value in Regression I-B, and it is of magnitude similar to the expectation coefficient.

The lack of precision in the social comparison coefficient in Regression II-C may be due to the reduction in variance in the data that results from collapsing seven response categories to

two. Because much information is lost due to this dependent variable transformation, it is worth asking if a different, statistically meaningful approach could retain that variation. One method to retain this variation and simultaneously account for fixed effects is to do fixed effects OLS, however, as mentioned earlier this approach assumes that the difference between response k and $k + 1$ is identical to the difference between response k' and $k' + 1$. A variation on this, called "probit OLS" by Van Praag (forthcoming) ("probit-adapted OLS by by Van Praag and Ferrer-i-Carbonnell (2004)), is to do OLS within a latent variable context that does not assume equidistant dependent variables.

Probit OLS (POLS for short) consists of transforming the discrete dependent variable according to properties of the sample distribution and then performing OLS estimation on the transformed variable. As in ordered probit and logit analysis, POLS assumes the satisfaction responses arise from an underlying latent variable process: the probability respondent i reports satisfaction level k is equal to the probability that a latent variable z_i is in interval $[\mu_{k-1},\mu_k]$. Unlike in ordered probit and logit analysis, in POLS the μ cut-points are defined according to the overall sample distribution. With k categories, $\mu_0 = -\infty$ and $\mu_k = \infty$, $\mu_{k'}$ is derived as the solution to $F(\mu_x) - F(\mu_{x-1}) = p_x$ for $x = 1, 2, \ldots, k - 1$ where p_x is the proportion of observations in the sample that had response x and F is the normal distribution function. The response variable k is then replaced by $z(k) = (f(\mu_{x-1}) - f(\mu_x)) / (F(\mu_x) - F(\mu_{x-1}))$, where f is the normal density function. In words, the transformation represents the average value in between the two cut-points. The rationale is as follows: because we do not know where in the range the latent variable will be, we use the average value of that range.

When applied to happiness studies, the POLS regressions coefficients are the same (given a multiplicative factor) as those from ordered probit regressions (see Van Praag (forthcoming)

and Van Praag and Ferrer-i-Carbonnell (2004)). Not only does the POLS approach allow us to retain the inherent discreteness in ordinal satisfaction data and obtain the same results as ordered probit, it also retains the flexibility of linear regression analysis. More important for this paper is the fact that fixed effects linear regressions do not suffer from the incidental parameters problem that plagues fixed effects ordered probit regressions.

Table 4 Panel III presents results from POLS regressions with fixed effects. The bottom part of the panel displays the cut-points derived as described in the previous paragraph, while the top part displays the regression coefficients. These regressions explain a large percent of the variation in the data (overall R^2s between 0.68 and 0.75) and also exhibit a pattern similar to those in Panels I and II. The subject's payment has the single largest impact on the reported satisfaction latent variable, and the expectation effect is roughly half as large. Glancing at the cut-points, we see that these magnitudes are meaningful. For example, suppose a subject's latent variable is -0.8, which predicts reported satisfaction of 2. All else equal, an increase in payment of 1 in any treatment increases the predicted satisfaction to 4, and a further increase in payment of 1 increases the predicted satisfaction to 6. A change in expected payment has a smaller though still meaningful effect. If we start with a latent variable of 0.6 in Treatment B, which is the average value in the response 6 category in that treatment, we must increase expected payment by 1.13 for predicted satisfaction to drop to 5. The coefficients on prior payment, average payment through the prior round, and the round all have very small magnitudes, thus indicating a negligible impact on reported satisfaction.

We also see that the coefficient on average payment of all others in Treatment B now achieves greater statistical significance. The magnitude of the coefficient is meaningful, as well.

Starting with a latent variable of 0.6 in Treatment B, an increase in the average payment of all others by 1.68 will decrease predicted satisfaction from 6 to 5.

Altogether, we can say that there is strong evidence that the expected payment effect is meaningfully at work, that there is evidence that the social comparison effect is meaningfully at work, and that there is no evidence that the past payment effect is meaningfully at work. Moreover, we can say that the expected payment and social comparison effects appear to be similar in magnitude both within and across treatments (within the same regression framework), and that these effects are of much smaller than the direct impact of payment on reported satisfaction.

Indeed, it appears that the expectations and social comparisons work separately to reduce satisfaction. That this appears true in Treatment C is of particular interest. Clearly, the higher the expected payment for one partner-type, the higher the average payment of those matched with that partner-type, so that the two measures are highly correlated in Treatment C (unconditional correlation about 0.75, as seen in Table 2C). Although this raises the possibility of multicollinearity, a linear regression test for multicollinearity using variance inflation factors finds no effect of multicollinearity. [17]

The evidence also indicates that subjects in Treatment prefer to compare themselves with those who faced similar partner-types rather than with all other subjects. Key here is that subjects in Treatment C had information by partner-type while those in Treatment B did not. If the type of information given the subject determines the type of comparison made, then a regression using Treatment A data but including the Treatment B social comparison variable or

[17] The variance inflation factor for variable j is $1/(1-R^2_j)$, where R^2_j is the square of the multiple correlation coefficient from the regression of the jth expanatory variable on the remaining variables. One rule of thumb is that a factor of 10 or higher indicates problematic multicollinearity, and the VIFs for the comparison and expected payment coefficients are not greater than 3 in the linear regression performed.

the Treatment C social comparison variables should yield insignificant social comparison coefficients. I conducted a series of regressions to test these hypotheses (results not shown), and in each case the prediction was confirmed. I conclude that having information about others' payments is necessary for social comparison effects to influence reported satisfaction. Similar predictions are that type specific averages reported in Treatment C comparison should not impact reported satisfaction when using Treatment B data, and that the overall average payment should not impact reported satisfaction when using Treatment C data. Regressions confirm these predictions (results not shown).

Why the Treatment C subject identifies only those who faced the same partner-type, as opposed to the entire session population, as her social reference group is likely related to the notion of what is a proper comparison. Festinger (1954) first hypothesized that an individual compares herself with someone of similar ability or opinion, and subsequent research has refined the hypothesis so that the compared individual is one whose performance or characteristics relating to performance is close to those of the comparer (Goethals and Klein 2000). In my experiment, this translates to similarity in payoff opportunities and expectations instead of personal attributes because knowledge of others' payoff opportunities is all the subjects know about other subjects in Treatment C. Intuitively, to assess your success by comparing your outcome with the outcomes of others who were expected to do much better (because of a higher expected payoff) or much worse (because of a lower expected payoff) would not be proper because their opportunities do not match your own. When a subject does not have the specific information about others by partner-type, then her reference group expands to include all subjects. Yet, it appears that a person will only consider all others if she lacks more specific

information about the others, so that the set of all others is not the preferred reference group for comparison.[18]

The coefficients on the prior round payment and average prior payments are always very small and with signs opposite of predicted. One possible explanation for the small magnitude positive sign is that the adaptation mechanism proposed in the income-happiness literature—that individuals essentially get "accustomed to" certain payoff levels—does not have time to take effect during the short duration of the laboratory experiment (each round lasted usually a minute or two, and the entire experiment lasted approximately an hour). Indeed, it appears that in the short run, the effect, though very, very small and not always significant, is opposite. A higher past payment actually decreases a subject's aspiration level and increases satisfaction, though only in a miniscule manner. This could be due to a positive but very small "glow" effect from getting a high payment.

Another explanation is changes in the decision rule used by the subject. If a subject receives a high payment and thus reports a high satisfaction level, then it is likely that the subject used an appropriate decision rule and will stick to that rule in the next round, thereby increasing the chance of a good outcome and high satisfaction in the next round. The positive coefficient could indicate an endogeneity problem. That the correlation between past payment and current payment is neither large nor statistically significant (Table 2) suggests that this is not happening. Another way to assess this hypothesis is to regress a measure of the current round's decision on last period's satisfaction. I performed such a regression using the change in difference between

[18] I also ran regressions (not shown) that controlled for the partner-type when making the comparison with the prior round payment. I replaced the prior round payment with two variables, one that took the prior round payment if the partner-type was the same as the last round and zero otherwise, and another that took the prior round payment if the partner-type differed. This made little difference in the regressions, which suggests that when comparing own payments across rounds, the subjects do not consider the partner-type faced in prior rounds.

the "expected payoff—maximization" and "expected payoff—choices" measures. The results (not shown) suggest that if this effect is at work, it has a negligible effect.

A third possible interpretation suggests a rethinking of prior research on income aspirations. Suppose income expectations form the largest component of income aspirations, just as payoff expectations form the largest component of payoff aspirations in the experiment. Suppose also that, unlike payoff expectations and past payment in the experiment, income expectations and past income are highly correlated. In other words, suppose that when individuals form their income expectations, they base them largely on past income. In this case, the significant coefficient on past income in income-happiness regressions (e.g., Stutzer 2004) may be due to the influence of past income on income expectations instead of a direct effect of individuals getting accustomed to past consumption levels. Future empirical work must determine if this is indeed the case.

It is worth noting that the payment coefficient in each regression is larger than the sum of the aspiration coefficients which implies that an increase in the payment has a larger impact on satisfaction than a simultaneous equivalent change in each aspiration factor. The A-1 regression can be represented as

$$z(k_{it}) = \alpha_i + \beta_0 + \beta_1 y_{it} + \beta_2 (\theta_1 \mathrm{E}y_{it} + \theta_2 y_{it\text{-}1} + \theta_1 \Sigma_{t'=1}^{t\text{-}1} y_{it'} /(t\text{-}1)) + \beta_3 round_t + \varepsilon_{it},$$

where the results shown in Table 5 include aspiration factor estimates $\beta_2\theta_1$, $\beta_2\theta_2$, and $\beta_2\theta_3$. If an increase in payment is perfectly offset by corresponding increases in aspiration factors, then a fixed effects POLS regression with the restriction $\beta_2\theta_1 + \beta_2\theta_2 + \beta_2\theta_3 = -\beta_1$ should yield statistically similar estimates. A test of this hypothesis for regression III-A strongly rejects this hypothesis with a $F(1,823)$-statistic of 212.04 for regression III-A. Adding the social comparison coefficients for Treatments B and C and conducting similar tests also strongly reject

the hypothesis with $F(1,730) = 43.70$ for Treatment B and $F(1,821)=22.34$ for Treatment C. An increase in payment is not completely offset by equivalent changes in the aspiration factors.

Other Issues

All regressions displayed in Table 4 used the Expected Payment—Maximization measure, which is the expected payoff if the subject chose consistently with expected payoff maximization. There are good reasons to use that measure. First, when a subject's coin choices are not perfectly consistent with expected payoff maximization, it is not clear what she perceives her expected payoff to be, and the Expected Payment—Maximization measure, given that so many choices are consistent with expected payoff maximization (Table 3), is one measure that should closely approximate the subject's subjective measure of her expected payment. Second, any deviation by a subject from the expected payoff maximizing behavior would yield her an actual expected payment lower than if her choices were consistent, but we might suspect that she would not deviate unless she perceived an increase in her expected payment. In this sense, the Expected Payment—Maximization variable would better represent the subject's subjectively calculated expected payment than the Expected Payment—Choices measure. Nonetheless, I have done these same regressions using the Expected Payment—Choices variable, and the results (not shown) do not differ in any substantive way. This is not surprising given the very high correlation between the two measures (0.79 to 0.90 across the treatments). Because all other independent variables exactly represent the information given to the subjects, we need not worry about alternate measures for the other variables.

Another issue is possible misspecification in the regression. As described earlier in the discussion of Table 4, I ran a series of regressions (results not shown) with various combinations

of independent variables and found that the comparative magnitudes of the aspiration coefficients retain their same rankings. Another dimension of specification is in the dependent variable. One specification of interest collapses the seven category reported satisfaction variable to two categories—all observations with a report of "dissatisfaction" (reported satisfaction 1, 2, or 3) take value 0, and all observations with a "satisfied" report (4, 5, 6, or 7) take value 1—and then runs a fixed effects binary logit regression. I argued above that this regression was not ideal for the primary purpose of this paper because it loses valuable variation in the satisfaction variable, yet it is of interest for other reasons. First, we might suspect that a subject is more likely to accurately report if they are "satisfied" or "dissatisfied" than accurately report if her satisfaction is, say, 6 or 7 on the satisfaction scale. Second, it might also be true that the cognitive process a subject uses in deciding whether she is satisfied or dissatisfied is different than that she uses to identify how satisfied she is within the dissatisfied or satisfied subranges.

Table 5 Panel I displays the results from a fixed effects logit regression for each treatment using this satisfaction-dissatisfaction binary variable. The relative magnitudes of the coefficients within treatments remain similar to the Table 4 regressions. The primary thing to note is that the social comparison coefficients for Treatments B and C are not statistically significant and that they are much smaller in magnitude than the expectation coefficients. In the Treatment B regression, the social comparison coefficient is about half as large as the expectation coefficient, and in the Treatment C regression, it is roughly a quarter as large. One reason for this finding is that the expectation and comparison effects may differ at different outcomes. For example, in the income-happiness literature, McBride (2001) finds that the social comparison effect at higher income levels is stronger than at lower income levels, the idea being that relative standing increases in importance once a subsistence living has been achieved.

Table 5: Fixed Effects Probit OLS Regression Results for Low and High Satisfaction

	Panel I: Low-High FE Logit			Panel II: Low and High FE POLS			
Treatment	A	B	C	B		C	
Satisfaction Categories	{1-3,4-7}	{1-3,4-7}	{1-3,4-7}	{1,2,3}	{4,5,6,7}	{1,2,3}	{4,5,6,7}
Constant	--	--	--	-0.024 (0.640)	-2.73** (0.404)	-0.717 (0.777)	-2.49** (0.568)
Payment	5.35** (0.524)	5.33** (0.554)	5.57** (0.552)	0.869** (0.051)	0.934** (0.035)	0.728** (0.044)	0.792** (0.033)
Exp. Payment–Max.	-2.34** (0.392)	1.887** (0.373)	-1.607** (0.518)	-0.305** (0.071)	-0.155** (0.047)	-0.196** (0.093)	-0.062 (0.071)
Prior Round Payment	0.375** (0.162)	0.161 (0.150)	0.197 (0.144)	-0.007 (0.030)	0.041* (0.021)	0.038 (0.028)	0.009 (0.020)
Avg. Pymt. through Prior Rd.	-0.092 (0.161)	0.173 (0.215)	0.248 (0.191)	0.025 (0.048)	0.040* (0.023)	0.046 (0.037)	-0.003 (0.024)
Overall Average of Others	--	-0.965 (0.810)	--	-0.231 (0.174)	-0.196* (0.113)	--	--
Avg. of All Type Averages	--	--	-0.881 (1.027)	--	--	-0.041 (0.190)	-0.011 (0.137)
Own Type Average	--	--	-0.430 (0.379)	--	--	-0.105 (0.078)	-0.100* (0.057)
Round	-0.059** (0.027)	-0.024 (0.026)	0.004 (0.025)	-0.012** (0.024)	0.004 (0.004)	-0.001 (0.005)	-0.003 (0.004)
Cut-point 1/2	--	--	--	-0.531	--	-0.307	--
Cut-point 2/3	--	--	--	0.032	--	0.495	--
Cut-point 4/5	--	--	--	--	-0.606	--	-0.658
Cut-point 5/6	--	--	--	--	-0.106	--	0.090
Cut-point 6/7	--	--	--	--	0.552	--	0.449
Observations	840	768	840	272	496	319	545
R^2	0.79†	0.78†	0.80†	0.38††	0.53††	0.30††	0.48††

Notes: Each regression used the last 24 rounds of the experiment session. Standard errors are listed in parentheses. Treatments A and C had 36 subjects each, and Treatment B had 32 subjects. Observation for a subject were dropped in Panel I regressions each for Treatments A and C. * and ** denote significance at 10% and 5% levels, respectively. Cut-points are derived directly from the sample distribution as described in the text using the appropriate sample. † denotes Psuedo R2, and †† denotes Overall R2.

To see if a similar relationship arises in this experiment, I ran two additional regressions for Treatments B and C each. The first is a fixed effects POLS regression using only the dissatisfied responses (responses {1,2,3}), and the second is a fixed effects POLS regression

using only the satisfied responses (responses {4,5,6,7}). Performing these regressions involved using a new transformation of the dependent variable. The cut-points from this new transformation and the regression coefficients are displayed in Table 5 Panel II. We cannot directly compare coefficients across regressions because they use different data, cut-points, and categories. However, we are more interested here in the relative magnitudes of the coefficients within regressions. Bearing this in mind, we do observe a dramatic change in the relative magnitudes of the coefficients. Whereas the expectation coefficient is much larger than the social comparison coefficient in each dissatisfied regression, the expectation coefficients are smaller than the social comparison coefficients in the satisfied regressions. Moreover, the social comparison coefficients are not statistically significant in either low category regression but are in each high category regression, and the expectation coefficient is not statistically significant in the satisfied regression for Treatment C.

Taken at face value, these results imply that the nature of aspirations depends on the satisfaction level, with social comparisons increasing in importance at high satisfaction levels. Some caution must be used when making this claim because the standard errors are large enough to warrant second thought. Nonetheless, we can interpret this conclusion in a manner that synthesizes the various regressions. The expectation effect plays a stronger role than the social comparison effect in determining whether a subject is satisfied or dissatisfied, while the social comparison effect plays a relatively larger role in determining how satisfied a subject is within the range of satisfied but not dissatisfied responses. This interpretation is consistent with the basic conclusion that expectations matter more in overall aspiration formation than social comparisons because expectations play a larger role in the determination of the boundary between satisfied and dissatisfied. It also implies that the aspiration function is not linear in the

aspiration factors. Instead, social comparisons increase in relative significance as subjective achievement improves and expectations decrease in relative significance.

Surprisingly, these experimental results support the finding reported in the income-happiness literature that comparisons with others' incomes increase in importance after basic needs are met. That this result is found in an experimental setting with relatively small experimental payoffs suggests that the income-happiness finding is not merely an artifact of the data but instead a reflection of a more fundamental process of aspiration formation.

5. Conclusion

Subjects' reported satisfaction levels do depend on their aspiration levels as well as their outcomes. The largest factor in payoff aspirations is the subject's expected payment, which comprises the primary factor a subject considers when deciding whether or not her achievement is satisfactory or dissatisfactory. The next highest aspiration factor is social comparison with other subjects, and there is evidence that this factor is stronger at higher satisfaction levels. When a subject makes a social comparison, she compares her outcome with all others if she only knows the average of all others payoffs, but she prefers to compare herself with subjects similar to herself. As predicted, an increase in either expected payment or the average payoff of her social reference group increases the subject's aspiration level, thereby decreasing satisfaction when holding outcome constant. Contrary to the prediction, there is a negligible impact of previous high payments. Overall, these findings support many of the claims in the recent income-happiness literature. Happiness does depend on aspirations, and these aspiration levels vary in measurable ways according to circumstances.

There are many avenues for future research. Future work should look more closely at differences in aspiration formation across individuals. While my work accounts for fixed individual factors, it assumes that the aspiration formation factors have the impact for all individuals. Because prior work finds evidence of differences in the marginal impact of income and happiness (Clark *et al* 2005), further examination of differences in coefficients could yield additional insights into the variation in reported happiness observed in the data. Another issue relates to the intriguing evidence reported that the comparison effects appear to be stronger at higher satisfaction ranges than across the whole range of possible satisfaction. In the spirit of Konow and Earley's (2003) experiment, a future experiment could examine how the different aspiration factors affect other subjective measures in addition to satisfaction, such as "How happy are you with your outcome?" and "How bad do you feel about your outcome?" It may be the case that the different aspiration factors affect positive and negative feelings in different manners. A closer examination of this phenomenon can help us understand in more detail the nature of aspiration formation. Finally, researchers should study how aspirations matter across different strategic environments.

Laboratory experiments provide a fruitful way to study these and others questions related to happiness. The experimenter can not only control many factors of interest, such as subjects' information, but can also obtain accurate measures of the factors thought to affect aspiration levels, such as expectations, previous outcomes, and information about others' outcomes at a small fraction of the cost of a large longitudinal survey. Such work will improve our understanding of the determinants of aspirations and happiness.

Appendix: Experiment Instructions

[Introduction.] Hello, and welcome to the California Experimental Social Science Laboratory. Thank you for coming. I am Michael McBride, a professor in the Department of Economics at UC Irvine, and my email address is mcbride@uci.edu.

By participating in today's experiment, you will receive a show-up payment of $5, but you will also receive additional monetary payments. Your actual total payment will depend on your decisions and decisions made randomly by the computer.

As the experiment progresses, you will amass a number of computer "coins." When we are finished, you will receive actual US dollars according to a coin-dollar exchange rate. The more coins you amass, the more US dollars you receive. The exchange rate is 8 coins for 1 dollar. Each participant in the room is likely to take home a different amount. You are under no obligation to tell your amount to anyone.

We should finish in approximately 60-75 minutes. At any time during the experiment, you are free to end your participation. Should you decide to stop your participation, you will still receive the show-up payment.

I will be giving you verbal instructions. You will not be deceived in any way by my instructions, and you must follow my directions. Do not take any initiative by typing or mouse-clicking before I tell you. You are not allowed to communicate with other students in the room. When you have a question, please raise your hand. Please do not kick the computer or the computer wires under your desk. And please silence your cell phones.

[Informed Consent.] The purpose of this experiment is to study decision making in a particular environment. Today you will be placed in that decision making environment through the use of the computer, and your choices will be recorded. Because all decisions are made privately over the computer, there are no foreseeable risks or discomforts to any participants. This study will benefit society by providing information about decision making. CASSEL will store the data obtained from your decisions on its computer. I, the researcher, will also maintain a copy of the data on your choices only. I will not have access to your names nor any other identifying personal information.

In order to participate, you must give your consent to participate. By registering on the CASSEL web page for today's session, you acknowledged that your participation is voluntary.

You are again reminded that your participation is voluntary, and that there is no penalty to ending your participation during the experiment. Should you end your participation, you will still receive the show-up payment. If you wish to end your participation now, please notify me by raising your hand.

[Basic Experiment Set-up.] Today you will participate in 2 practice rounds and 25 real rounds of decision making. In each round, you will do two things. First, you will choose "heads" or "tails" for a sequence of coins. Second, you will answer a question about the outcome of the rounds. After all rounds are over, you will next answer a brief questionnaire. The experiment ends after you have completed the questionnaire.

Let me now describe the heads-or-tails coin choices. In each round, you will be paired with a partner. For each of 5 coins—coin 1, coin 2, coin 3, and so on—you will choose heads or tails, and your partner will do the same. If you and your partner both choose the same coin side

for coin 1, then you will receive coin 1. That is, if you match, so that you both choose heads or you both choose tails, then you receive the coin. But if the coins mis-match, so that one of you chose heads and the other chose tails, then your partner receives the coin.

Your partner's choices will be determined randomly by the computer, however, the exact likelihood that the computer chooses heads or tails for each coin will depend on what we will call the partner type.

There are five possible partner types, and they are shown on the screen. The first partner type will choose heads with 20% likelihood and tails with 80% likelihood. The second type will choose heads with 35% and tails with 65%. The third will choose heads 50% and tails 50%. The fourth will choose heads 65% and tails 35%. And the fifth will choose heads 80% and tails 20%.

At the start of the round, the computer will randomly select your partner to be one of these types. Your computer will then tell you which type was chosen. Next, you make your coin choices. While you make your coin choices, the computer will make the partner's coin choices according to the percent likelihoods associated with your partner's type.

Notice that the computer does two things. First, the computer randomly selects your partner's type. Second, the computer randomly selects heads or tails for each coin based on the partner type it already selected.

[Begin Software and Subject Login.] Let us now login to the computer to start the practice rounds.

Please double-click the icon on your computer. Please double-click only once. If nothing happens, please raise your hand, and I will help you.

When prompted by the computer to type your name, do not type your name but instead type the computer number on the sticker on your monitor. For example, it might say SSEL85. Type that in for your name, and press SUBMIT.

Once you have done so, please wait for the main layout to appear on your monitor. Do not type anything else or mouse-click anywhere, even after the main layout appears.

[Practice Rounds.] You should now see the main layout on your monitor. It should look like what is displayed on the screen at the front of the room. The layout is divided into three sections, Section A on the top, B in the middle, and C on the bottom.

In Section A, you should see a number of columns running across the top of the screen. These record information about the experiment and about the choices that have been made.

Look at the first two columns on the left. The first column is the round number, and right now it says round 1. The second column states the partner type the computer selected for you. To remind you, the partner type is the percent likelihoods that any given coin will be heads or tails. The 50-50 "%H" means the percent likelihood that "heads" is chosen is 50%, and the percent that tails is chosen is 50%. For practice round 1, everybody is paired with a 50-50 partner.

During the real rounds, the computer will select your partner type randomly, and it will select other participants' partners randomly, as well. So, your neighbor might or might not have the same partner type that you have.

The columns under the heading "Coin Matching Outcomes" will summarize the coin choices for that round, and the last columns on the right record other information about the round.

The practice rounds are only for you to familiarize yourself with the computer layout and decision making environment. Coins amassed during the practice rounds will not be counted towards your overall payment.

Let's do the first practice round. In Section B is where you make you coin choices. For each of the 5 coins listed, please select either heads or tails. When you have finished making your coin choices, please press the SUBMIT button on the right side of Section B. Once you press SUBMIT, your coin choices will appear in the "Coin Matching Outcomes" columns.

Please note three important facts about the coin choices. First, once you press SUBMIT, you cannot go back and change your coin choices. Pressing SUBMIT means you are done with your coin choices for that round. Second, each coin choice made by the computer is done independently. That is, whether or not the computer's coin choice is heads or tails for coin 1, it still uses the partner type probabilities when making the coin choice for coin 2, and so on. Third, you must press SUBMIT in order for the experiment to progress.

After SUBMIT buttons have been pressed, the main layout will then display the outcomes in the columns labeled "Coin Matching Outcomes." Please raise your hand if you do not see the coin matching outcomes.

In each out of the outcome columns, the first entry is your coin choice, and the second entry is the coin choice made by the computer. Look in the payment column in Section A. Your payment for that round is the number of coins you won, which is the number of coin matches.

[IF TREATMENT B] The computer will also tell you the average payment of all other subjects' in the room. This is reported in the "Others' Average" column.

[IF TREATMENT C] The computer will also tell you the average payment of all other subjects' in the room according to partner-type. This is reported in the "Others' Average by Partner Type" columns. In this first practice round, since everyone faced the 50/50 type, the column now tells you what the average payment was everyone else.

After observing the payment outcomes, you will then describe your satisfaction with the outcome of that round. Report your satisfaction in Section C of the main layout by choosing a satisfaction level on a scale of 1 to 7, where 1 means "very dissatisfied" and 7 means "very satisfied." After making your selection, press SUBMIT on the right in Section C to complete the round.

Are there any questions about round 1?

The round advances only after everyone has finished round 1. Let's now continue the rest of the practice rounds. Please make your coin choices and report your satisfaction levels for practice round 2. You will notice that the computer type is now different that in practice round 1. Raise your hand if you have any questions.

[Begin Real Rounds.] Let me remind you that there is no talking or communicating with any other student in the room during the real rounds. If you have a question, please raise your hand and I will come to you.

Finally, let me remind you about how the partners are chosen. The computer will select your partner type randomly, and it will select other participants' partners randomly, as well.

Any questions?

Please start the real rounds.

[Questionnaire.] You will now be asked to fill out the questionnaire. Please use the mouse or keyboard to answer all questions. Answering all questions should take approximately two minutes. There is no penalty for skipping a question. However, please note that if you give a response for each question on the questionnaire, then you will receive an additional __ dollars.

When you are done giving your responses, press the SUBMIT QUESTIONNAIRE button at the bottom of the page. You will then be instructed to fill out the receipt that was handed to you as you entered the lab today.

Thank you for participating today.

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