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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 choose to compare themselves with similar subjects when possible. These findings support an aspirations-based theory of happiness.

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.

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[1] Consider, for example, an 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 et al. (2008a,b) for a recent review of the economics of happiness literature. McMahan (2006) traces interest in happiness back through time to ancient Greece.

[2] For example, Lane (2000) argues that income maximization should not be a top policy priority, while Stevenson and Wolfers (2008) present evidence that implies raising income is an important policy priority.

[3] Frey and Stutzer (2002b) review 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), and a very recent treatment is Clark et al. (2008b). For a general discussion of key issues related to happiness of interest to economists, see Frey and Stutzer (2002a,b).

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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; Di Tella et al., 2003, 2007; 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; Senik, 2004, 2009; Ferrer-i-Carbonell, 2005; Luttmer, 2005; Graham and Felton, 2006; Knight and Song, 2009). Third, her aspirations depend positively on her expected outcome, such that a higher expected income affects 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 without these 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 determine not only 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. 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 von Winden, 2002; Konow, 2010; Konow and Early, 2008). However, 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 role of other aspiration factors.

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 who play at announced probability distributions. After being told outcome information for a round, each subject reports her subjective satisfaction with the outcome.

This paper reports multiple findings. 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 clear evidence that aspirations affect happiness. Second, the three aspiration factors influence satisfaction to different degrees. Both expected payment and the comparison payment negatively affect reported satisfaction in similar magnitudes, though they are an order of magnitude less than the actual payment. Previous payments have a negligible effect. Third, when making social comparisons with sufficiently detailed information, subjects do not compare themselves with all other subjects but instead compare themselves with those other subjects most similar to themselves. Specifically, they compare themselves with others who faced the same partner-type but not those who faced other partner-types.

These findings support an aspirations-based theory of happiness and pecuniary rewards, and they match certain 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.[8] 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

[5] This mechanism has received relatively little attention in the income-happiness literature by economists (Clark et al., 2008b). See Kahneman (1999) and Frederick and Loewenstein (1999) for larger discussions of adaptation and expectations. The work by Senik (2004, 2008, 2009) suggests a potential relationship between expected future earnings and others' income.

[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 et al. (2004) find that rising income in reunified Germany had a lasting impact on happiness for former East Germans.

[7] The experiment by Lazear et al. (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.

[8] Expectations in this experiment refer to expectations about the current round's payment, not the payment in future rounds. They should thus be distinguished from the positive effect of expectation on happiness found by Frijters et al. (2008) because expectations there refer to future income levels and not just the current period.

further examine this issue to determine how and to what extent aspirations over small outcomes aggregate to overall life happiness. Nonetheless, 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.[9]

Although economists have done relatively little experimental work closely related to this study,[10] 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).[11] 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 usually considered to be the status quo; however, in my experiment and in the income-happiness literature, the reference point depends on multiple aspiration factors.[12] 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 et al., 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 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 et al., 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,b), 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.

Fig. 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 Fig. 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.

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

[9] For discussion and research of how various life experiences aggregate to subjective happiness, see Kahneman (1999) and Van Praag et al. (2003).

[10] In addition to Charness and Grosskopf (2001), Bosman and von Winden (2002), Konow (2010), and Konow and Early (2008), there is work on the relationship between satisfaction and familiarity with the decision making environment (Novarese and Rizzello, 2005), and there is also some experimental work on reference points in auctions (Ham et al., 2005) and expectations and bargaining (Oliver et al., 1994).

[11] 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 (Kahneman, 1999). It is often the case that both types of adaptation will be in effect, as in my experiments.

[12] 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. Carter and McBride (2009) use my data set to more directly estimate a satisfaction function with the explicit goal of identifying whether its shape mimics the S-shape in Prospect Theory.

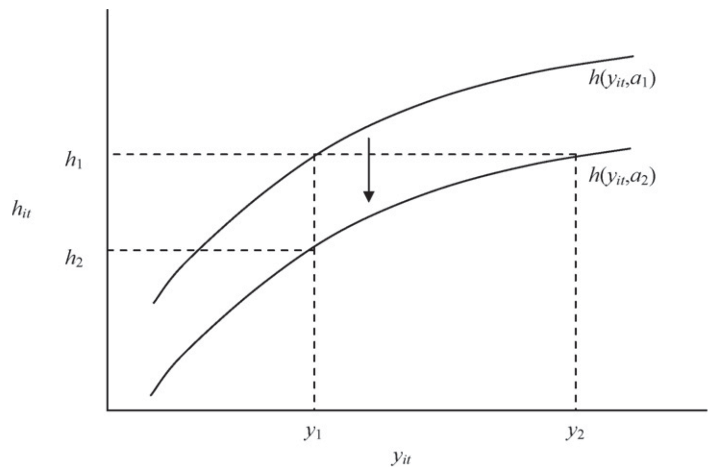

Fig. 1. A shift in a happiness function due to changing aspirations.

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 non-linear 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 $y_{it} \geq a_{it}$ 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 percent heads–80 percent tails.
35 percent heads–65 percent tails.
50 percent heads–50 percent tails.
65 percent heads–35 percent tails.
80 percent heads–20 percent 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 percent chance of being matched with a 20–80 type, a 20 percent 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. Fig. 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.

(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.
   d) The computer chooses heads or tails for five coins for each subject according to the partner-type distribution.
   e) The computer tells each subject her resulting coin matches and payment 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.
3. After 2(a)-(f) are repeated for each of 25 rounds, each subject answers a brief questionnaire.
4. Subjects receive US dollars according to 8 coins to 1 dollar exchange rate.

Fig. 2. The experiment design.

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 1–7, with 1 signifying "very dissatisfied," 4 signifying "satisfied," and 7 signifying "very satisfied."[13] 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.[14]

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[15] 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).[16] 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.

13 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.

14 See Diener (1984) for greater discussion on happiness and satisfaction questions.

15 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.

16 More information about CASSEL is available at their web site www.cassel.ucla.edu.

After entering the lab, each subject sits at a computer terminal, receives verbal instructions (available from the author upon request) 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. Fig. 2B summarizes the experiment order. Each treatment lasted approximately 1 h, 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 on 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 0–5 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 discussion

4.1. Preliminary examination of data

Fig. 3A pools the data across the treatments to display the average reported satisfaction as a function of payment received. This average is calculated treating the ordinal reported satisfaction variable as if it was cardinal. A larger circle represents a larger number of observations. 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. Fig. 3B breaks down the data by payment and treatment. Not only does reported satisfaction depend positively on the payment in all treatments, but also the treatment averages are similar to the pooled averages. This suggests consistency in reported satisfaction across treatments.

Fig. 3C displays the average satisfaction levels by payment and partner-type. For illustrative purposes, the data from subjects facing 20–80 and 80–20 partner-types were grouped, as were the data from students facing 35–65 and 65–35 partner-types. For each partner-type grouping, we observe the same positive relationship between satisfaction and payment. Observe the negative relationship between satisfaction and expected payment. For example, holding payment fixed at 3, the average satisfaction is highest for those with partner-type 50–50 (the partner-type that yields the lowest expected payment under expected payoff maximization), while the lowest satisfaction is for those with partner-type 20–80 or 80–20 (the partner-type that yields the highest expected payment). The same pattern is found for other payment levels. Overall, higher expectations decrease reported happiness, holding the payment fixed.

We can also assess the viability of using the expected payment under payoff maximization as a proxy for the subjects' subjectively perceived expected payment. As stated earlier, payoff maximization 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. We might ask, then, if using the actual expected payment given the subject's choices, would be a better proxy. However, the two measures are highly correlated (0.79, 0.90, and 0.86 in Treatments A, B, and C, respectively). This fact is not surprising because most of the coin choices are likely to be consistent with expected payoff maximization, as shown in Table 1. The first column in Table 1 reports the number of coin choices consistent with expected payoff maximization, which is overall and by treatment about 92 percent. The variation comes across partner-

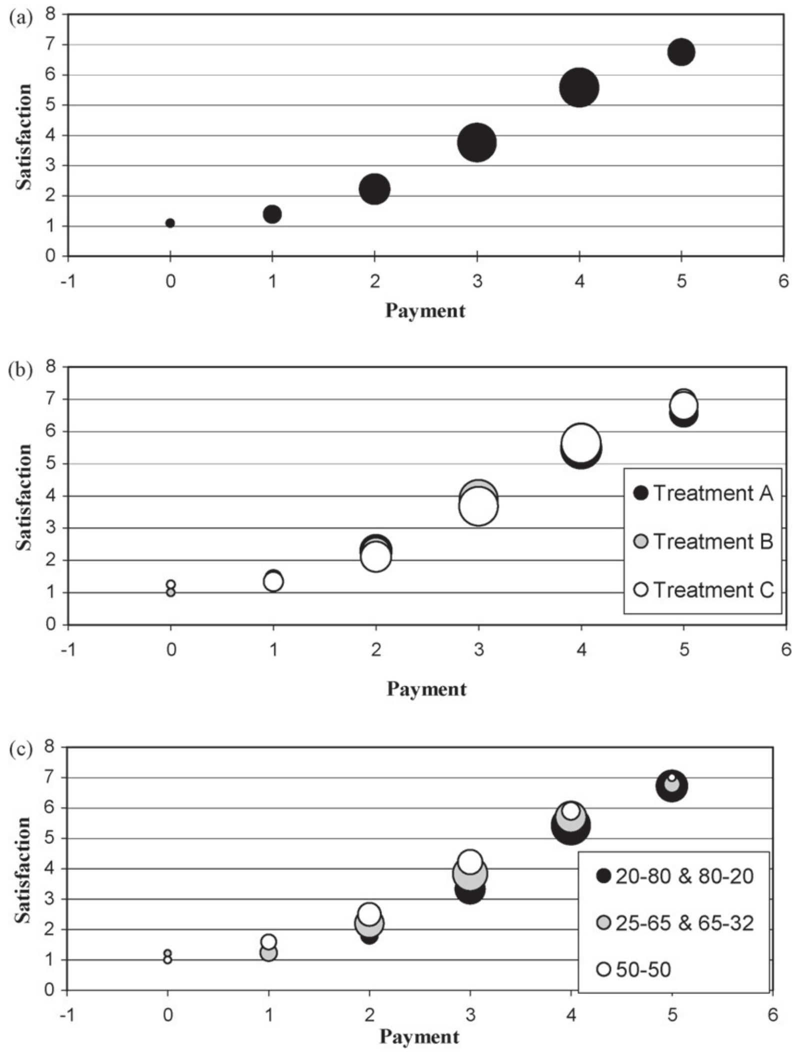

Fig. 3. (A) Average reported satisfaction by payment, weighted by number of observations. (B) Average reported satisfaction by payment and treatment, weighted by number of observations. (C) Average reported satisfaction by payment and expected payment, weighted by number of observations.

Table 1
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 fexoected Davment 4)	95%	81%	na

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

types, with the lowest choice consistency coming when players are matched with the 35–65 and 65–35 partner-types. The 100 percent 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. We see that 33 percent 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.

4.2. Regression analysis

Regression analysis must confront two critical econometric issues. 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; Ferrer-i-Carbonell and Frijters, 2004; Frijters et al., 2004). The second is that the reported satisfaction variable is a discretely ordinal. Individual level factors, such as personality traits, are normally captured using fixed effects (FE) OLS estimation, yet OLS in this context assumes an explicit cardinalization between the satisfaction responses. Economists dislike this explicit cardinalization because it assumes that the difference between response k and $k+1$ is identical to the difference between response k' and $k'+1$. They instead prefer to use ordered probit or logit analysis. However, it is known that using fixed effects in ordered probit or logit analysis yields inconsistent estimates due to the incidental parameters problem.[17] Methods have been developed for fixed effects in a binary discrete dependent variable case (see Winkelmann and Winkelmann, 1998; Ferrer-i-Carbonell and Frijters, 2004), yet applying those methods here involves collapsing seven categories to two, thereby losing valuable variation in the dependent variable. Another option is to use the mean value of each regressor as an additional regressor in an ordered probit, the idea being that the regressors' mean values would be correlated with the fixed effect.

I rely on Ferrer-i-Carbonell and Frijters's (2004) conclusion that the FE OLS is the best overall approach to use in analyzing happiness data. As they explain, linear regressions and ordered probit results are largely consistent with each other, while the linear regressions have a more direct interpretation and can more easily control for fixed effects. Thus, similar to other studies (e.g., Frijters et al., 2008), I report here only the results from linear regressions. Results from ordinal regressions are relegated to Appendix A. As seen there, ordinal regressions present an overall picture similar to the linear regressions.

Table 2 displays the estimates from two linear specifications. Panel I presents estimates from standard OLS, and Panel II presents estimates from FE OLS. The FE OLS specification is the preferred specification; the OLS results are presented for comparison purposes only. Robust standard errors to account for correlation at the individual level are reported. Each regression has reported satisfaction as the dependent variable, and, as independent variables, a constant, the payment, the expected payment (assuming expected payoff maximizing behavior), the prior round's payment, the average payment through all prior rounds, and the round. Treatment A provided no information to the subjects about others' payments, so the Treatment A regressions did not include a social comparison variable. In Treatment B, the subjects were told the average of all others' payments, so that variable in included in the Treatment B regressions to capture the social comparison effect. In Treatment C, subjects were told the average payments by partner-type. I use two social comparison variables. The first is the average payment of all other partner-types not including one's own partner-type (Avg of Other Type Averages). The second is the average payment of those with the same partner-type (Own Type Average). This breakdown is to capture the possibility that subjects consider the payments received by subjects who played against similar partner-types when given such information.

We observe that that the payment has the largest effect of any factor on a subject's reported satisfaction; all other effects, when they do exist, are an order or two smaller of magnitude. The payment coefficient is similar in size and significance across all treatments, and it is an order of magnitude larger than the coefficients on the other regressors. There is also evidence that the expected payment and social comparison effects are meaningfully at work in the predicted manner, i.e., an increase in each acts to lower a subject's reported satisfaction. In Treatment B, both effects are highly significant and of similar magnitudes. In Treatment C, the key social comparison effect is the comparison with others of similar partner-type (Own Type Average). This coefficient is highly significant under the preferred FE OLS specification. Reported satisfaction in Treatment C does not appear to be related to the payments received by subjects who faced other partner-types. The expected payment is not significant in Treatment C; however, I will discuss below how there is evidence that both the expected payment and social comparison effects appear to be operating in Treatment C. The coefficient on the prior round's payment is always positive contrary to the prediction, and it is sometimes significant; however, it is also very small in magnitude. The coefficient on the average of all past round payments is also very small and usually positive but never significant. The coefficient on round number is usually negative suggesting that subjects expressed lower satisfaction as the round progressed, but this variable is very small in magnitude and only significant for Treatment A in the FE OLS specification. I note that in other regressions (not shown), I used variations on the round, such as round squared, and only ever obtained coefficients of very small magnitude and usually not statistically significant.

[17] 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 Greene, 2003).

Table 2
Linear regression results.

	Panel I: OLS			Panel II: FE OLS		
	I-A[a]	I-B[a]	I-C[a]	II-A[a]	II-B[a]	II-C[a]
Constant	0.819[*]	1.494[***]	0.242	1.325[***]	1.379[***]	0.311
	(0.432)	(0.403)	(0.726)	(0.468)	(0.437)	(0.772)
Payment	1.468[***]	1.545[***]	1.517[***]	1.420[***]	1.540[***]	1.525[***]
	(0.071)	(0.058)	(0.076)	(0.083)	(0.055)	(0.076)
Exp. payment-max.	−0.487[***]	−0.420[***]	−0.134	−0.482[***]	−0387[***]	−0.155
	(0.098)	(0.088)	(0.089)	(0.100)	(0.079)	(0.098)
Prior round payment	0.041	0.070[**]	0.058[*]	0.0003	0.067[**]	0.051[*]
	(0.032)	(0.029)	(0.030)	(0.038)	(0.025)	(0.026)
Avg. Payment through prior Rd.	0.079	0.013	−0.001	0.017	0.061	0.001
	(0.057)	(0.037)	(0.058)	(0.034)	(0.037)	(0.044)
Overall average of others	–	−0.299[**]	–	–	−0.342[**]	–
		(0.126)			(0.128)	
Avg. of other type averages	–	–	−0.016	–	–	−0.010
			(0.168)			(0.168)
Own type average	–	–	−0.149[*]	–	–	−0.175[**]
			(0.090)			(0.076)
Round	−0.008	−0.001	−0.008	−0.011[*]	0.001	−0.009
	(0.006)	(0.009)	(0.007)	(0.006)	(0.009)	(0.007)
Satisfaction Average	–	–	–	–	–	–
Observations	864	768	864	864	768	864
R^2	0.68[†]	0.75[†]	0.72[†]	0.68[‡]	0.75[‡]	0.72[‡]

Notes: Each regression dropped the first round of the experiment session. Standard errors robust to correlation at the individual level are listed in parentheses. Treatments A and C had 36 subjects each, and Treatment B had 32 subjects.

[a] Panel-Treatment.
[*] Significance at 10 percent level.
[**] Significance at 5 percent level.
[***] Significance at 1 percent level.
[†] R^2.
[‡] Overall R^2.

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. An F-test for each treatment confirms this conclusion; it rejects the hypothesis that the coefficients on aspirations factors sum to a value equal to the coefficient on payment.

Fig. 4 puts the coefficients for Treatment B (regression II-B from Table 2) in perspective. The thickest line plots the predicted satisfaction by holding expected payment and others' average equal to 3, prior payment and average prior payment equal to 3.5, and round equal to 13. The dark and light gray lines, which are nearly on top of each other, increase expected payment and others' average to 4, respectively, holding the other variables constant. The thick dotted line has both expected payment and others' average equal to 4. An increase in payment by 1 has a much larger effect on predicted satisfaction than does an increase in both of the two aspiration factors. For example, if the payment, expected payment, and others' average all equal

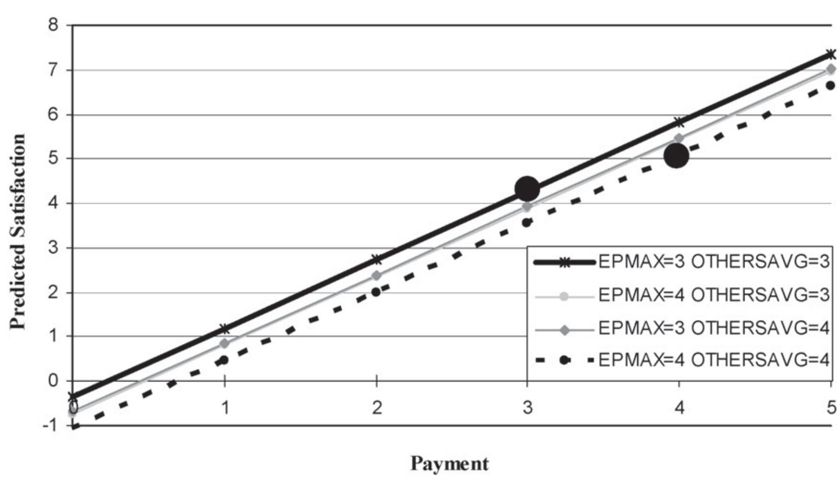

Fig. 4. Predicted satisfaction by payment, Treatment B.

Table 3
Linear regression results with comparison variables.

	Treatment A		Treatment B	Treatment C
Constant	1.883***	2.107**	0.821	0.484
	(0.649)	(0.814)	(0.552)	(0.674)
Payment	1.419***	1.419***	1.539***	1.523***
	(0.083)	(0.083)	(0.055)	(0.076)
Exp. Payment–Max.	-0.481***	-0.488***	-0.345***	-0.315***
	(0.099)	(0.112)	(0.090)	(0.077)
Prior Round Payment	0.001	0.002	0.065	0.048*
	(0.038)	(0.034)	(0.036)	(0.026)
Avg. Pymt. through Prior Rd.	0.020	0.022	0.056	-0.003
	(0.034)	(0.0j34)	(0.036)	(0.045)
Overall Average of Others	-0.175	--	--	-0.037
	(0.154)			(0.174)
Avg. of Other Type Averages	--	-0.204	-0.127	--
		(0.168)	(0.144)	
Own Type Average	--	-0.052	-0.092	--
		(0.066)	(0.070)	
Round	-0.011	-0.009	0.002	-0.010
	(0.006)	(0.006)	(0.009)	(0.008)
Observations	864	864	768	864
Overall R^2	0.68	0.68	0.75	0.72

Notes: Each regression dropped the first round of the experiment session. Standard errors robust to correlation at the individual level are listed in parentheses.
* Significance at 10 percent level.
** Significance at 5 percent level.
*** Significance at 1 percent level.

3, then the predicted satisfaction is 4.27 (marked by the large dot on the solid black line), and if all three increase to 4, then the predicted satisfaction is 5.08 (marked by the large dot on the dotted black line). To go back down after an increase in payment from 3 to 4, the expected payment and others' average would both have to increase from 3 to 5.112, which is more than twice the increase in payment. In general, an increase in payment will increase the predicted happiness by roughly 1.5, while an increase in either expected payment or comparison payment decreases predicted happiness by less than 0.5, and even much less in Treatment C.

4.3. Social comparisons

As previously stated, subjects in Treatment C compared themselves with those who faced similar partner-types rather than with all other subjects. Treatment C subjects had information by partner-type while Treatment B subjects 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 the Treatment C social comparison variables should yield insignificant social comparison coefficients. These regressions have a "placebo experiment" interpretation as we may expect to see different coefficients purely due to the type of information revealed. The Treatment A results displayed in Table 3 report the coefficients from a series of regressions to test these hypotheses; the coefficients of interest have been boxed. This prediction was confirmed in both cases. I conclude that having information about others' payments is necessary for social comparison effects to influence reported satisfaction. Two related predictions are that type specific averages reported in Treatment C comparison should not impact reported satisfaction in the Treatment B data, and the overall average payment for other subjects should not impact reported satisfaction in Treatment C data. Both of these predictions are also confirmed, as shown in Table 3, where none of the coefficients of interest (in boxes) are significant.

Why a 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 what subjects consider to be the appropriate 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 chosen reference group for comparison.[18]

The income-happiness literature suggests another relevant interpretation for this finding. Senik (2004, 2008) emphasizes that the sign on comparison income can be positive if others in my reference group doing well signals that I should do well in the future. There is, in effect, a positive "anticipatory" effect on my current happiness (dubbed a "tunnel effect" by Hirschman and Rothschild, 1973) that arises from the information learned about myself from others' income. A negative sign, on the other hand, indicates feelings of relative deprivation. Both forces could be at work in principle, with one dominating the other. For these experimental subjects, however, there could be a negative informational effect: instead of the payments of similar others signaling higher future payments for me, the higher payments by similar others signals my own missed past opportunity. The negative impact of the social comparison is not due to relative deprivation but instead due to regret about what could have been; in other words, it is instead a perception of bad luck and not jealousy. It is not possible to establish which of these two forces drives the negative sign on comparison payment, though some speculation is possible. First, given that it is the revealing of information that leads to the comparison effects (see Table 3 results), there is reason to believe that the information effect is valid, operating in a negative rather than positive way. Second, we might expect feelings of relative deprivation to appear when comparing with all subjects, yet subjects only compare with others facing the same partner-type, which suggests that the informational content of the comparison information plays a role.

The evidence also indicates that social comparisons and expectations work separately to reduce satisfaction. This is immediately apparent in Treatment B where the coefficients on expected payoff and others' average payment are both highly significant, though it is less apparent in the Treatment C regression. 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 (the unconditional correlation is 0.75).[19] This fact raises the possibility of multicollinearity, a hypothesis which I reject by a formal test of multicollinearity.[20] Nonetheless, it is instructive to examine some other specifications that are presented in Table 4.

Regression II-C in Table 4 is taken directly from Table 2 for comparison. Regression 1 uses all variables from regression II-C except Own Type Average. We observe that the expected payment coefficient in Regression 1 is much larger than the corresponding coefficient in Regression II-C and highly significant. That the coefficient is higher is to be expected because this coefficient is now capturing both the expected payment and social comparison effect. The reason is that Own Type Average is, effectively, the expected payment plus an error term for each other subject. A similar conflation occurs in Regression 2 which uses all variables from Regression II-C except Expected Payment—Maximization. The coefficient on Own Type Average is now higher than in Regression II-C. Regression 3 demonstrates this point another way. It replaces Own Type Average with Own Type Average minus Expected Payment—Maximization. This new variable, in effect, captures how much higher than expected were the payments of others whom were matched against the same partner-type. This new variable, by construction, is uncorrelated with Expected Payment—Maximization. The coefficient on this new variable is highly significant, thus demonstrating that the social comparison effect matters. The coefficient on Expected Payment—Maximization is also highly significant, and it is larger than that in Regression II-C because, again, it captures the direct of effect of one's expected payment on her satisfaction but also the indirect effect of the expectation of others' payments on her satisfaction. In effect, being matched with a high partner-type raises one's expected payment but also increases what you expect others in your reference group to receive, and both effects are at work. Finally, I note that the coefficients on Expected Payment-Maximization and Own Type Average are found to be highly significant in the discrete dependent variable regressions shown in Appendix A.

4.4. Other issues

Why the coefficient on prior round payment and average prior payments is not negative and is often statistically insignificant is not clear. It is possible 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, there could be a short-lived, positive, and small "glow" effect from getting a high

[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.

[19] Payment is also correlated with expected payment in Treatment C but at a much lower amount of 0.43, so the concerns are less warranted for these two variables.

[20] One way to test for multicollinearity is to calculate variance inflation factors (VIFs). The VIFs for the comparison and expected payment coefficients are not greater than 3, which is well below the rule of thumb that a factor of 10 or higher indicates problematic multicollinearity.

Table 4
Additional linear regression results for Treatment C.

Regression	II-C	1	2	4
Constant	0.311	-0.231	-0.255	0.311
	(0.772)	(0.730)	(0.771)	(0.773)
Payment	1.525***	1.525***	1.513***	1.525***
	(0.076)	(0.076)	(0.075)	(0.076)
Exp. payment-max.	−0.155	−0.272***	–	−0.331***
	(0.098)	(0.081)		(0.085)
Prior round payment	0.051*	0.049*	0.053**	0.051*
	(0.026)	(0.026)	(0.026)	(0.026)
Avg. Payment through prior Rd.	0.001	−0.010	0.000	0.001
	(0.044)	(0.046)	(0.044)	(0.044)
Avg. of other type averages	−0.010	0.155	0.102	−0.010
	(0.168)	(0.162)	(0.168)	(0.168)
Own type average	−0.175**	–	−0.242***	–
	(0.076)		(0.063)	
Own type Avg. minus ExpPayMax	–	–	–	−0.175**
				(0.076)
Round	−0.009	−0.011	−0.008	−0.009
	(0.007)	(0.008)	(0.007)	(0.007)
Observations	864	864	864	864
Overall R^2	0.72	0.72	0.72	0.72

Notes: Each regression dropped the first round of the experiment session. Standard errors robust to correlation at the individual level are listed in parentheses. Regression II-C is taken from Table 2.
* Significance at 10 percent level.
** Significance at 5 percent level.
*** Significance at 1 percent level.

payment that produces the positive coefficient in some regressions. Future research is necessary for a more definitive answer.

All regressions 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 measure I used should closely approximate the subject's subjective measure of her expected payment given that so many coin choices are consistent with expected payoff maximization (Table 1). 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 deviates because she perceives the deviation yielding a higher expected payment. In this sense, the Expected Payment—Maximization variable would better represent the subject's subjectively calculated expected payment than the expected payment from the actual choices. I have done these same regressions using the expected payment given the actual, 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 (0.79–0.90 across the treatments).

I ran a series of regressions with various combinations of independent variables and found that the comparative magnitudes of the aspiration coefficients retain their same rankings. One particular combination of note involves adding a dummy variable to capture whether the subject chose consistently with expected payment-maximization. The concern is that the payment variable may itself be endogenously determined by the subject's behavior, which will in turn be correlated with satisfaction: subjects whose choices do not reflect expected payoff maximization will have lower payments and lower satisfaction on average. Including the dummy variable for expected payoff maximization does not substantively alter the other coefficients or their significance (not shown), suggesting that the main results presented earlier are not undermined by an endogeneity problem.

5. Conclusion

Subjects' reported satisfaction levels depend on their aspiration levels as well as their outcomes. The subject's realized payment has the single largest effect on her reported satisfaction. The subject's expected payment and her comparison payment negatively affect her reported satisfaction in similar magnitudes, though at an order of magnitude smaller than the effect of the realized payment. 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. 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 same 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. In the spirit of Konow and Early's (2008) 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 control not only 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.

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Appendix A. Ordinal regressions

Table 5 presents results from three discrete dependent variable 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 when the dependent variable is discretely ordered with more than two response categories. Panel I displays results from three such ordered probit regression, one for each treatment. Probit regressions do not account for individual fixed effects, and Ferrer-i-Carbonell and Frijters (2004) suggest one way to capture fixed effects while retaining the ordinal response assumption. Their method takes advantage of the fact that we can do fixed effects in a binary dependent variable setting. Define x_i to be the average response given by individual i. With k response categories, recode all dependent variables for person i to take value 1 if the actual response is equal to or greater than x_i, and take value 0 otherwise. Meaningful variance in the dependent variable is lost but accounting for individual fixed effects is gained. I present the results from this collapsed FE Logit specification in Panel II of Table 5. Panel III presents coefficients from three ordered probit regression that treat

Table 5
Discrete regression results.

	Panel I: ordered probit			Panel II: collapsed FE logit			Panel III: mean reg. O. probit		
	I-A[a]	I-B[a]	I-C[a]	II-A[a]	II-B[a]	II-C[a]	III-A[a]	III-B[a]	III-C[a]
Payment	1.504***	1.752***	1.553***	4.717**	4.330**	5.532**	1.499***	1.77***	1.591***
	(0.172)	(0.205)	(0.191)	(0.384)	(0.380)	(0.518)	(0.178)	(0.209)	(0.191)
Exp. payment-max.	−0.529***	−0.468***	−0.192**	−1.388**	−0.779**	−0.941**	−0.557***	−0.442***	−0.213**
	(0.093)	(0.127)	(0.095)	(0.307)	(0.258)	(0.480)	(0.090)	(0.118)	(0.102)
Prior round payment	0.077**	0.06*	0.052	0.338**	0.156	0.314**	0.026	0.059**	0.048*
	(0.035)	(0.031)	(0.034)	(0.130)	(0.119)	(0.137)	(0.036)	(0.027)	(0.028)
Avg. Payment through prior Rd.	0.002	0.003	0.0001	0.168	0.133	0.284	0.002	0.003*	0.001
	(0.002)	(0.002)	(0.002)	(0.152)	(0.136)	(0.176)	(0.002)	(0.002)	(0.002)
Overall average of Others	–	−0.249	–	–	−0.854	–	–	−0.254*	–
		(0.158)			(0.632)			(0.0156)	
Avg. of all type averages	–	–	−0.017	–	–	−0.476	–	–	0.005
			(0.153)			(0.861)			(0.166)
Own type average	–	–	−0.137	–	–	−0.926**	–	–	−0.179**
			(0.087)			(0.358)			(0.072)
Round	0.014**	−0.001	−0.008	0.001	0.002	0.020	−0.014***	−0.001	−0.007
	(0.006)	(0.010)	(0.007)	(0.021)	(0.022)	(0.025)	(0.006)	(0.010)	(0.007)
Mean regressors included	No	No	No	No	No	No	Yes	Yes	Yes
Observations	864	768	864	864	768	864	864	768	864
Pseudo R^2	031	0 36	0 33	0.74	0.71	0.79	0.32	0.37	0.34

Notes: Each regression dropped the first round of the experiment session. Standard errors are listed in parentheses, with robust standard errors clustered by individual. Treatments A and C had 36 subjects each, and Treatment B had 32 subjects.

[a] Panel-Treatment.
* Significance at 10 percent level.
** Significance at 5 percent level.
*** Significance at 1 percent level.

the mean values of the regressors as regressors. This method has been proposed as one way to capture fixed effects in an ordered probit setting.[21]

The coefficients from these discrete dependent variables tell a story that is qualitatively similar to what we learned from the FE OLS regressions. Payment has the single largest effect that works in the predicted manner at an order of magnitude larger than the other effects. Prior round payments have a small effect opposite of predicted, and expected payments have a meaningful effect that is smaller than the payment but about equal to the social comparison effect. When controlling for fixed effects in Panels II and III in Table 3, we see both the expected payments and social comparisons to be statistically significant factors in subjects' reported satisfaction except in the Treatment B regression in Panel II where the coefficient has the anticipated sign but is not significant at normal levels.

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