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A Glance into the Tunnel: Experimental Evidence of Expectations Versus Comparison Considerations

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A Glance into the Tunnel: Experimental Evidence of Expectations Versus Comparison Considerations

February 25, 2015

Abstract

Learning that others earn more may reduce individual well-being but can also be informative about the own income prospects. This paper provides experimental evidence that separates direct effects of income comparisons on well-being and informational effects from observing changes in the income of others. In an environment of uncertainty about the own income we find that both the direct comparison effects and the informational effects are asymmetric. Individual beliefs about the own income are adjusted downwards when observing that others are likely to earn less but do not change significantly when observing that others are likely to earn more. Individual satisfaction decreases when observing that others are likely to earn more but does not change significantly when observing that others are likely to earn less. Overall, individuals are more reactive to “bad news” than to “good news.”

JEL codes: C91, D31, D63, D84

Keywords: Tunnel effect, relative income, expectations, belief formation, subjective well-being, experiment

1 Introduction

When people care about relative standing, observing changes in the income of others will affect their utility. At the same time, however, observing that others' earnings increase or decrease can be informative for the personal future income prospects. In this case individuals adjust expectations about the personal income accordingly, with consequences for individual well-being. This indirect effect of observing others' income can countervail the direct effect of relative standing concerns and affect the individual tolerance of income inequality. Using data from a controlled laboratory experiment we separate the direct comparison effect from the purely informational effect and examine their importance for subjective well-being.

The information driven effect has received less attention in the literature and was first discussed in a seminal paper by Hirschman (1973). Hirschman claims that the positive informational value of observing that others' earnings increase may even outweigh the negative effect driven by relative standing concerns, illustrating such a situation using a tunnel anecdote: Suppose you are in a tunnel, being stuck in a traffic jam. As far as you can see, nothing is moving and you are dejected. All of a sudden, in the lane next to you the cars start to roll on. Even though still being stuck in your lane, you feel relieved as the traffic jam seems to be broken. While your relative position is deteriorating the positive signal about the possibly dissolving traffic jam leaves you on balance more satisfied than you have been before.

Some empirical approaches have been undertaken to study Hirschman's "tunnel effect," usually relying on survey data. Using data for Russia, Ravallion and Lokshin (2000) provide evidence that individuals who expect their economic situation to improve show a weaker support for redistribution. Senik (2004, 2008) finds evidence that personal life satisfaction may react positively to changes of the income of a reference group. Similarly, Clark et al. (2009) match Danish employer-employee data with survey data and find supportive evidence for a positive correlation between job satisfaction and the income of colleagues. Whereas empirical evidence for the joint occurrence of comparison consideration and informational effects from the field is a natural and important starting point, generally these studies suffer from eminent problems. First, the measurement of the relevant variables can be defective in several ways. For instance, income runs at risk to be under-declared and measures of individuals' expectations about future income development are usually crude in survey data. Second, income can be endogenous to satisfaction. A common example is that satisfied people might be extraverted and, as a possible result, more successful in their job. Third, in the field it is difficult to identify the income of a relevant reference group. Even if one finds an arguably well-defined reference group it is impossible to confirm in the field how much of the reference group's income development people actually observe or whether they learned about

it at all. Furthermore, the usually yearly frequency of observations in survey data leaves a lot of room for unobserved effects that might influence results in an unobserved way.¹

Many problems in the field can be ruled out in the laboratory and, differently to studies in the field, we can concentrate on a separate and clearly causal identification of relative-income considerations and informational effects. The controlled environment allows us to observe exactly the income of participants and of a clear defined reference group while we can directly measure satisfaction and beliefs about the income development and control the type of additional information received. Most importantly, we can identify information driven effects and comparison considerations in isolation. This more detailed identification enables us to analyze directly adjustments in beliefs caused by the information rather than focusing on changes in satisfaction that are supposed to be caused by changes in beliefs.

In our experiment we endow participants with income in form of a portfolio where only the final portfolio value is payoff relevant. The portfolio value follows a stochastic process and in fixed time intervals the participants are asked a) for their satisfaction and b) for their belief about the personal final portfolio value. We compare individual satisfaction and beliefs across three treatments: In the baseline experiment, the participants observe their own portfolio only. In a second treatment, the subjects observe the same own portfolio but, additionally and simultaneously, a second portfolio which is of purely informational value and not assigned to any other participant of the experiment. Finally, the participants of a third treatment observe the same own portfolio and, in addition, a portfolio that is assigned to another participant. Holding direct signals about the own income constant, we can separate purely informational effects of receiving additional signals (“information effect”) from direct comparison effects of being informed about the relative expected income (“relative-income effect”). The experimental setting keeps the informativeness of additional signals uncertain but subjects are shown a distribution of possible income realizations.

We find evidence for the “information effect” and for the “relative-income effect.” Both effects turn out to be asymmetric. On the one hand, expectations about own income are more reactive when participants observe portfolios with lower values, that is, obtain “negative additional information.” On the other hand, relative-income concerns only affect satisfaction in situations where individuals are “behind” in terms of relative income. Finally, our setting of uncertain informativeness of observed signals suggests that our findings on the “information effect” are rather strong and can be expected to be even stronger when it becomes more likely that income levels across individuals are correlated.

Our paper relates to the literature on expectations formation (e.g. Schmalensee 1976,

¹Note that some of the problems can be and are addressed in one or another way in the studies cited above. But nevertheless it remains generally true that a completely clean identification is inaccessible in the field.

Dwyer et al. 1993, Hey 1994, Hommes 2011, R  theli 2011 and Beshears et al. 2013). However, we are not primarily interested in the expectations people form about a time series (in our setting, their observed income). We focus on how people adjust their expectations about their own income (that they observe in form of a portfolio) when they observe another person’s income (in form of an additional portfolio). In this respect we investigate whether people believe that the personal expected income depends on additionally observed signals about income of others when the underlying income-generating process is unknown.

How people adjust expectations about personal income when they observe signals about income of others has implications for the literature on “the prospect of upward mobility” (POUM). Benabou and Ok (2001) formalize and provide conditions for the POUM hypothesis that states that even when societies experience a distinct level of inequality a majority of voters does not prefer more redistribution because poor people believe that they themselves or their children are likely to move upward in terms of wealth and income in the future. Our experiment investigates beliefs about future income in an environment where the informativeness of observing income of others for the own income is uncertain. We believe this is particular interesting because outside the laboratory people might observe income signals about others, however, the underlying correlation between the incomes, and thus, the informativeness of these signals, is in most cases uncertain. In this respect, our paper also relates to Piketty (1995) who is concerned about upward mobility and its implications for preferences for redistribution, taking into account that agents may exhibit heterogeneous beliefs about upward mobility based on personal experience. Our results for the asymmetry of the “information effect” may be interpreted as subjects being on average rather pessimistic, which might strengthen concerns about downward mobility.

Our paper also contributes to the empirical literature on relative-income considerations.² Evidence for a negative relationship between subjective well-being and the income of a defined reference group find Van de Stadt et al. (1985), Clark and Oswald (1996), McBride (2001), Ferrer-i-Carbonell (2005), Luttmer (2005), Senik (2009), Clark and Senik (2010). Consistent with the ideas of Duesenberry (1949), Ferrer-i-Carbonell (2005), Senik (2009) and Clark and Senik (2010) find that the relative-income considerations are asymmetric, meaning that people compare mostly upwards. Experimental evidence from India provide Fehr et al. (2008) who find that participants costly punish others motivated by an increase in relative payoff. Using hypothetical choices of participants, Solnick and Hemenway (1998), Johansson-

²The discussion on relative income comparison dates back to Veblen (1899) and Duesenberry (1949). Important implications for economics have been discussed comprehensively in the literature (see for instance Leibenstein (1950), Easterlin (1974, 1995), Boskin and Sheshinski (1978), Frank (1984, 1985), Konrad (1992), Konrad and Lommerud (1993), Mui (1995) or Dur and Glazer (2008)). For a review on income comparison see Clark et al. (2008).

Stenman et al. (2002) and Alpizar et al. (2005) find that people prefer situations of considerably lower absolute income but higher relative income. Finally, Dohmen et al (2011a) scan brain regions of experiment participants and find that a decrease in relative-income negatively correlates with reward-related brain activity. We contribute to this empirical literature in two respects. First we focus on relative-income considerations under uncertainty controlling for informational effects that become important in an uncertain environment. Second, we provide experimental evidence in a novel and, as we believe, particularly simple setting, in which we show that the seemingly minor information (the second portfolio observed is assigned to another participant instead of not directly payoff-relevant) in an otherwise exactly similar situation induces comparison considerations.

The article is organized as follows. Section 2 discusses the theoretic framework, the design and the procedures of the experiment. In Section 3 follows the empirical analysis, with a focus on the “information effect” in Subsection 3.1 and on the “relative-income effect” in Subsection 3.2. Section 4 concludes.

2 Framework

2.1 Information and relative income concerns

Consider a model with two individuals 1 and 2. Individual $i \in \{1, 2\}$ realizes future income denoted by $y_i \in \mathbb{R}_+$. We assume that individual i cares about relative standing and, hence, both about his own future income and about the future income of individual $j \neq i$. The preferences of i are described by the utility function

$$u_i(y_i, y_j) = y_i - \lambda_i y_j,$$

where the parameter $\lambda_i \geq 0$ reflects i 's concerns about relative standing.

Future income of the individuals is uncertain. Individual i observes a signal $s_i \in \mathbb{R}$ about the own future income y_i as well as a signal $s_j \in \mathbb{R}$ about the other individual's future income y_j . Denote by $E_i(y_k)$ the expectation of i about y_k . Then, i 's expected utility conditional on the signals (s_i, s_j) is equal to

$$E_i[u_i(y_i, y_j) | (s_i, s_j)] = E_i[y_i | (s_i, s_j)] - \lambda_i E_i[y_j | (s_i, s_j)].$$

We assume that i 's beliefs about y_k are strictly increasing in the signal s_k , that is,

$$\frac{\partial E_i[y_k | (s_1, s_2)]}{\partial s_k} > 0, \quad k = 1, 2. \tag{1}$$

Moreover, i 's beliefs about the own income y_i may also depend on what i observes about j 's income, that is, on s_j . (Similarly, i 's expectation about the other individual's income y_j may depend on the signal s_i about the own income.) Thus, changes in s_j affect i 's expected utility through changes in his expectations of his own income and of the other individual's income:

$$\frac{\partial E_i[u_i(y_i, y_j) | (s_i, s_j)]}{\partial s_j} = \frac{\partial E_i[y_i | (s_i, s_j)]}{\partial s_j} - \lambda_i \frac{\partial E_i[y_j | (s_i, s_j)]}{\partial s_j}. \quad (2)$$

The second term in (2) is negative if $\lambda_i > 0$ and (1) holds. A higher signal s_j about j 's income has a direct negative effect on i 's expected utility whenever i has concerns about relative standing: A higher expected income of j makes i worse off in relative terms. We call this direct effect “relative-income effect.” The first term in (2) depends on how i interprets information about j 's income regarding his own future income. If i expects own future income y_i and the other individual's future income y_j to be positively correlated, then the first term in (2) may be positive, that is,

$$\frac{\partial E_i[y_i | (s_i, s_j)]}{\partial s_j} > 0. \quad (3)$$

In this case, there is an “information effect” that countervails the direct negative effect on $E_i(u_i)$ from observing a higher signal s_j . Positive signals about the income of others can increase i 's expected utility if these signals convey positive information about the own income.

If (3) holds, the total effect in (2) can be positive or negative, depending on whether the “information effect” or the “relative-income effect” dominates. The experimental treatments described next isolate the two effects and test them separately.

2.2 Experimental treatments

The experiment consists of three treatments, which are implemented in a between-subjects design. In each of the treatments, participant i receives a “portfolio” P_i whose value follows a stochastic process. Participant i observes the value $\tilde{y}_i(t)$ of portfolio P_i at points in time $t = 0, 1, 2, \dots, T$. The value $\tilde{y}_i(0)$ is identical for all portfolios/participants; the final value $\tilde{y}_i(T)$ is ex ante uncertain and determines i 's income in the experiment. Hence, the values $\tilde{y}_i(t)$ at $t < T$ represent signals about i 's income.

The participants' task is to repeatedly answer questions on their *beliefs* about the final

portfolio value $\tilde{y}_i(T)$ and on their *satisfaction*³ with the assigned portfolio.⁴ The first main question asks directly for an individual’s satisfaction with the assigned portfolio, on a scale from 0 (highly unsatisfied) to 10 (very satisfied). This question serves as a self-reported measure of utility.⁵ The second main task is to give an estimate of the final portfolio value $\tilde{y}_i(T)$; this task is incentivized. The subjects’ answers to this second question measure the individual beliefs about the final value of the income-generating process.

As a plausibility check for the self-reported measure of satisfaction, we also include a control question in which subjects have the option to receive as their earnings the final value of another randomly assigned portfolio. Subjects should be more likely to choose this option the less satisfied they are with their current portfolio; thus we can test whether this choice is (negatively) correlated with the self-reported satisfaction.^{6,7}

Treatment Base In the baseline treatment, each subject observes only the value $\tilde{y}_i(t)$ of the own portfolio P_i at points in time $t = 0, 1, \dots, T$. The BASE treatment will be used to establish a benchmark for the individuals’ beliefs about the own final portfolio value (i.e. income) in the absence of information about other individuals’ income.

Treatment P2-Info In the P2-INFO treatment, subject i observes the value $\tilde{y}_i(t)$ of the own portfolio P_i and, in addition, the value $\tilde{y}_j(t)$ of a second portfolio P_j at points in time $t = 0, 1, \dots, T$. This second portfolio has no payoff relevance for any other individual; it is common knowledge that it is not assigned to any other participant of the experiment. Using the BASE treatment as a counterfactual, this intermediate treatment P2-INFO isolates

³Although this might be a bit imprecise we use the terms satisfaction, subjective well-being and utility interchangeable. For our experiment we rely on the general conclusion in the literature that satisfaction or subjective well-being is a meaningful measure (for a survey of the work on subjective well-being see Diener (1984)). However, we also implement a control question relying on “revealed preference” for a consistency check of our subjective well-being measure in our experimental framework. For a discussion on action-revealed preferences and satisfaction judgments see Frey and Stutzer (2002).

⁴For the exact description of the task see the experimental instructions in Appendix C.

⁵Although utility is an ordinal concept in our analysis we treat this measure as cardinal following Ferreri-Carbonell and Frijters (2004) and more generally the literature on subjective well-being.

⁶This option to have the individual earnings determined by another randomly generated portfolio is included in about half of the sessions of each treatment. If this option is chosen, the subject is assigned and shown the new portfolio at the end of the experiment; subjects answer all questions on beliefs and satisfaction with respect to the originally assigned portfolio P_i . In the other half of the sessions, there is no option to have the earnings determined by another portfolio, but subjects are only asked “hypothetically” whether they would prefer to be assigned another portfolio. We use these two different versions of the control questions to control for possible interference of this control question with the self-reported measure of satisfaction. Note already that these two different types of sessions are very similar in terms of results obtained.

⁷We find a correlation of -0.63 between the choice of this option to receive the value of another portfolio and self-reported satisfaction with the assigned portfolio. Thus, we are confident that the non-incentivized measure of satisfaction is meaningful. Also see Table 6 in Appendix A.

Treatments	BASE	P2-INFO	P2-INCOME
# sessions	4	4	4
# participants	96	96	96

Note: In BASE, subjects only observe their own portfolio; in P2-INFO, subjects observe an additional portfolio which has no payoff relevance; in P2-INCOME, subjects observe their own portfolio and the portfolio of another subject.

Table 1: Summary of the experimental treatments.

the effect of *additional information* ($\tilde{y}_j(t)$) on an individual’s beliefs about the own income (“information effect”), in a situation in which this information is not directly informative about the income of another participant of the experiment.

Treatment P2-Income The P2-INCOME treatment differs from the P2-INFO treatment only in that the second portfolio P_j is assigned to another participant of the experiment (which is common knowledge). More precisely, two participants i and j of the experiment are randomly matched and both observe the values $\tilde{y}_i(t)$ and $\tilde{y}_j(t)$ at points in time $t = 0, 1, \dots, T$.⁸ Using treatment P2-INFO as a counterfactual, the P2-INCOME treatment isolates the effect of observing income of others on own satisfaction (“relative-income effect”). The P2-INFO counterfactual controls for the purely informational value of observing other portfolios, which may also affect the satisfaction with the own portfolio. In other words, we separate the “relative-income effect” from the “information effect” derived in Section 2.1.

Table 1 summarizes the treatments and the number of sessions and participants per treatment.

2.3 Experimental procedures

Each of the three treatments BASE, P2-INFO, and P2-INCOME consists of 10 structurally identical but independent rounds. Hence, participant i observes 10 own portfolios; in the treatments P2-INFO and P2-INCOME i observes also 10 additional portfolios. In the P2-INCOME treatment, the participants are randomly matched in groups of two in each of the

⁸Note that participant i can only observe $\tilde{y}_j(t)$ but not participant j ’s choices (reported satisfaction and estimate of $\tilde{y}_j(T)$), and vice-versa.

10 rounds.⁹

To allow for perfect counterfactuals we assign the portfolios such that a subset of players across all treatments observes an identical sequence of portfolios (own portfolios and potentially co-players' portfolios) in the 10 rounds.¹⁰ Therefore, the treatment comparisons control for history effects, that is, for information about portfolio values in previous rounds and in a given round (up to t).

Portfolios are generated by a random walk with $\tilde{y}_i(0) = 300$ and

$$\tilde{y}_i(t) = \tilde{y}_i(t-1) + \alpha_i + \beta \varepsilon_{it}. \quad (4)$$

The shocks ε_{it} are independent draws from a standard normal distribution, and the parameter β is a constant to scale the shock ε_{it} (we set $\beta = 10$). Moreover, the drift parameter α_i is randomly drawn from the set $\{-1.5, 0, 1.5\}$ in order to obtain different types of portfolios (low-value, medium-value, and high-value portfolios) and corresponding situations of subjects observing higher or lower portfolio values of others.¹¹ The final period is $T = 100$ and the subjects observe the dynamic process of the portfolio on the screen in a diagram (with the time dimension on the horizontal axis and the portfolio value on the vertical axis; see a screenshot in Figure ?? of Appendix ??).

The participants are not informed about the exact stochastic process that governs the portfolios. Instead, the experimental instructions contain a graph which shows a large number of portfolios generated by the stochastic process in (4) (compare Appendix C). This ensures that subjects have a comparable prior about the portfolio-generating process and about the probability distribution of final portfolio values, without imposing too much structure or exploiting differences in computational skills.

The questions on satisfaction and beliefs about $\tilde{y}_i(T)$ above are asked four times for a given portfolio P_i , at points in time $t = mT/5$ where $m \in \{1, 2, 3, 4\}$. At later points in time, individuals have observed more information (i.e. signals). At each point in time t , the subjects can give their answers independently of their previous answers.¹² At the end of the

⁹Note that participants do not interact or observe the co-players' decisions. We implement random re-matching to assure that relative income comparisons refers to the particular current round rather than referring to all previously observed rounds.

¹⁰To be precise, all participants observe the same 10 "own" portfolios. Moreover, participants in P2-INFO and P2-INCOME observe the same 10 additional portfolios. We generated 6 random sequences in which these portfolios are shown to the subjects; subjects are then randomly assigned to one of these sequences.

¹¹The drift parameters for the 20 selected portfolios are chosen such that each possible combination of drift parameters (α_i, α_j) occurs at least once to ensure some variation in terms of the observed portfolio pairs. Otherwise, the portfolio selection is completely random.

¹²Recall that the control question allows a subject to receive the final value of another, unknown, randomly drawn portfolio as his income. If a subject prefers another portfolio he nevertheless observes the initial

experiment we randomly choose one out of the 10 rounds; we randomly select one point in time \hat{t} in this round at which the questions have been answered. The participants' choices at this point in time determine their earnings in the experiment, as follows: First, subjects receive a payment for their estimate of their final portfolio value at this point in time; this payment increases in the precision of the estimate.¹³ Second, each subject receives the final value $\tilde{y}_i(T)$ (in experimental currency) of the portfolio assigned in the selected round.¹⁴ The payment received in experimental currency is converted to Euros at a rate of 25 ECU = 1 Euro (that is, the value $\tilde{y}_i(0) = 300$ is equal to 12 Euros). Third, subjects receive a lump-sum payment of 2 Euros for reporting their satisfaction and a show-up fee of 4 Euros.

The experiment was programmed and conducted with the experiment software z-Tree (Fischbacher 2007) and run at the University of Munich. 288 students from more than 10 different fields of study participated.¹⁵ After having completed the main experiment, subjects answered a set of post-experimental questions on individual characteristics and attitudes. At this point, we also conducted a set of incentivized post-experimental tasks, including a question on risk aversion (Dohmen et al. 2011b) and tasks to measure distributional preferences (Balafoutas et al. 2012), loss aversion (Fehr and Goette, 2007) and ambiguity aversion. One of the incentivized post-experimental tasks was randomly selected for payment in top of the earnings from the main experiment. On average subjects earned 29 Euros in total and a session lasted for approximately 90 minutes.

2.4 Predictions

Individuals form beliefs about their final portfolio value based on information received during the experiment; these beliefs affect an individual's expected utility (satisfaction). Using pairwise treatment comparisons we analyze how information about others affects individual beliefs and what this may imply when individuals have relative standing concerns.

The first prediction focuses on the effect of additional information (a second observed portfolio) on individuals' beliefs about own income. Since individual portfolios are drawn independently, if subjects knew the exact process individual beliefs about the own final portfolio value should be independent of any additional information about other portfolios. In the experiment, even though subjects do not learn the exact process, they are shown a "probability distribution" of possible portfolios (see the instructions in Appendix C). This

portfolio of the current round until T and answers all questions on this initially assigned portfolio.

¹³The payoff (in experimental currency) for an estimate \hat{y} is $\max\{250 - 0.1(\tilde{y}_i(T) - \hat{y})^2, 25\}$.

¹⁴In sessions with the control question offering the option to receive as a payment the final value of a new randomly generated portfolio, a subject receives either the final value of the assigned portfolio or the final value of a new portfolio, depending on his choice at the selected point in time \hat{t} .

¹⁵The participants were recruited using the software ORSEE (Greiner 2004).

approach closely maps a situation in which the individuals hold a common prior about the portfolio-generating process and it reduces the within-treatment variation. Therefore, in theory there should be no treatment effect of observing an additional portfolio. Subjects may, however, still perceive the additional information in P2-INFO as informative and adapt their beliefs according to the additional signals received, as summarized in the following testable prediction.

Prediction 1 (“Information effect”) (i) *Compared to the BASE treatment, observing an additional portfolio P_j in the P2-INFO treatment lowers individual i ’s beliefs about $\tilde{y}_i(T)$ whenever $\tilde{y}_j(t) < \tilde{y}_i(t)$.*
(ii) *Compared to the BASE treatment, observing an additional portfolio P_j in the P2-INFO treatment increases individual i ’s beliefs about $\tilde{y}_i(T)$ whenever $\tilde{y}_j(t) > \tilde{y}_i(t)$.*

Intuitively, Prediction 1 states that individuals’ beliefs about the own final portfolio value (income) should react negatively to “bad news” (part (i)) and positively to “good news” (part (ii)). “Good news” and “bad news” refer to information about a second portfolio and are defined taking the own current portfolio value as a benchmark. Prediction 1 maps a situation in which subjects expect some common (but unknown) trend in the portfolios observed. We analyze the cases of $\tilde{y}_j(t) < \tilde{y}_i(t)$ and $\tilde{y}_j(t) > \tilde{y}_i(t)$ separately to allow for an asymmetric effect of observing a second portfolio with higher and with lower relative value, respectively.

By comparing the individuals’ beliefs about the final portfolio value in P2-INFO and in BASE we test Prediction 1 against the alternative hypothesis that individuals interpret the additional information as uninformative for their own final portfolio value. Recall that a subset of individuals across treatments observe the same portfolios; thus the comparison of P2-INFO to BASE controls for the information received about the own portfolio in the respective round and in previous rounds.

Second, holding the beliefs about own income constant, additional information about others’ income may have a direct effect on own satisfaction if and only if individuals care about the income of others. The second testable prediction should hold if subjects have relative standing concerns.

Prediction 2 (“Relative-income effect”) (i) *Compared to the P2-INFO treatment, observing information about individual j ’s income in the P2-INCOME treatment lowers individual i ’s satisfaction whenever $\tilde{y}_j(t) > \tilde{y}_i(t)$.*
(ii) *Compared to the P2-INFO treatment, observing information about individual j ’s income in the P2-INCOME treatment increases individual i ’s satisfaction whenever $\tilde{y}_j(t) < \tilde{y}_i(t)$.*

Controlling for the “information effect,” whenever individuals observe that relative to the own portfolio another participant has a higher current portfolio value (i.e. likely a higher

expected income) the average satisfaction should be lower (Prediction 2(i)); when observing that others are worse off, average satisfaction should be higher (Prediction 2(ii)). Again, we will test whether there is an asymmetric effect of observing higher and lower income of others, respectively, on own satisfaction. If; instead, individuals do not care about relative standing, then under this alternative hypothesis average satisfaction in P2-INCOME and in P2-INFO should be the same (both for $y_j(t) > \tilde{y}_i(t)$ and for $\tilde{y}_j(t) < \tilde{y}_i(t)$).

To summarize, a comparison of P2-INFO and BASE identifies the purely informational value of observing signals about another income-generating process (the term $\partial E_i[y_i | (s_i, s_j)] / \partial s_j$ in the linear specification in (2)). A comparison of P2-INCOME and P2-INFO reveals whether signals about the actual income of others affect an individual's satisfaction (the term $\lambda_i \partial E_i[y_j | (s_i, s_j)] / \partial s_j$ in (2)), controlling for the effect on $E_i[y_i | (s_i, s_j)]$. By construction, this term is zero in the P2-INFO treatment where the additional information obtained is not payoff-relevant for any other participant. Note, however, that even in the P2-INFO treatment individuals may draw conclusions from the additional information on the income of others, if individuals believe that the additional observed portfolio is generally informative for the possible assigned portfolios of other participants. In this case satisfaction might already be affected by additional information in P2-INFO, and therefore, the comparison of P2-INCOME and P2-INFO may under-estimate the “relative-income effect” of observing to be ahead or behind in terms of expected income.

3 Results

In a nutshell the empirical results show that when subjects observe bad additional information (a second portfolio with lower current value), they lower their beliefs about their own income. Observing good additional information has, however, no statistically measurable effect on beliefs about own income. Moreover, observing signals that indicate a lower expected income than others has a negative effect on individual satisfaction, while observing signals that indicate a higher expected income than others has no statistically measurable effect on satisfaction. Hence, the results can be interpreted as subjects being more reactive to “bad news” than to “good news.”

3.1 Information effect

First we are interested in the effect of additional information on the beliefs about the own end-of-period portfolio value (Prediction 1). To assess the effect of observing an additional portfolio (additional signals) it is crucial to perfectly control for all the information about the

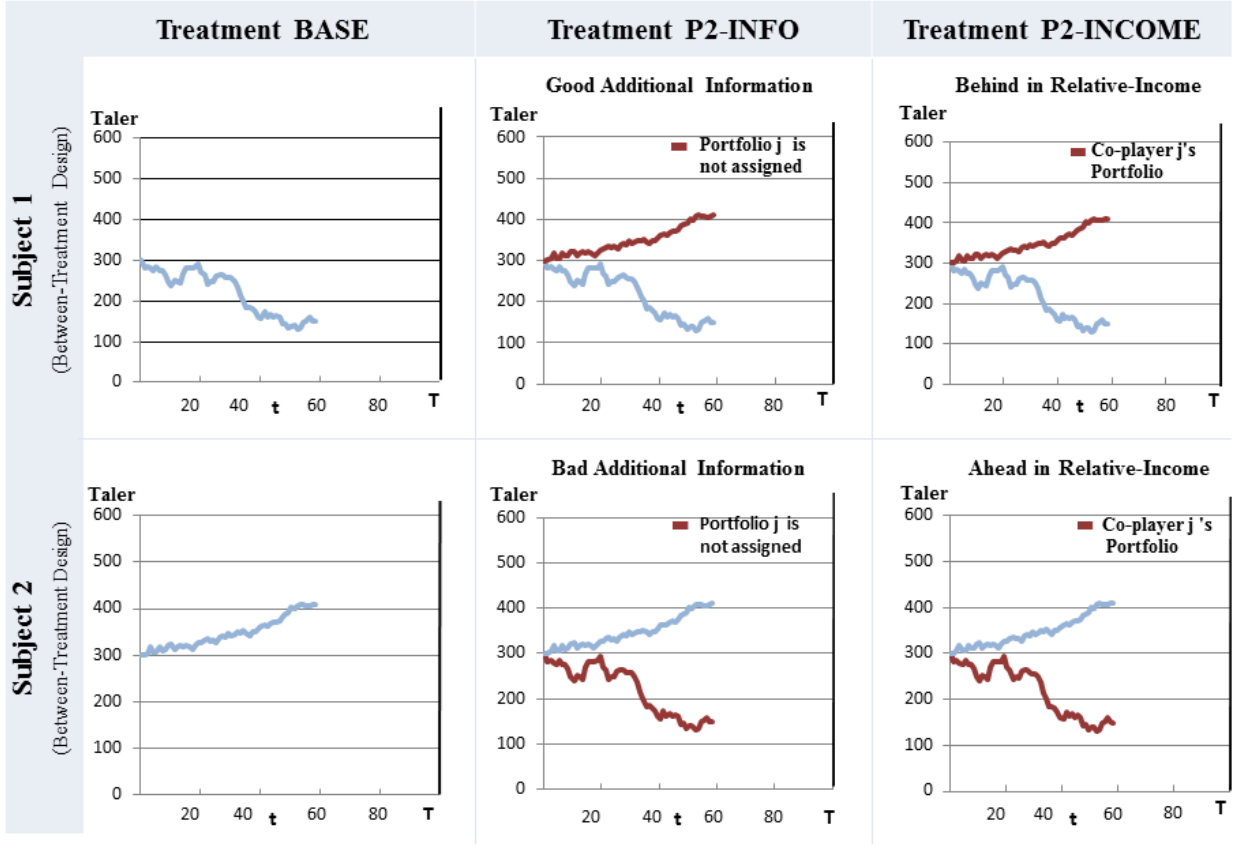


Figure 1: Identification Strategy

own portfolio. We compare beliefs in the P2-INFO treatment to beliefs in the BASE treatment in which reference groups of subjects observe the exact same own portfolios as in P2-INFO but no second portfolio. Moreover, we separate the “information effect” for situations in which subjects observe a second portfolio with a lower current value and with a higher current value, respectively. Figure 1 illustrates our identification strategy of comparing beliefs in P2-INFO to those in BASE for a given own portfolio.

We start with a simple comparison of average stated beliefs in treatments BASE and P2-INFO.¹⁶ We split the observations into a situations of good additional information (observing a second portfolio P_j with value $\tilde{y}_j(t) > \tilde{y}_i(t)$ in P2-INFO) and a group of bad additional information (observing a second portfolio P_j with value $\tilde{y}_j(t) < \tilde{y}_i(t)$ in P2-INFO).¹⁷ Figure

¹⁶See also Table 7 in Appendix A for descriptive statistics.

¹⁷Observations in BASE are split accordingly, that is, depending on whether the second portfolio (which subjects are not shown in BASE but only in P2-INFO) would have a higher or lower value than the own

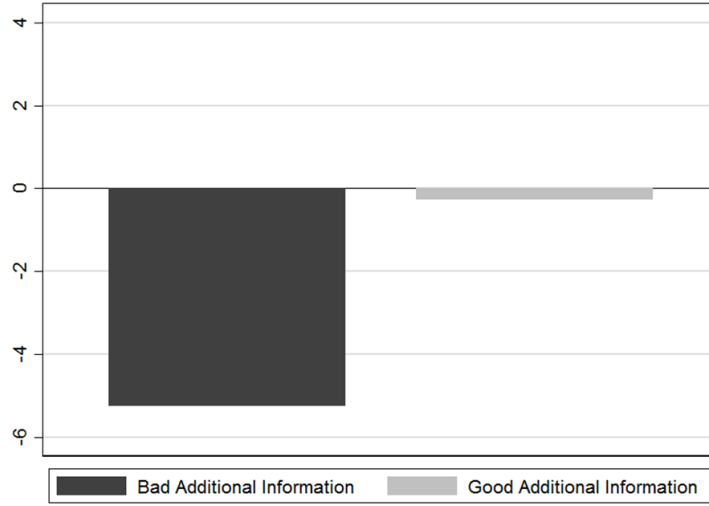


Figure 2: Change in average beliefs (in experimental currency) from BASE to P2-INFO.

2 suggests partial evidence for Prediction 1: While bad additional information lowers average beliefs in P2-INFO compared to BASE, good additional information shows no evident effect on average beliefs. In the following we will further investigate and confirm this observed asymmetry.

To test Prediction 1 on the effect of additional information we estimate a crossed effects linear regression model on a sample of the observations from BASE and P2-INFO.^{18,19} Our main specification is given by

$$belief_{it} = \beta_o + \beta_1 y_{it} + \beta_2 P2-INFO + \beta_3 I_{y_{jt} > y_{it} <} + \beta_4 I_{y_{jt} > y_{it}} \times P2-INFO + \beta X_{it} + \varepsilon_{it}. \quad (5)$$

The dependent variable $belief_{it}$ represents reported beliefs by subject i at point in time t about the end-of-period portfolio value $\tilde{y}_i(T)$. Our main variables of interest are the dummy portfolio.

¹⁸The crossed effects model allows us to specify random effects on the subject level and additional random effects on the portfolio level. The random effects on the subject level account for time constant subject specific effects. Because half of all subjects are assigned to the exact same portfolios, we include additional random effects on the portfolio level to account for portfolio specific effects. Note the all results are qualitatively robust to using a simple random effects regression model or a pooled OLS model with clustered standard errors on subject and session level.

¹⁹We exclud subjects from the analysis that showed “extrem” or “implausible” replies (for instance, when subjects stated in all questions about the final portfolio value beliefs below 10 ECU). In total we exclude 4 out of 288 subjects (1.38%). Note that we could not implement pre-tests before the main experiment because we did not want to bias subjects by showing them specific portfolios. This comes at the risk that some subjects might not have understood the instructions. However, generally answers of subjects are reasonable indicating that subjects understood the experiment.

variable *P2-INFO* (which takes a value of 1 for observations from the P2-INFO treatment) and the indicator variable $I_{y_{jt} > y_{it}}$ which is equal to one in situations of good additional information (if portfolio j has a higher current value than portfolio i) and equal to zero otherwise.²⁰ Moreover, we interact the dummy *P2-INFO* with the indicator $I_{y_{jt} > y_{it}}$, and we include the observed own current portfolio value $y_{it} := \tilde{y}_i(t)$ as explanatory variable as well as a vector X_{it} of additional control variables.²¹ In the model specified in equation (5), the coefficients of interest are β_2 (which measures the effect of bad information, that is, the treatment effect if $I_{y_{jt} > y_{it}} = 0$) and $\beta_2 + \beta_4$ (which measures the effect of good information, that is, the treatment effect if $I_{y_{jt} > y_{it}} = 1$).

In specification (1) of Table 2, the estimated coefficient of *P2-INFO* is -11.74 and significant at the 5%-level (p -value < 0.041). Hence, observing a second portfolio with lower value significantly lowers the subjects' beliefs in the P2-INFO treatment, compared to the reference group (same own portfolios) in BASE. Second, we analyze the treatment effect of observing a second portfolio with higher value; the sum of the coefficients of $I_{y_{jt} > y_{it}} \times P2-INFO$ and *P2-INFO* is negative but statistically indistinguishable from zero (p -value > 0.199).²² Finally, as we would expect, the current value of the own portfolio (y_{it}) has strong explanatory power with a positive coefficient that is close to 1 and significant at the 1% level.

Specification (2) is identical to specification (1) but additionally includes individual-specific controls generated after the main part of the experiment, including self-reported socioeconomic characteristics and post-experimental tasks to elicit distributional and risk preferences. The estimated coefficients of our main variables of interest remain very similar. The effect of negative additional information captured by *P2-INFO* decreases marginally in magnitude and the p -value increases slightly to 0.077 but the estimated effect remains significant at the 10% level.

In specification (3) of Table 2 we further disaggregate the type of information observed. Instead of separating the additional information into “good” and “bad” information, we generate quartiles at each observation point t : Being in the first quartile indicates that

²⁰The case of exactly the same current portfolio values ($\tilde{y}_j(t) = \tilde{y}_i(t)$) never occurs in the data.

²¹This includes controls such as gender, age and a dummy for economics students as well as individual-specific characteristics elicited in an extended post-experimental questionnaire (including measures for risk aversion, loss aversion, ambiguity aversion, distributional preferences and self-reported measures for optimism and patience). Furthermore, we include controls that are unrelated to post-experimental responses of individuals: fixed effects for the round of the experiment, for the point in time t within a round and for the sequence in which subject i observes the assigned 10 portfolios as well as session fixed-effects.

²²Note that the negative coefficient of $I_{y_{jt} > y_{it}}$ results from the fact that the comparison group in the BASE treatment has a relatively low own current portfolio value whenever $y_{jt} > y_{it}$. In other words, situations in which good additional information is observed are, at the same time, situations in which the own portfolio value and hence beliefs are relatively low (compare also rows 1 and 2 in Figure 1). The significantly positive coefficient of the interaction term $I_{y_{jt} > y_{it}} \times P2-INFO$ confirms the treatment difference of BASE and P2-INFO with respect to comparisons of situations of relatively low and relatively high own portfolio value.

	(1)	(2)	(3)	(4)
	belief	belief	belief	belief
y_{it}	0.848*** (0.0129)	0.848*** (0.0129)	0.844*** (0.0136)	0.844*** (0.0136)
P2-INFO	-11.74** (5.740)	-10.55* (5.972)	-9.649 (5.897)	-8.418 (6.122)
$I_{y_{it} < y_{-it}}$	-6.762*** (2.114)	-6.764*** (2.114)		
$I_{y_{it} < y_{-it}}$ *P2-INFO	4.364** (2.063)	4.364** (2.063)		
$Q1_{y_{it}-y_{-it}}$			-10.77*** (2.752)	-10.77*** (2.752)
$Q2_{y_{it}-y_{-it}}$			-3.063*** (2.561)	-3.053*** (2.561)
$Q4_{y_{it}-y_{-it}}$			4.078 (2.787)	4.088 (2.787)
$Q1_{y_{it}-y_{-it}}$ *P2-INFO			3.135 (2.699)	3.129 (2.699)
$Q2_{y_{it}-y_{-it}}$ *P2-INFO			1.042 (3.066)	1.010 (3.066)
$Q4_{y_{it}-y_{-it}}$ *P2-INFO			-5.271* (3.142)	-5.307* (3.142)
Constant	82.13*** (7.515)	82.76*** (7.571)	82.88*** (7.585)	83.50*** (7.640)
Individual controls	No	Yes	No	Yes
Time and session fixed effects	Yes	Yes	Yes	Yes
N	7600	7600	7600	7600

Note: Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. “Individual controls” include gender, age, whether the field of study is business related, risk aversion, loss aversion, ambiguity aversion, distributional preferences, optimism and patience. “Time and session fixed effects” include period fixed effects, point-in-time fixed effects, fixed effects for the sequence in which portfolios are shown and session fixed-effects.

Table 2: Information Effect: Regression Results

Additional Information	Effect (p-value)	Tested Hypothesis
Very Good ($y_{it} < y_{jt}$)	-6.5 (0.268)	$H_0: P2-INFO + Q1_{y_{it}-y_{jt}} * P2-INFO = 0$
Good ($y_{it} < y_{jt}$)	-8.6 (0.152)	$H_0: P2-INFO + Q2_{y_{it}-y_{jt}} * P2-INFO = 0$
Bad ($y_{it} > y_{jt}$)	-9.7 (0.102)	$H_0: P2-INFO=0$
Very Bad ($y_{it} > y_{jt}$)	-14.9** (0.013)	$H_0: P2-INFO + Q4_{y_{it}-y_{jt}} * P2-INFO = 0$

Note: The effect of additional information as estimated in specification (3) of Table (??). For simplicity, notation is a bit imprecise; in this table with "*P2-INFO*" and "*quartile* _{$y_{it}-y_{jt}$} 1 * *P2-INFO*" to "*quartile* _{$y_{it}-y_{jt}$} 4 * *P2-INFO*" we refer the coefficients of these variables estimated in specification (3).

Table 3: Disaggregated effect of negative and positive information

the observed additional information is of the most positive type (very good) while being in the last quartile indicates that the observed additional information is of the most negative type (very bad).²³ We include an indicator variable for each quartile ($Q1_{y_{it}-y_{jt}}$ to $Q4_{y_{it}-y_{jt}}$) and an interaction term of each quartile dummy and the treatment dummy *P2-INFO*: $Q1_{y_{it}-y_{jt}} \times P2-INFO$ to $Q4_{y_{it}-y_{jt}} \times P2-INFO$. The baseline category is the third quartile where subjects observe “rather bad” information. The treatment effect in this case is given by *P2-INFO*, with an estimated coefficient of -9.649 which is borderline significant (p -value < 0.102). For the treatment effect in the other quartiles, we have to add the respective interaction term to the dummy *P2-INFO*; the results of the hypothesis tests are summarized in Table 3. As the results show, the lower the additional signals that a subject observes the lower are the beliefs. While “very good” and “rather good” additional signals (quartiles 1 and 2) do not significantly affect the beliefs, “very bad” additional information in *P2-INFO* leads to beliefs which are by 14.9 points lower than for the reference observations of the *BASE* treatment (p -value < 0.013). These results are also confirmed in specification (4) of Table 2 where we add individual-specific control variables from the post-experimental

²³Hence, for each of the four points in time within a round at which beliefs are reported, we generate quartiles of the distance $y_{it} - y_{jt}$. Then, we assign each observation to its quartile, for the *P2-INFO* treatment as well as for the reference portfolios in *BASE* (where the second portfolio is not observed). Our observations are balanced such that the observation for i at point in time t is assigned to the first or the second quartile if and only if $y_{jt} > y_{it}$.

questionnaire. Overall, additional information appears to have a monotonic effect on beliefs when going from very good to very bad additional signals; the treatment effect is strongest when subjects observe a second portfolio with very low value.²⁴

Result 1 *Additional signals of uncertain informativeness affect beliefs about own income. Bad additional information (signals $\tilde{y}_j(t) < \tilde{y}_i(t)$) lead to a downward adjustment of beliefs while good additional information (signals $\tilde{y}_j(t) > \tilde{y}_i(t)$) have no statistically significant effect on beliefs.*

Generally we find that subjects react to additional information even when they “know” the probability distribution of their income and the informativeness of additional information is uncertain. This uncertain informativeness of additional information is an important feature of our experiment. However, we find an asymmetric effect of additional information: Subjects lower their beliefs about their own income after observing additional portfolios with relative to the own portfolio lower values. On the other hand, after observing additional portfolios with higher values subjects do not adjust their beliefs. In light of the uncertainty about the informativeness of the additional portfolios the results appear to be even stronger. Responses are likely to be more pronounced when subjects know with certainty the income-generating processes and possible correlations.

3.2 Relative-Income Effect

In this section we analyze how satisfaction is affected when subjects observe signals about another subject’s expected income (Prediction 2). By comparing the P2-INCOME treatment to the P2-INFO treatment, we can perfectly control for all signals that could be directly informative for the own income. Figure 1 illustrates our identification strategy using the treatments P2-INFO (middle column) and P2-INCOME (right column).²⁵ We distinguish between situations in which subjects are behind in terms of relative income ($\tilde{y}_i(t) < \tilde{y}_j(t)$; row 1 in Figure 1) and situations in which subjects are ahead in terms of relative income ($\tilde{y}_i(t) > \tilde{y}_j(t)$; row 2 in Figure 1), that is, between favorable and unfavorable income comparisons.

For a first overview of the data, consider the change in simple means of satisfaction when comparing the P2-INCOME treatment to the reference observations in the P2-INFO

²⁴In line with the coefficient of $I_{y_{jt} > y_{it}}$, we find a similar monotonic effect for indicator variables $Q1_{y_{it}-y_{jt}}$ to $Q4_{y_{it}-y_{jt}}$: The more positive the additional information obtained in P2-INFO compared to y_{it} , the lower must have been the reference portfolio in BASE. The results on $Q1_{y_{it}-y_{jt}}$ to $Q4_{y_{it}-y_{jt}}$ confirm that observing a comparably low own portfolio value leads to lower beliefs.

²⁵Recall that the only difference of the two treatments is that the second portfolio observed in P2-INCOME is directly payoff-relevant for another subject and should therefore have an effect on satisfaction, while it should have no effect (or a weaker effect) on satisfaction in P2-INFO.

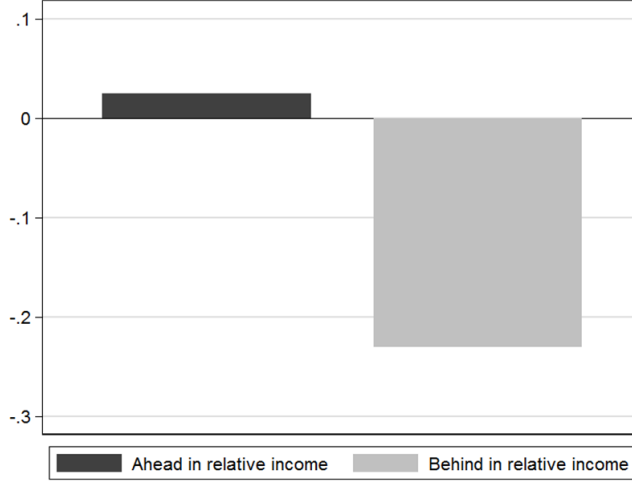


Figure 3: Change in satisfaction from treatment P2-INFO to P2-INCOME

treatment.²⁶ As Figure 3 indicates, we find partial evidence for Prediction 2: When subjects are behind in the sense that they have a lower current portfolio value ($\tilde{y}_j(t) > \tilde{y}_i(t)$), their satisfaction is lower than in the comparison group of P2-INFO, while being ahead ($\tilde{y}_j(t) < \tilde{y}_i(t)$) has no evident effect on average satisfaction.

To further investigate this result we estimate a crossed effects linear regression model similar to Section 3.1, on a sample of the observations from the treatments P2-INFO and P2-INCOME:^{27,28}

$$\begin{aligned}
satisfaction_{it} = & \beta_o + \beta_1 belief_{it} + \beta_2 y_{it} + \beta_3 P2-INCOME \\
& + \beta_4 I_{y_{jt} > y_{it}} + \beta_5 I_{y_{jt} > y_{it}} \times P2-INCOME + \beta X_{it} + \varepsilon_{it}
\end{aligned} \tag{6}$$

The dependent variable $satisfaction_{it}$ represents subject i 's reported satisfaction at point in time t . Our main variables of interest are the treatment dummy $P2-INCOME$ (which indicates observations stemming from the P2-INCOME treatment) and the interaction of $P2-$

²⁶See also Table 7 in Appendix A for descriptive statistics.

²⁷Note that we pool the observations from the sessions with the two different versions of the control question for the measure of satisfaction (compare Section 2.2), as the results obtained are very similar. See Table 8 in Appendix A for estimations that separate these two types of sessions.

²⁸The reasoning for using a crossed effects model is identical to the previous subsection. Also in this section all results are qualitatively robust to using a simple random effects regression model or a pooled OLS model with two dimensional clustered standard errors on subjects and session level. As satisfaction is an ordinal concept we also apply a random effects ordered probit model. In line with the findings of Ferrer-i-Carbonell and Frijters (2004) we find that the results are robust.

INCOME with the indicator variable $I_{y_{jt} > y_{it}}$, which now indicates that subject i is behind in terms of current portfolio value ($\tilde{y}_j(t) > \tilde{y}_i(t)$). Just as in specification (5) for the information effect, additional explanatory variables are the current own portfolio value y_{it} and the set X_{it} of controls (time and session fixed effects and individual-specific controls). Moreover, we include the reported beliefs $belief_{it}$ as explanatory variable. In (6), the coefficient β_3 reflects the treatment effect of being ahead (when $I_{y_{jt} > y_{it}} = 0$) compared to the reference group in P2-INFO, and the sum $\beta_3 + \beta_5$ corresponds to the treatment effect of being behind (when $I_{y_{jt} > y_{it}} = 1$), again compared to the reference group in P2-INFO.

In specification (1) of Table 4, the estimated coefficient of *P2-INCOME* is -0.179 and insignificant (p -value > 0.48). Since this coefficient measures the treatment effect in situations where $\tilde{y}_j(t) < \tilde{y}_i(t)$, we conclude that being ahead has no statistically measurable effect on satisfaction. For the treatment effect in situations where $\tilde{y}_j(t) > \tilde{y}_i(t)$ we have to analyze the sum of the coefficients of *P2-INCOME* and its interaction term with $I_{y_{jt} > y_{it}}$, which has the expected negative sign (-0.415) and is borderline significant (p -value < 0.105).²⁹ In specification (2) where we add individual-specific control variables from the post-experimental questionnaire, the treatment effect of being behind becomes slightly stronger (-0.447) and significant at the 10% level (p -value < 0.074). The treatment effect of being ahead remains insignificant. Moreover, as expected, the current value of the own portfolio (y_{it}) and the beliefs about the own end-of-period portfolio value ($belief_{it}$) have strong explanatory power throughout all specifications with positive coefficients that are significant at the 1% level.

Specification (3) further disaggregates the income comparisons using quartiles of the difference $y_{it} - y_{jt}$ just as in the estimation of the beliefs in Section 3.1. The variable $Q1_{y_{it}-y_{jt}}$ indicates that the difference $y_{it} - y_{jt}$ is in the first quartile among the observations at point in time t , meaning that subject i is “far behind” subject j . Correspondingly, the variable $Q4_{y_{it}-y_{jt}}$ indicates the forth quartile and, hence, that i is “far ahead.” Baseline category is the second quartile where subjects are “rather behind.” The treatment effects for the different categories are obtained when adding the indicator variable for the respective quartile and its interaction term with the treatment dummy *P2-INCOME*.

The results of the hypothesis tests summarized in Table 5 confirm a monotonic effect of relative income comparisons on the reported satisfaction, and are robust to adding individual-specific control variables from the post-experimental questionnaire (specification (4)).³⁰ The

²⁹Note the significantly negative coefficient of $I_{y_{jt} > y_{it}}$ reflecting the fact that within P2-INFO in situations of $\tilde{y}_j(t) > \tilde{y}_i(t)$ a subject’s own portfolio is comparably low, and hence, also satisfaction is low. Since the interaction term of P2-INCOME with $I_{y_{jt} > y_{it}}$ is significantly negative, this effect becomes more pronounced within P2-INCOME, in line with the result of the negative treatment effect of being behind.

³⁰The coefficients of $Q1_{y_{it}-y_{jt}}$ to $Q4_{y_{it}-y_{jt}}$ by themselves control again for the effect of comparably low or high own portfolio value in the control group. The monotonicity of the coefficients of $Q1_{y_{it}-y_{jt}}$ to $Q4_{y_{it}-y_{jt}}$ shows that satisfaction in the control group is increasing in the level of their current portfolio

	(1)	(2)	(3)	(4)
	satisfaction	satisfaction	satisfaction	satisfaction
y_{it}	0.0166*** (0.000524)	0.0166*** (0.000524)	0.0158*** (0.000542)	0.0158*** (0.000541)
$belief_{it}$	0.00369*** (0.000388)	0.00369*** (0.000388)	0.00359*** (0.000385)	0.00358*** (0.000385)
P2-INCOME	-0.179 (0.256)	-0.207 (0.247)	-0.246 (0.261)	-0.272 (0.253)
$I_{y_{it} < y_{-it}}$	-0.863*** (0.0663)	-0.862*** (0.0663)		
$I_{y_{it} < y_{-it}} * P2-INCOME$	-0.236*** (0.0652)	-0.236*** (0.0652)		
$Q1_{y_{it} - y_{-it}}$			-0.529*** (0.0884)	-0.528*** (0.0884)
$Q3_{y_{it} - y_{-it}}$			0.758*** (0.0799)	0.759*** (0.0799)
$Q4_{y_{it} - y_{-it}}$			1.107*** (0.104)	1.107*** (0.104)
$Q1_{y_{it} - y_{-it}} * P2-INCOME$			-0.274*** (0.0942)	-0.276*** (0.0942)
$Q3_{y_{it} - y_{-it}} * P2-INCOME$			0.0645 (0.0963)	0.0623 (0.0963)
$Q4_{y_{it} - y_{-it}} * P2-INCOME$			0.0698 (0.101)	0.0701 (0.101)
Constant	-0.825*** (0.289)	-0.808*** (0.281)	-1.254*** (0.292)	-1.237*** (0.284)
Individual controls	No	Yes	No	Yes
Time and session fixed effects	Yes	Yes	Yes	Yes
N	7600	7600	7600	7600

Note: Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. “Individual controls” include gender, age, whether the field of study is business related, risk aversion, loss aversion, ambiguity aversion, distributional preferences, optimism and patience. “Time and session fixed effects” include period fixed effects, point-in-time fixed effects, fixed effects for the shown sequence of portfolios and session fixed-effects.

Table 4: Relative-Income Effect: Regression Results

Relative-Income	Effect (p-value)	Tested Hypothesis
Far behind ($y_{it} < y_{jt}$)	-0.520** (0.044)	$H_0: P2\text{-INCOME} + Q1_{y_{it}-y_{-it}} * P2\text{-INCOME} = 0$
Behind ($y_{it} < y_{jt}$)	-0.246 (0.347)	$H_0: P2\text{-INCOME} = 0$
Ahead ($y_{it} > y_{jt}$)	-0.182 (0.484)	$H_0: P2\text{-INCOME} + Q3_{y_{it}-y_{-it}} * P2\text{-INCOME} = 0$
Far ahead ($y_{it} > y_{jt}$)	-0.176 (0.502)	$H_0: P2\text{-INCOME} + Q4_{y_{it}-y_{-it}} * P2\text{-INCOME} = 0$

Note: The effect of relative-income as estimated in specification (3) of Table (??). For simplicity, notation is a bit imprecise; in this table with " $P2\text{-INCOME}$ " and " $quartile_{y_{it}-y_{-it}} 1 * P2\text{-INCOME}$ " to " $quartile_{y_{it}-y_{-it}} 4 * P2\text{-INCOME}$ " we refer the coefficients of these variables estimated in specification (3).

Table 5: Disaggregated effect of negative and positive information

treatment effect of observing income of others becomes stronger the worse the own portfolio performs relative to the other subject's portfolio. In particular, satisfaction of subjects who are "far behind" is significantly lower (at the 5% level) in the P2-INCOME treatment compared to the reference group in P2-INFO treatment. Recall that the subjects in the control group of P2-INFO observe the exact same portfolios; hence, the treatment effect controls for the own portfolio history as well as for any information on the own portfolio value which subjects derive from observing a second portfolio. Since already in P2-INFO subjects may interpret the second portfolio as a signal of, for instance, expected income of the remaining participants of the experiment, the estimated treatment effect can be seen as a lower bound for the effect of being behind on satisfaction.

Result 2 *Observing signals about the income of others affects individual satisfaction. Being behind (signals $\tilde{y}_j(t) > \tilde{y}_i(t)$) has a negative effect on satisfaction while being ahead (signals $\tilde{y}_j(t) < \tilde{y}_i(t)$) has no statistically significant effect on satisfaction.*

It is interesting to note that we find asymmetric results for additional information on beliefs (Result 1) and for relative-income considerations (Result 2). These asymmetries, however, appear exactly in the opposite way. Beliefs are only significantly affected when

when moving from the first to the forth quartile.

subjects observe a *lower* additional portfolio (bad information), while satisfaction is only significantly affected when subjects observe a *higher* additional portfolio of another subject (i.e. subjects are behind). One possible interpretation could be that in either case subjects respond to the “negative prospect” rather than to the “positive prospect.” Put differently, while negative signals about the expected personal income and negative signals about the expected relative standing trigger significant reactions, positive signals do not or less so.

While our experiment focuses on the “information effect” and “relative-income effect” in isolation, we can also look at the “total effect” as the interplay of both effects. As derived in the theory section (equation (2)), the information effect can countervail the relative-income effect: Being behind may make individuals feel worse, but less so if they conclude from a higher income of others that their own income might increase as well.

A way to identify this total effect is to compare satisfaction in the P2-INCOME treatment to satisfaction in BASE (moving from the left to the right column in Figure 1). While the BASE treatment again provides a control group of subjects who observe the exact same own portfolios, the second portfolio observed in P2-INCOME might yield additional information about the own income and introduces directly aspects of relative income comparisons. To test for the “total effect” we estimate the same model as in equation (6), but on a sample of observations from treatments BASE and P2-INCOME. Moreover, we do not include $belief_{it}$ as independent variable, which would already be a control for the “information effect.” The estimation results are reported in Table A of appendix A. The effect of observing that the portfolio of another participant has a lower value (being ahead) remains highly insignificant and close to zero. The effect of observing another participant’s portfolio in case it has a higher value (being behind) remains negative but has a lower magnitude and turns insignificant (p -value > 0.263). This result can be interpreted as being in line with the theory section 2.1, when the “information effect” counteracts the “relative-income effect”. Individual i experiences less of a deterioration in satisfaction when he is behind in expected income because he interprets a relatively higher portfolio value of individual j as a positive signal for his own income prospects. The counteracting “information effect”, however, is not strong enough to overcome the “relative-income effect”, which is congruent with the finding that subjects’ beliefs do not react significantly to good information.

4 Conclusion

We investigate direct comparison effects (“relative-income effect”) and indirect belief-based effects (“information effect”) when individuals observe income of others in an environment characterized by uncertainty about the own income prospects. The empirical results of our

experiment show that when subjects observe negative additional information (others are likely to earn less than oneself), they lower their beliefs about their own income. Observing positive additional information (others are likely to earn more) has, however, no statistically measurable effect on beliefs about the own income. Moreover, observing signals that indicate a lower expected income relative to others has a negative effect on individual well-being, while observing signals that indicate a higher expected income relative to others has no statistically measurable effect on individual well-being. Hence, we find an asymmetric effect of information and of comparison considerations. This asymmetry can be interpreted as subjects being more reactive to “bad news” than to “good news”, where “bad news” refer to a bad signal about the own income prospects and to being relatively “behind” with respect to income comparison considerations.

The asymmetry of either effect and their interplay have implications for redistributive preferences and the acceptance of income inequality. First, individuals dislike if others earn more but consider the informational value of such signals is rather weak. Hence, regarding the total effect on subjective well-being, upward comparison and unfavorable inequality matter: An increase of the income of the reference group will leave individuals discontent. Second, individuals gain no or little subjective well-being when others earn less but consider such “negative additional information” as informative for their own income expectations and adjust their beliefs downwards. As a consequence, favorable inequality appears less important: A decrease in income of others can even leave subjects unsatisfied when the negative information strongly raises the fear to experience a similar drop in income. In summary, individuals might dislike both favorable and unfavorable inequality.

A Appendix

	Correlation Coefficient			
	BASE	P2-INFO	P2-INCOME	Total
CQ	-0.61	-0.61	-0.57	-0.60
Behind	-0.61	-0.59	-0.53	-0.58
Ahead	-0.46	-0.47	-0.42	-0.45
CQH	-0.66	-0.66	-0.60	-0.64
Behind	-0.63	-0.62	-0.56	-0.60
Ahead	-0.56	-0.54	-0.48	-0.52
Pooled	-0.63	-0.64	-0.59	-0.62
Behind	-0.62	-0.60	-0.54	-0.59
Ahead	-0.51	-0.50	-0.45	-0.48

Note: correlation coefficients of satisfaction and the control question “CQ”. The variable “CQ” takes on a value of 0 when subjects prefer the payoff of the currently assigned portfolio over the payoff of new, randomly drawn portfolio; otherwise “CQ” takes on a value of 1. “CQH” is a hypothetical version of “CQ” where subjects are hypothetically asked whether they prefer the payoff of the currently assigned portfolio (“CQH” = 0) or whether they would prefer the payoff of a new, randomly drawn portfolio (“CQH” = 1). “Behind” refers to being behind in relative-income, while “ahead” refers to being ahead in relative-income.

Table 6: Correlation of Satisfaction and Control Question

	BASE	P2-INFO	P2-INCOME	Total			
	Mean	Mean	Mean	Mean	S.D.	Max	Min
Satisfaction	4.57	4.49	4.39	4.49	2.69	10	0
Behind	3.37	3.25	3.02	3.21	2.32	10	0
Ahead	5.78	5.73	5.75	5.76	2.42	10	0
Belief	309.2	306.4	308.9	308.2	101.5	902	0
Bad Add. Info.	359.4	354.1	359.3	357.6	85.5	902	0
Good Add. Info.	258.9	258.7	258.6	258.7	91.7	750	1
Male	0.47	0.37	0.34	0.39	0.49	1	0
Age	23.8	22.8	22.8	23.1	4.2	52	17
Econ	0.33	0.29	0.40	0.34	0.47	1	0

Note: “Male” takes on a value of 1 for male subjects. Econ takes on a value of 1 for subjects that study in business related fields such as economics. “Behind” refers to the case of being behind in relative-income. “Ahead” refers to situations of being ahead in relative-income. “Bad Add. Info.” refers to situations when subjectes observe an additional portfolio of a lower value than their own portfolio (bad additional information). “Good Add. Info.” refers to situations when subjectes observe an additional portfolio of a higher value than their own portfolio (good additional information).

Table 7: Summary Statistics

	CQ (1) satisfaction	CQH (2) satisfaction	Pooled (3) satisfaction
y_{it}	0.0158*** (0.000700)	0.0176*** (0.000752)	0.0166*** (0.000524)
$belief_{it}$	0.00446*** (0.000526)	0.00287*** (0.000572)	0.00369*** (0.000388)
P2-INCOME	-0.0766 (0.260)	-0.175 (0.246)	-0.207 (0.247)
$I_{y_{it} < y_{-it}}$	-0.889*** (0.0875)	-0.780*** (0.0968)	-0.862*** (0.0652)
P2-INCOME* $I_{y_{it} < y_{-it}}$	-0.221*** (0.0865)	-0.256*** (0.0976)	-0.236*** (0.0652)
Constant	-0.825*** (0.350)	-0.808*** (0.343)	-1.254*** (0.281)
Individual controls	Yes	Yes	Yes
Time and session fixed effects	Yes	Yes	Yes
N	3840	3760	7600

Note: “CQ” refers to a subsample of treatments incorporating the control question about the preferred final payoff. “CQH” refers to a subsample of treatments incorporating the hypothetical version of the control question about the preferred final payoff. “Pooled” refers to the full sample, treatments incorporating the control question and treatments incorporating the hypothetical version of the control question. Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. “Individual controls” include gender, age, whether the field of study is business related, risk aversion, loss aversion, ambiguity aversion, distributional preferences, optimism and patience. “Time and session fixed effects” include period fixed effects, point-in-time fixed effects, fixed for the sequence portfolios are shown and session fixed-effects.

Table 8: Relative-Income Effect: Subsample Regression Results

	(1)	(2)
	satisfaction	satisfaction
y_{it}	0.0196*** (0.000398)	0.0187*** (0.000425)
P2-INCOME	0.0385 (0.255)	-0.207 (0.265)
$I_{y_{it} < y_{-it}}$	-0.749*** (0.0656)	
$I_{y_{it} < y_{-it}} * \text{P2-INCOME}$	-0.336*** (0.0644)	
$Q1_{y_{it} - y_{-it}}$		-0.599*** (0.0873)
$Q3_{y_{it} - y_{-it}}$		0.577*** (0.0792)
$Q4_{y_{it} - y_{-it}}$		0.949*** (0.103)
$Q1_{y_{it} - y_{-it}} * \text{P2-INCOME}$		-0.129 (0.093)
$Q3_{y_{it} - y_{-it}} * \text{P2-INCOME}$		0.276*** (0.0951)
$Q4_{y_{it} - y_{-it}} * \text{P2-INCOME}$		0.226*** (0.0992)
Constant	-0.725*** (0.275)	-1.071*** (0.273)
Individual controls	Yes	Yes
Time and session fixed effects	Yes	Yes
N	7520	7520

Note: Regression analysis on observations of treatment BASE and treatment P2-INCOME. Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. “Individual controls” include gender, age, whether the field of study is business related, risk aversion, loss aversion, ambiguity aversion, social preferences, optimism and patience. “Time and session fixed effects” include period fixed effects, point-in-time fixed effects, fixed effects for the shown sequence of portfolios and session fixed-effects.

Table 9: Total Effect: Regression Results

B



Note: Illustration of all portfolios (time series) as shown to participants during the experiment. Y-axis: Portfolio value in experimental currency. X-axis: time.

C Experimental Instructions

Welcome to the Experiment!

Please read this instruction carefully and completely. Thoroughly understanding this instruction will help you to earn more money.

Your earnings during the experiment are measured in Taler. At the end of the experiment the Talers you earned will be converted into Euros and we will pay you accordingly. The following conversion rate holds: **25 Talers = 1 Euro**. Additionally, each participant receives a show-up fee of 4 Euros.

We ensure your anonymity throughout the experiment. Please keep in mind that you are not allowed to communicate with other participants during the experiment. If you do not obey this rule you will be asked to leave the laboratory without getting paid. At any time, when you have questions please raise your hand and we will help you.

Your Task:

In the experiment, each participant is assigned a portfolio whose value growth you will observe in a graph on your screen. You can think of your “portfolio” as a part of your earnings that you receive at the end of the experiment. Portfolios are generated according to a stochastic process by the computer. At the end of this instruction you find a graph illustrating possible portfolio courses.

You will randomly be assigned into groups of two. However, you will not learn the identity of your group member. Each participant will observe the value growth of the own portfolio and of the group member’s portfolio. The starting value of all portfolios is 300 Talers and the final portfolio value (a whole number larger than zero) represents the major part of your earnings of the experiment.

The value growth of portfolios will stop frequently and the following questions will appear on your screen:

1. How satisfied are you with your current portfolio on a scale 0 (very dissatisfied) to 10 (highly satisfied)?
2. What do you think: what will be the final value of your current portfolio (in Talers)?
3. Choose one of the following options:
 - (a) I want to be paid the final value of my current portfolio.
 - (b) I want to be paid the final value of a new portfolio, which is randomly assigned at the end of the experiment.

You and your group member answer repeatedly and independently the same 3 questions.

At each point in time you choose your answers anew and independent of your previously given answers. Your group member will not observe your answers.

During the complete period you and your group member keep the assigned portfolios and answer each set of 3 questions by referring to the current portfolio. This also applies for the case when you have chosen as a payment the final value of a new, randomly assigned portfolio in question 3.

Procedure:

Overall, you will repeat this task 10 times. Consequently, you will observe 10 portfolio performances where these 10 rounds are completely independent of each other: for each period the groups of two are randomly re-assigned and each time you and your new group member receive new randomly assigned portfolios.

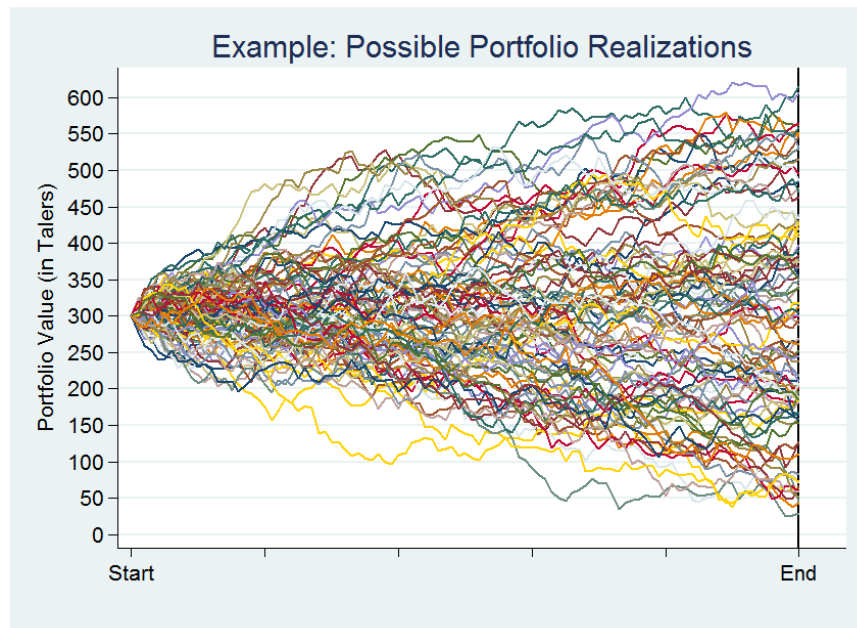
At the end of the experiment, in a first step, the computer will randomly select one of the 10 rounds. In the selected rounds the computer will select exactly one point in time at which you answered the above described 3 questions. Your payment will be determined by your answers that you have given at this selected point in time and includes three components:

- For your answer with respect to your satisfaction you receive 50 Talers flat, independent of the value entered.
- The closer your estimate of the final portfolio value at the selected point in time matches the actual final portfolio value, the more money you receive:
 - If you predicted exactly the realized final portfolio value, you receive 250 Talers.
 - The exact formula to calculate your payment is:
$$\text{Payment (in Talers)} = 250 - \frac{1}{10} (\text{estimate} - \text{actual final value})^2;$$
 at least, however, 25 Talers.
- You receive the final value of your portfolio as a payment:
 - If you chose **Option 3(a)** at the selected point in time, you will receive the final value of the portfolio assigned in the selected period.
 - If you chose **Option 3(b)** at the selected point in time, a new portfolio will be randomly assigned to you and you will receive the final value of this new portfolio as a payment.
 - Note: In case you receive the final value of a new, randomly selected portfolio you will see the complete portfolio development at the end of the experiment on your screen.

In total your payment consists of the final portfolio value, the Talers paid dependent on your estimate of the final portfolio value and the Talers you receive for your answers with respect to your satisfaction. These Talers are converted into Euros and paid to you in cash. After the experiment we ask you to provide some more information; as a matter of course, all of your provided information will only be used anonymized.

Thank you very much for showing up and good luck!

The following graph illustrates possible portfolio realizations. The starting value of all portfolios is 300 Talers. On the horizontal axis the points in time are indicated (4 in total) when you will be asked to answer the 3 questions explained above.



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