SOCIAL COMPARISONS UNDER UNCERTAINTY IN PUBLIC FINANCE

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Harald Wilhelm Lang

Referent:	Prof. Dr. Kai A. Konrad
Korreferent:	Prof. Dr. Martin G. Kocher
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Für meinen Vater.

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‘Creativity always comes as a surprise to us; therefore we can never count on it and we dare not believe in it until it has happened.’

Albert O. Hirschman (1967, p.13)

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Chapter 1

Introduction and summary

Envy is a powerful human emotion. Anthropologists and sociologists recognize envy as a possible driving force of individual behavior and it is not just by chance that envy is one of the seven deadly sins in Christianity. In the field of economics, envy translates to the idea of relative standing concerns or social comparisons: Individuals might care not only about their absolute consumption but also about their position relative to others.[1] Economic models incorporating social comparisons deviate from traditional models in which individuals care purely about absolute consumption. If relative standing matters, important questions arise: What changes with respect to traditional economic models? What are the implications of social comparisons in economics and how important are these? Not least, should we question the wisdom of many economic policy recommendations from models that ignore social comparisons, as Frank (2005, p.137) suggests?

Evidence shows that social comparison does matter and that it is essential to understand its implications for the field of economics.[2] The motivation of the present thesis is to deepen our understanding of social comparisons with a focus on social comparisons under uncertainty. While individuals are commonly exposed to uncertainty, to date, few empirical studies addressed social comparisons in an uncertain enviroment. Evidence on the interaction of information and social comparisons, and also for behavioral implications, is still rare. This is remarkable as Albert O. Hirschman discussed information-related effects and social comparisons as early as 1973. In a seminal contribution, Hirschman (1973) introduced

[1]More precisely, relative standing concerns imply that individuals dislike being behind but gain satisfaction from being ahead of others. Thus, individuals could be described as envious when behind and as gloating when ahead of others.

[2]Evidence on relative standing concerns will be discussed in detail in Section 1.1 of this chapter. See Clark and D'Ambrosio (2015) for a recent overview.

the "tunnel effect,"and analyzed situations when informational effects countervail social comparison effects.[3] The interplay of informational effects and social comparisons described by the tunnel effect is an important theme throughout the present thesis and motivates chapters 2 and 3. As a natural next step the question of implications for decisions under uncertainty arises. Consequently, Chapter 4 addresses effects of social comparisons on risk taking.

The main contribution of the present thesis is experimental and empirical evidence of the effects of social comparisons under uncertainty. The methodologies used to generate results are theory-guided controlled laboratory experiments and an empirical investigation applying a global survey dataset that incorporates macro- and microeconomic variables. Generally, the empirical investigation of social comparisons under uncertainty is intrinsically challenging. Essential variables for an analysis are often not observable in the field or are at risk of being defectively measured. For these reasons, controlled laboratory experiments are a suitable method to derive clean and causal evidence on fundamental effects. The controlled environment allows me to precisely measure variables such as the income and expectations of individuals, the reference group's income, and the information that individuals learn about the reference group. The outcome of experiments should be understood as positive results that are complementary to other methodologies such as classical empirical methods.

Before I continue with the detailed analyses in the following chapters, this introductory chapter proceeds with a discussion of social comparisons and demonstrates its manifold implications in economics. Section 1.2 discusses social comparisons under uncertainty with subsections on information-related effects and risk taking. Section 1.4 continues with methodological aspects of an experimental method to investigate social comparisons. Section 1.5 provides an outline of the present thesis and a summary of the main contributions.

1.1 Social comparisons in economics

The role of social preferences has provoked a large body of literature in economics in recent decades.[4] Social preferences are understood to be a form of individual preferences that depend not only on the personal material payoff (consumption)

[3]The "tunnel effect" is named after a tunnel anecdote in Hirschman's (1973) article and will be discussed in more detail in Section 1.2 of this chapter.

[4]For an overview see, for instance, Fehr and Schmidt (2006).

but also on resources that are allocated to others (Fehr and Schmidt 2006).[5] These types of preferences have been widely studied in different forms, such as altruism (e.g., Andreoni and Miller 2002) or inequity aversion (e.g., Fehr and Schmidt 1999, Bolton and Ockenfels 2000).[6] Social comparisons are maybe the oldest type of social preferences that have been studied in the field of economics, with early contributions reaching back as far as Veblen (1899).[7] Outside the economic profession people would most commonly refer to social comparisons as envy. Probably most people would agree that envy is a powerful human emotion, and thus, we might expect behavioral implications, one way or another. Consider the following thought experiment for a demonstration of how social comparisons may lead to different outcomes compared to a traditional economic model that is based purely on absolute consumption.[8]

Imagine you could choose for your grandchild to live in either society A or B. Both societies are identical but for the levels of income (per annum). Importantly, both societies exhibit identical price levels. You are supposed to choose the society in which your grandchild would be most content. Which society would you choose:

- *Society A: Your grandchild's income is 54,000 EUR; the average income in the society is 60,000 EUR.*
- *Society B: Your grandchild's income is 54,000 EUR; the average income in the society is 48,000 EUR.*

Traditional models that assume absolute consumption-based preferences predict that people should be indifferent between both societies. However, most people would choose society B. In fact, when I presented this hypothetical question to a sample of 239 participants of an economic laboratory experiment, 84 percent of the

[5]Note that social preferences, as they are defined here, are not necessarily in contradiction to neoclassical economics (Binmore and Shaked 2010).

[6]Besides models of social preferences further "other-regarding preferences" theories found much attention in economics. These models include interdependent preferences and intention-based reciprocity (see, for instance, Sobel 2005).

[7]I use the terms social comparisons, comparison considerations, relative standing concerns, and income comparisons interchangeably throughout the present thesis.

[8]This experiment is inspired by Alpizar et al. (2005). Earlier hypothetical experiments of a similar nature have been conducted by, for instance, Tversky and Griffen (1991) and Zeckhauser (1991). Also Frank (1985a) addresses his reader with a similar thought experiment about living in hypothetical societies.

students chose society B over society A.[9] The case becomes even more clear when I asked the same sample of participants slightly adjusted versions of this question. For these questions I adjust the income level for the grandchild in society B while everything else remains unchanged. Figure 1.1 shows that about 60 percent of the participants would still prefer society B, even though their grandchild would earn less than in society A (#2 in Figure 1.1). Thus, in this hypothetical example, a majority of participants seem to be willing to forgo absolute income in return for a better relative position. In a following question, 31 percent of the participants would still prefer society B, even when their grandchild would not only earn less than in society A but also less than the average in society B (#3 in Figure 1.1). Apparently, these participants find the (negative but still) higher relative income position in society B more important than the higher absolute income level in society A. Unambigious preferences for society A, with about 99 percent of participants choosing A over B, emerge only when the absolute *and* the relative income positions are both worse in society B (#4 in Figure 1.1).

The purpose of this short experiment is to demonstrate that social comparisons matter for individuals and may offer relevant implications.[10] Such implications have been widely discussed in the economic literature. In one of the earlier contributions, Duesenberry (1949) discusses how household consumption can be affected by the expenditure of the household's neighbors. Galbraith (1958) even argues that most consumer demands are determined by society rather than by innate needs only. One well-discussed implication of social comparisons is that an increase in one's own position imposes a negative externality on others. When one individual improves her, for instance, income position then all who compare themselves with her will experience a loss in utility due to the deterioration in the relative position. Frank (1985a) refers to a "positional treadmill" for the situation when all spend effort to gain advantage but remain in the same relative position because of everybody's struggle to get ahead. However, social comparisons can also provide positive externalities that benefit the society. If social comparisons foster competition among certain groups, such as entrepreneurs or scientists, the

[9]The data were collected at the econlab of the Max Planck Institute for Tax Law and Public Finance. The hypothetical questions were presented to participants of an economic experiment in the post-experimental questionnaire, following the main part of the experiment.

[10]While this hypothetical experiment focuses on income, social comparisons do not need to be restricted to income. Consumption (e.g., Veblen 1899), leisure (e.g., Frijters and Leigh 2008) or other socioeconomic domains (e.g., Mujcic and Frijters 2015) may also serve as a medium for comparison concerns. Observable domains may serve as a medium to signal wealth. For instance, Glazer and Konrad (1996) show that one motive for donating to charity can be the desire to demonstrate wealth.

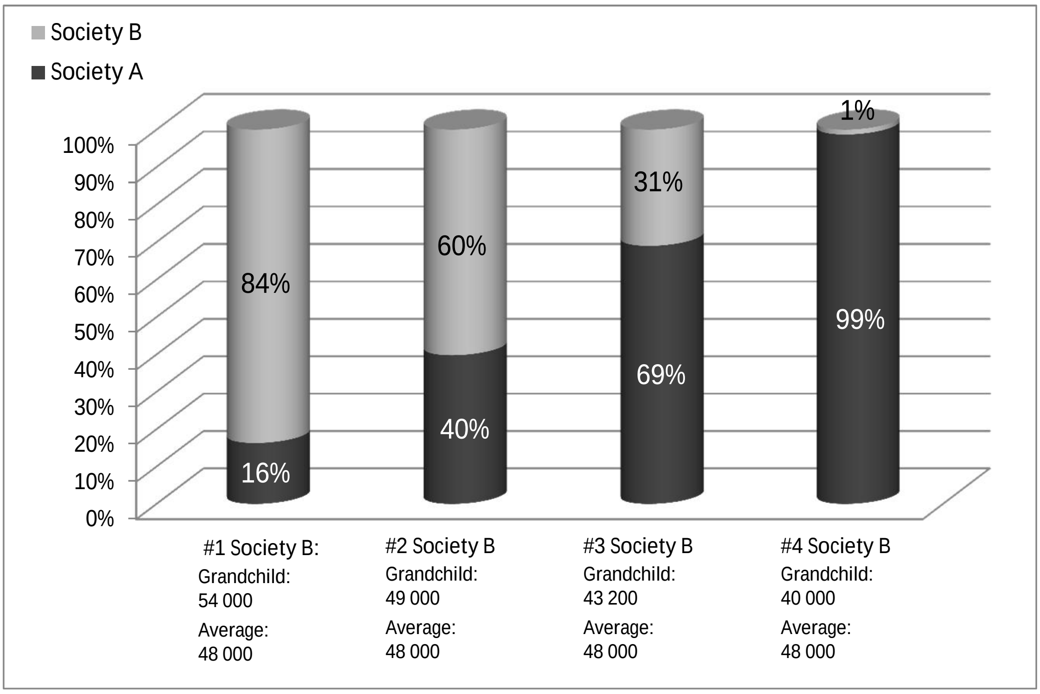

Figure 1.1: Would you choose society A or B for your grandchild?

Note: Answers of 239 participants to four versions of the question of whether they would choose for their hypothetical grandchild to rather live in the hypothetical society A or B. In all versions of the question the income levels in society A remain fixed at 54,000 EUR for the own hypothetical grandchild and at 60,000 EUR as the average income level. The income level of the hypothetical grandchild changes between societies 1 B to 4 B. In all cases, the price level is assumed to be identical in societies A and B.

result can be higher effort and output (Congleton 1989).[11,12]

These possible external effects of social comparisons offer implications in the field of public finance. For instance, income taxes can be less distortive if individuals exhibit social comparisons with respect to income (Layard 1980). Boskin and Sheshinski (1978) demonstrate in an optimal taxation framework that relative income concerns lead to higher optimal marginal tax rates. Aronsson and Johansson-Stenman (2008) show in a framework with nonlinear income taxation and provision of public goods that social comparisons imply higher marginal income tax rates compared to the conventional case.[13,14] Furthermore, when some goods, such as luxury goods, are more prone to social comparisons than other goods, taxes on such "status goods" are less distortive (Konrad 1990).[15] Konrad (1992) shows in a neoclassical growth model that social comparisons with respect to wealth can lead to an overaccumulation of capital and, ceteris paribus, a capital income tax and a wealth tax can be welfare-improving. Dupor and Liu (2003) demonstrate that social comparisons can lead to overconsumption. Ng (1987b) discusses that the level of public expenditure can be too low due to social comparisons. From an international perspective, social comparisons can lead to an underprovision of national and global public goods in case of social comparisons toward other domestic residents and residents in foreign countries (Aronsson and Johansson-Stenman 2014).[16]

The discussion on the implications of social comparisons, such as the "posi-

[11]Congleton (1989) refers to some individuals that engage in "status games,"such as capitalists that compete in accumulating capital. Social comparisons are operative in these "status games" and fosters competition. Athletes and the mass entertainment of sport events provide another example. Congleton (1989) emphasizes that good institutions could make use of social comparisons to benefit the society.

[12]Weimann et al. (2015) provide a recent discussion on the role of social comparison concerns for society. They argue that, on balance, social comparisons are probably beneficial for society because of the intensifying effect on beneficial competition.

[13]Wendner and Goulder (2008) investigate linear optimal taxation and optimal provision of public goods when individuals exhibit social comparisons. Similarly, they find that social comparisons lead to a lower excess burden of consumption taxes and labor taxes compared to purely absolute consumption based preferences.

[14]Aronsson et al. (2016) have analyzed the implications of social comparisons for optimal capital and labor income taxation in a small open economy (i.e., mobile capital), using an overlapping generations framework. They find that optimal tax rules change considerably when savings abroad cannot be observed. Among other results, capital income taxation becomes ineffective and tax rules for marginal labor income become rather complex.

[15]Some luxury goods may even be taxed without any excess burden. Ng (1987a) refers to such goods as "diamond goods."Konrad (1990) discusses factors that complicate the taxation of luxury goods such as changes of the "status-property" of goods over time.

[16]Social comparisons can also provide a motive for a countercyclical tax policy over the business cycle, from a macroeconomic perspective (Ljungqvist and Uhlig 2000).

tional treadmill," imposes the question: Why would individuals behave in such a way? What could explain individuals experiencing social comparison concerns? Many economists have pointed out that social concerns may well be rational from an individual point of view, when a higher relative position helps to achieve other important objectives (e.g., Hirsch 1976, Sen 1983). For instance, Frank (1985b) explains that high observable consumption (relative to others) might signal past success and high labor income to employers and business partners, and therefore increase the chances of finding a new or better job. Konrad (1990) offers a sociobiological explanation for social comparisons. Preferences for the relative position of individuals can be the outcome of the evolutionary process. If a better relative position in observable factors helps to signal the unobservable quality as a partner for reproduction, then striving for the best relative position becomes a successful strategy to pass on genes. As a possibly successful strategy in the evolutionary process, comparison concerns may have become a part of human preferences. Samuelson (2004) gives a related but alternative explanation of how social comparisons can be the optimal outcome of the evolutionary process. He shows that relative consumption preferences can be an evolutionary solution to induce more effective individual behavior in an uncertain environment. In hunter-gatherer times consumption levels of others could have contained important information. Relative comparison preferences can trigger reactions and induce individuals to respond to such information more effectively.[17]

Evidence in line with the comparison concerns of individuals is well-documented in the economic literature (e.g., Clark et al. 2008).[18] A large body of empirical studies analyze subjective well-being and the effect of other people's income changes (e.g., Clark and Oswald 1996, Ferrer-i-Carbonell 2005, Luttmer 2005, McBride 2001). The Easterlin paradox (Easterlin 1974) provided an initial impetus for this literature and stimulated much debate (e.g., Stevenson and Wolfers 2008).[19] Generally, these studies document a negative relationship between sub-

[17]Rayo and Becker (2007) make a related argument. They model happiness as a biological "measurement instrument" of individuals to evaluate different choices. In their theory, the "measurement instrument" works more accurately when individuals apply relative measures (i.e., relative to past experiences and relative to others). Therefore, they argue, the relative measurement has been successful in the evolutionary process of genetic multiplication and became part of individuals' preferences.

[18]For a recent overview of the literature see Clark and D'Ambrosio (2015).

[19]The Easterlin paradox was motivated by deviating correlations between GDP and subjective well-being in cross-sectional and time-series analyses. It states that at any point in time richer individuals are happier than poorer individuals, but as per capita GDP increases over time, average subjective well-being does not increase. For a recent discussion see, for instance, Weimann et al. (2015).

jective well-being and the income of a defined reference group. A different strand of the literature provides evidence of social comparisons using survey-experimental studies (e.g., Alpizar et al. 2005, Carlsson et al. 2007, Johansson-Stenman et al. 2002, and Solnick and Hemenway 2005). The hypothetical experiment discussed above is an example of these types of survey-experiments. Further evidence of social comparisons provide studies that rely on revealed preference approaches, including laboratory experiments (e.g., Bolton 1991) and, more recently, natural experiments (e.g., Kuhn et al. 2011).[20]

1.2 Social comparison and informational effects

After a glance at the extensive empirical literature on social comparisons, it seems remarkable that social comparisons under uncertainty has received comparably less attention. After all, income is to a large part dependent on future developments. Consequently, social comparisons often occur in an uncertain environment. Indeed, Samuelson (2004) even argues that uncertainty and the information value of the consumption of others could be the very reason why social comparisons evolved in individual preferences. Following this reasoning, investigating social comparisons under uncertainty seems relevant and promising.

The first to discuss the relevance of informational effects when income is uncertain was Albert O. Hirschman (1973). His theoretical analysis builds on the intuitive idea that observing the income of others can be informative about the own future income prospects. Hirschman pointed out that such informational effects can even outweigh social comparison concerns and illustrated his idea in a well-known anecdote:

"Suppose that I drive through a two-lane tunnel, both lanes going in the same direction, and run into a serious traffic jam. No car moves in either lane as far as I can see (which is not very far). I am in the left lane and feel dejected. After a while the cars in the right lane begin to move. Naturally, my spirits lift considerably, for I know that the jam has been broken and that my lane's turn to move will surely come any moment now. Even though I still sit still, I feel much better off than before because of the expectation that I shall soon be on the move,"

[20]Furthermore, Kirchsteiger (1994) theoretically shows that individuals' behavior in the ultimatum game could be explained by envious players (i.e., social comparisons) and strategic concerns. The ultimatum game was first introduced by Güth et al. (1982) and inspired much research due to its simplicity and the striking deviations from predictions based on traditional economic theory.

(Hirschman 1973, p.545).
Named after this anecdote, Hirschman refers to the "tunnel effect" for the situations when positive informational effects outweigh social comparisons and, therefore, individuals experience an increase in satisfaction when they observe the income advances of others.[21]

In its nature, this interaction of informational and social comparison effects is quite general and might apply in many situations of social comparisons. One interesting application is in the area of public finance. When Hirschman wrote his 1973 paper he focused on the implications of the tunnel effect for the tolerance toward inequality in societies. Consider a growing economy and the exploding income of certain groups of a society, while a majority of people remain stuck in utter poverty. As a result of the advances of some while the broad mass of people face poverty, one might suspect that social cohesion, possibly even peace, is at risk. Hirschman (1973) explains that, quite contrary to this first suspicion, a majority of people might be quite satisfied with such a development – because of the tunnel effect. The hope of the poor majority to receive a piece of the cake, and possibly become rich themselves (or their children), renders them satisfied. Put more technically, the poor people in this example believe in a positive correlation between their future income and the increasing income of the privileged groups in their society. Of course, when their hope is disappointed, and they lose their belief in the positive correlation of incomes, social cohesion will finally be at risk, as Hirschman (1973) points out. Then, social comparisons will dominate and produce anger at losing in relative terms compared to the rich minority.

For Hirschman, a scholar with a strong interest in development economics, the transitional economies of developing countries was the starting point of his analysis. However, the idea of the tunnel effect carries over to the more general case of preferences for redistribution. Redistribution is more common for developed countries and varies considerably across these countries (Alesina et al. 2004). One important factor for the preferences for redistribution, and thus also redistributive taxation, is the social mobility in a society (Alesina and La Ferrara 2005).[22] If

[21]Hirschman also points to an interesting dynamic aspect. He continues his anecdote by stating that the initial satisfaction may eventually give way to anger when his hope for progression in his lane is disappointed. When the cars in the own lane remain stuck while others pass by, the disappointed hope may induce illegal and even violent reactions of individuals, such as illegal double line crossings to squeeze onto the other lane.

[22]Social mobility is also an important issue in the currently effervescent debate on inequality. A high social mobility implies that we may worry less about advances in the income and wealth of the very rich because every individual has a chance of becoming rich. For a recent discussion of arguments that govern the debate on inequality, see, for instance, Piketty (2015).

a society exhibits high social mobility and poor people expect that they or their children have a fair chance to become high-income earners, they may favor a moderate level of redistribution. Put differently, they experience a tradeoff between higher potential future (net) income and higher current income in the form of redistributive transfers from the government. Social mobility ensures that poor people believe (to some extent) in a positive correlation between the top-earners' income and their future income – in line with Hirschman's informational effects. One important assumption for this reasoning is that once redistributive policies are implemented then these are long-lasting. Bénabou and Ok (2001) formalize this idea of prospects of upward mobility (POUM). They show that prospects of upward mobility lead indeed to a lower demand for redistribution compared to the classical "workhorse" political economic model of Meltzer and Richards (1981).[23] One important aspect is that it is the perceived rather than the actual social mobility that affects preferences for redistribution. Alesina et al. (2004) find evidence that differences in (perceived) social mobility can explain differences in preferences for redistribution between the United States and European countries. The latter redistribute considerably more than the United States. Kopczuk et al. (2010) and Chetty et al. (2014a, 2014b) provide evidence that the more optimistic view of people in the United States on social mobility is only partially justified. While social mobility has been stable over the last decades for the average citizen in the United States, social mobility deteriorated for men and shows a considerable heterogeneity among different regions of the United States.[24,25]

While the discussion on the prospects of upward mobility relates to positive informational effects à la Hirschman, informational effects can also work the other way around. The declining incomes of others can provide a bad signal for prospects of future income. Increasing unemployment rates and declining aggregate income in a recession could be one example. Furthermore, the correlation between incomes is not always unambiguously positive. In an overall stagnant economy with no economic growth, the increasing income of others could imply that people expect less for themselves. In this case, the increasing income of other people can imply that they receive more of a fixed amount of resources, while less remains available

[23] Bénabou and Ok (2001) show that certain conditions are necessary for their results. These include that individuals are not too risk averse, that expected income is a concave function of today's income and a skewed distribution of random shocks to income.

[24] Some regions in the United States offer levels of social mobility that are persistently lower than in most other developed countries (Chetty et al. 2014).

[25] Kopczuk et al. (2010) analyze data that reach back to the 1950s. While social mobility deteriorated for men, it increased for women during this time period.

for oneself. In either of these two cases, individuals may fear decreases in income that could lead to a more favorable attitude toward redistribution.[26] Finally, income uncertainty can generally provide an insurance motive for redistributive taxation (Varian 1980).

The evidence on informational effects à la Hirschman usually relies on survey data. Ravallion and Lokshin (2000) find that individuals who expect their economic situation to improve show a weaker support for redistribution. Senik (2004, 2008) investigates self-reported life satisfaction. She finds a positive reaction of satisfaction if the income of a defined reference group increases that is in line with the tunnel effect. Clark et al. (2009) consider job satisfaction. They match Danish employer-employee data with survey data and find a positive correlation between job satisfaction and the income of colleagues. These studies are an important starting point but suffer from identification problems. Chapter 2 of the present thesis contributes to the literature by providing clean and causal evidence on informational and comparison effects from a controlled laboratory experiment.[27]

1.3 Implications for risk taking behavior

When we think about social comparisons under uncertainty, an important question is: What are the implications for decisions under uncertainty? Individual decisions under uncertainty are one of the most studied subject in economics. However, the impact of the social context on risk taking is not well-understood so far and the literature addressing risk taking in a social context is still in its infancy (Trautmann and Vieider 2012, Fafchamps et al. 2015). Research that contributes to a deepening of our knowledge in this matter is relevant as most decisions under uncertainty involve a social context. Managerial decisions in a firm or decisions involving risk in the family are only two examples. In the financial industry commonly available rankings for investment funds provide a context for comparisons and potential social influences on investment decisions.[28]

"Social context" can refer to many things that could affect risk taking in several

[26]In line with this reasoning, the fear of a decreasing income and socially falling behind can be particularly relevant for highly developed, sluggish-growth countries, such as Germany. Indeed, discussions about a declining middle class and higher risk for falling behind in the public debate in Germany provide anecdotal evidence that this might be the case. See, for instance, the Frankfurter Allgemeine Zeitung (online), "In der Abstiegsgesellschaft" ("In the descent society"), published on June 15th, 2016.

[27]See Section 1.4 for a discussion on the potential identification problems of studies that analyze social comparisons in the field.

[28]Investment funds ratings are published by, for instance, *Forbes* and *Morningstar*.

ways.[29] The present thesis focuses on the effects of relative standing comparisons on decisions under uncertainty. The first to discuss relative standing comparisons and risk taking were Robson (1992) and Konrad and Lommerud (1993). Robson (1992) models social comparisons as individuals who are concerned about their rank in the income distribution. In Robson's (1992) model individual utility can be concave in wealth itself but convex over some range due to an additional social comparisons part in the utility function. Robson shows that social comparisons can explain that individuals simultaneously purchase insurance and participate in lotteries.[30] This observation, that individuals play high-risk lotteries but still buy insurance, cannot easily be explained in a standard expected utility framework and puzzled economists for many decades. Social comparisons provide an alternative and a maybe more natural explanation for this phenomenon than Friedman and Savage's (1948)'s idea of an utility function that is "concave-convex-concave" in consumption.

Konrad and Lommerud (1993) model social comparisons as individuals caring about the distance in income to others. They distinguish cases of systematic (correlated) and non-systematic (uncorrelated) risk. Konrad and Lommerud (1993) show that, for non-systematic risk, risk taking can be higher or lower compared to standard expected utility as individuals can be risk averse or risk loving with respect to their relative position. Thus, the utility function can become more concave or convex when social comparisons are incorporated. They continue their analysis showing that social comparisons lead to excessive risk taking in their model, and thus, social comparisons provide a rationale for regulations that discourage risk taking.

Only recently some studies have provided evidence on social comparisons and risk taking. Because it is difficult to pursue an investigation of risk taking and social comparisons in the field, the evidence predominantly stems from experimental studies – the results are partially contradictory. For instance, Rohde and Rohde (2011) find no convincing evidence of social comparisons affecting risk taking while Bault et al. (2008, 2011) do find evidence. Other studies find evidence but draw deviating conclusion from their results. Linde and Sonnemans (2012) find that subjects take less risk when they can win at most as much as a certain payoff of a reference subject ("social loss situation") compared to the case when they can win at least as much as a reference subject ("social gain situation"). In

[29] See Trautmann and Vieider (2012) for an overview of social influence on risk taking.

[30] Additionally, Robson's (1992) analysis is informative about stable equilibrium income distributions (also see Becker et al. 2005).

contrast to Linde and Sonnemans, Schwerter (2013) finds that subjects take more risk when they observe a higher certain payoff of others (to surpass these) compared to observing a lower certain payoff of others (in order to stay ahead). The motivation for both studies is a possible extension of loss aversion à la Kahneman and Tversky (1979) to the social dimension ("social loss aversion"). As a result, Schwerter interprets his results in favor of social loss aversion, whereas Linde and Sonnemans (2012) argue that loss aversion does not easily extend to the case of social comparisons.

In light of the lack of distinct evidence, Chapter 4 investigates social comparisons and decisions under uncertainty and contributes to a better understanding of the matter.

1.4 An experimental method for a direct analysis of social comparisons

Empirical studies on social comparisons rely to a large part on subjective well-being as a direct measure of satisfaction.[31] Traditionally, many economists favor revealed preference approaches. Revealed preference approaches rely on the observed behavior of individuals that allows the researcher to learn indirectly about an individual's preference over different choices (Frey 2008). Social comparisons, however, imply that utility is affected by the choices of others: When others earn more (less) than we do, we are less (more) satisfied. The direct approach of measuring utility as self-reported satisfaction allows economists to measure changes in satisfaction after changes in the income of others. The evidence reported above shows that the "direct approach," based on self-reported satisfaction, turns out to be highly useful for the analysis of social comparisons.[32]

Many studies on social comparisons and subjective well-being rely on survey data from the field.[33] Studying social comparisons in the field is an important approach but gives rise to considerable difficulties. For instance, income runs at risk of being under-declared and can be endogenous to satisfaction. It is difficult to identify the income of a relevant reference group and to confirm to what extent

[31] See Section 1.1.

[32] The use of self-reported life satisfaction, and the "happiness" literature in general, have received considerable methodological critique in economics. A full-fledged discussion is out of the scope of the present thesis. For a recent discussion and overview see Weimann et al. (2015).

[33] Perez-Truglia (2015) provides a validation test for subjective well-being and finds that life satisfaction is a meaningful measure.

the reference group's income is observable to individuals. An analysis of social comparisons and informational effects, that operate through changes in expectations (see Section 1.2 above), complicate an investigation even further. To address these issues, I combine a direct utility measure with the experimental methodology by applying a self-reported measure of satisfaction in the controlled environment of a laboratory experiment. The measure for satisfaction allows a direct analysis of social comparison effects. Precise measures for income, income of a reference group, and control over the information that participants receive allow a clean and causal investigation that is hardly possible outside the laboratory.

Although this methodological approach offers the advantages of a controlled environment and direct utility measurement, one might wonder: Does self-reported satisfaction in the laboratory provide a meaningful measure that captures well-being? Evidence from a sample of 120 participants in a laboratory experiment suggests that "Yes" is the answer to this question. In the experiment I endow participants with income in the form of a "portfolio."The portfolio value follows a stochastic process and the final portfolio value determines a participant's earnings. In regular time intervals I measure changes in an individual's well-being (the self-reported satisfaction with their portfolio).[34,35] To test whether self-reported satisfaction provides meaningful results, I implement an additional, incentivized revealed preference measure. At each point in time when participants report their satisfaction, they additionally face the choice of receiving as their earnings the final value of an alternative portfolio instead of the final value of their currently observed portfolio. Participants are given no information on the optional alternative portfolio but know that this portfolio would be randomly generated by the same stochastic process of their current portfolio.[36] Overall, subjects participate in 10 independent rounds and thus observe 10 times the development of a new randomly generated portfolio over time.

The choice between the current portfolio and an alternative portfolio provides

[34]This analysis builds on a pooled subsample of the participants in treatments "Base" and "Base-C" of Chapter 2. I analyze all 120 participants of both treatments that have the incentivized "choice" to receive the final value of an alternative portfolio. For a more detailed description of the design and experimental procedures see Chapter 2.

[35]Satisfaction is recorded on a scale of 0 (highly dissatisfied) to 10 (highly satisfied). See Chapter 2 for more details.

[36]Regardless of their choice, participants continue to observe their currently assigned portfolio. If participants choose the unknown alternative portfolio at some point in time, they just receive and observe the alternative portfolio before they are paid at the end of the experiment. For payment only decisions at one point in time are randomly selected, and thus, participants can choose the alternative or the current portfolio anew and independently of the previous choices at each point in time.

information on the participant's preference toward the current portfolio. Participants are expected to choose an alternative portfolio less (more) often if satisfaction with respect to their current portfolio is high (low). Indeed, we observe a positive correlation of 0.58 between the participants' satisfaction and the incentivized choice for the currently assigned portfolio.[37] Furthermore, I calculate the average satisfaction for each participant and classify observations for the situation when a participant reports above-personal-average satisfaction ("satisfied") and below-personal-average situations ("dissatisfied"). Figure 1.2 summarizes the result. When participants are "satisfied" I observe in only 10.4 percent of the decisions that participants choose the alternative portfolio. Whereas in case of "dissatisfied" participants, I observe that participants choose the alternative portfolio in 64.4 percent of the decisions.

This evidence shows that the self-reported satisfaction in the experiment provides results that are consistent with an incentivized revealed preference measure. These results suggest that satisfaction is indeed a meaningful measure for the analysis in the present thesis.[38] Furthermore, the direct measurement of satisfaction in laboratory experiments can, if applied in a reasonable framework, provide a constructive methodological extension that should be regarded as complementary to the standard tools of experimental economists.[39]

1.5 Outline of the thesis and main contributions

The present thesis investigates social comparisons under uncertainty and implications for risk taking behavior. The first two chapters directly address social comparison and informational effects while the last chapter focuses on behavioral implications. Chapter 2 provides causal evidence of the social comparison and informational effects in a theory-guided experimental investigation. Chapter 3 takes an international perspective of social comparisons under uncertainty and relies on subjective well-being, growth and trade data to provide evidence of social comparison and informational effects when applying a global dataset. Chapter 4 addresses

[37] I define an indicator variable ("CHOICE") that takes a value of 1 when participants choose their currently assigned portfolio and 0 when participants choose the alternative portfolio. The correlation of 0.58 refers to the correlation between "CHOICE" and satisfaction.

[38] A further analysis in Chapter 2 shows that also the incentivized measure for the participants' expected final portfolio value and reported satisfaction produce consistent results.

[39] Indeed, the direct measurement of satisfaction in laboratory experiments opens opportunities for new applications. For example, Herbst (2016) applies self-reported satisfaction in an experimental analysis of the joy of winning in contest experiments.

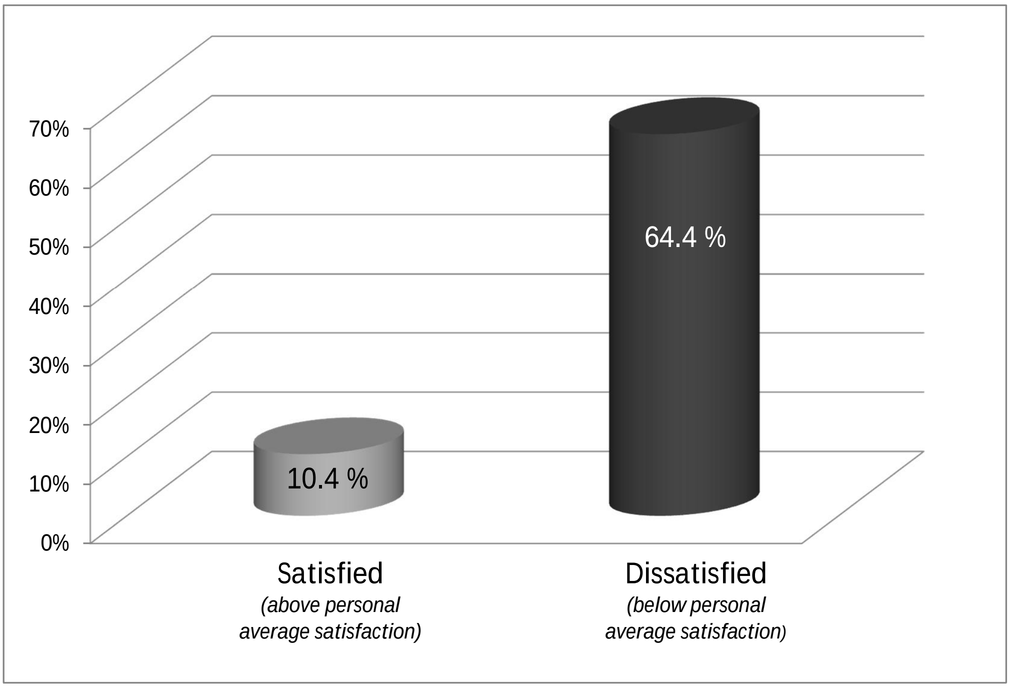

Figure 1.2: Revealed preferences and satisfaction

Note: The percentage of participants that choose a random alternative portfolio over their current portfolio when they report being "satisfied" or "dissatisfied" with their current portfolio. "Satisfied" refers to decisions when participants reported a higher level of satisfaction than the personal overall-average in the experiment. "Dissatisfied" refers to decisions when participants reported a lower level of satisfaction than the personal overall-average in the experiment.

behavioral consequences of social comparisons on risk taking in a theory-guided experiment. In the following I summarize each chapter and its main contribution in more detail.

Hirschman's (1973) seminal contribution on the tunnel effect motivates Chapter 2. The tunnel effect refers to the idea that learning that others earn more may reduce individual well-being due to social comparisons but can also be informative of the own income prospects. In an environment of uncertainty about the own income, this chapter provides experimental evidence on the direct income-comparison effects on well-being and informational effects from observing signals about others' income prospects. The clean and causal evidence on informational and social comparison effects from a controlled laboratory experiment contributes to the empirical literature on the tunnel effect. Previous studies, that rely on survey data, exhibit a higher external validity but face obvious identification problems due to unobserved or possibly defectively measured variables. The more precise and causal evidence from the experimental study in Chapter 2 complements the existing literature. I find evidence both of informational effects on the expectations about the own income and for direct social comparison effects. Both types of effects turn out to be asymmetric. Individual expectations 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 others are likely to earn more but does not change significantly when others are likely to earn less. For the overall effect on satisfaction, informational effects countervail direct social comparison effects if and only if the uncertainty about the own income is sufficiently large. In situations of low uncertainty the social comparison effects of learning about the income prospects of others prevail.

Chapter 3 takes an international perspective on Hirschman's tunnel effect and investigates informational and comparison effects on a global level. Since the surge of globalization, the internet, mass media, and other advances in communication, individuals have become increasingly aware of the economic situation of people in foreign countries. This has made it more likely that individuals compare their own economic situation with that of foreigners as well as that of more local reference groups. At the same time, globalization induces closer economic ties among countries, which could imply spillover effects from the economic performance of one country to another, and thus potential income-prospect effects between countries. I contribute to the existing literature by focusing on residents of foreign countries as a reference group. More specifically, I focus on the relationship between individuals' subjective well-being and the economic performance of reference countries. The underlying idea is that performance measures, such as economic growth, are readily observable and informative about the aggregate development of the economic situation of foreign residents. I find evidence that individuals' subjective well-being depends on the macroeconomic performance of other countries as well as that of their home country. Given the home country's economic growth, an individual's life satisfaction is positively associated with the economic growth of important trade partners. It is negatively associated with the economic growth of neighboring countries that engage in relatively little trade with the home country. Findings in Chapter 3 are consistent with individuals who care about their economic situation relative to that of people in other countries, but who at the same time anticipate positive spillovers from the economic performance of countries that share an important economic tie with their home country.

Chapter 4 investigates individual behavior under uncertainty and social comparisons. Decisions involving risk, similar to most other things that we do, usually take place in a social context. Individuals face decisions on pursuing personal education, choosing an occupation, making savings or financial investment decisions while being aware of the situation of others. To understand risk taking behavior in such situations it is important to understand the implications of social comparisons. In light of the broad scope of application, remarkably few studies consider risk taking and social comparisons. Furthermore, the results that these studies generate are often contradictory. Chapter 4 contributes to the literature by investigating the effect of social comparisons on risk taking in an experimental study, with a focus on the case of income-rank comparisons. At first sight, implications on risk taking when individuals care about their rank in income are not obvious. Chapter 4 derives theoretical predictions showing that the properties of the respective probability distribution of a lottery are crucial factors. The model shows that, compared to standard expected utility theory, income-rank comparisons lead to less (more) risk taking in case of lotteries with more downside (upside) probability mass. The empirical results of the experimental investigation provide evidence of income comparisons affecting risk taking decisions. In line with our theoretical predictions, individuals take significantly less risk in situations of lotteries with more downside probability mass. I do not find a significant effect for lotteries with more upside probability mass. Individuals who face a reference subject of the same gender exhibit a larger comparison effect on risk taking. I interpret this finding as evidence for Festinger's (1954) idea that individuals prefer to compare themselves to more rather than to less similar other individuals.

Chapter 2

A glance into the tunnel: Experimental evidence on income comparisons under uncertainty

2.1 Introduction

When individuals care about relative standing, observing changes in the income of others will affect their utility.[1] At the same time, however, individuals may make inferences about their own future income prospects from observing that others' earnings increase or decrease. If positive experiences of others cause an upward adjustment of the beliefs about the own income prospects, the informational value of observing advances of others can countervail the direct effect on subjective well-being caused by relative-standing concerns and affect the individual tolerance for income inequality. Using data from a controlled laboratory experiment we separate the direct comparison effect from the purely informational effect of learning about others' income and examine their importance for subjective well-being. Overall, our findings suggest that individuals are more reactive to "bad news" than to "good news," both in how they adjust their expectations about the own income prospects and in how subjective well-being is affected. In environments with sufficiently strong uncertainty about the own income prospects, informational effects on the expectations of own future income may offset direct income-comparison effects caused by concerns for relative standing.

The information-driven effect of increases in well-being following advances of

[1] This chapter is based on joint work with Florian Morath, University of Frankfurt. See Lang and Morath (2015).

others 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 earnings of peers increase may even outweigh the negative effect driven by relative-standing concerns, illustrating such a situation with a tunnel anecdote: Suppose your 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 move. Even though still being stuck in your lane, you may 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, altogether, more satisfied than you have been before.

Hirschman (1973) concludes that information-driven effects can be important determinants for attitudes towards inequality and redistribution. When future (lifetime) income is uncertain learning about others' experiences may lead to individuals adjusting their perceptions of income mobility within their society, thereby affecting attitudes towards redistribution. Our experimental results not only provide support in favor of the importance of experiences of peers; it also hints at a potential asymmetry in the process of how individuals update their beliefs about the mobility process.[2] We find that a higher weight is given to signals that indicate the potential of downward mobility. This asymmetry may directly affect reactions of individual well-being to inequality and the demand for redistributive policies; more broadly, individual perceptions of social mobility (rather than actual mobility) may shape general political attitudes and social cohesion.

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. Studies by Senik (2004, 2008) find evidence that personal life satisfaction may react positively to an increase in the income of a reference group. 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 considerations and informational effects from the field is a natural and important starting point, studies

[2]Individual perceptions of social mobility can be influenced by many factors such as past experience, parental background or the social environment and need not necessarily mirror the actual mobility rates; see, for instance, Alesina et al. (2004) on differences in beliefs about social mobility as an explanation for differences in views on inequality between the United States and Europe.

based on field data generally 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, measures of individuals' expectations about future income prospects are usually crude in survey data and income can be endogenous to satisfaction.[3] Furthermore, it is difficult to identify the income of a relevant reference group and to confirm to what extent (or whether at all) the reference group's income is observable.[4] Many problems in the field can be addressed in the laboratory. The controlled environment allows us to observe the income of participants and of a clearly defined reference group. We can directly measure individual satisfaction levels and the beliefs about their income prospects, controlling for the information received about the income-generating process. This more detailed and causal identification enables us to directly analyze adjustments in beliefs as a consequence of additional information, rather than focusing on changes in satisfaction that are supposed to be caused by changes in beliefs. Thus, we can separate the income-comparison and belief-based effects resulting in Hirschman's (1973) "tunnel effect."

In the experiment we endow participants with income in form of a "portfolio." The portfolio value follows a stochastic process and the final portfolio value determines a subject's income. Hence, subjects are ex ante uncertain about their income and about the income of others but receive additional information about the final portfolio value (their income) in the course of the experiment. In regular time intervals we measure changes in the subject's beliefs about their final income and in individual well-being (the self-reported satisfaction with their portfolio). To isolate purely belief-based effects of receiving additional signals about the underlying income-generating process ("information effects") we compare beliefs of a control group that only observes their own portfolio to a treatment group (treatment "P2-INFO") that observes the exact same own portfolio but, in addition, another portfolio which may have informational value for the own income but is not assigned to any other participant of the experiment. To measure direct "income-comparison effects" we use observations from this P2-INFO treatment as control group and compare the self-reported satisfaction levels to another treatment group (treatment "P2-INCOME") in which subjects are matched in groups of two and observe each other's income-generating process. Thus, holding constant

[3]For instance, satisfied people might be extraverted and possibly more successful in their job.

[4]Some of the problems are addressed in one or another way in the studies cited above. Nevertheless, it remains generally true that a completely clean identification is inaccessible in the field.

the information that subjects may use to infer about their own income prospects (i.e., portfolio values) we provide precise information about another subject's likely income and estimate its effect on self-reported satisfaction. The main experimental treatments keep the informativeness of additional signals uncertain by not providing precise information about the income-generating process; instead, subjects are shown a distribution of possible income realizations. In additional control treatments we vary the subjects' priors by keeping them completely uncertain about the distribution of final incomes.

We find evidence both for "information effects" on the beliefs about the own income and for direct "income-comparison effects." Both types of effects turn out to be asymmetric. On the one hand, expectations about the own income only react significantly when participants observe additional portfolios with lower values, in which case subjects lower their beliefs. On the other hand, relative-standing concerns most strongly affect satisfaction in situations where individuals observe that others are likely to earn more, in which case subjects report lower satisfaction levels. Belief-based effects and income-comparison effects offset each other in how they affect well-being when the uncertainty about individual incomes is substantial and, hence, information-driven effects are important; in situations of low uncertainty the income-comparison effects of learning about the income prospects of others prevail.

The discussion on relative-income comparisons dates back to Veblen (1899) and Duesenberry (1949) and there is a vast literature on the importance of relative-income considerations for economic outcomes.[5] More specifically, a substantial amount of evidence documents a negative relationship between subjective well-being and the income of a defined reference group (see, e.g., Van de Stadt et al. 1985; Clark and Oswald 1996; McBride 2001; Ferrer-i-Carbonell 2005; Luttmer 2005; Senik 2009; Clark and Senik 2010). Ferrer-i-Carbonell and Ramos (2014) survey the literature on the relation between inequality and subjective well-being. Consistent with the ideas of Duesenberry (1949), studies by 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. We contribute to this empirical literature in two respects. First, we focus on income-comparison considerations under uncertainty, controlling for informational effects that become important in an uncertain environment. Second, we provide

[5]For early contributions see, for instance, Leibenstein (1950), Easterlin (1974, 1995), Boskin and Sheshinski (1978), Frank (1984, 1985a, 1985b), Konrad (1992), and Konrad and Lommerud (1993). Clark et al. (2008) review the literature on income comparisons and well-being.

experimental evidence in a novel and, as we believe, particularly simple setting, in which we show that seemingly minor institutional changes (individuals learn about the income prospects of another participant, instead of only observing a second portfolio which is not payoff-relevant for any other participant) in an otherwise exactly similar situation induces significant income-comparison effects.

Under uncertain future and, hence, lifetime earnings income comparisons involve directly the perception of social mobility. Bénabou and Ok (2001) rationalize and provide conditions for the "prospect of upward mobility" (POUM) hypothesis that a majority of individuals may expect to become richer than average in the future.[6] Their work on the POUM hypothesis, explaining the lack of support for high levels of redistributive taxation, assumes that individuals know the income-generating mobility process. Our experiment investigates expectations of future income in an environment where the income-generating process and, hence, the informativeness of learning about others' income prospects for the own future income is uncertain. We believe this is particular interesting because outside the laboratory people might observe income signals about the income of others; however, the underlying correlation between future incomes is in most cases uncertain. In this respect, our paper also relates to Piketty (1995) who takes into account that individuals may exhibit heterogeneous beliefs about upward mobility and focuses on learning about the relative importance of individual effort as compared to parental background.[7] Our results for the asymmetry of how subjects take into account additional information may be interpreted as subjects being mostly concerned about downward mobility.

Finally, our paper relates to the literature on expectations formation (e.g. Schmalensee 1976; Dwyer et al. 1993; Hey 1994; Hommes 2011; Rötheli 2011; Beshears et al. 2013). However, we are not primarily interested in the expectations individuals form about a time series (in our setting, their income prospects).

[6]This and further explanations for why in democracies the low-income majority does not implement high levels of redistribution are discussed by Putterman (1997); see also Fong (2001) on beliefs about distributive justice and Luttmer and Singhal (2011) on the role of cultural background. For empirical studies on the relation between perceptions of social mobility and preferences for redistribution see Ravallion and Lokshin (2000), Corneo and Grüner (2002), Alesina and La Ferrara (2005), Guillaud (2013), and Cojocaru (2014). Checchi and Filippin (2004), Krawczyk (2010), Konrad and Morath (2013) and Durante et al. (2014) experimentally investigate preferences for redistributive taxation under different income mobility regimes.

[7]Our setting takes individual incomes as fully exogenous and predetermined and abstracts from questions of the sources of inequality, which have been extensively discussed in the literature on redistributive preferences. For seminal contributions on the role of beliefs about the sources of inequality for redistributive outcomes see Alesina and Angeletos (2005) and Bénabou and Tirole (2006).

Our experiment focuses on how subjects adjust their expectations when they observe another individual's income prospects. We deliberately refrain from inducing the individuals to believe in a particular correlation structure but investigate how individual beliefs react to signals about a second mobility process, in situations where the underlying income-generating process is unknown.

2.2 Theoretical framework

2.2.1 Information and income-comparison concerns

Consider a model with two individuals. 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 income and about the 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 $E_i(y_k)$ individual i's expectation 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 i's beliefs about y_k to be strictly increasing in the signal s_k, that is,

$$\frac{\partial E_i[y_k|(s_1, s_2)]}{\partial s_k} > 0, \; k = 1, 2. \tag{2.1}$$

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 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 and the other individual's income:

$$\frac{\partial E_i[u_i(y_i, y_j)|(s_i, s_j)]}{\partial s_j} = \frac{\partial E_i[y_i|(s_i, s_j)]}{\partial s_j} - \lambda_i \frac{\partial E_i[y_j|(s_i, s_j)]}{\partial s_j}. \tag{2.2}$$

The second term of the derivative in (2.2) is negative if $\lambda_i > 0$ and (2.1) 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 an "income-comparison effect." The first term in (2.2) 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 of the derivative in (2.2) may be positive, that is,

$$\frac{\partial E_i\left[y_i|\left(s_i,s_j\right)\right]}{\partial s_j} > 0. \tag{2.3}$$

In this case, there is an "information effect" on own expected income 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 (2.3) holds, the total effect in (2.2) can be positive or negative, depending on whether the "information effect" or the "income-comparison effect" dominates. The experimental treatments described next isolate the two effects and test them separately.

2.2.2 Experimental treatments

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

Portfolios are generated by a random walk with drift, with $y_i(0) = 300$ and

$$y_i(t) = y_i(t-1) + \alpha_i + \beta\varepsilon_i(t). \tag{2.4}$$

The final period is $T = 100$ and the drift parameter α_i is randomly drawn (with equal probabilities) from the set $\{-1.5, 0, 1.5\}$ in order to obtain different types of portfolios (low-value, medium-value, and high-value portfolios).[8] 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; for a screenshot see Figure 2.5 in the 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 (2.4) (compare Appendix 2.C). This ensures that subjects have a comparable prior about the income-generating process and about the probability distribution of final incomes, and it reduces the within-treatment variation, without imposing too much structure or exploiting differences in computational skills.

The participants' task is to repeatedly answer questions on their *beliefs* about the final portfolio value $y_i(T)$ and on their *satisfaction* with the assigned portfolio.[9] The first main task is to give an estimate of the final value $y_i(T)$ of the income-generating process; this task is incentivized. The second 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.[10]

As a plausibility check for the self-reported satisfaction we also include a control question in which subjects have the choice to receive as their earnings the final value of an alternative portfolio to be randomly generated by the same process. Subjects should be more likely to choose this option if they are less satisfied with their current portfolio; we can test whether their choice is correlated with the self-reported satisfaction.[11]

[8] The shocks $\varepsilon_i(t)$ are independent draws from a standard normal distribution, and the parameter β is a constant to scale the shock $\varepsilon_i(t)$ (we set $\beta = 10$).

[9] For the exact description of the task see the experimental instructions in Appendix 2.C.

[10] Although this might be a bit imprecise we use the terms satisfaction, subjective well-being and utility interchangeably. For our experiment we rely on the general conclusion in the literature that self-reported satisfaction or subjective well-being is a meaningful measure (for a recent survey see Weimann et al. 2015). For a discussion on action-revealed preferences and satisfaction judgments see Frey and Stutzer (2002).

[11] We include this control question in two variants: In about one half of the sessions of each treatment, if the option to have the individual earnings determined by another randomly generated portfolio is chosen, the subject is assigned and shown the new portfolio at the end of the experiment. In the other half of the sessions, subjects are only asked "hypothetically" whether they would prefer to be assigned another portfolio. In both cases, subjects answer all questions on beliefs and satisfaction with respect to the originally assigned portfolio P_i (even if they prefer the value of another portfolio as their final earnings). We use these two variants to control for possible interference of the control question (the possibility to receive the final value of another portfolio) with the self-reported measure of satisfaction. Note already that these two different types of sessions are very similar in terms of results obtained.

Treatment BASE In the baseline treatment, each subject observes only the value $y_i(t)$ of the own portfolio P_i at points in time $t = 0, 1, ..., T$. 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 $y_i(t)$ of the own portfolio P_i and, in addition, the value $y_j(t)$ of a second portfolio P_j at points in time $t = 0, 1, ..., T$. 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 the effect of *additional information* $(y_j(t))$ 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 $y_i(t)$ and $y_j(t)$ at points in time $t = 0, 1, ..., T$ (but not the other participant's choices). Using treatment P2-INFO as a counterfactual, the P2-INCOME treatment isolates the effect of observing the income prospects of others on own satisfaction ("income-comparison effect"). Since we use the same sets of portfolios across treatments (for more details see below), the comparison of P2-INCOME to P2-INFO controls for any informational effect that observing portfolio j may have on i's beliefs about the *own income* (and, hence, on satisfaction with the own portfolio). In other words, we separate the "income-comparison effect" from the "information effect" derived in Section 2.2.1.[12]

[12]By making others' income prospects more salient the "income-comparison effect" is also based on additional information. We refer to "information effect" in the context of effects on beliefs about the *own* income; the "income-comparison effect" relates to the channel which works through specific information about another participant's expected income and, hence, potential income inequality.

2.2.3 Experimental procedures

Each of the three treatments Base, P2-Info, and P2-Income consists of ten structurally identical but independent rounds indexed by $r \in \{1, ..., 10\}$. Hence, participant i observes a sequence of ten own portfolios; in the treatments P2-Info and P2-Income i observes also ten additional portfolios in total. In the P2-Income treatment, the participants are randomly matched in groups of two in each of the ten rounds.[13]

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 rounds $r = 1, ..., 10$.[14] Therefore, the treatment comparisons control for portfolio history effects, that is, for information about portfolio values in previous rounds and in a given round (up to t).

In each round r, participant i answers the questions on satisfaction and beliefs about $y_{i,r}(T)$ at points in time $t \in \{T/5, 2T/5, 3T/5, 4T/5\}$ where at later points t individuals have observed more signals and uncertainty about $y_{i,r}(T)$ is reduced. At each point in time t, the subjects can give their answers on beliefs and satisfaction independently of their previous answers. At the end of the experiment the computer randomly selects one round $\hat{r}$ out of the ten rounds; then the computer randomly selects one point in time $\hat{t}$ of this round at which the questions have been answered. The participants' choices at this selected point in time $\hat{t}$ determine their earnings in the experiment as follows: First, subjects receive a payment for their estimate $\tilde{y}_{i,\hat{r}}(\hat{t})$ of their final portfolio value in round $\hat{r}$; this payment increases in the precision of the estimate.[15] Second, each subject receives the final value $y_{i,\hat{r}}(T)$ (in experimental currency) of the portfolio assigned in the selected round.[16] The

[13]The participants do not interact or observe other participants' decisions. We implement random re-matching to assure that income comparison refers to the current round and to avoid that subjects take into account information about the assigned co-player's earnings in previous rounds.

[14]We randomly selected 20 portfolios to be used in all treatments (see Appendix 2.B.4), which are assigned such that subsamples of participants in each treatment observe the exact same ten "own" portfolios over the ten rounds. Moreover, in P2-Info and P2-Income all participants of a subsample observe the exact same ten additional portfolios. We generated six random sequences in which these portfolios are shown to the subjects; subjects are then randomly assigned to one of these sequences. When selecting the 20 portfolios we made sure that each possible combination of the drift parameters (α_i, α_j) occurs at least once (recall that $\alpha_k \in \{-1.5, 0, 1.5\}$) to ensure some variation in terms of the observed portfolio pairs; otherwise, the portfolio selection was completely random.

[15]The payoff (in experimental currency) for an estimate $\tilde{y}_{i,\hat{r}}(\hat{t})$ is $\max\{250 - 0.1\left(y_{i,\hat{r}}(T) - \tilde{y}_{i,\hat{r}}(\hat{t})\right)^2, 25\}$.

[16]In sessions with the control question offering the choice to receive as a payment the final

payment received in experimental currency units (ECU) is converted to Euros at a rate of 25 ECU = 1 Euro. 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. Each treatment consisted of four sessions with 24 subjects each; the participants were students from all different fields of study and were recruited using the software ORSEE (Greiner 2004); for an overview of the treatments and summary statistics see Tables 2.4 and 2.5 in the appendix. After having completed the main experiment, subjects answered a set of post-experimental questions on individual characteristics and attitudes. At this point, we conducted a set of incentivized post-experimental tasks, including a question on risk aversion (Dohmen et al. 2011) 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 on top of the earnings from the main experiment. On average subjects earned 29 Euros in total and a session lasted for approximately 90 minutes.

2.2.4 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 concerns for relative standing.

The first prediction focuses on the effect of additional information (a second observed portfolio) on individuals' beliefs about the own income. Individual portfolios are drawn independently; thus, if subjects knew the exact income-generating process, individual beliefs about the own final income should be independent of any additional information about other portfolios and, hence, not be different in the treatments Base and P2-Info. In the experiment, even though subjects do not

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 $\hat{t}$. Recall that even if a subject opts for a new portfolio at some point in time, he nevertheless observes the initially assigned portfolio of the current round until T and answers all questions on this initially assigned portfolio. Just at the end of the experiment a subject will get to see the alternative portfolio in case he chose an alternative portfolio at the randomly selected point in time $\hat{t}$.

learn the exact income-generating process, they are shown a "probability distribution" of possible portfolio values (see the graph in the instructions in Appendix 2.C). This approach closely maps a situation in which individuals hold a common prior about the income-generating process. However, even though it is common knowledge that the portfolios are independently and randomly assigned, subjects may still perceive the additional information in P2-INFO as informative and adapt their beliefs according to the additional signals received. If the individuals expect some common (but unknown) trend in the income-generating processes observed, this yields the following testable prediction which is in line with Hirschman (1973).

Prediction 2.1 ("Information effect") *(i) In the* P2-INFO *treatment, observing an additional portfolio* P_j *with value* $y_j(t) < y_i(t)$ *lowers individual* i*'s beliefs about* $y_i(T)$*, compared to the control group in the* BASE *treatment.*
(ii) In the P2-INFO *treatment, observing an additional portfolio* P_j *with value* $y_j(t) > y_i(t)$ *increases individual* i*'s beliefs about* $y_i(T)$*, compared to the control group in the* BASE *treatment.*

By comparing the individuals' beliefs about the final portfolio value in P2-INFO and in BASE we test Prediction 2.1 against the alternative hypothesis that individuals interpret the additional information on a second portfolio as uninformative for their own final income. Taking the own current portfolio value as a benchmark we analyze the cases of $y_{j,r}(t) < y_{i,r}(t)$ and $y_{j,r}(t) > y_{i,r}(t)$ separately to allow for an asymmetric effect of observing a second portfolio with higher and with lower value, respectively. Since a subset of individuals across treatments observe the same portfolios, 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 constant the information that subjects receive about the own income, observing signals about another individual's income prospects may have a direct effect on own satisfaction whenever individuals care about their relative income.

Prediction 2.2 ("Income-comparison effect") *(i) In the* P2-INCOME *treatment, observing information about individual* j*'s income lowers individual* i*'s satisfaction whenever* $y_j(t) > y_i(t)$*, compared to the control group in the* P2-INFO *treatment.*
(ii) In the P2-INCOME *treatment, observing information about individual* j*'s income increases individual* i*'s satisfaction whenever* $y_j(t) < y_i(t)$*, compared to the control group in the* P2-INFO *treatment.*

Controlling for the "information effect" on beliefs about the own income, average satisfaction should be lower when individuals observe that another participant has a relatively high current portfolio value and is, hence, likely to have a higher income (Prediction 2.2(i)); average satisfaction should be higher when observing that others are worse off (Prediction 2.2(ii)). If, instead, individuals do not care about income comparison then average satisfaction in P2-Income and in P2-Info should be the same (both for $y_j(t) > y_i(t)$ and for $y_j(t) < y_i(t)$) since the information received about the own income is identical in both treatments. Again, we will test whether there is an asymmetric effect on own satisfaction when observing higher and lower income of others, respectively.

To summarize, a comparison of P2-Info and Base identifies the purely informational value that observing additional signals about the income-generating process may have for the expectations about the own income (i.e., the term $\partial E_i[y_i|(s_i, s_j)]/\partial s_j$ in (2.2)), in situations in which status concerns do not directly take effect. 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 (2.2)), controlling for the effect on $E_i[y_i|(s_i, s_j)]$. By construction, the direct effect on satisfaction is zero in the P2-Info treatment where the additional portfolio observed is not payoff-relevant for any other participant. However, even in the P2-Info treatment individuals may draw conclusions on the income of others when observing an additional portfolio, for instance, because they believe that the second portfolio is generally informative regarding the portfolios that other participants may be assigned to. In this case, satisfaction might already be affected by additional information in P2-Info; therefore, the comparison of P2-Income and P2-Info may underestimate the "income-comparison effect" of observing to be ahead or behind in terms of expected income relative to the assigned co-player.

2.3 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 expectations about their own income prospects. 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. Combining these effects shows that information-based effects and direct income-comparison effects may offset each other when the uncertainty about the income is large; their joint effect on satisfaction is statistically indistinguishable from zero in early points in time within a round. But as the uncertainty is reduced, income-comparison effects dominate the value of information about the others' experiences for the own income prospects such that, in total, satisfaction goes down when observing that others most likely earn more.

Before we derive these results in more detail it is important to note that the self-reported measures for beliefs and satisfaction are sensitive to changes in the information observed and react as predicted to the parameters of the experiment. For instance, stated beliefs and satisfaction levels shift upwards under higher (though unknown) trends of the income-generating process (compare the histograms in Figure 2.4 of Appendix 2.A; the resulting cumulative distribution functions can be ranked in terms of first-order stochastic dominance). Similarly, stated beliefs and satisfaction are significantly positively correlated (the correlation coefficient is 0.71). The same is true (i) for stated beliefs and the current or the final (not yet known) portfolio value (correlation coefficients are 0.88 and 0.70, respectively) and (ii) for stated satisfaction and the current or the final portfolio value (correlation coefficients are 0.78 and 0.65, respectively). The correlation of stated beliefs and the final portfolio value becomes stronger as the points in time t, in which the portfolio is observed, approach the end point T of a round (the correlation coefficient increases from 0.43 to 0.93): As to be expected, the beliefs become more accurate when the uncertainty decreases.[17] Finally, we can use as a plausibility check the incentivized control question on the option to receive as income the final value of a new, randomly drawn portfolio. Here, subjects are more likely to prefer the final value of their current portfolio as their income if (i) their beliefs are higher (the correlation coefficient of this choice and reported beliefs is 0.63) and (ii) their reported satisfaction is higher (the correlation coefficient of this choice

[17]For the subsequent analysis we exclude 4 (out of 288) subjects which either always stated "implausible" beliefs below 10 (presumably used a wrong scale given the fact that final portfolio values were between 81 and 585) or always reported the exact same number for their satisfaction. While for the latter subjects it is conceptually less clear whether or not these subjects should be excluded, our results are robust to including them. Since we did not want to bias the subjects' priors by showing them specific portfolios, we could not implement pre-tests before the main experiment. In general, however, the subjects' choices together with their answers to the post-experimental questions indicate that the vast majority of subjects understood the experimental tasks.

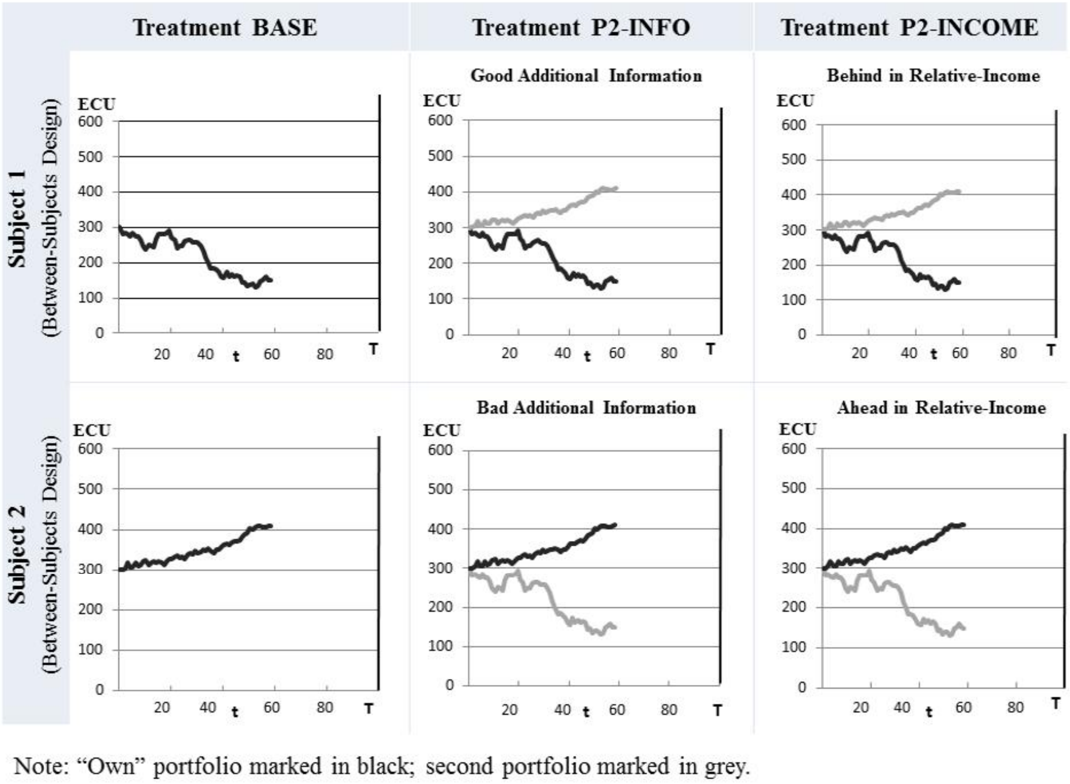

Figure 2.1: Identification strategy

and satisfaction is 0.62).[18]

2.3.1 Information effects

First we are interested in the effect of information about another income-generating process on the beliefs about the own end-of-period portfolio value (Prediction 2.1). To assess the effect of observing additional signals in the form of an additional portfolio it is crucial to perfectly control for all the information about the own portfolio. We compare the beliefs in the P2-Info treatment to the beliefs in the Base treatment in which reference groups of subjects observe the exact same own portfolios as in P2-Info but no additional portfolio within a round. Moreover, we separate the "information effect" for situations in which subjects observe (i) "good additional information" (the second portfolio has a higher current value, that is,

[18]More precisely, for satisfaction, the correlation coefficient is 0.60 if the choice to be assigned a new portfolio at the end of the experiment is binding and is 0.64 if the choice of a new portfolio is only "hypothetical" and not actually implemented (and thus has no payoff consequence). Recall that each of these variants of the control question was used in about half of the sessions.

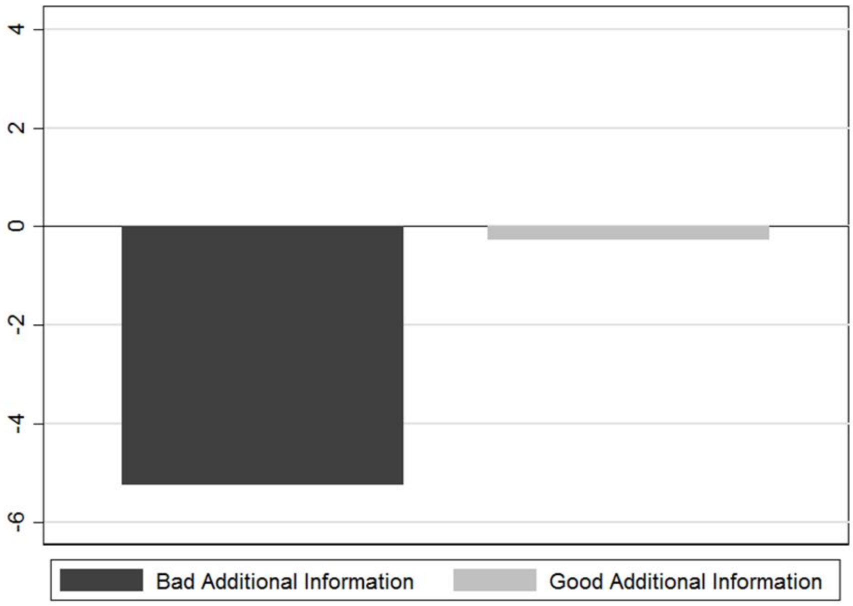

Figure 2.2: Change in average beliefs (in experimental currency) from BASE to P2-INFO.

$y_j(t) > y_i(t)$) and (ii) "bad additional information" (the second portfolio has a lower current value, that is, $y_j(t) < y_i(t)$). Figure 2.1 illustrates our identification strategy of comparing beliefs in P2-INFO (middle column) to those in BASE (left column), for a given own portfolio.

We start with a simple comparison of average stated beliefs in treatments BASE and P2-INFO; see also Table 2.5 in Appendix 2.A for descriptive statistics. Splitting the observations into situations of good and bad additional information,[19] Figure 2.2 suggests partial evidence for Prediction 2.1: 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 in the reaction to additional information.

To test Prediction 2.1 on the effect of additional information we estimate a crossed-effects linear regression model on the sample of the observations from BASE and P2-INFO.[20] Using as dependent variable subject i's beliefs $belief_{i,r}(t)$ about

[19]Observations in BASE are split accordingly (even though the second portfolio is not observed) such that the treatment group in P2-INFO and the control group in BASE observe the exact same *own* current portfolio values (income prospects), both under good and under bad additional information.

[20]The crossed-effects model allows us to specify random effects on the subject level and addi-

the own end-of-period portfolio value as reported at point in time t of round r, our main specification is given by

$$belief_{i,r}(t) = \beta_0 + \beta_1 y_{i,r}(t) + \beta_2 P2\text{-}INFO + \beta_3 I_{y_{j,r}(t)>y_{i,r}(t)} + \beta_4 I_{y_{j,r}(t)>y_{i,r}(t)} \times P2\text{-}INFO + \gamma X_{i,r}(t) + \varepsilon_{i,r}(t). \tag{2.5}$$

The main variables of interest are the treatment variable $P2\text{-}INFO$ (which is equal to one for observations from the P2-Info treatment and zero otherwise) and the indicator variable $I_{y_{j,r}(t)>y_{i,r}(t)}$ which is equal to one in situations of good additional information (if the second portfolio j has a higher current value than subject i's portfolio) and equal to zero otherwise.[21] Moreover, we interact the dummy $P2\text{-}INFO$ with the indicator $I_{y_{j,r}(t)>y_{i,r}(t)}$, and we include the observed own current portfolio value $y_{i,r}(t)$ as explanatory variable as well as a vector $X_{i,r}(t)$ of additional control variables.[22] Thus, in equation (2.5), β_2 measures the effect of bad information (the treatment effect if $I_{y_{j,r}(t)>y_{i,r}(t)} = 0$) and $\beta_2 + \beta_4$ measures the effect of good information (the treatment effect if $I_{y_{j,r}(t)>y_{i,r}(t)} = 1$). The main estimation results are summarized in Table 2.1.

In specification 1 of Table 2.1, the estimated coefficient of $P2\text{-}INFO$ is -11.74 and significant at the 5%-level (p-value < 0.041). Hence, observing a second portfolio with a lower value significantly lowers the subjects' beliefs in the P2-Info treatment, compared to the reference group (with identical own portfolios) in Base. Second, the sum of the coefficients of $I_{y_{j,r}(t)>y_{i,r}(t)} \times P2\text{-}INFO$ and $P2\text{-}INFO$ is negative but statistically indistinguishable from zero (p-value > 0.199); observing a second portfolio with a higher value does not yield a statistically measurable effect on stated beliefs.[23] Finally, the current value of the own portfolio

tional random effects on the portfolio level. The random effects on the subject level account for time-constant subject-specific effects. Random effects on the portfolio level allow us to reduce potential portfolio noise in the error term. Note that 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.

[21] The case of the exact same current portfolio values ($y_{j,r}(t) = y_{i,r}(t)$, $t > 0$) never occurs in the data.

[22] We include fixed effects for the round r 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. Moreover, some specifications further include controls such as gender, age and a dummy for business-related fields of study 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).

[23] Note that the negative coefficient of $I_{y_{j,r}(t)>y_{i,r}(t)}$ results from the fact that the comparison group in the Base treatment has a relatively low own current portfolio value whenever $y_{j,r}(t) >$

	(1) belief	(2) belief	(3) belief	(4)[a] belief	(5)[b] belief
$y_{i,r}(t)$	0.848*** (0.013)	0.848*** (0.013)	0.814*** (0.017)	0.900*** (0.026)	0.867*** (0.024)
P2-INFO	-11.74** (5.740)	-10.55* (5.972)	-8.336 (5.885)	-1.418 (7.041)	-9.426 (7.318)
$I_{y_{j,r}(t)>y_{i,r}(t)}$	-6.762*** (2.114)	-6.764*** (2.114)			
$I_{y_{j,r}(t)>y_{i,r}(t)}$×P2-INFO	4.364** (2.063)	4.364** (2.063)			
$\Delta_{y_{j,r}(t)-y_{i,r}(t)}$			-12.99*** (2.928)	-7.45 (5.712)	-19.59*** (5.541)
$\Delta_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INFO			4.021** (1.947)	-4.954 (5.017)	8.613* (4.731)
Constant	82.13*** (7.515)	82.76*** (7.571)	89.66*** (8.015)	51.19*** (11.21)	86.02*** (10.32)
Individual controls	No	Yes	Yes	Yes	Yes
Time and session fixed effects	Yes	Yes	Yes	Yes	Yes
N	7600	7600	7600	3800	3800

[a]Subsample of good additional information ($\Delta_{y_{j,r}(t)-y_{i,r}(t)} > 0$).
[b]Subsample of bad additional information ($\Delta_{y_{j,r}(t)-y_{i,r}(t)} < 0$).

Note: Crossed-effects regression model with random effects on subject and portfolio level. Observations from treatments BASE and P2-INFO. Dependent variable: beliefs. 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 round fixed effects, point-in-time fixed effects, fixed effects for the sequence in which the selected portfolios are shown, and session fixed effects.

Table 2.1: Information effects: Regression results.

$(y_{i,r}(t))$ has strong explanatory power with a positive coefficient that is close to one, which also confirms the validity of the measure of beliefs. Even though the estimated coefficient of $P2\text{-}INFO$ and the corresponding significance level decrease slightly (p-value < 0.077), these findings are confirmed in specification 2 which adds individual-specific control variables elicited after the main part of the experiment.

As a natural extension beyond the binary case of good or bad additional information, specifications 3 to 5 include as explanatory variable the difference between the current value of the second portfolio and the own current portfolio value $(y_{j,r}(t) - y_{i,r}(t))$. Hence, positive (negative) values of this difference indicate good (bad) additional information and higher values indicate better additional signals. We normalize this difference in order to separate effects of additional information from time trends within a round (since all portfolios start with the same value, the range of $y_{j,r}(t) - y_{i,r}(t)$ is usually increasing in t; at the same time, uncertainty is reduced) and define the normalized difference by $\Delta_{y_{j,r}(t)-y_{i,r}(t)}$.[24] Now, the treatment effect of observing an additional portfolio is captured by the coefficients of $P2\text{-}INFO$ and the interaction term $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INFO$. In specification 3, the estimated coefficient of $P2\text{-}INFO$ is -8.336 and indicates that beliefs are, for average portfolios, slightly lower in P2-Info than in Base. Moreover, higher values of the second portfolio compared to the own current portfolio value have a significantly positive effect on beliefs in P2-Info, again compared to the reference group in Base (the estimated coefficient of the interaction term $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INFO$ is positive and significant at the 5%-level).[25] Separating the sample into subsamples of good additional information (where $\Delta_{y_{j,r}(t)-y_{i,r}(t)}$ is positive) and bad additional information (where $\Delta_{y_{j,r}(t)-y_{i,r}(t)}$ is negative) shows that the observed effect of additional information (in specification 3) is driven by bad information and is, again, asymmetric: There is no statistically measurable treatment effect in case of good additional information (specification 4) but a significant treatment

$y_{i,r}(t)$. 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 2.1). The significantly positive coefficient of the interaction term $I_{y_{j,r}(t)>y_{i,r}(t)} \times P2\text{-}INFO$ confirms a treatment difference of Base and P2-Info with respect to comparisons of situations where the second portfolio would be relatively low and high, respectively.

[24] More precisely, we divide $y_{j,r}(t) - y_{i,r}(t)$ by the maximum value of $|y_{j,r}(t) - y_{i,r}(t)|$ over all portfolio combinations (i,j) at point in time t; thus, $\Delta_{y_{j,r}(t)-y_{i,r}(t)}$ takes values between -1 and 1 at each point in time t. Alternatively, the normalization could use the median or mean of the absolute distance over all portfolios, which yields qualitatively very similar results.

[25] An F-test shows that coefficients of $P2\text{-}INFO$ and $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INFO$ are jointly significant at the 5% level (p-value is 0.04).

effect in case of bad additional information (specification 5).[26]

While specifications 3 to 5 assume a linear effect of the difference $\Delta_{y_{j,r}(t)-y_{i,r}(t)}$, we can alternatively extend the interaction model of specifications 1 and 2 of Table 2.1 to disaggregate the effect of observing a second portfolio on beliefs. If we use dummy variables to separate cases of "very good," "rather good," "rather bad," and "very bad" information based on quartiles of the difference $y_{j,r}(t) - y_{i,r}(t)$, we find that lower additional signals lead to lower beliefs (see Tables 2.8 and 2.9 in Appendix 2.B for the estimation results). This effect appears to be monotonic going from very good to very bad additional information and is strongest when observing very bad additional information.

Result 2.1 *Additional signals of uncertain informativeness affect the beliefs about the own income prospects. Bad additional information (signals $y_j(t) < y_i(t)$) leads to a downward adjustment of beliefs while good additional information (signals $y_j(t) > y_i(t)$) has no statistically significant effect on beliefs.*

Generally we find that subjects react to additional information even when they "know" the probability distribution of their own income and when the informativeness of additional information is uncertain. This uncertain informativeness of additional information is an important feature of our experiment, as we do not "frame" subjects into one or the other direction by inducing them to believe in some particular correlation structure. Nevertheless, we find an effect of additional information but only in specific situations: Subjects lower their beliefs about their own income prospects after observing additional portfolios with relatively low values. But when observing additional portfolios with relatively high values subjects do not adjust their beliefs in a statistically measurable way. In light of the detailed information provided on the distribution of possible portfolios (compare the graph in the experimental instructions in Appendix 2.C) and the uncertain informativeness of the additional signals the results appear to be even stronger. Responses are likely to be more pronounced when subjects know the correlations between future incomes with certainty.

2.3.2 Income-comparison effects

In this section we analyze how satisfaction is affected when subjects observe signals about another subject's income prospects (Prediction 2.2). By comparing the

[26]This holds for both treatment variables of interest as well as their joint effect; the p-value of an F test on the joint significance of the estimated coefficients of $P2$-$INFO$ and $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2$-$INFO$ is 0.53 in specification 4 and 0.03 in specification 5.

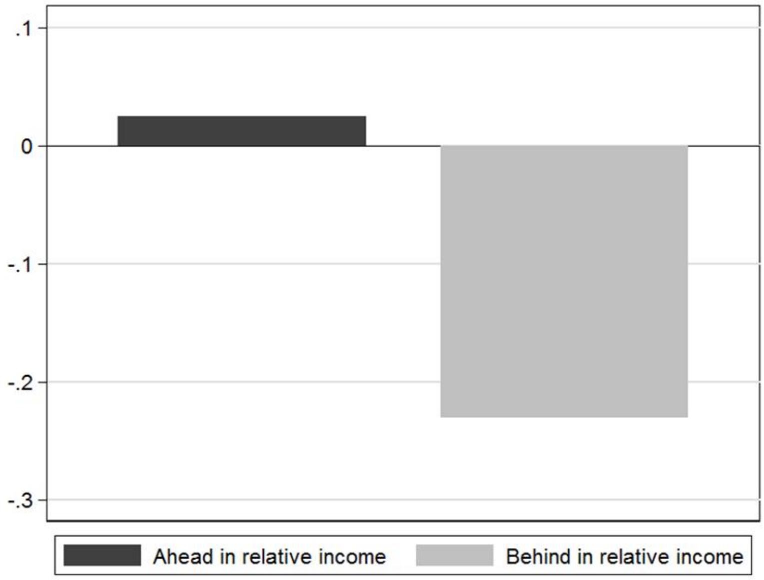

Figure 2.3: Change in satisfaction from treatment P2-Info to P2-Income.

P2-Income treatment to the P2-Info treatment, we can perfectly control for all signals that could be directly informative for the *own* income.[27] Figure 2.1 illustrates our identification strategy using the treatments P2-Info (middle column) and P2-Income (right column), which now distinguishes between situations in which subjects are behind in terms of relative income ($y_{j,r}(t) > y_{i,r}(t)$) and situations in which subjects are ahead in terms of relative income ($y_{j,r}(t) < y_{i,r}(t)$), that is, between unfavorable and favorable income comparisons.

For a first overview of the data, consider the change in simple means of reported satisfaction when comparing the P2-Income treatment to the reference observations in the P2-Info treatment.[28] As Figure 2.3 indicates, we find partial evidence for Prediction 2.2: When subjects are behind in the sense that they have a lower current portfolio value ($y_{j,r}(t) > y_{i,r}(t)$), their satisfaction is lower than in the comparison group of P2-Info, while being ahead ($y_{j,r}(t) < y_{i,r}(t)$) has no evident effect on average satisfaction.

To further investigate this result we estimate a crossed-effects linear regression model similar to Section 2.3.1, on the sample of the observations from the

[27]Recall 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) in P2-Info where it is not payoff-relevant for any other subject.

[28]See also Table 2.5 in Appendix 2.A for descriptive statistics.

treatments P2-Info and P2-Income:[29,30]

$$satisfaction_{i,r}(t) = \beta_o + \beta_1 belief_{i,r}(t) + \beta_2 y_{i,r}(t) + \beta_3 P2\text{-}INCOME + \beta_4 I_{y_{j,r}(t)>y_{i,r}(t)} + \beta_5 I_{y_{j,r}(t)>y_{i,r}(t)} \times P2\text{-}INCOME + \beta X_{i,r} + \varepsilon_{i,r}(t) \quad (2.6)$$

The dependent variable $satisfaction_{i,r}(t)$ represents subject i's reported satisfaction at point in time t of round r. Our main variables of interest are the treatment dummy $P2\text{-}INCOME$ (which indicates observations stemming from the P2-Income treatment) and the interaction of $P2\text{-}INCOME$ with the indicator variable $I_{y_{j,r}(t)>y_{i,r}(t)}$, which now indicates that subject i is behind in terms of current portfolio value ($y_{j,r}(t) > y_{i,r}(t)$). Just as in estimation equation (2.5) for the information effect, additional explanatory variables are the current own portfolio value $y_{i,r}(t)$ and the set $X_{i,r}(t)$ of controls (time and session fixed effects and individual-specific controls). Moreover, we include the reported beliefs $belief_{i,r}(t)$ as explanatory variable. In equation (2.6), the coefficient β_3 reflects the treatment effect of being ahead (when $I_{y_{j,r}(t)>y_{i,r}(t)} = 0$) compared to the reference group in P2-Info, and the sum $\beta_3 + \beta_5$ corresponds to the treatment effect of being behind (when $I_{y_{j,r}(t)>y_{i,r}(t)} = 1$), again compared to the reference group in P2-Info.

In specification 1 of Table 2.2, the estimated coefficient of $P2\text{-}INCOME$ is -0.179 and insignificant (p-value > 0.48); hence, we conclude that being ahead has no statistically measurable effect on satisfaction. The treatment effect of being behind measured by the sum of the coefficients of $P2\text{-}INCOME$ and its interaction term with $I_{y_{j,r}(t)>y_{i,r}(t)}$ has the expected negative sign (-0.415) and is borderline significant (p-value < 0.105). When adding individual-specific controls from the post-experimental questionnaire as in specification 2, 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.[31] Moreover,

[29] Note that we pool the observations from the sessions with the two different versions of the incentivized control question for the measure of satisfaction (the choice to receive as income the final value of another portfolio; compare Section 2.2.2), as the results obtained are very similar. See Table 2.7 in the appendix for estimations that separate these two types of sessions.

[30] The reasoning for using a crossed effects model is identical to the previous subsection. All results of this section are qualitatively robust to using a simple random-effects regression model, a random-effects Tobit 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 qualitatively robust.

[31] The significantly negative coefficient of $I_{y_{j,r}(t)>y_{i,r}(t)}$ reflects the fact that, within P2-Info, a subject's own portfolio is comparably low in situations of $y_{j,r}(t) > y_{i,r}(t)$; hence, also satisfaction is low. Since the interaction term of P2-Income with $I_{y_{j,r}(t)>y_{i,r}(t)}$ is significantly negative, this

	(1) satisfaction	(2) satisfaction	(3) satisfaction	(4)[a] satisfaction	(5)[b] satisfaction
$y_{i,r}(t)$	0.017***	0.017***	0.014***	0.021***	0.023***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$belief_{i,r}(t)$	0.004***	0.004***	0.004***	0.003***	0.003***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
P2-INCOME	-0.179	-0.207	-0.321	-0.277	-0.221
	(0.256)	(0.247)	(0.245)	(0.282)	(0.261)
$I_{y_{j,r}(t)>y_{i,r}(t)}$	-0.863***	-0.862***			
	(0.066)	(0.066)			
$I_{y_{j,r}(t)>y_{i,r}(t)}$ ×P2-INCOME	-0.236***	-0.236***			
	(0.065)	(0.065)			
$\Delta_{y_{j,r}(t)-y_{i,r}(t)}$			-1.338***	-0.846***	-0.462**
			(0.094)	(0.169)	(0.19)
$\Delta_{y_{j,r}(t)-y_{i,r}(t)}$ ×P2-INCOME			-0.244***	-0.338**	-0.054
			(0.062)	(0.143)	(0.153)
Constant	-0.825***	-0.808***	-0.305	-2.784***	-2.869***
	(0.289)	(0.281)	(0.307)	(0.467)	(0.409)
Individual controls	No	Yes	Yes	Yes	Yes
Time and session fixed effects	Yes	Yes	Yes	Yes	Yes
N	7600	7600	7600	3800	3800

[a]Subsample of being behind ($\Delta_{y_{j,r}(t)-y_{i,r}(t)} > 0$).
[b]Subsample of being ahead ($\Delta_{y_{j,r}(t)-y_{i,r}(t)} < 0$).

Note: Crossed-effects regression model with random effects on subject and portfolio level. Observations from treatments P2-INFO and P2-INCOME. Dependent variable: satisfaction. 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 round fixed effects, point-in-time fixed effects, fixed effects for the sequence in which the selected portfolios are shown, and session fixed effects.

Table 2.2: Income-comparison effects: Regression results.

the current value of the own portfolio ($y_{i,r}(t)$) and the beliefs about the own end-of-period portfolio value ($belief_{i,r}(t)$) have strong explanatory power throughout all specifications with positive coefficients that are significant at the 1% level. Hence, even after controlling for the current portfolio value differences in beliefs about the final income translate into differences in satisfaction levels.

In line with Section 2.3.1 above we can extend the binary case of being ahead or behind and directly investigate the treatment effect of the difference between the two observed portfolio values (the variable $\Delta_{y_{j,r}(t)-y_{i,r}(t)}$).[32] In specification 3, the estimated coefficient of the indicator variable $P2\text{-}INCOME$ is -0.321; hence, for average portfolios the stated satisfaction is slightly lower in P2-Income than in P2-Info. More importantly, the estimated coefficient of the interaction term $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INCOME$ is -0.244 and significant at the 1% level: An increasing difference between the current portfolio values of subjects j and i leads to significantly lower satisfaction levels of subject i, compared to the reference group in P2-Info where the exact same portfolios are observed but the second portfolio is not assigned to another subject.[33] Specifications 4 and 5 confirm that the effect of changes in the difference of portfolio values is mainly driven by situations where subjects are behind: In the subsample of observations where subjects face unfavorable inequality ($\Delta_{y_{j,r}(t)-y_{i,r}(t)} > 0$; specification 4) we observe a strong treatment effect; we observe, however, no statistically significant treatment effect in the subsample of observations where subjects face favorable inequality ($\Delta_{y_{j,r}(t)-y_{i,r}(t)} < 0$; specification 5).[34]

As in Section 2.3.1 we can also extend the interaction model in specifications 1 and 2 of Table 2.2 to disaggregate the income-comparison effect into cases of being "far behind," "behind," "ahead," and "far ahead" (see Tables 2.10 and 2.11 in Appendix 2.B for the estimation results). The treatment effect of P2-Income appears to be monotonic, and is strongest when subjects are "far behind," which, given the remaining uncertainty about the final income, makes it most likely that

effect becomes significantly more pronounced within the P2-Income treatment, in line with the result of the negative treatment effect of being behind.

[32]We again normalize the difference $y_{j,r}(t) - y_{i,r}(t)$ using the maximum observed difference at a given point in time (see Section 2.3.1) in order to separate the effect of a higher difference in portfolio values from time-related effects of an increasing difference $y_{j,r}(t) - y_{i,r}(t)$ within a round. Note again that normalizing the difference by the mean or the median yields very similar results.

[33]An F-test shows that the coefficients of $P2\text{-}INCOME$ and $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INCOME$ are jointly significant at the 1% level (p-value is 0.000).

[34]In specifications 4 and 5, the F-tests on joint significance of $P2\text{-}INCOME$ and $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INCOME$ yield p-values of 0.018 and 0.692, respectively.

the final income will be lower.

Result 2.2 *Observing signals about the income prospects of others affects individual satisfaction. Being behind (signals $y_j(t) > y_i(t)$) has a negative effect on satisfaction while being ahead (signals $y_j(t) < y_i(t)$) has no statistically significant effect on satisfaction.*

Since subjects in the control group of P2-INFO observe the exact same portfolios, the treatment effect of observing another subject's portfolio value $y_j(t)$ 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. However, already in P2-INFO subjects may interpret the second observed portfolio as a signal of, for instance, the likely income of the remaining participants of the experiment. Thus, the estimated treatment effect based on the difference between P2-INFO and P2-INCOME may be seen as a lower bound for the direct income-comparison effect.[35]

It is interesting to note that we find asymmetric results for additional information on beliefs (Result 2.1) and for relative-income considerations (Result 2.2). These asymmetries, however, appear exactly in the opposite way. Beliefs are most strongly affected when subjects observe a *lower* additional portfolio (that is, receive bad additional information), while satisfaction is most strongly affected when subjects observe a *higher* additional portfolio of another subject (that is, are behind). One possible interpretation could be that in either case subjects respond to the "bad prospect" rather than to the "good prospect." Put differently, while bad signals about the expected personal income and bad signals about the expected relative standing trigger significant reactions, good signals do not or less so.

2.3.3 Combining informational and income-comparison effects

Our experimental design does not only separate purely informational effects and income-comparison effects when observing signals about the income of others; it also allows to look at the interplay of the two potentially countervailing effects:

[35]Note that we can check this possibility by comparing reported satisfaction in the P2-INFO treatment to satisfaction in the BASE treatment. Running the specifications of Table 2.2 on observations from treatments BASE and P2-INFO yields, however, no significant difference in satisfaction levels, independent of whether the second portfolio observed has a higher or lower current portfolio value. Details are available on request.

Taking both effects together, do good signals about others' experiences lead to higher or lower satisfaction levels in situations where the own income is uncertain? Does the total effect depend on the degree of uncertainty and is, hence, different in early points in time as compared to late points in time where in the latter there is less uncertainty and income differences have become stable?

To investigate the total effect of observing signals about the income of others we can directly compare satisfaction levels in the P2-INCOME treatment and in the BASE treatment, combining both informational effects and income-comparison effects.[36] For this purpose we use the same estimation strategy as in the previous section (see, for instance, specification 3 of Table 2.2).[37] We separate possible effects in early points in time within a round from effects in later points in time to allow for changes in the combined effect over time when the uncertainty about income naturally decreases. The first two columns of Table 2.3 present the main results for the combined treatment effects on satisfaction levels based on the sample of observations from BASE and P2-INCOME; specification 1 only includes observations from the first two points in time t within a round for which satisfaction levels were elicited (situations of high uncertainty), while specification 2 is based on observations from the last two points in time t within a round where the uncertainty about the *own* and the *relative* income is reduced. (Recall that there are four such points in time in total within a round.) The main variables of interest are the treatment dummy $P2\text{-}INCOME$ and its interaction with the variable $\Delta_{y_{j,r}(t)-y_{i,r}(t)}$, which again denotes the (normalized) difference between subject j's and subject i's current portfolio value and takes values between -1 and 1. The coefficient of this interaction term reveals whether subjects in the treatment group P2-INCOME react differently to changes in the difference $y_{j,r}(t) - y_{i,r}(t)$, compared to the control group in BASE (where subjects do not observe the second portfolio but have been assigned the exact same own portfolios).

[36] The point becomes clear when considering Figure 2.1 once again. We simply move directly from the very left to the very right column of Figure 2.1 and thereby combine effects that additional signals may have on the expecations about the own income and about the relative income in one step.

[37] We do not include beliefs as explanatory variable since we are explicitly interested in the total effect which combines both purely informational effects and income-comparison effects.

	Total effect BASE vs. P2-INCOME		Income-comparison effect P2-INFO vs. P2-INCOME		Information effect BASE vs. P2-INFO	
	Early t	Late t	Early t	Late t	Early t	Late t
	(1)	(2)	(3)	(4)	(5)	(6)
	satisfaction	satisfaction	satisfaction	satisfaction	belief	belief
$y_{i,r}(t)$	0.016***	0.023***	0.016***	0.022***	0.815***	0.906***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.045)	(0.018)
P2-INCOME	-0.077	-0.170	-0.171	-0.374		
	(0.277)	(0.253)	(0.263)	(0.235)		
$\Delta_{y_{j,r}(t)-y_{i,r}(t)}$	-1.183***	-0.496***	-1.177***	-0.811***	-20.65***	-1.584
	(0.193)	(0.160)	(0.194)	(0.165)	(7.067)	(3.345)
$\Delta_{y_{j,r}(t)-y_{i,r}(t)}\times$P2-INCOME	-0.149	-0.402***	-0.298***	-0.250***		
	(0.097)	(0.073)	(0.100)	(0.074)		
P2-INFO					-11.27	-5.30
					(7.695)	(4.789)
$\Delta_{y_{j,r}(t)-y_{i,r}(t)}\times$P2-INFO					6.109	2.989
					(3.932)	(1.947)
Constant	-0.072	-1.940***	-0.103	-1.673***	92.42***	51.77***
	(0.476)	(0.389)	(0.476)	(0.409)	(15.89)	(7.285)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Time and session fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	3760	3760	3800	3800	3800	3800

Note: Crossed-effects regression model with random effects on subject and portfolio level. Dependent variables: *satisfaction* in specifications 1 to 4 and *beliefs* in specifications 5 and 6. 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 round fixed effects, point-in-time fixed effects, fixed effects for the sequence in which the selected portfolios are shown, and session fixed effects.

Table 2.3: Total effect: Regression results.

In specification 1 of Table 2.3, neither of the estimated coefficients of the two treatment variables is significantly different from zero.[38] Hence, in early points in time, satisfaction is not affected by the information about another subject's income. This changes, however, in later points in time: In specification 2, the estimated coefficient of the interaction term $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INCOME$ becomes larger in magnitude and significant at the 1% level.[39] To summarize, initially subjects do not become unhappier if they observe that another subject has been assigned a portfolio that outperforms their own portfolio; over time, however, this changes and satisfaction strongly reacts to differences in income prospects. Note that the latter effect is, again, mostly driven by situations in which subjects are behind in terms of relative income.

Two apparent and mutually non-exclusive interpretations for this finding, which is in line with Hirschman's prediction, are the following. First, in early points in time, potential inequality is rather unstable since the final income is still uncertain; even if the other subject's current portfolio value is higher, there is still some probability that this can be reversed. In later points in time, however, persisting differences in current portfolio values translate, with high likelihood, into inequality of final incomes. Second, the uncertainty about the own income in early points in time makes purely informational effects of observing another portfolio more important; as discussed in Section 2.2.1, however, such information effects can countervail the income-comparison effects. When the own final income becomes much less uncertain (as in late points in time), we would also expect those information effects to be much weaker and dominated by the income-comparison effects. To address the first interpretation, specifications 3 and 4 of Table 2.3 analyze how the isolated income-comparison effect (the treatment effect of P2-Income compared to P2-Info) changes over time. In contrast to the combined effect in specifications 1 and 2, the income-comparison effect turns out to be significant already in early points in time and is quite stable over time.[40] Even in situations with high uncertainty, satisfaction significantly reacts to increased inequality, given that we control for the informational effects on own expected income using P2-

[38] An F-test shows that the coefficients of $P2\text{-}INCOME$ and $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INCOME$ are jointly insignificant (p-value 0.299).

[39] An F-test shows that the coefficients of $P2\text{-}INCOME$ and $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INCOME$ are jointly significant at the 1% level (p-value is 0.000). Note that qualitatively very similar results on the dynamics are obtained when running estimations separately for each point in time.

[40] For both specifications 3 and 4, the coefficients of $P2\text{-}INCOME$ and $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INCOME$ are jointly significant at the 1% level.

INFO as control group. Looking at the dynamics of the information effects as in specifications 5 and 6 yields, however, some support for Hirschman's idea: The isolated information effect (the treatment effect of P2-INFO on beliefs about the own income, compared to BASE) is stronger in early points in time and fades out in late points in time.[41] In particular in early points in time, beliefs tend to be higher when the difference $y_{j,r}(t) - y_{i,r}(t)$ goes up (compare the coefficient of $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INFO$); higher beliefs, however, increase a subject's satisfaction (compare Table 2.2). Thus, the fact that the combined effect is indistinguishable from zero in early points in time may be interpreted as the information effect offsetting the income-comparison effect if and only if there is substantial uncertainty about the income prospects.[42]

Result 2.3 *The combined (information and income-comparison) effect is statistically indistinguishable from zero at early points in time where the two effects of observing additional information may offset each other. At late points in time, the relative-income effect dominates such that satisfaction decreases when observing to be behind in expected income ($y_j(t) > y_i(t)$).*

2.3.4 Information effect under increased uncertainty

Our main analysis on the effects of observing additional signals about the income distribution so far focuses on a scenario in which, at the beginning of the experiment, the individuals receive rather detailed information on the distribution of final portfolio values. An advantage of this setup is that the subjects start with a common prior and that learning dynamics become less important. This allows us to separate effects of additional information on the beliefs about the *distribution of incomes* and on the beliefs about the *own income*. At the same time, however, the value of additional information is weakened when detailed information about

[41]The coefficients of $P2\text{-}INFO$ and $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INFO$ are jointly marginally significant in specification 5 (p-value 0.103) and insignificant in specification 6 (p-value 0.166). Note also that the effect of information in early points time (specification 5 of Table 2.3) is more sizable than the effect for the complete sample (specification 3 of Table 2.1) but less precisely estimated due to the smaller sample size.

[42]These findings on the dynamics are confirmed when using the indicator variable $I_{y_{j,r}(t)>y_{i,r}(t)}$ for being ahead or behind (and good or bad information, respectively) to identify treatment effects, just as in specification 2 of Tables 2.1 and 2.2. As the only difference in terms of results obtained, the income-comparison effect (the treatment effect of P2-INCOME on satisfaction, as compared to P2-INFO) becomes stronger in later points in time. The latter may be caused by the fact that the indicator variable $I_{y_{j,r}(t)>y_{i,r}(t)}$ treats small and large inequalities in the same way, but observed income inequalities are larger at later points in time.

the income distribution is available at the beginning of the experiment. In addition, the "information effect" may generally be different in situations in which individuals face a considerably higher degree of uncertainty.

In further control sessions, we vary the information that subjects receive about the income distribution. More precisely, while the experimental instructions of main treatments display a "cloud" of possible portfolio developments (compare the graph in Appendix 2.C) from which the subjects can conclude on the income distribution, we do not provide this information in the control sessions. Hence, for subjects in the control sessions the experimental instructions contain no information at all about the income-generating process or the probability distribution of final portfolio values. Apart from this change in the information about the income distribution provided to the subjects, the resulting treatments called Base-C and P2-Info-C ("control") follow the exact same rules as the original Base and P2-Info treatments and are based on the same set of portfolios.[43] Therefore, "information effects" can be identified just as in the main analysis.

Before turning to the treatment comparisons of Base-C and P2-Info-C within the control sessions under increased uncertainty we briefly compare the subjects' stated beliefs in the control sessions to the data of the original sessions analyzed in the previous sections. For the very early observations (that is, the first points in time where beliefs are elicited) stated beliefs are less accurate in the control sessions than in the original sessions. This holds, however, only for the very early observations in the first round and is stronger in the Base treatment (where subjects observe their own portfolio only) than in the P2-Info treatment (where subjects also observe a second portfolio).[44] Already from the end of the first round on and in all future rounds, the stated beliefs in Base (P2-Info) are very similar in the original and in the control sessions. Overall, the data suggests that at the beginning of the control sessions subjects underestimate the variance of the final income distribution but rather expect their income to take some average value.[45]

[43]For the experimental instructions in the control sessions we use the exact same instructions as in the original treatments, except that we remove the last paragraph including the figure that shows the "cloud" of possible portfolios (compare Appendix 2.C). We run 3 sessions for the Base-C treatment and 4 sessions for the P2-Info-C treatment (168 subjects with 40 observations per subject in total).

[44]More precisely, for the first point in time where beliefs are elicited (where the uncertainty in the control sessions is likely to be most important), the correlation of the stated beliefs with the final portfolio value is only 0.26 in Base-C (compared to 0.44 in the original Base treatment). While this difference might already seem small, it becomes even smaller when comparing P2-Info-C to P2-Info (0.34 compared to 0.50), and it fades out the more observations from later rounds are included.

[45]In fact, in all sessions we observe that subjects on average underestimate the value of portfo-

The subjects' beliefs seem, however, to adjust very quickly towards the stated beliefs in the original sessions.

Taking this finding on learning dynamics into account we can estimate the "information effect" in the control sessions based on the same identification strategy as in Section 2.3.1. The estimation results are summarized in Table 2.6 in Appendix 2.A and are based on samples of observations from the treatments BASE-C and P2-INFO-C, contrasting the information effect in early rounds and early points in time t within a round to the effect in later rounds where the subjects have already received a number of signals about the income distribution.[46] When including only observations from the early rounds, the effects of additional information (the coefficients of $P2\text{-}INFO$ and of the interaction term $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INFO$ jointly) are very imprecisely measured and not significantly different from zero. In later rounds, however, the signs of the estimated coefficients change and the observed effects approach the results from the original sessions reported in Section 2.3.1: Focusing on the effect of bad additional information and taking into account that the normalized difference $\Delta_{y_{j,r}(t)-y_{i,r}(t)}$ reaches a value -1 for the "worst" information observed we find a highly insignificant effect between -3.45 and 12.15 in rounds 1 and 2 (see specification 1 of Table 2.6) that shifts, still insignificant, to an effect in the range between -4.05 and -0.98 in rounds 1 to 5 (see specification 2 of Table 2.6). For rounds 6 to 10, the estimated effect of bad additional information is between -14.53 to -10.25 (see specification 2 of Table 2.6; the coefficients of $P2\text{-}INFO$ and the interaction term $\Delta_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INFO$ are jointly significant at the 10% level). In the latter case, the estimated effects in the control sessions are very similar to the results obtained for situations in which subjects are endowed with a rather exact common prior about the income distribution (compare, for instance, specification 3 of Table 2.1 and specification 5 of Table 2.3).[47] Again, the information effect is driven by bad additional information and is insignificant

lios with a positive trend and overestimate the value of portfolios with a negative trend; this effect is, however, strongest in early observations of the control sessions with increased uncertainty.

[46]Note that we again exclude one subject (out of 168) whose beliefs are "implausible" in the sense that the responses were always below 10 points. Note also that due to unintentional heterogeneity in the composition of the sessions the set of observations of the control sessions is not perfectly balanced in the sense that the *number* of subjects who observe the exact same portfolio is not exactly the same in BASE-C and in P2-INFO-C. In the estimations we control for this issue with portfolio-specific random effects; moreover, estimations on subsamples which are perfectly balanced confirm the findings on the information effect discussed below.

[47]Using the entire sample of the additional control treatments (see in specification 4 of Table 2.6), however, we do not measure a statistically significant effect of additional (good or bad) information, which is not surprising given the learning dynamics presented in specifications 1 to 3.

in case of good additional information.[48]

Result 2.4 *Under higher uncertainty about the income distribution we do not measure a significant effect of observing additional information in early rounds. In later rounds, subjects' beliefs are significantly lower when observing bad additional information ($y_j(t) < y_i(t)$), while there is no significant effect on the subjects' beliefs when observing good additional information ($y_j(t) > y_i(t)$).*

The control sessions confirm the finding that subjects may react differently to "bad news" and to "good news," even in situations with higher uncertainty where much less information about the income distribution is available. For this asymmetric information effect to be measurable, it seems important that subjects have some idea of how the income distribution may look like. For early observations where subjects do not know anything about the income distribution, additional signals may have several and countervailing effects, affecting both the posterior about the income distribution as well as the expectation about the own income.[49] While the learning dynamics are interesting per se, the results of the control sessions with higher uncertainty can also be seen as a robustness check of our main results.

2.4 Conclusion

Guided by Hirschman's idea of the "tunnel effect" we analyze direct income-comparison effects and indirect belief-based information effects when individuals observe signals about the income of others, in an environment characterized by uncertainty about the own income prospects. The empirical results of our experiment show that when individuals observe bad additional information (others are

[48] When identifying the treatment effect of additional information based on the indicator variable $I_{y_{j,r}(t)>y_{i,r}(t)}$ for good additional information (as in specification 2 of Table 2.1) we find very similar results: In early rounds there is no significant treatment effect of additional information (neither for good nor for bad information). In later rounds, however, we find the asymmetric effect that only bad additional information significantly (and negatively) affects stated beliefs. These results and estimations on separate subsamples for good and bad additional information applying the specifications in Table 2.6 are available on request.

[49] As an illustration, suppose that subjects believe that the income distribution is concentrated around a value very close to the initial value $y(0)$ (that is, they underestimate the variance of portfolios). If a subject has a portfolio with a currently positive trend and observes a second portfolio with negative trend, this may provide information on the variance of final incomes and may, hence, lead to higher beliefs about the own final portfolio value. Such an effect would counteract the negative effect of "bad news" observed in the original sessions where the variance of the income distribution is basically known due to information provided in the instructions.

likely to earn less), they lower their beliefs about their own income. Observing good 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 asymmetric effects of information and of comparison considerations. For the combined "income-comparison effect" and "information effect" we find that under high uncertainty about final incomes both countervailing effects offset each other, leading to a statistical zero total effect. But as uncertainty decreases over time income-comparison effects dominate the informational effects such that individuals report significantly lower satisfaction when observing that others are ahead. Thus, our evidence suggests in line with Hirschman's idea that informational and comparison effects are simultaneously at work, with the dynamics being crucial: The countervailing forces of informational effects are particularly relevant in early points in time, when additional information first arrives and uncertainty is still substantial. At a later stage, stable inequalities and a lower informational value of additional signals about others' experiences lead to a situation in which income-comparison considerations clearly prevail. Since we intentionally leave individuals uncertain about the informativeness of additional signals our findings on informational effects can be interpreted as rather strong and might be expected to dominate in environments in which the income-generating processes are clearly correlated.

Maybe surprisingly, we find asymmetric effects both for informational effects on the beliefs about the own income and for income-comparison effects. We interpret this finding as subjects being more reactive to "bad news" than to "good news." This offers interesting implications for attitudes toward redistribution and for the acceptance of income inequality. First, and maybe most straightforward to see, an asymmetric "income-comparison effect" implies that individuals experience a lower tolerance for inequality (ceteris paribus) and favor more redistribution. Catching up to richer individuals will be more important than the possible disutility resulting from other individuals catching up in terms of income relative to oneself. Consequently, redistributing from richer to poorer individuals compared to oneself would be perceived as favorable. Second, when signals of upside potentials in future income are less recognized, but signals of downside potentials lead to an updating of the own expectations, this will increase the support for redistributive policies. Raising taxes on high incomes will be seen less critical, as individuals are less sensitive to signals that indicate good income prospects for themselves. On the other hand, as individuals are sensitive to potentially bad signals about the own income prospects individuals will consider social assistance programs in support of low income levels as relatively more important, reinforcing Varian's (1980) argument of "redistributive taxation as social insurance." Therefore, the asymmetries in the information-based and in the direct income-comparison effects imply that individuals experience a lower tolerance for inequality and favor more redistribution.

Appendix

2.A Appendix

2.A.1 Experimental treatments

Treatments	Base	P2-Info	P2-Income	Base-C	P2-Info-C
# sessions	4	4	4	3	4
# participants	96	96	96	72	96
# obs. per participant	40	40	40	40	40

Note: In BASE, subjects only observe their own portfolio; in P2-INFO, subjects observe their own portfolio and an additional portfolio which is not payoff relevant for any participant; in P2-INCOME, subjects observe their own portfolio and the portfolio of another participant. The control treatments BASE-C and P2-INFO-C are identical to treatments BASE and P2-INFO, except that subjects receive no information about the distribution of final portfolio values (see Section 2.3.4).

Table 2.4: Summary of the experimental treatments.

2.A.2 Descriptive statistics

	BASE	P2-INFO	P2-INCOME	Total			
	Mean	Mean	Mean	Mean	S.D.	Max	Min
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
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
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

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. "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). "Behind" refers to the case of being behind in relative-income. "Ahead" refers to situations of being ahead in relative-income.

Table 2.5: Summary statistics for the main treatments.

2.A.3 Histograms of stated beliefs and satisfaction levels

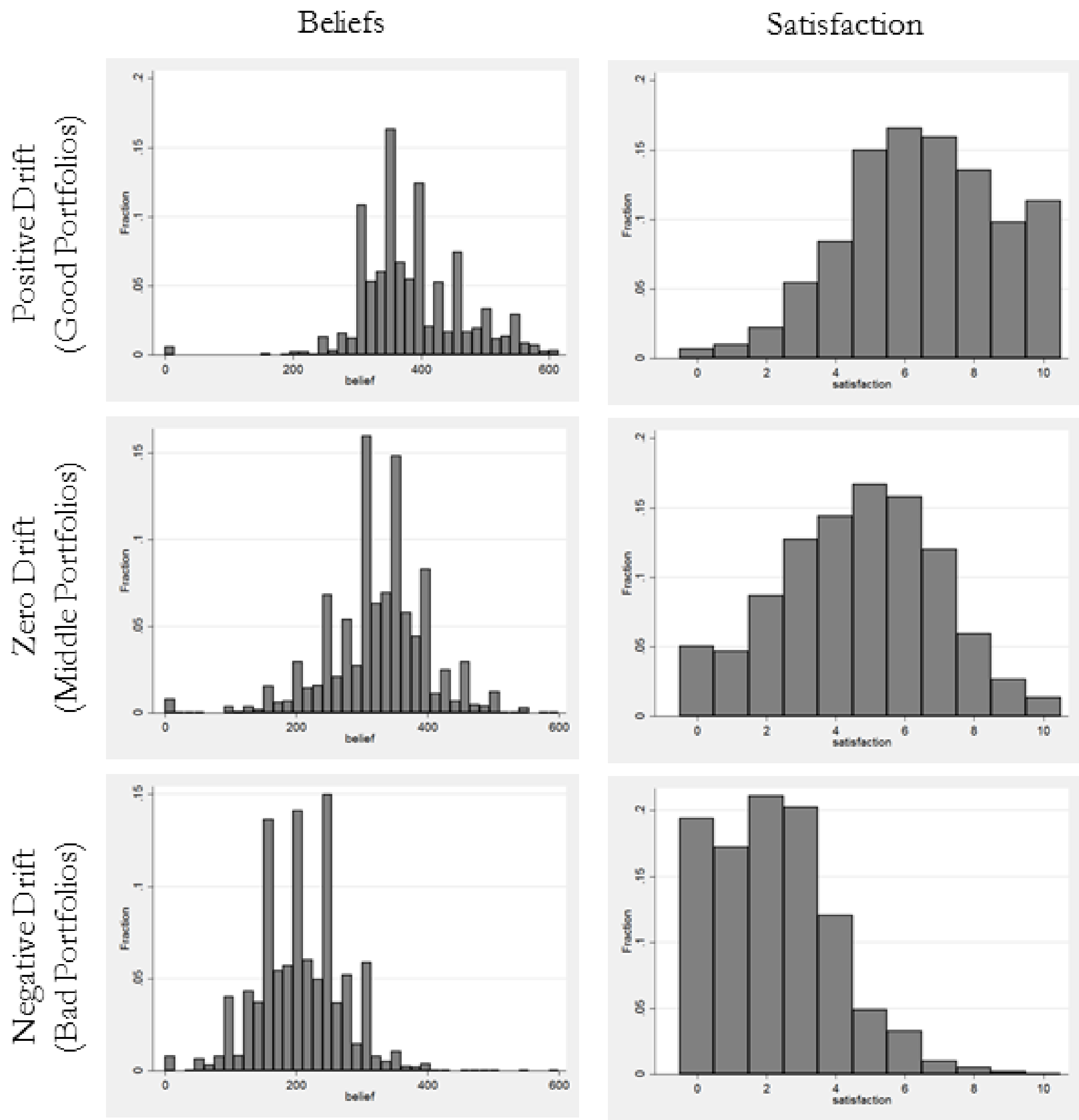

Figure 2.4: Distributions of measured beliefs and satisfaction for different portfolio types (with positive, zero, and negative drift of the stochastic portfolio-generating process).

2.A.4 Screenshot of the experimental task

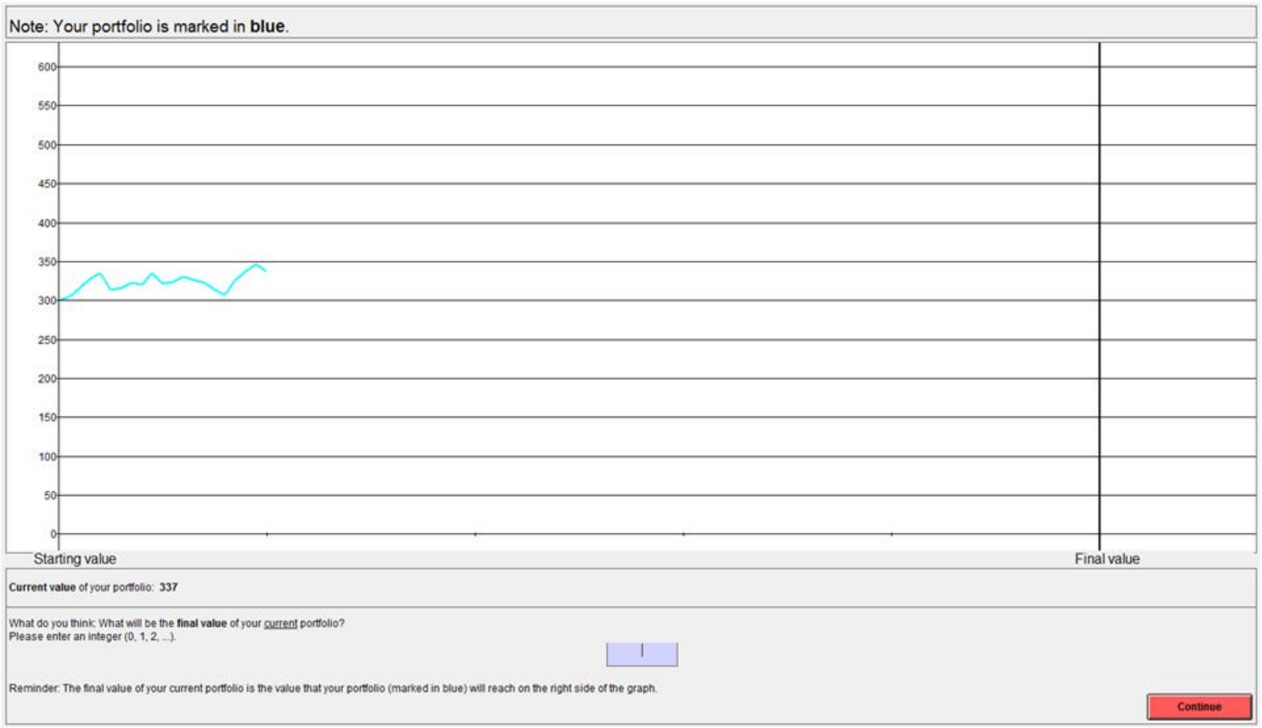

Figure 2.5: Screenshot of the experiment (for the question on beliefs in the Base treatment).

2.A.5 Estimation results for the sessions with increased uncertainty

	Round 1 to 2	Round 1 to 5	Round 6 to 10	Round 1 to 10
	Early t	Early t	Early t	All t
	(1)	(2)	(3)	(4)
	belief	belief	belief	belief
$y_{i,r}(t)$	0.867***	0.861***	1.009***	0.871***
	(0.071)	(0.039)	(0.038)	(0.017)
P2-INFO	-3.450	-4.049	-12.39**	-5.758
	(17.64)	(11.07)	(5.819)	(5.809)
$\Delta_{y_{j,r}(t)-y_{i,r}(t)}$	-10.41	-22.83***	-0.448	-1.277
	(12.30)	(7.430)	(6.334)	(3.076)
$\Delta_{y_{j,r}(t)-y_{i,r}(t)}$ $\times$P2-INFO	-15.60	-3.072	2.140	-3.156
	(12.97)	(7.954)	(4.584)	(2.230)
Constant	54.45	66.44***	0.599	55.31***
	(42.39)	(25.60)	(16.88)	(13.09)
Individual controls	Yes	Yes	Yes	Yes
Time and session fixed effects	Yes	Yes	Yes	Yes
N	668	1670	1670	6680

Note: Crossed-effects regression model with random effects on subject and portfolio level. Dependent variables: *beliefs*. 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 round fixed effects, point-in-time fixed effects, fixed effects for the sequence in which the selected portfolios are shown, and session fixed effects.

Table 2.6: Information effects under increased uncertainty: Regression results.

2.B Supplementary material

2.B.1 Income-comparison effects for the two variants of the control question

	CQ (1) satisfaction	CQH (2) satisfaction	Pooled (3) satisfaction
$y_{i,r}(t)$	0.016***	0.018***	0.017***
	(0.001)	(0.001)	(0.001)
$belief_{i,r}(t)$	0.005***	0.003***	0.004***
	(0.001)	(0.001)	(0.000)
P2-INCOME	-0.077	-0.175	-0.207
	(0.260)	(0.246)	(0.247)
$I_{y_{j,r}(t)>y_{i,r}(t)}$	-0.889***	-0.780***	-0.862***
	(0.088)	(0.097)	(0.065)
P2-INCOME$\times I_{y_{j,r}(t)>y_{i,r}(t)}$	-0.221***	-0.256***	-0.236***
	(0.087)	(0.098)	(0.065)
Constant	-0.825***	-0.808***	-1.254***
	(0.350)	(0.343)	(0.281)
Individual controls	Yes	Yes	Yes
Time and session fixed effects	Yes	Yes	Yes
N	3840	3760	7600

Note: Crossed-effects regression model with random effects on subject and portfolio level. Observations from treatments P2-INFO and P2-INCOME. Dependent variable: satisfaction. Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. The sample of observations depends on the variant of the control question. In "CQ" the subjects had the choice to have their earnings to be determined by the final value of another, randomly drawn portfolio; in "CQH" this control question was only asked "hypothetically" and was not actually implemented. "Pooled" refers to the full sample based on both variants of the control question. "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 round fixed effects, point-in-time fixed effects, fixed effects for the sequence in which the selected portfolios are shown, and session fixed effects.

Table 2.7: Income-comparison effects: Separate regression results depending on the variant of the control question used in the experiment.

2.B.2 Information effects: Additional results

	(1) belief	(2) belief
$y_{i,r}(t)$	0.841***	0.841***
	(0.013)	(0.013)
P2-INFO	-13.02**	-11.83*
	(5.936)	(6.160)
$Q2_{y_{j,r}(t)-y_{i,r}(t)}$	-1.090	-1.098
	(2.691)	(2.691)
$Q3_{y_{j,r}(t)-y_{i,r}(t)}$	-8.497***	-8.495***
	(3.173)	(3.173)
$Q4_{y_{j,r}(t)-y_{i,r}(t)}$	-13.28***	-13.29***
	(3.546)	(3.546)
$Q2_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INFO$	2.033	2.63
	(2.980)	(2.980)
$Q3_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INFO$	6.006**	6.007**
	(2.947)	(2.947)
$Q4_{y_{j,r}(t)-y_{i,r}(t)} \times P2\text{-}INFO$	6.044**	6.077**
	(3.033)	(3.033)
Constant	86.15***	86.76***
	(8.000)	(8.051)
Individual controls	No	Yes
Time and session fixed effects	Yes	Yes
N	7600	7600

Note: Crossed-effects regression model with random effects on subject and portfolio level. Observations from treatments BASE and P2-INFO. Dependent variable: beliefs. Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. The variables $Q2_{y_{j,r}(t)-y_{i,r}(t)}$ to $Q4_{y_{j,r}(t)-y_{i,r}(t)}$ are indicator variables for quartiles of the difference $y_{j,r}(t) - y_{i,r}(t)$ at a given point in time t; $Q2_{y_{j,r}(t)-y_{i,r}(t)}\times$P2-INFO to $Q4_{y_{j,r}(t)-y_{i,r}(t)}\times$P2-INFO are the respective interaction terms with the treatment dummy P2-INFO. Baseline category is $Q1_{y_{j,r}(t)-y_{i,r}(t)}$. Specification 2 adds "Individual controls": 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 round fixed effects, point-in-time fixed effects, fixed effects for the sequence in which the selected portfolios are shown, and session fixed effects.

Table 2.8: Information effects: Disaggregated interaction model.

Additional information	Effect (p-value)	Tested hypothesis
Very Bad ($y_{j,r}(t) << y_{i,r}(t)$)	-13.02** (0.028)	H_0: P2-INFO= 0
Bad ($y_{j,r}(t) < y_{i,r}(t)$)	-10.987 (0.063)	H_0: P2-INFO+$Q2_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INFO= 0
Good ($y_{j,r}(t) > y_{i,r}(t)$)	-7.014 (0.239)	H_0: P2-INFO+$Q3_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INFO= 0
Very Good ($y_{j,r}(t) >> y_{i,r}(t)$)	-6.976 (0.240)	H_0: P2-INFO+$Q4_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INFO= 0

Note: The effect of additional information as estimated in specification 1 of Table 2.8. *p<0.10, **p<0.05, ***p<0.01. For the baseline category (Q$1_{y_{j,r}(t)-y_{i,r}(t)}$), the treatment effect of additional information is given by the coefficient of P2-INFO. For the remaining quartiles, the treatment effect of additional information is given by the sum of the coefficients of P2-INFO and its interaction term with the indicator variable for the respective quartile (in the table, P2-INFO and Q$2_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INFO to Q$4_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INFO refer to the coefficients of the variables as estimated in specification 1 of Table 2.8).

Table 2.9: Disaggregated information effects: Hypothesis tests for good and bad additional information.

2.B.3 Income-comparison effects: Additional results

	(1) satisfaction	(2) satisfaction
$y_{i,r}(t)$	0.016***	0.016***
	(0.001)	(0.001)
$belief_{i,r}(t)$	0.004***	0.004***
	(0.000)	(0.000)
P2-INCOME	-0.170	-0.196
	(0.260)	(0.252)
$Q2_{y_{j,r}(t)-y_{i,r}(t)}$	-0.367***	-0.366***
	(0.084)	(0.084)
$Q3_{y_{j,r}(t)-y_{i,r}(t)}$	-1.344***	-1.343***
	(0.100)	(0.100)
$Q4_{y_{j,r}(t)-y_{i,r}(t)}$	-1.622***	-1.620***
	(0.111)	(0.111)
$Q2_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INCOME	0.000	-0.002
	(0.093)	(0.093)
$Q3_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INCOME	-0.161*	-0.161*
	(0.092)	(0.092)
$Q4_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INCOME	-0.345***	-0.347***
	(0.095)	(0.095)
Constant	-0.155	-0.146
	(0.314)	(0.308)
Individual controls	No	Yes
Time and session fixed effects	Yes	Yes
N	7600	7600

Note: Crossed-effects regression model with random effects on subject and portfolio level. Observations from treatments P2-INFO and P2-INCOME. Dependent variable: satisfaction. Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. The variables $Q2_{y_{j,r}(t)-y_{i,r}(t)}$ to $Q4_{y_{j,r}(t)-y_{i,r}(t)}$ are indicator variables for quartiles of the difference $y_{j,r}(t) - y_{i,r}(t)$ at a given point in time t; $Q2_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INCOME to $Q4_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INCOME are the respective interaction terms with the treatment dummy P2-INCOME. Baseline category is $Q1_{y_{j,r}(t)-y_{i,r}(t)}$. Specification 2 adds "Individual controls": 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 round fixed effects, point-in-time fixed effects, fixed effects for the sequence in which the selected portfolios are shown, and session fixed effects.

Table 2.10: Income-comparison effects: Disaggregated interaction model.

Income-comparison	Effect (p-value)	Tested hypothesis
Far ahead ($y_{j,r}(t) << y_{i,r}(t)$)	-0.170 (0.512)	H_0: P2-INCOME= 0
Ahead ($y_{j,r}(t) < y_{i,r}(t)$)	-0.170 (0.511)	H_0: P2-INCOME+$Q2_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INCOME= 0
Behind ($y_{j,r}(t) > y_{i,r}(t)$)	-0.331 (0.203)	H_0: P2-INCOME+$Q3_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INCOME= 0
Far behind ($y_{j,r}(t) >> y_{i,r}(t)$)	-0.515** (0.048)	H_0: P2-INCOME+$Q4_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INCOME= 0

Note: The effect of observing another participant's portfolio as estimated in specification (1) of Table 2.10. *p<0.10, **p<0.05, ***p<0.01. For the baseline category ($Q1_{y_{j,r}(t)-y_{i,r}(t)}$), the treatment effect of observing another participant's portfolio is given by the coefficient of P2-INCOME. For the remaining quartiles, the treatment effect of observing another participant's portfolio is given by the sum of the coefficients of P2-INCOME and its interaction term with the indicator variable for the respective quartile (in the table, P2-INCOME and $Q2_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INFO to $Q4_{y_{j,r}(t)-y_{i,r}(t)}$×P2-INCOME refer to the coefficients of the variables as estimated in specification (1) of Table 2.10).

Table 2.11: Disaggregated income-comparison effect: Hypothesis tests for being behind and being ahead.

2.B.4 Set of portfolios assigned in the experiment

Figure 2.6: Portfolios

2.C Experimental instructions

Welcome to the Experiment![1]

Please read these instructions carefully and completely. Thoroughly understanding the instructions will help you to earn more money.
Your earnings in the experiment are measured in Talers. At the end of the experiment we will convert the Talers you earned into Euros and pay you accordingly. The conversion rate is: 25 Talers = 1 Euro. In addition, 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. Whenever you have a question, please raise your hand and we will help you.

Your Task:

In the experiment, each participant is assigned a portfolio whose current value you will observe in a graph on your screen. You can think of your "portfolio" as a part of your earnings you receive at the end of the experiment. Portfolios are generated by the computer according to a random process. A graph at the end of these instructions illustrates possible portfolio processes.
You will randomly be assigned into groups of two. However, you will not know which of the other participants is assigned to you as your co-player. Each participant will observe the current value of the own portfolio and of the co-player's portfolio over time. 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 dynamic change in portfolio values will stop in regular interval and you will be asked the following questions on your screen:

1. How satisfied are you with your current portfolio on a scale from 0 (highly dissatisfied) to 10 (highly satisfied)?

2. What do you think: what will be the final value of your current portfolio (in Talers)?

[1] The experiment was conducted in German. This appendix contains a translated version of the instructions for the P2-Income treatment.

3. Please choose one of the following two options:
   (a) I prefer to be paid the final value of my current portfolio.
   (b) I prefer to be paid the final value of a new portfolio, which is randomly generated and assigned to me at the end of the experiment.

You and your co-player answer repeatedly and independently the same 3 questions. At each point in time you can choose your answers anew and fully independently of your previous answers. Your answers will not be displayed to your co-player.
Until the final portfolio value is reached you and your co-player keep the assigned portfolios and each answer the three questions with respect to the current portfolio. This also applies in case your answer to question 3 is to receive as a payment the final value of a new, randomly assigned portfolio.

Procedure:

Overall, you will repeat this task 10 times. Consequently, you will observe 10 such portfolio processes. These 10 rounds are completely independent of each other: In each round the participants will be randomly re-matched in groups of two and each time you and your new co-player will each be randomly and independently assigned a new portfolio.
At the end of the experiment, in a first step, the computer will randomly select one of the 10 rounds. For the selected round the computer will select exactly one point in time at which you answered the three questions described above. Your payment will be determined by your answers at this selected point in time and includes three components:

- For your answer with respect to your satisfaction you receive 50 Talers, independent of the value you entered.

- The better your estimate of the final portfolio value at the selected point in time matches the actual final portfolio value in the selected round, the more money you receive:
  – If you predicted precisely the realized final portfolio value, you receive 250 Talers.
  – The exact formula to calculate your payment is:
  $$\text{Payment (in Talers)} = 250 - \tfrac{1}{10}(\text{estimate} - \text{actual final value})^2;$$
  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 round.

– 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 process at the end of the experiment on your screen.

In total your payment consists of the final portfolio value (in Talers), of the Talers earned when predicting the final portfolio value, and of 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 three questions explained above.

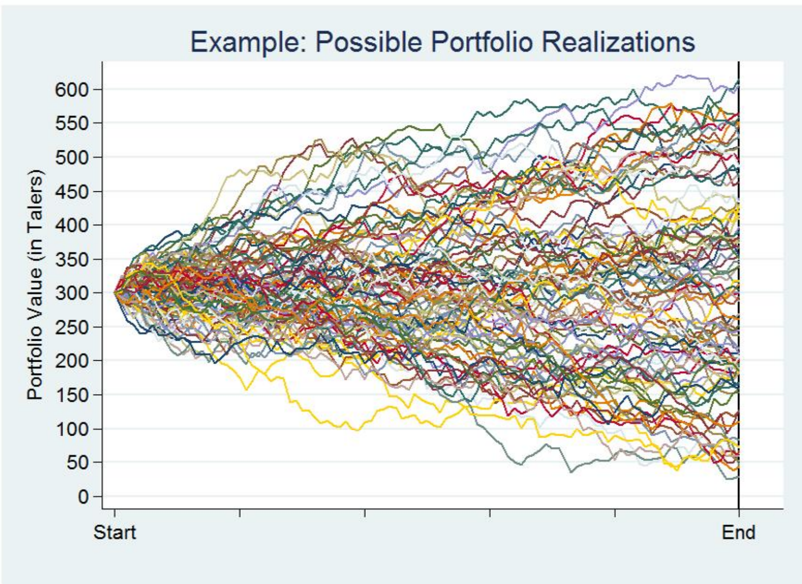

Figure 2.7: Example

Chapter 3

Hirschman's tunnel effect goes abroad: International dimensions of social comparison and subjective well-being

3.1 Introduction

Recent years have seen a steady stream of economic crises around the world.[1] The resolution of such crises more often than not involve international transfers to prevent the economic collapse of particularly severely hit countries. Politicians and pundits who argue in support for such transfers typically appeal to national self interest: it would adversely affect the own population if the crisis-struck country is not saved from destitution.[2] Such argument relies on a supposedly positive effect of other countries' economic performance on the own population's well-being. However, surprisingly little is known about how people's well-being is affected by the economic fate of foreign populations. This is in stark contrast to what we know

[1]This chapter is based on joint work with Aart Gerritsen, MPI for Tax Law and Public Finance. See Gerritsen and Lang (2016).

[2]For instance, during the European sovereign debt crisis, *The Guardian* wrote about the "German self-interest" of bailing out other European countries to protect the Euro (September 29, 2012). *The Times* argued for the self-interest of Great Britain to participate in bilateral and multilateral bailout measures for Ireland (May 15, 2012). *The Independent* even claimed that it is in the self-interest for China to eventually step in and financially contribute to a resolution of the European debt crisis (October 29, 2011). Such lines of argument have also been applied, for instance, to support the United States' efforts to help Mexico during the peso crisis or Japan's financial aid for South-East Asian countries during the Asian crisis (e.g., see *The Australian*, July 22, 1997).

about the well-being effects of more local reference groups such as neighbors, co-workers, and the like. This paper is an attempt at rectifying this by determining how people's subjective well-being is affected by the economic performance of foreign countries.

People's attitude towards other countries' economic performance is *a priori* unclear. On the one hand, people might dislike economic progress of other nations when they care about their own relative income position – a psychological phenomenon we refer to as social comparison. On the other hand, people might like economic progress in foreign countries if they expect it to spill over toward their home country, thereby raising their own income prospects. In a seminal study, Hirschman (1973) discusses both countervailing mechanisms in the context of people's attitude toward within-country income inequality. Situations in which the "income-prospect" effect dominates the "social-comparison" effect have come to be referred to as the "tunnel effect," after an anecdote by Hirschman that involves a person being stuck in a traffic jam in the middle of a two-lane tunnel. She feels dispirited as neither of the two lanes are moving. However, when suddenly the cars in the lane next to her start moving, her mood brightens as she becomes hopeful that the traffic jam might be broken. While her relative position deteriorates as other drivers pass by, her own prospects of moving on appear much better now, leaving her more satisfied than before.[3] In a similar way, Hirschman argues, individuals might have a higher tolerance for other people's income increases if these are expected to be informative of future increases in own income.

Previous empirical studies have found evidence in line with both social comparison and income-prospect effects. These studies typically rely on either survey data or lab experiments in which the reference group has a local connotation – neighbors, colleagues, or other fellow lab subjects.[4] But since the surge of globalization, internet, mass media, and other advances in communication, individuals have become increasingly aware of the economic situation of people in foreign countries.

[3] Hirschman (1973) continues his anecdote by stating that the driver's mood might reverse if these prospects do not materialize and only the other lane keeps moving. Soon, she and her fellow drivers would become furious and attempt to illegally cross the double line between the two lanes.

[4] Survey evidence of social-comparison effects of other people's income on own subjective well-being have been found by, amongst others, Easterlin (1995), Clark and Oswald (1996), McBride (2001), Blanchflower and Oswald (2004), Ferrer-i-Carbonell (2005), Luttmer (2005), Kingdon and Knight (2007), Card et al. (2012). Survey evidence of the tunnel effect – or a dominant income-prospect effect – have been established by, amongst others, Ravallion and Lokshin (2000), Senik (2004, 2008) and Clark et al. (2009). Lang and Morath (2015) provide experimental evidence on the tunnel effect.

This has made it more likely that individuals compare their own economic situation with that of foreigners as well as that of more local reference groups. At the same time, globalization induces closer economic ties among countries which could imply spillover effects from economic performance of one country to another, and thus, potential income-prospect effects between countries. This paper contributes to the existing literature by focussing on foreign countries as a reference group. More specifically, we focus on the relationship between individuals' subjective well-being and the economic performance – measured as economic growth rates – of these reference countries. We do this for a repeated cross-section covering about 180,000 individuals in roughly 80 countries across 5 survey waves of the World Values Survey.

As both social-comparison and income-prospect effects might simultaneously be at work, it is impossible for us to empirically disentangle the two effects – rather, we can only measure the net impact of the two countervailing effects. We do, however, focus our analysis on different groups of reference countries based either on criteria of economic integration or on criteria of geographical proximity. Our hypothesis is that income-prospect effects are stronger for reference countries that are economically more integrated with the home country. In line with this expectation, we find that individuals' subjective well-being is positively associated with economic growth rates of important trade partners. On the other hand, we find no statistically significant association between subjective well-being and the average economic growth of neighboring countries. However, when we separate the reference group of neighboring countries based on their economic integration with the home country, we find a positive association between well-being and growth rates of high-trade neighbors, and a negative association between well-being and growth rates of low-trade neighbors. These results suggest the simultaneous existence of both social-comparison and income-prospect effects on an international level, and the dominance of income-prospect effects when it concerns reference countries that are economically closely integrated with the home country.

Our paper relates to different strands of the literature on income comparisons and subjective well-being. It is most closely related to the studies on income comparisons mentioned above, and adds to that literature by focussing on the international dimension of comparison effects (see the references in footnote 4). Another related strand of the literature focusses on the effect of macroeconomic variables on subjective well-being. These studies attempt to determine the well-being effects of variables such as economic growth, unemployment and inflation in the home country, but typically do not include other countries' macroeconomic performance in their analysis (e.g., Di Tella et al. 2001, 2003, Alesina et al. 2004, Stevenson and Wolfers 2008, and Blanchflower et al. 2014).[5] To the best of our knowledge, Becchetti et al. (2013) is the only study that takes into account the effects of other countries' macroeconomic variables on subjective well-being. They find for a sample of 15 West-European countries that subjective well-being relates negatively to the gross national disposable income of the richest neighboring country. In contrast, we focus on a much larger sample of countries, a richer set of reference countries, and a different macroeconomic variable – economic growth – which we believe is more visible in the media for the general public and generally accepted as a measure of economic performance. More importantly, we show that income-prospect effects could play an important role and even dominate comparison effects when it comes to economically integrated reference countries. Finally, there is a small number of theoretical studies which establish that the international comparison effects we observe bear important policy implications for the optimal provision of public goods, as well as the optimal nonlinear income tax schedule (Aronsson and Johansson-Stenman 2014, 2015).[6]

The paper proceeds as follows. Section 3.2 introduces a simple theoretical model to flesh out the different ways in which foreign economic growth might impact a person's well-being. Section 3.3 discusses our empirical strategy and the data we use. In Section 3.4, we present the empirical results of our baseline estimations, as well as robustness analyses. We close with concluding remarks in Section 3.5.

3.2 Theoretical framework

We present a highly stylized two-period economic model of an individual's utility and his consumption decisions. The purpose of this model is to illustrate ways in which international comparisons could affect an individual's utility. The model is loosely based on a combination of multiple models in Clark et al. (2008). We assume that an individual earns income y_t and consumes c_t in period $t \in \{1, 2\}$. We furthermore assume that he has access to capital markets and can borrow and invest at a risk-free interest rate r. The individual's intertemporal budget

[5]For an early overview of the effects of macroeconomic variables on subjective well-being see Frey and Stutzer (2002).

[6]On a more local level, social comparison concerns can also have implications for the optimal amount of accumulated capital and domestic growth (e.g., Congleton 1989, Konrad 1990, 1992).

constraint equates the present values of income and consumption expenditures:

$$c_1 + \frac{c_2}{1+r} = y_1 + \frac{y_2}{1+r}. \tag{3.1}$$

The individual derives utility from consumption in both periods. Utility is assumed to be increasing and strictly concave in consumption. Moreover, we allow it to depend on a consumption-reference level $\bar{c}_t$ as well as on the individual's own consumption level. Lifetime utility is given by the discounted sum of period-specific subutility:

$$U \equiv u(c_1 - \alpha\bar{c}_1) + \beta u(c_2 - \alpha\bar{c}_2), \qquad u'(\cdot), -u''(\cdot) > 0, \tag{3.2}$$

where $\alpha \in [0,1]$ denotes the degree to which utility of consumption depends on the reference level $\bar{c}_t$, and $\beta \in [0,1]$ denotes the individual's discount factor. For simplicity, we abstract from uncertainty and assume that income in both periods is exogenously given. Thus, the individual's only decision is how to smooth his consumption over time. In the first period, the individual makes his consumption-savings decision so as to maximize eq. (3.2), subject to eq. (3.1). This yields the following consumption Euler equation:

$$u'(c_1 - \alpha\bar{c}_1) = (1+r)\beta u'(c_2 - \alpha\bar{c}_2). \tag{3.3}$$

Intuitively, the individual smooths his consumption in order to equalize the discounted marginal utility of consumption in both periods.

There are a number of ways in which international comparisons might affect an individual's utility. On the one hand, higher economic growth in foreign countries could raise individuals' consumption reference levels in either period. That is, as the individual observes that people in other countries are improving their own economic situation, he might himself be less satisfied with any given level of consumption. This would imply that higher economic growth abroad raises reference levels $\bar{c}_1$ and $\bar{c}_2$. On the other hand, an increase in foreigen economic growth might raise the individual's future income y_2. Better economic performance abroad could eventually spill over and improve economic performance at home. International trade would be an obvious way in which such spillover effects might materialize.

How would these two different effects of foreign economic performance affect individual utility? First, consider an increase in reference levels: $\mathrm{d}\bar{c}_1, \mathrm{d}\bar{c}_2 > 0$. Taking derivatives of eqs. (3.1) and (3.3) with respect to consumption and reference levels, yields the following effects of reference levels on first-period consumption:

$$\frac{\mathrm{d}c_1}{\mathrm{d}\bar{c}_1} = \left(\frac{u''(c_1 - \alpha\bar{c}_1)}{u''(c_1 - \alpha\bar{c}_1) + (1+r)^2 \beta u''(c_2 - \alpha\bar{c}_2)} \right) \alpha \in (0, \alpha), \tag{3.4}$$

$$\frac{\mathrm{d}c_1}{\mathrm{d}\bar{c}_2} = -\left(\frac{(1+r)^2 \beta u''(c_2 - \alpha\bar{c}_2)}{u''(c_1 - \alpha\bar{c}_1) + (1+r)^2 \beta u''(c_2 - \alpha\bar{c}_2)} \right) \frac{\alpha}{1+r} < 0. \tag{3.5}$$

An increase in the first-period reference level raises the marginal utility of first-period consumption and therefore the equilibrium level of first-period consumption. However, as indicated by eq. (3.4), a unit increase in the reference level leads to an increase in consumption that is less than α. As a result, this increase in consumption is insufficient to fully offset the negative utility effect of the higher reference level. Similarly, eq. (3.5) indicates that an increase in the second-period reference level causes the individual to consume less in the first period. It follows that an increase in either reference level always leads to a reduction in utility. This holds for both lifetime utility, as well as first-period subutility:

$$\frac{\mathrm{d}U}{\mathrm{d}\bar{c}_1}, \frac{\mathrm{d}U}{\mathrm{d}\bar{c}_2}, \frac{\mathrm{d}u(c_1 - \alpha\bar{c}_1)}{\mathrm{d}\bar{c}_1}, \frac{\mathrm{d}u(c_1 - \alpha\bar{c}_1)}{\mathrm{d}\bar{c}_2} < 0. \tag{3.6}$$

Thus, economic growth abroad could lead to a reduction in individuals' utility if it leads to higher consumption reference levels.

Second, consider that foreign economic growth raises future income, so that $\mathrm{d}y_2 > 0$. Again taking derivatives of eqs. (3.1) and (3.3), now with respect to consumption and future income levels, yields the following effect of future income on first-period consumption:

$$\frac{\mathrm{d}c_1}{\mathrm{d}y_2} = \left(\frac{(1+r)^2 \beta u''(c_2 - \alpha\bar{c}_2)}{u''(c_1 - \alpha\bar{c}_1) + (1+r)^2 \beta u''(c_2 - \alpha\bar{c}_2)} \right) \frac{1}{1+r} \in \left(0, \frac{1}{1+r}\right). \tag{3.7}$$

As the individual smooths out income shocks over time, higher future income leads to higher consumption in both periods. It immediately follows that this causes an increase in both lifetime utility and first-period subutility:[7]

$$\frac{\mathrm{d}U}{\mathrm{d}y_2}, \frac{\mathrm{d}u(c_1 - \alpha\bar{c}_1)}{\mathrm{d}y_2} > 0. \tag{3.8}$$

Summing up, improved macroeconomic performance abroad might affect an indi-

[7]Notice that people's access to financial markets plays an important role. If people would not be able to borrow – for instance, because they face liquidity constraints – they would be unable to smooth out future increases in income. As a result, their first-period consumption and subutility would remain unaffected. Future utility, as well as lifetime utility, would still increase.

vidual's utility negatively by raising his consumption reference level, or positively by improving future income prospects. We will refer to the former as the social-comparison effect and to the latter as the income-prospect effect. The net effect is ambiguous.

It is important to stress three underlying assumptions for this result to hold. First, individuals need to be aware of the macroeconomic performance of foreign countries. Second, for the negative relationship between utility and foreign economic performance to hold, individuals need to consider foreign people as a reference group. Third, for the positive relationship to hold, individuals need to be aware of economic ties between countries and anticipate that better economic performance abroad will translate into better economic performance at home. We believe it is conceivable that all three assumptions hold in reality. In a globalized world, and after the proliferation of mass media, it has almost become unavoidable that people – intentionally and unintentionally – consume information about the developments within at least some prominent neighboring countries or important trade partners. News media regularly report on the macroeconomic performance of the own country, often benchmarking this against the economic performance in other countries. Nevertheless, we are not able to test these three assumptions directly. Instead, we attempt to measure the resulting effect of foreign economic performance on individuals' utiliy.

3.3 Data and empirical strategy

3.3.1 Measuring utility

We use a combination of individual-level and country-level data for our empirical analysis. Individual-level data are obtained from the World Values Survey (WVS), which contains survey information on a large number of variables for a repeated cross section of individuals. Using the WVS offers several advantages. First of all, the survey has been extensively used in the literature on subjective well-being so that results are comparable to previous studies. More importantly, the WVS includes consistent data for a large number of countries, which we need to obtain enough variation in macroeconomic performance of both individuals' home countries and foreign countries.

The WVS data have been collected for over a hundred countries in six waves between 1981 and 2014. Every country-wave pair typically contains data for a nationally representative sample of approximately 1,000 respondents. We do not impose any restrictions on this dataset, except when necessitated by data availability. Because of a lack of data on some of our main variables for some of the respondents, we ultimately end up using the latest five waves of the WVS, with a sample of approximately 160,000 to 180,000 individuals from about 80 countries, depending on the exact specification of our empirical analysis. Because for many countries data are not available for all waves of the WVS, we arrive at 154 unique country-year pairs.

As discussed, we are interested in explaining individuals' utility. We use self-reported life satisfaction as a measure of utility. Specifically, it is measured by a respondent's answer to the following question:

> All things considered, how satisfied are you with your life as a whole these days?

Possible answers range from 1 ('completely dissatisfied') to 10 ('completely satisfied').

3.3.2 Macroeconomic performance and the choice of reference country

As a measure of macroeconomic performance, for both the home country and for reference countries, we use the growth rate of a country's real gross domestic product (GDP). These data are taken from the World Bank's Development Indicators database. While most studies on social comparison focus on income *levels*, we believe that in an international context economic growth better captures the concept of economic performance. Moreover, economic growth rates are a very common and widely presented economic indicator within the media, and thus, broadly observable for purposes of social comparison.

Even if individuals' utility is affected by the economic performance of a reference group of countries, it is *a priori* unclear which foreign countries comprise this reference group. For this reason, we use different types of reference countries, based on either economic ties or geographical proximity. We first adopt the most important trade partner as a reference country, defined as the foreign country that trades most with the home country. To identify the most important trade partner, we use bilateral trade data from the United Nations Commodity Trade Statistics Database (COMTRADE). As a second possible reference country we focus on neighboring countries. In that case, the macroeconomic performance of the reference country is taken to be the average economic growth of all neighboring countries.[8]

Notice that our theoretical discussion identifies two possible counteracting channels through which foreign countries' economic performance can affect an individual's utility. It might either reduce utility by increasing reference levels, or raise utility through improved income prospects for the home country. Accordingly, it is *a priori* unclear which of the two effects is dominant. However, we would expect that the income-prospect effect is relatively more important if the reference country is based on trade volumes than when it is based purely on geographical proximity. To further exploit this, we also consider neighbors with which the home country trades relatively much and neighbors with which it trades relatively little as separate reference countries. Again, we would expect the economic growth of the former reference group to have a more positive effect on utility than the economic growth of the latter reference group.

3.3.3 Other explanatory variables

When determining the effect of reference countries' economic growth on individuals' life satisfaction, we control for a wide range of confounding variables on both the individual and the country level. Individual-level control variables include: dummies for self-reported income decile; dummies for self-reported health, which ranges from 1 ("very good") to 5 ("very poor"); a dummy for being unemployed; dummies for marital status; a dummy for having children; dummies for age categories (younger than 25, 25 to 34, 35 to 44, 45 to 54, 55 to 64 and older than 65); dummies for having completed secondary education and having a university degree; a dummy for self-reported importance of religion in one's life; and a dummy for whether the subject indicates to be trusting in other people. These variables are often used as control variables in earlier empirical studies on life satisfaction (see, e.g., Helliwell 2003, Blanchflower and Oswald 2004, Clark and Senik 2010).

At the country level we control for the domestic unemployment rate, the inflation rate based on the consumer price index, (the logarithm of) the domestic GDP level, and the domestic growth rate of real GDP, all obtained from the World Bank's Development Indicators. Life satisfaction has been shown to depend on these domestic macroeconomic variables in a number of earlier studies (see, e.g., Di Tella et al. 2003, Blanchflower et al. 2014). Besides these macroeconomic

[8]For an overview of all the countries in the sample, including their neighboring countries and most important trade partners, see Table 3.7 in Appendix 3.A.

variables, we also control for time- and country-fixed effects.

3.3.4 Regression equation and estimation technique

To determine the relationship between utility and foreign economic performance, we estimate the following linear regression equation:

$$\begin{aligned}(3.9) \qquad satisfaction_{i,c,t} = b_0 + b_1 \cdot growth\_ref_{c,t} + \mathbf{b_M} \cdot \mathbf{macroControl}_{c,t} \\ + \mathbf{b_m} \cdot \mathbf{microControl}_{i,c,t} + \mathbf{D}_c + \mathbf{D}_t + e_{i,c,t},\end{aligned}$$

where $satisfaction_{i,c,t}$ refers to life satisfaction of individual i in country c and year t, $growth\_ref_{c,t}$ refers to the economic growth rate in year t of the reference country for individuals in country c, $\mathbf{macroControl}_{c,t}$ is a vector of macroeconomic control variables, and $\mathbf{microControl}_{i,c,t}$ is a vector of individual-level control variables. We moreover include country- and time-fixed effects, denoted by $\mathbf{D}_c$ and $\mathbf{D}_t$. Finally, $e_{i,c,t}$ denotes the error term. Naturally, subjective well-being is in its nature an ordinal concept, so that the applied linear regression model might be prone to error. For this reason we also estimate an ordered probit model and present its results in a section on the robustness of our results. In line with previous studies, we find that, qualitatively, our results are not sensitive to the estimation method we apply (cf. Ferrer-i-Carbonell and Frijters 2004).

We report robust standard errors that account for clustering at the country level (Moulton 1986, 1990). The coefficient b_1 is our coefficient of interest, capturing the effect on life satisfaction of the annual real GDP growth of a reference country. Note that we cannot rule out that variation in the explanatory variables is endogenous with respect to the error term. While we lack quasi-experimental data to truly rule out endogeneity, we try to limit this problem as much as we can by adding important confounding control variables, and by making use of the country-time variation in our data.

3.4 Results

3.4.1 Regression results

The results of estimating eq. 3.9 for various reference groups are reported in Table 3.1.[9] We find for all five specifications that a person's life satisfaction is positively associated with domestic GDP growth. Indeed, a percentage point increase in economic growth leads to an increase in life satisfaction of about 0.03 to 0.04. While at first sight this may appear small, it is economically significant and in line with results from earlier studies (e.g., Di Tella et al. 2003).[10]

Column (1) reports the results for the most important trade partner as reference country. We find that GDP growth in the reference country is in that case also positively associated with individuals' life satisfaction. Indeed, the effect of foreign economic growth on life satisfaction is of the same magnitude as that of domestic economic growth. Our point estimate indicates that a percentage point increase in foreign economic growth is associated with an increase in life satisfaction of 0.037. This finding is in line with an income-prospect effect that is dominant over the social-comparison effect for foreign countries that share an important economic tie with the home country.

Column (2) reports the results when we take the average neighboring country as the relevant reference country. We fail to discern any statistically significant effect of the average economic growth of neighboring countries on life satisfaction. One possible explanation for the latter finding is that the countervailing social-comparison and income-prospect effects cancel out for neighboring countries. For this reason, we separate the neighboring countries of each observation into two groups. One group includes the neighboring countries that are relatively important

[9]Here, we only report the estimated coefficients for the variables of interest. For the full output, see Table 3.2 in Appendix 3.A.

[10]We can also compare the coefficients of other macroeconomic control variables (reported in Table 3.2) to estimates in the literature. For instance, we find in line with Rehdanz and Maddison (2005), who also investigate a large international sample, no correlation between a country's unemployment rate and satisfaction. This is in contrast to other studies that do find a negative correlation between unemployment rates and life satisfaction (e.g., Helliwell 2003, and Alesina et al. 2004). Note that the correlation between the unemployment rate and life satisfaction could be positive when individuals who are already unemployed suffer less from this in situations of more widely spread unemployment (for micro evidence on such effects see, for instance, Clark (2003), and Knabe et al. (2012)). In line with other studies, we find a negative coefficient for the inflation rate in four out of five specifications (e.g., Di Tella et al. 2001). However, the coefficient of inflation is only statistically significant in two out of five specifications. Finally, the (natural log) of the level of real GDP per capita exhibits a positive coefficient in all specifications (in line with, e.g., Di Tella et al. 2003) that is marginally significant in three out of five specifications.

trade partners, and a second group includes those that are relatively unimportant trade partners. Whether the neighboring country is a relatively important trade partner or not is determined on the basis of median bilateral trade volume of the home country with its neighboring countries. We estimate a regression that includes the average economic growth rates for both above-median-trade neighbors and below-median-trade neighbors. The results are reported in column (3).

In line with our expectation, the findings in column (3) indicate that life satisfaction is positively associated with the economic growth in high-trade neighboring countries, and negatively associated with the economic growth in low-trade neighboring countries. Our point estimates suggest that a percentage point increase in the economic growth of high-trade neighbors is associated with a 0.029-point *increase* in life satisfaction, whereas a percentage point increase in the economic growth of low-trade neighbors is associated with a 0.024-point *decrease* in life satisfaction. This is in line with a dominant income-prospect effect for neighbors that share an important economic tie with the home country, and a dominant social-comparison effect for low-trade neighboring countries.

Finally, we apply as a reference growth rate the real GDP growth of the neighboring country that, of all neighboring countries, exhibits either the lowest or the highest trade volume with the home country. In column (4), we find a negative effect for the neighbor with the lowest trade volume, consistent with a dominant social-comparison effect. The magnitude of this effect is comparable to that for neighbors with a below-median trade volume as reported in column (3). In column (5), we find a positive effect for the neighbor with the highest trade volume, consistent with a dominant income-prospect effect. The magnitude of this effect is comparable to that for neighbors with an above-median trade volume.[11]

[11]Note that the number of observations varies across specifications (1)–(5) of Table 3.1. We lose some observations because trade volume data is missing for a number of country-year pairs, which lowers observations for columns (1) and (3)–(5). In columns (3)–(5) we further exclude observations that only have one neighboring country, as this would make it impossible to divide the group of neighboring countries into sub-groups. As a robustness test, we could impute missing data for country-year pairs by using trade volume data of earlier years. After all, there is only limited variation in the most important trade partner over time (see Table 3.7 in Appendix 3.A). Using these imputed data, we can estimate the same regression as reported in column (1) for more observations. Results from doing so are in line with our findings in Table 3.1 (results are available on request).

	(1) life satisf.	(2) life satisf.	(3) life satisf.	(4) life satisf.	(5) life satisf.
GDP growth	0.039*** (0.008)	0.025*** (0.009)	0.042*** (0.010)	0.046*** (0.011)	0.0352*** (0.009)
Max.-trade partner growth	0.037*** (0.011)				
Neighbors' av. growth		0.028 (0.022)			
Above-med.-trade neighbors' av. growth			0.029** (0.012)		
Below-med.-trade neighbors' av. growth			-0.024* (0.012)		
Max.-trade neighbor growth					0.022* (0.012)
Min.-trade neighbor growth				-0.017** (0.008)	
Constant	3.271* (1.730)	4.423** (2.148)	3.287** (1.475)	3.409** (1.627)	3.466** (1.608)
Country-level controls	Yes	Yes	Yes	Yes	Yes
Individual-level Controls	Yes	Yes	Yes	Yes	Yes
Country and time fixed effects	Yes	Yes	Yes	Yes	Yes
N	166526	182783	157699	157699	157699
R^2	0.272	0.281	0.271	0.270	0.270

Note: Linear repeated cross-sectional regression model with country and time fixed effects. Dependent variable: life satisfaction. Cluster-adjusted, heteroskedasticity robust standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. Country-level controls include: natural log of real GDP per capita, inflation rate (consumer price index) and unemployment rate. Individual-level controls include: controls for income, health, age, education, marital/relationship status, gender, children, religiosity, trust in other people.

Table 3.1: International income-prospect and social-comparison effects

3.4.2 Robustness

Our results are robust to a number of alternative model specifications. In our first robustness analysis we make use of additional individual-level information obtained from the World Values Survey. More specifically, one question in the survey allows us to infer to what extent individuals care about future economic growth in their home country. From a list of different potential aims for the country in the next ten years – including economic growth, strong defense forces, more freedom, and a better environment – respondents are asked which of them they would give first and second priority. Recall that, in previous sections, we hypothesized that income-prospect effects create a positive relationship between subjective well-being and foreign economic growth because the individual expects this growth to spill over to future economic growth in the home country. According to this logic, the more one cares about future economic growth in the home country, the more important such spillover effects should be. As a result, we expect the evidence of income-prospect effects to be particularly strong among those individuals who indicate that they would give high priority to future economic growth.

To test this, we create a dummy variable that equals one for individuals who indicate that future economic growth is *not* a first priority for them – constituting about 40 percent of the individuals in our dataset. We then rerun the regressions from the previous subsection, but this time add a variable that interacts this dummy with the economic growth in the reference country. The results are reported in Table 3.3 in Appendix 3.A. In line with our expectations, we find evidence that the income-prospect effect is especially strong among individuals who care about future economic growth. That is, columns (1), (3), and (5) show that the economic growth of important trade partners is positively associated with the subjective well-being of individuals who give high priority to future economic growth. This effect is smaller for individuals who do not give priority to future economic growth – as indicated by the negative and statistically significant coefficients for the interaction term. Furthermore note that we do not find a similar effect for the economic growth in (low-trade) neighboring countries, as shown in columns (2)–(4). This is what one would expect if the spillover effects from economic growth in these countries are less important than those of important trade partners.

If the positive well-being effect of important trade partners' economic growth is caused by income-prospect effects, then this effect might be most pronounced for economies that are particularly dependent on their export sector. Individuals might anticipate that expected future income is more affected by the growth of trade partners when the domestic export sector is relatively large. To elaborate on this idea we estimate an interaction model based on the specification in column (1) of Table 3.1. We interact GDP growth of the most important trade partner with a dummy variable that takes the value one if the home country has a relatively small export sector – i.e., when the proportion of exports in total GDP is smaller than the median value across the countries in our sample. Results are reported in column (1) of Table 3.4 in Appendix 3.A. For countries with a relatively large export sector, we find that the effect of the economic growth of the most important trade partner is significantly positive. Moreover, the coefficient's point estimate, 0.051, is larger than in the baseline specification, although this difference is statistically insignificant. The effect for countries with a relatively small export sector appears to be somewhat smaller, but the insignificant interaction term indicates that there is no significantly different effect compared to countries with a large export sector. Thus, we cannot reject the hypothesis that income-prospect effects are equally strong for countries with small or large export sectors.

For about a third of the countries in our sample, the most important trade partner is also a neighboring country. In those cases, it is possible that the positive well-being effect of the trade partner's economic growth is driven by commuting individuals who work in the country of the trade partner. In this case, learning about this country's GDP growth might have more direct implications for the personal income of individuals that is not directly related to the trade channel that we discuss above. One might wonder whether the results we find in specification (1) of Table 3.1 still hold when we focus on most important trade partners that are not neighboring countries, for which results cannot be driven by commuting individuals. When we estimate specification (1) in Table 3.1 for a sub-sample of countries for which the most important trade partner is not a neighboring country, we find that the coefficient of the reference country's economic growth increases slightly, but less than the respective standard error. As a result, p-values rise to 0.15, which is above conventional significance levels. When we estimate an interaction model to analyze whether there is a significant difference for the effect of the most important trade partner's GDP growth between the two sub-samples we find no evidence of such a difference. The estimates of the interaction model are reported in column (2) of Table 3.4 in Appendix 3.A.[12]

[12]To ensure that our results are not driven by the fact that some people in our sample are foreign citizens, and therefore might have a direct interest in higher growth abroad, we also rerun the regressions from the previous section on a subsample that excludes non-citizens. As they

To test our results' robustness to alternative estimation methods, we re-estimate the specifications presented in the previous subsection using an ordered probit model. As satisfaction is an ordinal concept, treating life satisfaction implicitly as a cardinal measure in our linear regression model could be problematic. The non-linear ordered probit model accounts for the ordinal dependent variable. Results are reported in Table 3.5 in Appendix 3.A and show that all of our main findings are robust to using an ordered probit model.[13]

Data such as the GDP growth or unemployment rates are usually published with a time lag. Furthermore, the initially published data might well be revised after some time. Therefore, recorded macroeconomic variables in our data might not perfectly coincide with the data that individuals actually observed at each point in time, leading to potential measurement error. To address this issue, we replace the macroeconomic variables by the average of their values in the survey year and their values in the year before. Using these averages ensures that we use information over a longer period and possibly get closer to the actual information that individuals observed at each point in time. As can be seen from Table 3.6 in the Appendix, results are largely robust to using such alternative explanatory variables.[14,15]

Finally, we experiment with the cluster-adjusted standard errors. Most studies that combine life satisfaction data with country-level data implement cluster-adjusted standard errors on the country-year level.[16] Contrary to this, we rely on somewhat more restrictive country-level cluster-adjusted standard errors in the estimates presented in the previous subsection. In line with what one would expect, when we apply country-year level cluster-adjusted standard errors, we obtain

represent little more than one percent of the people in our sample, excluding them from the analysis leaves all results virtually unaffected. Estimates are available on request.

[13] This is in line with Ferrer-i-Carbonell and Frijters (2004) who find that using linear OLS or models that assume ordinality makes little difference for estimations on life satisfaction.

[14] All coefficients remain very similar in sign and size. For the above-median-trade neighbors' (in specification 3) and minimum-trade neighbor (specification 4), standard errors increase slightly so that the coefficients are no longer significant. For the most important trade neighbor (specification 5), the coefficient increases and becomes more significant.

[15] One might argue that income-prospect effects are determined by forecasted rather than actual economic growth rates of important trade partners. Unfortunately, we do not have access to such forecasts for enough countries in our sample. Instead, we could use one-year leading values of economic growth – assuming that future realized economic growth rates are unbiased approximations for its earlier forecasts. When we estimate specification (1) in Table 3.1 relying on one year leading values of the most important trade partner's GDP growth rates, we find that results are robust.

[16] For recent examples, see Becchetti et al. (2013), Blanchflower et al. (2014), and Clark et al. (2015).

robust and statistically more significant results than the findings reported above.[17]

3.5 Conclusion

We take an international perspective on the tunnel effect – first discussed by Hirschman (1973) – and estimate the relationship between individuals' life satisfaction and the economic growth of reference countries. In line with the discussion by Hirschman, we find evidence for both social-comparison and income-prospect effects. When we focus on the most important trade partner as the reference country, we find a positive relationship between economic growth and life satisfaction. When we consider neighboring countries instead, we find that life satisfaction relates negatively to the economic growth of neighbors that exhibit relatively little economic integration with the home country.[18] At the same time, life satisfaction relates positively to the economic growth of neighbors that are economically more integrated with the home country. These findings suggest that income-prospect effects dominate in case of economically integrated reference countries, while comparison concerns dominate for less economically integrated countries.

We started this paper with the observation that international help for crisis-struck countries is often justified with the claim that other countries' economic betterment is beneficial for the own population. We find direct evidence in favor of this claim only when it regards the economic performance of countries that are economically relatively integrated with the own country. Our results thereby suggest a beneficial role for economic integration that goes beyond standard international-trade arguments. Economic integration further aligns economic interests, and it might therefore have the potential to enhance people's international solidarity and the political feasibility of international cooperation.[19]

[17] Results applying country-year cluster-adjusted standard errors are available on request.

[18] Our results also relate to the Easterlin (1974) paradox which states that subjective well-being remained relatively flat even though per capita income has sharply increased over the past decades (e.g., see for a recent discussion Weimann et al. 2015). A typical explanation for this paradox is that people derive satisfaction from their own situation relative to local reference groups – colleagues, family members, and the like. Our evidence suggests that similar comparison effects might also play a role on an international level. Thus, even when the home country's GDP does grow over time, any positive effect on subjective well-being might be neutralized by relatively higher economic growth of (low-trade) neighbors.

[19] Indeed, the idea that economic integration would lead to a path of political integration is at least as old as European cooperation, and was deliberately expressed by the founding fathers of the European Union. See, for instance, the so called Spaak Report from April 21st, 1956. The Spaak Report is the outcome of the experts group set up by the Messina Conference which led to the creation of the European Economic Community, available at www.cvce.lu.

Appendix

3.A Appendix

Table 3.2

	(1) life satisf.	(2) life satisf.	(3) life satisf.	(4) life satisf.	(5) life satisf.
GDP growth	0.039*** (0.008)	0.025*** (0.009)	0.042*** (0.010)	0.046*** (0.011)	0.035*** (0.009)
Max.-trade partner growth	0.037*** (0.011)				
Neighbors' av. growth		0.028 (0.022)			
Above-median trade neighbors' av. growth			0.029** (0.012)		
Below-median trade neighbors' av. growth			-0.024* (0.012)		
Max.-trade neighbor growth					0.022* (0.012)
Min.-trade neighbor growth				-0.017** (0.008)	
Ln GDP per capita	0.398* (0.214)	0.410 (0.251)	0.366* (0.176)	0.374* (0.198)	0.336 (0.206)
unemployment	0.015 (0.022)	-0.020 (0.020)	0.027 (0.023)	0.023 (0.024)	0.019 (0.024)
inflation rate	-0.001 (0.000)	-0.001 (0.001)	-0.001** (0.000)	-0.001** (0.000)	0.001 (0.000)

(continued on the next page)

Table 3.2 *(continued)*	(1) life satisf.	(2) life satisf.	(3) life satisf.	(4) life satisf.	(5) life satisf.
2^{nd} income decile	0.119	0.177**	0.123	0.126	0.125
	(0.075)	(0.076)	(0.078)	(0.078)	(0.079)
3^{rd} income decile	0.231***	0.290***	0.243***	0.243***	0.243***
	(0.086)	(0.087)	(0.089)	(0.090)	(0.090)
4^{th} income decile	0.495***	0.551***	0.513***	0.517***	0.518***
	(0.090)	(0.096)	(0.094)	(0.094)	(0.094)
5^{th} income decile	0.684***	0.734***	0.700***	0.701***	0.703***
	(0.098)	(0.102)	(0.101)	(0.101)	(0.101)
6^{th} income decile	0.875***	0.930***	0.897***	0.897***	0.898***
	(0.104)	(0.109)	(0.107)	(0.108)	(0.108)
7^{th} income decile	1.079***	1.137***	1.107***	1.106***	1.108***
	(0.109)	(0.112)	(0.112)	(0.112)	(0.112)
8^{th} income decile	1.193***	1.262***	1.239***	1.237***	1.239***
	(0.121)	(0.123)	(0.123)	(0.124)	(0.123)
9^{th} income decile	1.219***	1.304***	1.272***	1.267***	1.274***
	(0.122)	(0.127)	(0.125)	(0.125)	(0.125)
10^{th} income decile	1.240***	1.337***	1.295***	1.285***	1.292***
	(0.136)	(0.141)	(0.144)	(0.143)	(0.143)

(continued on the next page)

Table 3.2 *(continued)*

	(1) life satisf.	(2) life satisf.	(3) life satisf.	(4) life satisf.	(5) life satisf.
unemployed	-0.348*** (0.045)	-0.344*** (0.044)	-0.349*** (0.047)	-0.349*** (0.047)	-0.348*** (0.047)
Good health	-0.568*** (0.031)	-0.577*** (0.029)	-0.564*** (0.033)	-0.566*** (0.033)	-0.566*** (0.033)
Faire health	-1.215*** (0.043)	-1.222*** (0.040)	-1.200*** (0.045)	-1.204*** (0.045)	-1.204*** (0.045)
Poor health	-2.172*** (0.063)	-2.143*** (0.062)	-2.151*** (0.065)	-2.154*** (0.065)	-2.153*** (0.065)
Very poor health	-2.582*** (0.112)	-2.693*** (0.098)	-2.578*** (0.114)	-2.598*** (0.116)	-2.588*** (0.112)
gender (male=1)	-0.135*** (0.023)	-0.137*** (0.022)	-0.135*** (0.024)	-0.134*** (0.024)	-0.134*** (0.024)
married	0.311*** (0.042)	0.306*** (0.042)	0.310*** (0.044)	0.309*** (0.044)	0.308*** (0.044)
living as married	0.190*** (0.041)	0.172*** (0.041)	0.186*** (0.044)	0.184*** (0.043)	0.187*** (0.043)

(continued on the next page)

Table 3.2 *(continued)*

	(1) life satisf.	(2) life satisf.	(3) life satisf.	(4) life satisf.	(5) life satisf.
Divorced	-0.189*** (0.044)	-0.185*** (0.046)	-0.191*** (0.047)	-0.192*** (0.047)	-0.191*** (0.047)
Separated	-0.183*** (0.049)	-0.204*** (0.047)	-0.190*** (0.053)	-0.192*** (0.053)	-0.189*** (0.053)
Widowed	-0.044 (0.056)	-0.037 (0.054)	-0.053 (0.057)	-0.053 (0.057)	-0.052 (0.057)
Children	-0.033 (0.024)	-0.032 (0.026)	-0.040 (0.025)	-0.040 (0.025)	-0.040 (0.025)
Age (25-34)	-0.233*** (0.021)	-0.242*** (0.021)	-0.225*** (0.020)	-0.225*** (0.020)	-0.225*** (0.020)
Age (35-44)	-0.305*** (0.034)	-0.313*** (0.035)	-0.298*** (0.035)	-0.299*** (0.035)	-0.296*** (0.034)
Age (45-54)	-0.270*** (0.039)	-0.275*** (0.040)	-0.268*** (0.040)	-0.268*** (0.040)	-0.267*** (0.040)
Age (55-64)	-0.077* (0.045)	-0.077 (0.047)	-0.086* (0.046)	-0.087* (0.046)	-0.084* (0.046)
Age (64<)	0.215*** (0.062)	0.204*** (0.061)	0.195*** (0.065)	0.194*** (0.064)	0.198*** (0.064)

(continued on the next page)

Table 3.2 *(continued)*

	(1) life satisf.	(2) life satisf.	(3) life satisf.	(4) life satisf.	(5) life satisf.
Education (university)	0.050	0.061	0.057	0.055	0.055
	(0.040)	(0.038)	(0.043)	(0.043)	(0.043)
Education (secondary)	0.006	0.014	0.007	0.006	0.008
	(0.026)	(0.025)	(0.028)	(0.028)	(0.027)
Trust	0.158***	0.174***	0.158***	0.158***	0.158***
	(0.034)	(0.033)	(0.036)	(0.036)	(0.036)
Religious	0.318***	0.313***	0.321***	0.322***	0.322***
	(0.027)	(0.027)	(0.028)	(0.028)	(0.028)
Constant	3.271*	4.423**	3.287**	3.409**	3.466**
	(1.730)	(2.148)	(1.475)	(1.627)	(1.608)
Country-level controls	Yes	Yes	Yes	Yes	Yes
Individual-level Controls	Yes	Yes	Yes	Yes	Yes
Country and time fixed effects	Yes	Yes	Yes	Yes	Yes
N	166526	182783	157699	157699	157699
R^2	0.272	0.281	0.271	0.270	0.270

Note: Linear repeated cross-sectional regression model. Dependent variable: life satisfaction. At the country level cluster-adjusted, heteroskedasticity robust standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. Country-level controls include: log of real GDP per capita, inflation rate (consumer price index) and unemployment rate. Individual-level controls include: dummies for self-reported income group (1 (lowest) to 10 (highest), base category: 1), dummies for self-reported health (1 ("very good") to 5 ("very poor"), base category: 1), a dummy for being unemployed, dummies for being married, divorced, widowed, or living separated (base category: being single), a dummy for having children, dummies for age categories (25-34, 35-44, 45-54, 55-64, 65+, base category: younger than 25), a dummy for having completed secondary education, a dummy for having a university degree, a dummy for considering religion to be very important in one's life, a dummy for being trusting in other people.

Table 3.2: International income-prospect and social-comparison effects

Table 3.3

	(1) life satisf.	(2) life satisf.	(3) life satisf.	(4) life satisf.	(5) life satisf.
GDP growth	0.039^{***} (0.008)	0.042^{***} (0.010)	0.042^{***} (0.010)	0.047^{***} (0.011)	0.034^{***} (0.009)
Max.-trade partner growth	0.039^{***} (0.011)				
Max.-trade partner growth * growth-not-priority	-0.014^{***} (0.004)				
Neighbors' av.growth		0.035 (0.022)			
Neighbors' av.growth * growth-not-priority		-0.008 (0.006)			
Above-med.-trade neighbors' av. gr.			0.034^{***} (0.011)		
Above-med.-trade neighbors' av. growth * growth-not-priority			-0.012^{***} (0.004)		
Below-med.-trade neighbors' av.growth			-0.021^{*} (0.012)		
Below-med.-trade neighbors' av.growth growth-not-priority			-0.001 (0.005)		

(continued on the next page)

Table 3.3 *(continued)*

	(1) life satisf.	(2) life satisf.	(3) life satisf.	(4) life satisf.	(5) life satisf.
Max.-trade neighbor growth					0.030***
					(0.012)
Max.-trade neighbor growth * growth-not-priority					−0.011***
					(0.004)
Min.-trade neighbor growth				-0.020**	
				(0.009)	
Min.-trade neighbor growth * growth-not-priority				-0.000	
				(0.004)	
Growth-not-priority	0.051*	0.052*	0.056**	0.009	0.051**
	(0.021)	(0.028)	(0.024)	(0.024)	(0.020)
Constant	3.162*	4.378**	3.2655**	3.926**	3.958**
	(1.813)	(1.935)	(1.443)	(1.641)	(1.602)
Country-level controls	Yes	Yes	Yes	Yes	Yes
Individual-level controls	Yes	Yes	Yes	Yes	Yes
Country and time fixed effects	Yes	Yes	Yes	Yes	Yes
N	159585	171875	152140	152140	152140
R^2	0.268	0.281	0.271	0.270	0.270

Note: Linear repeated cross-sectional regression model with country and time fixed effects. Dependent variable: life satisfaction. Cluster-adjusted, heteroskedasticity robust standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. "growth-not-priority" is a dummy variable that takes a value of 0 for individuals that consider economic growth to be an important aim for their home country for the next ten years and 1 otherwise. Country-level controls include: the natural log of real GDP per capita, inflation rate (consumer price index) and unemployment rate. Individual-level controls include: controls for income, health, age, education, marital/relationship status, gender, children, religiosity, trust in other people.

Table 3.3: Interaction model: Economic growth as priority

	Export sector	Neighboring max. trade partner
	(1)	(2)
	life satisf.	life satisf.
GDP growth	0.040*** (0.008)	0.039*** (0.008)
Max.-trade partner growth	0.051** (0.024)	0.039 (0.026)
Max.-trade-partner gr *small export	-0.021 (0.025)	
Small export	0.16 (0.481)	
Max.-trade-partner gr *max.-trade-neighbor		-0.007 (0.028)
Max.-trade-neighbor		0.075 (0.143)
Constant	3.084** (1.424)	2.934 (1.923)
Country-level controls	Yes	Yes
Individual-level Controls	Yes	Yes
Country and time fixed effects	Yes	Yes
N	166526	166526
R^2	0.272	0.272

Note: Linear repeated cross-sectional regression model with country and time fixed effects. Dependent variable: life satisfaction. Cluster-adjusted, heteroskedasticity robust standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01."Small export" is a dummy that takes a value of 1 for below-median export-to-GDP ratio countries and 0 otherwise. "Max.-trade-neighbor" is a dummy that takes a value of 1 when a country's most important trade partner is a geographically neighboring country and 0 otherwise. Country-level controls include: log of real GDP per capita, inflation rate (consumer price index) and unemployment rate. Individual-level controls include: controls for income, health, age, education, marital/relationship status, gender, children, religiosity, trust in other people.

Table 3.4: Large/small export sectors and neighboring max. trade partners

	(1) life satisf.	(2) life satisf.	(3) life satisf.	(4) life satisf.	(5) life satisf.
GDP growth	0.019*** (0.004)	0.012*** (0.004)	0.020*** (0.005)	0.022*** (0.005)	0.017*** (0.005)
Max.-trade partner growth	0.016*** (0.005)				
Neighbors' av. growth		0.015 (0.011)			
Above-median trade neighbors' av. growth			0.015** (0.006)		
Below-median trade neighbors' av. growth			-0.010* (0.006)		
Max.-trade neighbor growth					0.011* (0.006)
Min.-trade neighbor growth				-0.007* (0.004)	
Country-level controls	Yes	Yes	Yes	Yes	Yes
Individual-level Controls	Yes	Yes	Yes	Yes	Yes
Country and time fixed effects	Yes	Yes	Yes	Yes	Yes
N	166526	182783	157699	157699	157699

Note: Ordered probit repeated cross-sectional regression model with country and time dummies. Dependent variable: life satisfaction. Cluster-adjusted standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. Country-level controls include: log of real GDP per capita, inflation rate (consumer price index) and unemployment rate. Individual-level controls include: controls for income, health, age, education, marital/relationship status, gender, children, religiosity, trust in other people.

Table 3.5: Income-prospect and comparison effects: Ordered probit

	(1) life satisf.	(2) life satisf.	(3) life satisf.	(4) life satisf.	(5) life satisf.
GDP growth (mlag)	0.028** (0.014)	0.031* (0.017)	0.023 (0.015)	0.033* (0.017)	0.015 (0.015)
Max.-trade partner growth (mlag)	0.039*** (0.012)				
Neighbors' av. growth (mlag)		0.019 (0.024)			
Above-med.-trade neighbors' av. growth (mlag)			0.028 (0.017)		
Below-med.-trade neighbors' av. growth (mlag)			-0.024* (0.014)		
Max.-trade neighbor growth (mlag)					0.033** (0.014)
Min.-trade neighbor growth (mlag)				-0.018 (0.014)	
Constant	6.056*** (2.202)	8.929*** (3.356)	7.048*** (2.312)	6.483*** (2.286)	7.390*** (2.347)
Country-level controls	Yes	Yes	Yes	Yes	Yes
Individual-level Controls	Yes	Yes	Yes	Yes	Yes
Country and time fixed effects	Yes	Yes	Yes	Yes	Yes
N	166526	182783	157699	157699	157699
R^2	0.272	0.282	0.271	0.270	0.270

Note: Linear repeated cross-sectional regression model with country and time fixed effects. Dependent variable: life satisfaction. Cluster-adjusted, heteroskedasticity robust standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. Country-level controls include the mean of the current value and first lag of: the natural log of real GDP per capita, inflation rate (consumer price index) and unemployment rate. Individual-level controls include: controls for income, health, age, education, marital/relationship status, gender, children, religiosity, trust in other people.

Table 3.6: Timing of macroeconomic variables: Mean current value and first lag

Table 3.7

Country	Neighboring countries	Country level obs.	Individ. level obs.	Waves	Max trade partner
Algeria	Libya Mali Mauritania Morocco Niger Tunisia	1	844	1999-2004	United States
Armenia	Azerbaijan Georgia Iran Turkey	2	2,734	1994-1998 2010-2014	Russia Russia
Australia	Indonesia New Zealand Papua New Guinea Solomon Islands Vanuatu	3	3,934	1994-1998 2005-2009 2010-2014	Japan Japan China
Azerbaijan	Armenia Georgia Iran Russia Turkey	2	2,556	1994-1998 2010-2014	Russia Italy
Bangladesh	India Myanar (Burma)	2	2,395	1994-1998 1999-2004	United States United States
Belarus	Latvia Lithuania Poland Russia Ukraine	2	3,063	1994-1998 2010-2014	- [a] Russia
Brazil	Argentina Bolivia Colombia French Guiana Guyana Paraguay Peru Suriname Uruguay Venezuela	2	2,974	1989-1993 2005-2009	United States United States

(continued on the next page)

Table 3.7 *(continued)*

Country	Neighboring countries	Country level obs.	Individ. level obs.	Waves	Max trade partner
Bulgaria	Greece Macedonia Romania Serbia Turkey	2	1,422	1994-1998 2005-2009	Russia Germany
Burkina Faso	Benin Côte d'Ivoire Ghana Mali Niger Togo	1	645	2005-2009	Switzerland
Canada	United States	2	3,402	1999-2004 2005-2009	United States United States
Chile	Argentina Bolivia Peru	1	856	2010-2014	China
China	Afghanistan Bangladesh Bhutan Hong Kong India Japan Kazakhstan Kyrgyzstan Laos Mongolia Myanmar (Burma) Nepal North Korea Russia South Korea Taiwan Tajikistan Viet Nam	4	4,558	1994-1998 1999-2004 2005-2009 2010-2014	Japan Japan United States United States

(continued on the next page)

Table 3.7 *(continued)*

Country	Neighboring countries	Country level obs.	Individ. level obs.	Waves	Max trade partner
Colombia	Brazil Ecuador Panama Peru Venezuela	2	4,386	1994-1998 2010-2014	United States United States
Cyprus	Greece Turkey	2	1,943	2005-2009 2010-2014	Greece Greece
Dominican Rep.	Haiti Puerto Rico	1	279	1994-1998	- [a]
Egypt	Israel Jordan Libya Saudi Arabia Sudan	2	4,204	1999-2004 2005-2009	United States United States
El Salvador	Guatemala Honduras	1	994	1999-2004	United States
Estonia	Latvia Russia	2	2,392	1994-1998 2010-2014	Finnland Russia
Ethiopia	Eritrea Djibouti Kenya Somalia Sudan	1	1,126	2005-2009	China
Finland	Norway Russia Sweden	2	1,777	1994-1998 2005-2009	Germany Germany
France	Andorra Belgium Germany Great Britain Italy Luxembourg Spain Switzerland	1	869	2005-2009	Germany

(continued on the next page)

Table 3.7 *(continued)*

Country	Neighboring countries	Country level obs.	Individ. level obs.	Waves	Max trade partner
Georgia	Armenia Azerbaijan Russia Turkey	1	1,396	2005-2009	Turkey
Germany	Austria Belgium Czech Rep. Denmark France Luxembourg Netherlands Poland Switzerland	2	3,199	1994-1998 2005-2009	France France
Ghana	Burkina Faso Côte d'Ivoire Togo	2	2,592	2005-2009 2010-2014	South Africa South Africa
Great Britain	Belgium France Ireland Netherlands	1	761	2005-2009	Germany
Guatemala	Belize El Salvador Honduras Mexico	1	904	1999-2004	United States
Hong Kong	China	1	959	2005-2009	China
Hungary	Austria Croatia Romania Serbia Slovakia Slovenia Ukraine	1	941	2005-2009	Germany

(continued on the next page)

Table 3.7 *(continued)*

Country	Neighboring countries	Country level obs.	Individ. level obs.	Waves	Max trade partner
India	Bangladesh Bhutan China Myanmar (Burma) Nepal Pakistan Sri Lanka	3	3,626	1994-1998 1999-2004 2005-2009	United States United States United States
Indonesia	Australia Malaysia Papua New Guinea Philippines Singapore	2	2,286	1999-2004 2005-2009	Japan Japan
Iran	Afghanistan Armenia Azerbaijan Iraq Pakistan Turkey Turkmenistan	2	3,644	1999-2004 2005-2009	Japan Japan
Iraq	Iran Jordan Kuwait Saudi Arabia Syria Turkey	3	4,761	1999-2004 2005-2009 2010-2014	- [a] - [a] - [a]
Italy	Austria France Slovenia Switzerland	1	606	2005-2009	Germany
Japan	China South Korea Russia	3	3,157	1999-2004 2005-2009 2010-2014	United States United States China
Jordan	Egypt Iraq Israel Saudi Arabia Syria	1	995	1999-2004	Iraq

(continued on the next page)

Table 3.7 *(continued)*

Country	Neighboring countries	Country level obs.	Individ. level obs.	Waves	Max trade partner
Kazakhstan	China Kyrgyzstan Russia Turkmenistan Uzbekistan	1	1,497	2010-2014	China
Kyrgyzstan	China Kazakhstan Tajikistan Uzbekistan	2	2,368	1999-2004 2010-2014	Russia Russia
Latvia	Belarus Estonia Lithuania Russia	1	1,036	1994-1998	Russia
Lithuania	Belarus Latvia Poland Russia	1	829	1994-1998	Russia
Macedonia	Albania Bulgaria Greece Kosovo Serbia	2	1,531	1994-1998 1999-2004	Germany Germany
Malaysia	Indonesia Philippines Singapore Thailand Viet Nam	2	2,416	2005-2009 2010-2014	United States China
Mali	Algeria Burkina Faso Côte d'Ivoire Guinea Mauritania Niger Senegal	1	458	2005-2009	South Africa

(continued on the next page)

Table 3.7 *(continued)*

Country	Neighboring countries	Country level obs.	Individ. level obs.	Waves	Max trade partner
Mexico	Belize Guatemala United States	4	6,038	1994-1998 1999-2004 2005-2009 2010-2014	United States United States United States United States
Moldova	Romania Ukraine	3	2,706	1994-1998 1999-2004 2005-2009	Russia Russia Ukraine
Morocco	Algeria	3	1,191	1999-2004 2005-2009 2010-2014	France France France
Netherlands	Belgium Germany Great Britain	2	2,235	2005-2009 2010-2014	Germany Germany
New Zealand	Australia	3	2,273	1994-1998 1999-2004 2010-2014	Australia Australia China
Nigeria	Benin Cameroon Chad Niger	3	4,793	1994-1998 1999-2004 2010-2014	United States United States United States
Norway	Finland Russia Sweden	2	1,959	1994-1998 2005-2009	Great Britain Great Britain
Pakistan	Afghanistan India Iran	2	1,952	1999-2004 2010-2014	- [a] United Arab Emirates
Peru	Bolivia Brazil Chile Colombia Ecuador	4	4,810	1994-1998 1999-2004 2005-2009 2010-2014	United States United States United States United States
Philippines	Indonesia Malaysia	2	2,330	1999-2004 2010-2014	United States Japan

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Table 3.7 *(continued)*

Country	Neighboring countries	Country level obs.	Individ. level obs.	Waves	Max trade partner
Poland	Belarus Czech Rep. Germany Lithuania Russia Slovakia Ukraine	2	1,770	2005-2009 2010-2014	Germany Germany
Qatar	Bahrain Saudi Arabia United Arab Emirates	1	993	2010-2014	- [a]
Romania	Bulgaria Hungary Moldova Serebia Ukraine	3	3,878	1994-1998 2005-2009 2010-2014	Italy Italy Germany
Russia	Azerbaijan Belarus China Estonia Finland Georgia Japan Kazakhstan Latvia Lithuania Mongolia North Korea Norway Poland Ukraine	3	5,332	1994-1998 2005-2009 2010-2014	- [a] Germany China
Rwanda	Burundi D.R. Kongo Tanzania Uganda	2	2,214	2005-2009 2010-2014	Kenya Tanzania

(continued on the next page)

Table 3.7 *(continued)*

Country	Neighboring countries	Country level obs.	Individ. level obs.	Waves	Max trade partner
Saudi Arabia	Bahrain Iraq Jordan Kuwait Oman Qatar United Arab Emirates Egypt Yemen	1	1,281	1999-2004	- [a]
Serbia	Albania Bosnia Bulgaria Croatia Hungary Romania Macedonia Montenegro Kosovo	2	2,052	1994-1998 1999-2004	- [a] - [a]
Singapore	Indonesia Malaysia	1	1,833	2010-2014	China
Slovakia	Austria Czech Rep. Hungary Poland Ukraine	1	833	1994-1998	Germany
Slovenia	Austria Croatia Hungary Italy	2	1,904	2005-2009 2010-2014	Germany Germany
South Africa	Botswana Lesotho Mozambique Namibia Swaziland Zimbabwe	2	4,714	1994-1998 1999-2004	- [a] United States
South Korea	China Japan North Korea	3	3,455	1999-2004 2005-2009 2010-2014	United States China China

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Table 3.7 *(continued)*

Country	Neighboring countries	Country level obs.	Individ. level obs.	Waves	Max trade partner
Spain	Andorra France Gibraltar Portugal	4	3,670	1994-1998 1999-2004 2005-2009 2010-2014	France France France France
Sweden	Denmark Finland Norway	3	2,850	1994-1998 2005-2009 2010-2014	Germany Germany Germany
Switzerland	Austria France Germany Italy Lichtenstein	2	1,875	1994-1998 2005-2009	Germany Germany
Tanzania	Burundi D.R. Kongo Kenya Malawi Mozambique Rwanda Uganda Zambia	1	831	1999-2004	Great Britain
Thailand	Cambodia Laos Malaysia Myanmar (Burma)	1	1,354	2005-2009	Japan
Trinidad and Tobago	Barbados Venezuela	2	1,909	2005-2009 2010-2014	United States United States
Turkey	Armenia Bulgaria Cyprus Georgia Greece Iran Iraq Syria	4	6,702	1994-1998 1999-2004 2005-2009 2010-2014	Germany Germany Russia Germany

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Table 3.7 *(continued)*

Country	Neighboring countries	Country level obs.	Individ. level obs.	Waves	Max trade partner
Uganda	D.R. Kongo Kenya Rwanda Sudan Tanzania	1	538	1999-2004	Kenya
Ukraine	Belarus Hungary Moldova Poland Romania Russia Slovakia	3	4,087	1994-1998 2005-2009 2010-2014	Russia Russia Russia
United States	Canada Mexico	3	4,507	1994-1998 1999-2004 2010-2014	Canada Canada Canada
Uruguay	Argentina Brazil	3	2,536	1994-1998 2005-2009 2010-2014	Brazil Argentina Brazil
Viet Nam	Cambodia China Laos	2	2,239	1999-2004 2005-2009	Japan China
Zambia	Angola Botswana D.R. Kongo Malawi Mozambique Namibia Zimbabwe Tanzania	1	1,063	2005-2009	South Africa
Zimbabwe	Botswana Mozambique Namibia South Africa Zambia	1	761	1999-2004	South Africa
Total		154	182,783		

Note: [a] no COMTRADE data available

Table 3.7: Sample countries

Chapter 4

You are not alone: Experimental evidence of risk taking when social comparisons matter

4.1 Introduction

Since Veblen (1899) economists have considered income comparisons to be an important objective for individuals that yields manifold implications in economics. While theorists extended the analysis of income comparisons to its consequences for taking risk (e.g., Robson 1992, Konrad and Lommerud 1993, Becker et al. 2005) less empirical evidence has since followed. We contribute to the literature by investigating the case of income-rank comparisons in a laboratory experiment. Our theoretical predictions show that, compared to standard expected utility theory, income-rank comparisons lead to less (more) risk taking in case of lotteries with more downside (upside) probability mass. Overall, we find that individuals respond in their risk taking to comparison concerns. Individuals take less risk when lotteries have more probability weight on the downside. However, we do not find an effect for lotteries with more upside probability mass. Individuals respond more strongly to comparison concerns when reference subjects are of the same sex.

The economic outcomes of others matter for individuals in many contexts (e.g., Sobel 2005, Fehr and Schmidt 2006). However, empirical research did not embrace the interaction of social comparisons and decisions under uncertainty until relatively recently even though decisions involving risk usually take place in a social context.[1] Individuals usually face decisions on pursuing personal education, choos-

[1]Early experimental papers on social concerns and risk taking investigate, for instance, pro-

ing an occupation, making savings or financial investment decisions while being aware of the situation of others. Consider an investor facing the decision on how to allocate his savings to financial investment opportunities. He could invest a major part of his funds in risky stocks or rather in a low return but safe savings account. While his investment choice will impact his future consumption, at the same time, his investments will have implications for his relative income position, for instance, relative to his sparkish brother-in-law. Higher financial risk might offer the chance to do better or fall behind in terms of income. As the investor of this example, we are not alone in this world but we have siblings, colleagues, friends, and neighbors. When we feel comparison concerns toward one or the other it seems natural that we incorporate not only the implications for the absolute but also for the relative income into our risk taking decisions.

To investigate the effect of comparisons on risk taking, we first need to take a stand in which form comparison concerns enter the individual utility. One possibility is that individuals care about their rank in the income distribution, as first formalized by Frank (1985b), thereby assuming that comparison concerns are intrinsically ordinal.[2] Alternatively, the distance between the own income and the income of others might enter the utility, implying that income comparison concerns are cardinal.[3] While the empirical literature generally finds that individuals care about income comparisons (see, e.g., Easterlin 1995, Clark and Oswald 1996, McBride 2001, Ferrer-i-Carbonell 2005, and Luttmer 2005) few studies refer to a possible difference between comparison considerations based on the rank in the income distribution and the distance in income to others.[4] In the following, we focus on income-rank comparisons but address preferences for the distance in incomes in an additional treatment.

cedural fairness (e.g., Bolton et al. 2005, Krawczyk and Le Lec 2010) or the relationship of other regarding concerns and risk attitudes (e.g., Brennan et al. 2008, Güth et al. 2008). For an overview of the literature on social concerns and risk attitudes see Trautmann and Vieider (2012).

[2]Also, see Frank (2013) for applications of utility that is dependent on the rank relative to others beyond monetary wealth or income.

[3]The assumption that individuals care about the distance in incomes was first formalized by Duesenberry (1949) (see Clark et al. 2008 for an overview of the literature). Note that the choice to apply the distance or the rank in income has an impact on the results in many theoretical applications (Bilancini and Boncinelli 2008).

[4]One exception is a study by Clark et al. (2009). They find that a lower rank in the local income distribution of a neighborhood relates negatively to subjective well-being. However, conditional on the personal income, having richer neighbors shows a positive effect. Clark et al. (2009) argue that having rich rather than poor neighbors provides other positive externalities than implied by relative standing concerns. These positive externalities could explain the positive effect of richer neighbors on subjective well-being.

Robson (1992) and Konrad and Lommerud (1993) are the first to theoretically analyze the impact of social comparisons on risk attitudes. The former study assumes income-rank comparisons while the latter models income comparisons as the distance between incomes. Following Robson (1992) we rely on income-rank comparisons to derive theoretical hypotheses. Inspired by Konrad's and Lommerud's (1993) distinction hat the correlation structure of own risks with risks of others can matter, we analyze in our baseline scenario an environment where the risk of individuals is perfectly correlated. In an additional treatment we vary the correlation structure and investigate the case of uncorrelated risk.

At first sight, the effect of income-rank comparisons on risk taking behavior is not obvious and one might come up with examples concluding that income-rank comparisons simply induce more risk taking (e.g., Frank 2008, p. 1778). We show that the properties of the respective probability distribution of a lottery are crucial factors. Compared to standard expected utility theory subjects are expected to take more risk in case of lotteries with more upside probability mass but less risk in case of more probability weight on the downside. Intuitively, individuals shift more income to the state of the world that is more likely to materialize so that they are more likely to be ahead in income-rank once this state does materialize. We apply these predictions to empirically test the effect of income-rank comparisons on risk taking.

An empirical analysis of the impact of social comparisons on individual decisions is intrinsically difficult in the field. The controlled environment in the laboratory allows us to focus on a clean isolation of the effect of comparison concerns on risk taking. We can compare situations, in which we eliminate income comparisons to the greatest possible extent, to similar situations, in which we expect income comparisons. In the experiment, subjects face classical portfolio choice decisions where they allocate their endowment between a risky lottery and a risk-free investment opportunity. The risky lotteries differ in terms of upside and downside probability mass (i.e., whether a gain or loss is more likely) but offer identical expected returns. In the baseline treatment "*Alone*" subjects face portfolio choices while being alone in the laboratory so that income comparisons should not be of any immediate concern. In the reference treatment "*Social*" subjects enter the laboratory in groups of two and simultaneously face the same portfolio choices as the other participant. The change in the share of the endowment that subjects invest into the risky lottery, comparing treatments *Alone* and *Social*, allows us to investigate the effect of income comparisons on risk taking. Both treatments *Alone* and *Social* are identical except for the present second participant. Importantly, subjects neither observe decisions nor earnings of the other participant nor do they interact, so that we can rule out other peer effects.

We find evidence of an effect of income comparisons on risk taking. In line with our theoretical predictions, individuals take less risk for lotteries with more downside probability mass. However, we do not find a significant effect in the case of more upside probability weight. We test whether this asymmetric finding can be explained by other forms of comparison concerns, such as a preference for the distance in incomes à la Konrad and Lommerud (1993) or social loss aversion. To do so we run an additional treatment in which income risk is uncorrelated among participants. Consequently, we increase the risk for the *relative* income position while we leave the risk for the personal income unchanged. In contrast to preferences for the distance in incomes and social loss aversion we find that the effect of comparisons on risk taking turns out to be similar but weaker in the additional treatment. Furthermore, for all treatments holds that the effect of comparison considerations on risk taking is predominantly present in situations where the reference subject is of the same sex as the deciding subject. Thus, attributes of reference subjects are a crucial piece of the picture and Festinger's (1954) idea of social comparisons being stronger among more similar individuals seems to extend to the income and risk taking dimension. Independent of the reference subject, measured effects appear to be strongest for female individuals.

We contribute to a small but growing number of experimental studies that are inspired by social comparisons and risk taking.[5] These studies generated partly contradicting results. For instance, Rohde and Rohde (2011) do not find evidence of social comparisons affecting risk taking. However, applying physiological measures, Bault et al. (2008) and Bault et al. (2011) find that lottery outcomes do affect subjects differently for different lottery outcomes of others. They find that social gains lead to stronger responses than social losses. Focusing directly on risk taking behavior, Linde and Sonnemans (2012) find that subjects take less risk when they can win at most as much as a certain payoff of a reference subject (social loss situation) compared to the case when they can win at least as much as a reference subject (social gain situation). In contrast to Linde and Sonnemans, Schwerter

[5]Comparison concerns are effectively a form of peer effects and our work also relates to the literature on peer effects under uncertainty (e.g., Bougheas et al. 2013, Cooper and Rege 2011, Viscusi et al. 2011). In this strand of the literature the work of Lahno and Serra-Garcia (2015) is closest to us. They investigate peer effects in a binary lottery choice setting and find that the probability to switch a previous stated lottery choice can be explained by concerns about the observed choices of other individuals and by relative payoff concerns.

(2013) finds that subjects take more risk when they observe a higher rather than a lower certain payoff of others, in order to surpass or to stay ahead of others. Consequently, Schwerter interprets his results in favor of social loss aversion while Linde and Sonnemans argue that their findings suggest that loss aversion does not easily extend to the case of social comparisons.[6] Motivated by the insurance market, Friedl et al. (2014) find experimental evidence that social comparisons make insurance policies less attractive when risks are correlated. Dijk et al. (2014) focus on rank-comparisons and performance pay and apply a dynamic setting where subjects accumulate investment returns over several rounds. They find evidence that under-performers adjust their investments toward positively skewed assets while over-performers adjust their investments toward negatively skewed assets. This holds similarly for situations when subjects only observe the performance of others, and for situations when individuals are actually paid according to their relative performance.

The setup of this paper deviates from the previous studies in important aspects. First, in our experiment, subjects neither observe decisions of others (as in many peer effect studies) nor what others earn (as, for instance, in Linde and Sonnemans 2012, Schwerter 2013 or Dijk et al. 2014). The former point is important to control for any sort of peer effect that is not related to comparison concerns, the latter feature ensures that subjects do not adjust expectations about likely experimental earnings when they observe earnings of others before they make their decisions. Second, while other studies vary payoffs or lotteries of reference subjects when social comparison concerns are at work, we identify the effect of comparisons on risk taking by comparing situations *with* social comparisons to situations *without* social comparisons. Third, in contrast to other studies we apply a classical portfolio choice setting.[7] Finally, while for many studies the theoretical starting point is an extension of Kahneman's and Tversky's (1979) prospect theory to the social dimension, we focus on income-rank comparisons and subsequently analyze whether other forms of social comparisons, such as social loss aversion, can explain our results.[8,9]

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[6]Recently, Grimm et al. (2015) also find evidence that individuals more often choose more risky decisions when they are in a disadvantaged initial position relative to another party.

[7]In the experimental literature a portfolio choice setup to investigate risk taking was first introduced by Gneezy and Potters (1997) and has been applied in different contexts (e.g., Sutter 2007, Charness and Gneezy 2010).

[8]Note that the results of the studies investigating social loss aversion do not necessarily disagree with income-rank comparisons (e.g., Schwerter's 2013 results seem to be in line with income-rank-dependent preferences).

[9]The effect of preferences for distributional fairness (i.e., people dislike inequity) on risk taking

4.2 Theoretical framework

4.2.1 Risk taking and income-rank comparisons

Consider a model with two individuals $i \in \{1, 2\}$. Individuals care about absolute income $y_i \in \mathbb{R}_+$ and the rank in income. Their utility function exhibits the following form:

$$U = u(y_i) + S(y_i, y_{-i}), \tag{4.1}$$

$$with\ S(\cdot) = \begin{cases} W & if\ y_i > y_{-i} \\ 0 & if\ y_i = y_{-i} \\ L & if\ y_i < y_{-i} \end{cases}, \quad where\ W > 0 > L. \tag{4.2}$$

While the part $u(y_i)$ represents a standard (consumption) part of the utility function $S(y_i, y_{-i})$ adds a positive, negative or zero utility value depending on the relative income position to the other individual $-i$. Income y_i depends on the endowment E that is identical for individuals and normalized to one, and on i's investment decision of allocating E across a risky and a risk-free investment opportunity. The risky investment offers a return $\theta_G > 1$ with a probability p_G and a return of $\theta_B = 0$ with a probability $1 - p_G$ and is perfectly correlated for both subjects. The risk-free investment allows individuals to store income at zero costs (return of 1). We assume that $p_G\theta_G > 1$ holds at any time to keep the problem non-trivial in the case of risk aversion. Defining a_i as share of E invested into the risky project, expected income is

$$E(y_i) = 1 + (p_G\theta_G - 1)a_i. \tag{4.3}$$

As we focus on the effect of income-rank-dependent utility on risk taking decisions, we abstract from strategic interaction between individuals to simplify the analysis. Thus, we assume that individual 1 actively chooses the share of risky investment a_1, while individual 2 is passive in the sense that a_2 is randomly drawn from a continuous uniform probability distribution $F(a_2)$ with a corresponding

also attracted some interest in the literature. Recently Bolton et al. (2015) find no evidence that social risk taking is driven by this type of preferences. Other studies that investigate social risk taking and whose results do not fit social preferences for distributional fairness include Bereby-Meyer and Roth (2006), Güth et al. (2008), Brennan et al. (2008), Bolton and Ockenfels (2010) and Cappelen et al. (2013).

probability density function $f(a_2)$. The active individual faces the following maximization problem:

$$\max_{a_1} E[u(y_1(a_1)) + S(y_1(a_1), y_2(a_2))]. \tag{4.4}$$

Abstracting from $S(y_1, y_2)$ for a moment and only considering the standard part of the utility function $E[u(y_1(a_1))]$, it is straightforward to derive an optimal share a_1^* that depends on risk preferences when we rely on standard assumptions with respect to $u(\cdot)$. Starting from a_1^* the question of interest can be reformulated as follows: How does risk taking, i.e. the optimal choice of a_1, change when the income comparison part $S(y_1, y_2)$ enters the situation? This can be done by considering the following trade-off: adjusting a_1 away from a_1^* causes costs related to the standard part of the utility function, $u(y_1(a_1))$, but offers the chance to gather an income comparison gain (or loss) due to $S(y_1, y_2)$. Starting from the optimum without income comparisons, a_1^*, we can define the costs of deviating from a_1^* as

$$C(a_1) = E[u(y_1(a_1^*))] - E[u(y_1(a_1))], \tag{4.5}$$

where the first term on the right-hand side is a constant representing the utility level of the standard part of the utility function given the optimal a_1^*.[10] It follows that $C(a_1^*) = C'(a_1^*) = 0$ as a minimum, $C'(a_1) > 0$ if $a_1 > a_1^*$ and $C'(a_1) < 0$ if $a_1 < a_1^*$. A sufficient condition for this cost function to be convex, $C''(a_1) > 0$, is that $u(y_1(a_1))$ is strictly concave, and thus, that individuals are risk averse.[11,12] The maximization problem of individual 1 can be reformulated to

$$\begin{aligned} \max_{a_1} \pi_1 &= p_G \times [F(a_1)W + (1 - F(a_1))L] \\ &\quad + (1 - p_G) \times [F(a_1)L + (1 - F(a_1))W] - C(a_1). \end{aligned} \tag{4.6}$$

The income comparison gain or loss for individual 1 in (4.6) is determined by the outcome of the risky investment and by the personal share invested into the

[10]Note that we define $C(\cdot)$ in utils rather than pecuniary certainty equivalents. Although this approach seems a bit unconventional, it proves to be constructive for our purpose.

[11]For a derivation of the sufficient condition for the convexity of $C(\cdot)$ see Appendix 4.B.1.

[12]We assume from now on that individuals are risk averse in the standard part of the utility function (i.e. $u(y_1(a_1))$ is strictly concave).

risky asset a_1 relative to the other individual's share a_2. Note that both individuals face a perfectly correlated investment project, and thus, both individuals will end up receiving simultaneously the return θ_G in the good state or a return of 0 in the bad state. Therefore, in the good state individual 1 will gain comparison utility $S^1(y_1(a_1), y_2(a_2)) = W$ iff $a_1 > a_2$ and lose utility $S^1(y_1(a_1), y_2(a_2)) = L$ iff $a_1 < a_2$. Analog, in the bad state $S^1(y_1(a_1), y_2(a_2)) = L$ iff $a_1 > a_2$ and $S^1(y_1(a_1), y_2(a_2)) = W$ iff $a_1 < a_2$. Solving problem (4.6) yields the first-order condition

$$f(a_1^s)[W - L][2p_G - 1] = C'(a_1^s). \tag{4.7}$$

When the left-hand side of (4.7) is larger than zero, $C'(a_1^s)$ must be positive, and hence, the optimal share invested into the risky project when incorporating income-rank comparisons (a_1^s) must be larger than the optimal share without comparison considerations (a_1^*), i.e. we observe $a_1^s > a_1^*$ (more risk taking). Analog follows that when the left-hand side of equation (4.7) is smaller than zero, $a_1^s < a_1^*$ must hold (i.e. subjects take less risk when incorporating income-rank comparisons). When the left-hand side of (4.7) is zero $a_1^s = a_1^*$ holds.

Considering $W > 0 > L$ and $f(a_1)$ being a density function, it is easy to see that the sign of the left-hand side of (4.7) is solely determined by p_G. When $p_G > \frac{1}{2}$ holds, the sign of the left-hand side of (4.7) will be positive, while in case of $p_G < \frac{1}{2}$ the sign will be negative. For $p_G = \frac{1}{2}$ the left-hand side of (4.7) will be zero. Intuitively, introducing income-rank comparisons will lead to more (less) risk taking in case more probability mass is on the good (bad) possible outcome of the risky project (i.e. more upside (downside) probability weight). Income-rank comparison will have no effect on risk taking in case of symmetric lotteries. Note that this holds even though we hold the expected return of the risky project constant. The first-order condition is sufficient as $C(a_1)$ is convex and as we assume that a_2 is uniformly distributed (i.e. $f(a_1)$ in (4.7) is a constant).

Furthermore, we can consider the effect of a shift in probability weight between the bad and the good state on equilibrium risk taking. From (4.7) we see that[13]

$$\frac{\partial a_1}{\partial p_G} = \frac{2f(a_1) \times (W - L) + (\theta_G - 1) \times u'(a_1\theta_G + (1 - a_1)) + u'(1 - a_1)}{C''(a_1)} > 0, \tag{4.8}$$

again relying on $C(a_1)$ being convex, $W > 0 > L$ and $f(a_1)$ being a constant.

[13]Note that $C'(a_1)$ depends directly on p_G (see equation (4.11) in Appendix 4.B.1).

Equation (4.8) shows that income-rank comparisons lead to an equilibrium share invested into the risky project that is increasing in p_G. Consequently, comparing lotteries with more upside (downside) probability weight, the equilibrium risk taking is higher for increasing upside probability weight (increasing in p_G) and lower for decreasing downside probability weight (decreasing in $(1-p_G)$). Put differently, the *absolute* effect of income-rank comparisons on risk taking is *increasing* for more asymmetrically allocated probability weight to the upside or to the downside.

4.2.2 Experimental design

The main part of the experiment consists of two treatments that are implemented in a between-subjects design. In each treatment subjects receive an endowment of 175 experimental currency units (ECU) that they allocate between a risky project (i.e. a lottery) and a risk-free project. ECUs invested into the risk-free project will be paid to subjects one-to-one while the return of the lottery depends on the outcome of independent dice rolls at the end of the experiment. A lottery pays a return $\theta_G > 1$ with some probability p_G while with a probability of $(1 - p_G)$ the lottery pays nothing.[14]

Before the experiment starts subjects participate in a quiz in which each subject has to answer questions to ensure a good understanding of the experiment.[15] Subjects play 9 independent rounds. In each round subjects receive a new endowment and face a new lottery that exhibits different probabilities p_G and $(1 - p_G)$ and different payoffs but an identical expected payoff of $p_G \times \theta_G$ (Table 4.1 shows all lotteries).[16] The risk-free project remains unchanged over all rounds.

The computer randomly selects exactly one of the 9 rounds for payment.[17] At

[14] In the following we refer to p_G as upside probability and to $1 - p_G$ as downside probability.

[15] More precisely, subjects observe an investment decision and have to answer questions about implications for the payoff given certain realizations of the lottery. At the beginning of the quiz subjects are asked to enter a number between 0 and 175 without knowing for what reason. This stated number determines the investment decision for the example that is used during the quiz. This twist allows us to present an example without introducing a default investment decision of the experimenter that could possibly influence the subsequent decisions of subjects during the main part of the experiment.

[16] Lotteries with different probabilities p_G and $(1-p_G)$ ensure that each subject faces lotteries with more upside and lotteries with more downside probability weight.

[17] In principle, participants might perceive the 9 independent rounds as one compounded lottery even though at the end of the experiment exactly one round is randomly drawn and payoff relevant. If so, this would introduce an effect of earlier rounds on later rounds. However, such a behavior seems unlikely to be at work. First, in the instructions subjects are explicitly told to consider each round as independent from the earlier rounds. Second, usually narrow bracketing is a common behavior of individuals, and thus, we should expect that subjects treat the lotteries in different rounds as independent lotteries (for instance, see Rabin and Weizsäcker 2009). Finally, to

Lottery	Probabilities		Return (multipliers)		Exp. Return (multipliers)	Std. Dev.
	Good state	Bad state	Good state	Bad state		
n	p_G	$(1-p_G)$	$\frac{\theta_G}{a_l \times 175}$	θ_B	$\frac{\theta_G}{a_l \times 175} \times p_G$	
more downside prob. mass						
L_1	33%	67%	3.75	0	1.25	1.77
L_3	17%	83%	7.5	0	1.25	2.90
L_5	8%	92%	15	0	1.25	4.15
L_7	6%	94%	22.5	0	1.25	5.15
more upside prob. mass						
L_2	67%	33%	1.88	0	1.25	0.56
L_4	83%	17%	1.5	0	1.25	0.88
L_6	92%	8%	1.36	0	1.25	0.38
L_8	94%	6%	1.32	0	1.25	0.30
Symmetric						
L_9	50%	50%	2.5	0	1.25	1.25

Note: Each subject plays all lotteries and participates in one treatment only (between-subjects design). Returns of lotteries are stated as the multiplier for the invested experimental currency units (ECUs). All probabilities are explained to subjects in terms of dice roll results, and thus, all probabilities are presented to subjects in an intuitive way. For example, consider Lottery 1 (L_1): in case the experimenter rolls a 6 using a regular die (i.e. out of 1, ..., 6) the subject's invested ECUs will be multiplied by 3.75, converted to Euros and paid to the subject at the end of the experiment. When the outcome is 5 or below the subject won't receive any money from the risky project (the invested ECUs are multiplied by zero). While we focus in our analysis on lotteries with more upside or downside probability mass according to our theoretical predictions, we also implement one symmetric lottery (L_9) that we can use for a plausibility check.

Table 4.1: Lotteries

the end of the experiment the lottery of the selected round appears on the screen of the participant. The experimenter enters the room and rolls dice clearly visible for the subject using a dice cup.[18] The experimenter enters the results of the dice rolls into the computer and payoffs are accordingly calculated by the computer.[19,20]

Treatment *Alone* In the baseline treatment *Alone* each session consists of one subject so that each subject is alone in the room during the experiment. Nothing in the instructions indicates that other participants take part in the experiment. Subjects are invited with a time-lag to ensure that they never meet and that they are alone in the laboratory. During the experiment subjects observe the lottery for the current decision on a screen that is placed next to them but in the middle of the room. Subjects enter their decisions into a "private" screen in front of them.[21]

Treatment *Social* In treatment *Social* two subjects participate in each session and participants are aware of the other participant.[22] In the laboratory room subjects sit in cubicles to ensure privacy. Importantly, both subjects know that in each round they invest into the same lottery as the other subject and that the final realization of the lottery at the end of the experiment is the same for both subjects. Subjects observe this "social" lottery on a single screen placed in the middle of the room. All decisions of subjects are entered into fields on their private screen in front of them. Before the experiment starts, subjects learn that one participant will be passive while the other will actively invest. It is commonly known that for the passive participant the computer randomly draws an investment share a_2 for the investment into the risky lottery from an unknown distribution. The com-

control for such a possible, effect we implement two groups of subjects that see the 9 independent rounds in different sequences. As subjects only learn about lotteries of a round once a round starts, a different sequence of rounds leaves participants with a different history of information at the beginning of each round. Because we do not observe a significant difference between the two groups we conclude that participants indeed treat rounds as independent situations.

[18]We implement physical dice rolls as this is a generally accepted, trustworthy mechanism to implement randomness. We intend to avoid the possibility that subjects doubt the fair randomness implementation of a computer (that is basically a "black box").

[19]The experimenter does not observe the investment decision of the subject when the dice roll results are entered into the computer, and thus, the experimenter does not learn the experimental income of the subjects.

[20]Dependent on the lottery, the experimenter rolls one die or two dice (also see Table 4.1).

[21]See Figure 4.3 in Appendix 4.A for an illustration of the setup.

[22]Subjects had to wait in front of the laboratory rooms (only for treatment *Social*). However, in most cases subjects sat separated by several chairs between them and we observed no interaction between subjects.

puter draws a_2 anew and independently for each round and displays the current randomly drawn a_2 on the *private* screen of the *passive* subject only. The other participant decides actively on her investment share a_1, i.e. how much to invest into the risky lottery. While the active subject is aware of the passivity of the other subject the active subject never observes the passive subject's investments share a_2.[23] Analog, also the passive subject never observes the active participant's decision a_1.

The design feature of assigning one subject to be passive is important to rule out strategic interaction, but also costly due to many incentivized passive subjects to generate observations. To make use of passive subjects we introduce a simple measure of subjective well-being in all treatments that we can later apply to test the effect of income-rank comparisons along this dimension. After each investment decision (or random draw in case of passive subjects) each participant is asked for her satisfaction with respect to the current lottery on a scale of 0 to 10. To test the effect of income-rank comparisons using measured satisfaction we need a baseline treatment analog to treatment *Alone*.

Treatment *Alone Passive* This treatment is identical to treament *Alone* but that the single subject of each session is assigend to be passive meaning that the computer randomly draws a from some distribution identical to the case explained above.

4.2.3 Experimental procedures

The experiment was programmed and conducted using the experiment software z-Tree (Fischbacher 2007) and run at the Max Planck Laboratory for Experimental Research in Social Science (econlab) in Munich. The participating subjects are enrolled at the Technical University of Munich and the University of Munich, and recruited from all different fields of study using ORSEE (Greiner 2004); for summary statistics and an overview of the treatments see tables 4.5, 4.6, and 4.7 in Appendix 4.A. After having completed the main experiment, subjects answer 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 test to assess risk aversion (Holt and Laury 2002).[24] During the incentivized

[23] For more details see the instructions presented in Appendix 4.C.

[24] We also implement as an alternative risk aversion measure a general risk-question (Dohmen et al. 2011).

post-experimental tasks participants earned on average 1.2 Euros on top of the earnings from the main part of the experiment. Overall, participants earned on average 14.8 Euros including a show-up fee of 6 Euros and a session approximately lasted for 45 minutes.

4.2.4 Predictions

Pairwise comparisons between treatments allow us to investigate whether income-rank comparison considerations affect risk taking. While subjects face identical investment decisions in all treatments, in treatment *Alone* the investment decision affects only the personal income while in treatment *Social* the investment decision will also determine the relative performance (rank) compared to another participant in the laboratory. Thus, we can test the effect of income-rank-dependent utility analyzing the investment choice a_1: Assuming that subjects care about their rank relative to another participant they adjust the share invested into the risky lottery, a_1, when we compare situations where investment decisions have implications for the relative income position (rank) (in treatment *Social)* to situations where investment decisions have solely implications for the personal income (in treatment *Alone*):

Prediction 4.1 *Comparing treatment Social to treatment Alone,*
(i) for lotteries with more downside than upside probability mass ($p_G < \frac{1}{2}$) subjects invest a lower share a_1 into the risky lottery (take less risk);
(ii) for lotteries with more upside than downside probability mass ($p_G > \frac{1}{2}$) subjects invest a higher share a_1 into the risky lottery (take more risk).

Prediction 4.1 emanates from equation (4.7) of Section 4.2.1. Intuitively, the probability to be ahead of the other participant increases when subjects adjust their investment (relative to the situations without comparison considerations) so that the income is higher in the state that most probably materializes.[25] We test Prediction 4.1 against the alternative hypothesis that individuals do not experience comparison concerns, and thus, that investment is similar in all treatments.

We can further analyze implications of income-rank comparison considerations for risk taking by considering the treatment difference between *Social* and *Alone* for lotteries with more or less asymmetrically allocated probability mass:

[25] More precisely, subjects tradeoff an expected utility gain (loss) from being ahead (behind) in rank with costs of deviating from their investment choice that would be optimal abstracting from comparison considerations (see Section 4.2.1).

Prediction 4.2 *Comparing treatment Social to treatment Alone,*
(i) for lotteries with more downside than upside probability mass subjects' investment into the risky lottery a_1 *(risk taking) is decreasing in the downside probability mass* $(1 - p_G)$*;*
(ii) for lotteries with more upside than downside probability mass subjects' investment into the risky lottery a_1 *(risk taking) is increasing in the upside probability mass* p_G*.*

Prediction 4.2 follows from equation (4.8) of Section 4.2.1 and intuitively states that the absolute effect of income-rank comparisons on risk taking is larger when probability weights of a lottery are more asymmetrically allocated to either the downside or the upside. To test Prediction 4.2 we will exploit variation in the lotteries that subjects face and analyze the treatment difference of a_1 not only for lotteries with asymmetrically allocated probability mass but also for *more or less* asymmetrically allocated probability mass.

Focusing on passive subjects and measured satisfaction we can derive further testable predictions in line with Section 4.2.1. Passive subjects cannot adjust their investment, and thus, we can implement identical investment shares a_2 for both treatments *Social* and *Alone Passive*.[26] Thus, for passive subjects, comparing situations in which lotteries have implications for the rank in income (in treatment *Social*) to situation where lotteries have solely implications for the personal income (in treatment *Alone Passive*) participants should be less (more) satisfied when they are more likely to be behind (ahead) in expected income. To assess whether one is more likely to be ahead or behind, first, subjects need to form expectations about a plausible probability distribution of active subjects' investment choices a_1. Intuitively, such plausible expectations should incorporate that active subjects take less (more) risk in situations of more risky (less risky) lotteries.[27] From this intuition, it follows that passive subjects should expect to be more likely to be behind in *income-rank* when lotteries have more upside probability weight and their randomly drawn investment share a_2 is very low or when lotteries have more downside probability weight and their investment share a_2 is very high.[28] Thus,

[26]Recall, that a_2 (a_1) represents the share of risky investment for passive (active) subjects.

[27]This reasoning assumes that passive subjects anticipate that active subjects behave on average risk averse even for small stakes, a common behavior among individuals as a large body of experimental literature shows. Also note that the empirical distribution of investment decisions in this study also clearly shows that subjects behave risk averse in their investment decisions, and thus, passive subjects are correct when expecting risk averse behavior of active subjects.

[28]Again, this follows from Section 4.2.1. Also note that lotteries with more probability mass on the downside exhibit by construction higher standard deviations (are more risky) than lotteries

in such situations passive subjects should be less satisfied in treatment *Social* compared to treatment *Alone Passive*.

Prediction 4.3 *Comparing treatment Social to treatment Alone Passive, passive subjects are less satisfied*
(i) in situations of lotteries with more upside than downside probability mass and low a_2*, and*
(ii) in situations of lotteries with more downside than upside probability mass and high a_2*.*

To test Prediction 4.3 we implement groups of passive subjects with high and low randomly drawn a_2.[29] Furthermore, we ensure that randomly drawn investment shares a_2 of each group are identical between treatments *Social* and *Alone Passive* to perfectly control for any effect of a_2 on satisfaction that is unrelated to comparison considerations.[30,31]

with more probability mass on the upside (see Table 4.1).

[29]We implement two groups of passive subjects: a "high-risk" group that receives high risk shares and a "low-risk" group that receives low risk shares. In a first step, we recorded the empirical distribution of the first 10 active subjects in treatment *Alone*. Second, each group of passive subjects receives random draws from a different part of this recorded empirical distribution. The "high-risk" group receives random draws from the right part outside the 95% confidence interval of the empirical distribution of risk shares (i.e. relatively high a_2). The "low-risk" group receives random draws from the left part outside the 95% confidence interval of the empirical distribution of risk shares (i.e. relatively low a_2). Note that all risk-shares are determined (drawn) exactly once for each group and each portfolio choice problem, and thus, risk shares for each group of passive subjects are identical in both treatments, *Social* and *Alone Passive*.

[30]Note that analog to Prediction 4.3, one might suspect that passive subjects should be more satisfied when being more likely to be ahead in rank when comparing treatment *Social* to treatment *Alone*. One might argue that this is the case in situations of lotteries with more upside probability weight and very high a_2 and in situations of lotteries with more downside probability weight and very low randomly drawn a_2. However, the case of being ahead is generally less clear because active subjects should react strategically to the comparison situations in treatment *Social* (in line with Section 4.2.1). Consequently, the uncertainty of being ahead in income in such situations is larger compared to being behind in income (as stated in Prediction 4.3) because active subjects will adjust their investment decision a_1 into the *unfavorable* direction for passive subjects.

[31]Note that it is not precisely clear how high (low) the randomly drawn investment share a_2 must be to observe Prediction 4.3. In principle, this depends on the expectations of passive subjects: subjects should be more (less) satisfied in treatment *Social* compared to treatment *Alone Passive* whenever they expect $a_2 > a_1$ ($a_2 < a_1$), meaning a higher (lower) risky investment than the active subject in situations of lotteries with more upside probability weight. For lotteries with more downside probability weight passive subjects should be more (less) satisfied in treatment *Social* whenever they expect $a_2 < a_1$ ($a_2 > a_1$).

4.3 Results

In a nutshell, we find that income comparisons lead to less risk taking in case of lotteries with more downside probability weight. For lotteries with more upside probability weight we find no significant effect. Furthermore, the characteristics of the reference individual are crucial for the effect of income comparisons on risk taking. Effects are stronger when the deciding (active) subject and the reference (passive) subject exhibit the same sex, and thus, when subjects are more similar. Independent of the reference subject, measured effects appear to be strongest for female subjects.

Before we continue with the main analysis it is important to note that investment choices and satisfaction are sensitive to different portfolio choice situations. For instance, subjects invest a higher share of their endowment into the risky lottery (i.e. higher risk share a_1) when lotteries are less risky. Figure 4.4 in Appendix 4.A shows that subjects invest on average 59.0 percent of their endowment into the risky lottery when lotteries have a below median standard deviation compared to on average only 33.9 percent in case of an above median standard deviation lotteries.[32] A negative correlation coefficient of -0.38 between the standard deviation of lotteries and the risk share a_1 confirms this result. When we consider measured risk aversion we find a negative relationship between being more risk averse and higher risk shares a_1 with a correlation coefficient of -0.20, and thus, more risk averse subjects invest indeed less into the risky lottery choice just as one would expect.[33]

Finally, when we eyeball recorded satisfaction of passive subjects we observe that subjects report satisfaction in a sensible way. In case of an above median standard deviation lotteries (i.e. higher risk) passive subjects for which the computer draws a high (low) risk share are generally less (more) satisfied (see the left graph in Figure 4.5, Appendix 4.A). In case of a below median standard deviation (i.e. lower risk) lotteries passive subjects are more (less) satisfied if they receive a

[32]Note that by construction all lotteries with an above (below) median standard deviation are lotteries with more downside (upside) probability weight (see Table 4.1). However, here we focus on risk defined as variation (std. dev.) rather than on the asymmetric allocation of probability mass for the sake of this argument.

[33]We use a risk aversion measure à la Holt and Laury (2002) that essentially applies a multiple price list (MPL) of binary choices between lotteries. Note that this risk aversion measure is considerably less incentivized compared to the main part of the experiment. Thus, the consistent results support the general notion that the risk aversion measure à la Holt and Laury works well in the pecuniary dimension even when using comparably small stakes to incentivize subjects.

high (low) risk share (see the right graph in Figure 4.5, Appendix 4.A).[34]

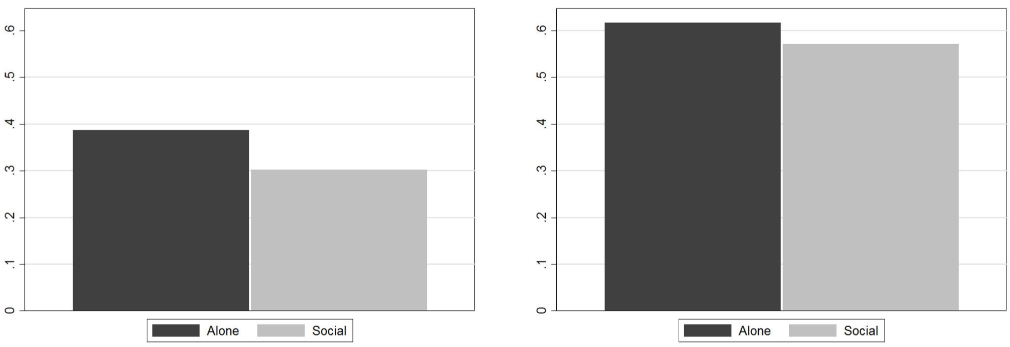
4.3.1 Individual risk taking

To investigate the effect of social comparisons on risk taking we consider the change in investment (risk share) comparing treatment *Alone* and treatment *Social*. For a first test of Prediction 4.1 we separately compare risk taking behavior for lotteries with more downside and lotteries with more upside probability mass between treatments *Alone* and *Social*.[35] Our starting point is a first glance at simple averages. Figure 4.1 shows for more downside probability weighted lotteries a lower average risk share of 0.30 in treatment *Social* compared to an average risk share of 0.39 in treatment *Alone* in line with Prediction 4.1. Thus, in case of lotteries with more downside than upside probability mass subjects invest on average about 9% less of their endowment into the risky lottery when the otherwise identical lottery is payoff relevant for another subject. The treatment difference is significantly different when we conduct a Wilcoxon rank-sum test (p-value 0.050) or a two-sided t-test assuming unequal variances (p-value 0.067). Next, in case of lotteries with more upside than downside probability weight, we find a lower risk share of 0.57 in treatment *Social* compared to a risk share of 0.62 in treatment *Alone*. This difference is not significantly different using a Wilcoxon rank-sum test (p-value 0.250) or a two-sided t-test assuming unequal variances (p-value 0.302). Hence, for lotteries with more upside probability mass we find the opposite sign as Prediction 4.1 suggests but this difference is statistically indistinguishable from zero while for lotteries with more downside probability mass we find a significant negative effect in line with Prediction 4.1.[36] Before we proceed with a regression analysis note that independent of the treatment subjects take significantly more risk in case of lotteries with more upside probability mass compared to lotteries with more downside probability mass, as we would expect. By construction, the lotteries with more downside probability mass are mean-preserving spreads of the lotteries with more upside probability mass, and therefore, the lotteries with more upside probability mass second-order stochastically dominate the lotteries with

[34]Recall that we implement two groups of passive subjects: a "high-risk" group receiving high risk shares and a "low-risk" group receiving low risk shares (see more details in Section 4.2.4).

[35]Subjects also face one "symmetric" lottery (Lottery 9 in Table 4.1 has neither a probability overweight on the upside nor on the downside) that we can analyze for a plausibility check. In line with Section 4.2.1 we find no significant treatment effect in risk taking for Lottery 9 comparing treatment *Social* to *Alone*. This holds for simple means and for a regression analysis (not reported, available on request).

[36]Also see Table 4.6 in Appendix 4.A for descriptive statistics.



More downside probability mass   More upside probability mass

Figure 4.1: Average share invested into risky lottery

more downside probability mass (also see Table 4.1 above).[37,38]

To further test Prediction 4.1 we run a random effects linear panel regression model on the observations of treatment *Alone* and of active subjects in treatment

[37]Note that we construct lotteries with the goal to shift probability weight between the downside and the upside of the distribution (i.e., altering p_G). For a better comparability between lotteries, we hold the expected return constant over all lotteries by adding dispersion to the upside of the lotteries that have higher probability weights at the downside. While this procedure allows us to keep the simple binary structure of lotteries in our transformations, it effectively leads to two simultaneous changes w.r.t. to riskiness. First, risk changes by an intended change in the placement of risk between the upside and downside of a lottery. Second, risk changes by a "pure risk increase or decrease" in the sense of Menezes et al. (1980) (more probability mass placed into the tails of the distribution leading to a higher variance). Menezes et al. (1980) show that risk averters and risk preferrers that exhibit prudence (i.e., the third derivative of a von Neumann-Morgenstern utility function is positive) dislike "pure downside risk", that is a placement of risk from the left to the right of a distribution, without altering neither the mean nor the variance.

[38]Note that the lotteries with more downside probability weight exhibit the properties of "long shots" (a high return with a low probability and positively skewed). Empirical evidence shows that bettors are attracted by "long shots" and are willing to forgo expected return for a higher positive skewness (Golec and Tamarkin 1998, Garrett and Sobel 1999). While such patterns in field data could be explained by risk preferring individuals (Quandt 1986), experimental evidence on the isolated effect of skewness on risk taking finds that individuals do indeed take more risk for more positively skewed lotteries (Grossman and Eckel 2015). Grossman and Eckel (2015) find that about 35 precent of individuals take on higher levels of risk in case of positively skewed lotteries. This seems roughly consistent with the behavior of subjects in this experiment. We find that about 22 percent of the subjects invest more in *more risky* but positively skewed lotteries (that is more downside probability weight lotteries) than in *less risky,* negatively skewed lotteries (i.e., more upside probability weight lotteries).

Social.[39] The main specification is given by[40]

$$riskshare_{i,n} = \beta_0 + \beta_1 Social + \beta_2 I_{upside} + \beta_3 Social \times I_{upside} + \beta X_{i,n} + \varepsilon_{i,n} \quad (4.9)$$

The dependent variable $riskshare_{i,n}$ represents the share of endowment that subject i invests into the risky lottery for lottery (portfolio choice) n and takes values between 0 and 1. I_{upside} is an indicator variable that takes a value of 1 if lottery n has more upside than downside probability mass and a value of 0 in case of more downside than upside probability mass. Our variables of main interest are the treatment dummy *Social* that takes a value of 1 for observations from treatment *Social*, 0 otherwise and the interaction term $Social \times I_{upside}$. $X_{i,n}$ represents a vector of control variables.[41] Coefficient β_1 of the interaction model (4.9) measures the treatment effect of comparison considerations (treatment *Alone* vs. treatment *Social*) on subjects' risk share in situations of lotteries with more downside probability weight ($I_{upside} = 0$). The coefficients β_1 and β_3 jointly measure the treatment effect in situations of lotteries with more upside probability weight ($I_{upside} = 1$).

In specification (1) of Table 4.2 we estimate model (4.9) without control variables.[42] The coefficient of variable *Social* is -0.091 and significant at the 10 percent level (p-value 0.054). Thus, in line with Prediction 4.1, social comparison leads to significantly less risk taking in case of lotteries that have more downside than upside probability weight. More precisely, subjects invest 9.1 percentage points less of their endowment into the risky lottery. On the other hand, the joint effect of variables *Social* and $Social \times I_{LeftS}$ is -0.049 and not significantly different from zero (p-value 0.292) as an F-test shows. Hence, the treatment effect in situations of

[39]We exclude subjects that inconsistently behaved in the test for risk aversion à la Holt and Laury (one subject in treatment *Alone* and 4 subjects in treatment *Social*). However, all results are robust to including all subjects.

[40]Results are robust to using a simple pooled OLS model with clustered standard errors at the subject level or a random effects tobit model that accounts for potential problems related to censoring.

[41]We include as individual controls a measure of risk aversion à la Holt and Laury, gender, age, an indicator variable whether the field of study is related to business or economics, an indicator variable whether subjects participated in laboratory experiments before, measures of patience and impulsiveness, and a measure whether subjects tend to be envious. We also include a fixed effect for each round of the experiment (point in time) and a control variable that controls for the sequence of the rounds (i.e. sequence of lotteries; more precisely, we implemented two groups that saw lotteries in two different randomized sequences; the group dummy is our control for the sequence of lotteries).

[42]Note that all individual control variables are self-reported after the main experiment, and thus, could be potentially endogenous. Therefore, it is comforting to see that our findings are robust even without including control variables.

	All asymmetric lotteries (1) risk share	All asymmetric lotteries (2) risk share	More asymmetric lotteries (3) risk share	Less asymmetric lotteries (4) risk share
$Social$	-0.091*	-0.115**	-0.150***	-0.080*
	(0.047)	(0.045)	(0.058)	(0.046)
I_{upside}	0.231***	0.217***	0.331***	0.166***
	(0.045)	(0.044)	(0.067)	(0.042)
$Social \times I_{upside}$	0.042	0.042	0.067	0.017
	(0.060)	(0.061)	(0.090)	(0.057)
$risk\ aversion_i$		-0.033***	-0.039***	-0.027***
		(0.012)	(0.015)	(0.013)
$Constant$	0.384***	0.414***	0.545***	0.246*
	(0.034)	(0.115)	(0.136)	(0.145)
Individual controls	No	Yes	Yes	Yes
Time and sequence fixed effects	No	Yes	Yes	Yes
N	672	672	336	336
$Clusters$	84	84	84	84

Note: Random effects panel regression model. Dependent variable: share invested into the risky lottery. Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. Specifications (1) and (2) include all observations of asymmetric lotteries (i.e., lotteries with more upside or downside probability mass) from treatments *Alone* and *Social*. Specification (3) includes the subsample of more asymmetric lotteries (see L_5, L_6, L_7, L_8 in Table 4.1). Specification (4) includes the subsample of less asymmetric lotteries (see L_1, L_2, L_3, L_4 in Table 4.1). "Individual controls" include gender, age, whether the field of study is business related, risk aversion, impulsiveness and patience, a measure for enviousness, a dummy whether subjects participated in laboratory experiments before. "Time and sequence fixed effects" include period fixed effects and a group-indicator variable for groups that observe the lotteries in different sequences.

Table 4.2: Income-rank dependent preferences: Regression results for risk-taking

lotteries with more upside probability mass shows the opposite (negative) sign as predicted by Prediction 4.1 but is statistically indistinguishable from zero. Finally, the positive and highly significant coefficient of 0.231 (p-value 0.000) for variable I_{upside} shows that independent of the treatment effect subjects generally take considerably more risk in situations of lotteries with more upside probability mass compared to situations of lotteries with more downside probability mass. This is as one would expect because of the generally higher standard deviations (risk) of lotteries with more downside probability weight.[43] Specification (2) confirms these findings where we include individual-specific socioeconomic characteristics, variables from post-experimental tasks as well as time and sequence-of-lotteries fixed effects. Note that the individual control $risk\ aversion_i$ has a coefficient of -0.033 that is significant at the 1 percent level (p-value 0.008), and thus, more risk-averse subjects generally invest significantly less into the risky lottery of a portfolio choice problem as one would suspect.[44]

Result 4.1 *Comparison considerations affect risk taking: Individuals invest less risky in situations of lotteries with more downside than upside probability mass. In situations of more upside than downside probability mass comparison considerations do not significantly affect risk taking behavior.*

To shed some light on Prediction 4.2, whether more asymmetrically allocated probability mass affects risk-taking more (less), we estimate model (4.9) on subsamples of lotteries with more or less asymmetrically allocated probability mass. In specification (3) we consider only a subset of more asymmetric lotteries (lotteries L_5, L_7, L_6, L_8 of Table 4.1). More asymmetric lotteries show a more significant and in absolute size larger treatment effect of -0.150 (p-value 0.009) in situations of lotteries with more downside probability mass in line with Prediction 4.2. In situations of lotteries with more upside probability mass the effect remains insignificant.[45] In specification (4) we consider a subset of less asymmetric lotteries (lotteries L_1, L_3, L_2, L_4 of Table 4.1) and find a less significant and in absolute

[43]Recall that lotteries with more downside probability weight are mean preserving spreads of lotteries with more upside probability weight by construction.

[44]Note that we experimented with different measures of risk aversion. In our regressions we use a multiple price list measure à la Holt and Laury as this measure shows the highest explanatory power. Using the general risk-question of Dohmen et al. (2011) as a measure of risk aversion shows the correct sign in most specifications but is only weakly or not at all significant. Thus, our results imply that in the financial dimension an incentivized multiple price list measure seems to be a better measure to assess risk aversion.

[45]In specification (3) the sum of coefficients of variables $Social$ and $Social \times I_{upside}$ is insignificant (p-value 0.34) using an F-test.

size smaller effect of −0.0795 (p-value 0.086) for lotteries wiht more downside probability mass in line with Prediction 4.2. In case of lotteries with more upside probability mass the effect remains again statistically indistinguishable from zero.[46]

Result 4.2 *The effect of comparison considerations on risk taking appears to be larger (smaller) in absolute terms for lotteries with more downside probability mass when probability mass is allocated even more asymmetrically. In case of lotteries with more upside than downside probability mass neither a more nor a less asymmetric allocation of the probability mass shows a significant effect.*

A natural next step is to analyze factors that foster the effect of comparison considerations on risk taking. An influential idea first discussed by Festinger (1954) is that individuals compare themselves to more similar rather than to less similar individuals, and thus, we might expect stronger comparison effects for more similar subjects.[48] During the experiment we intentionally provide a large degree of anonymity to limit potential noise from the interaction of individuals that might be related to subjective sympathy or other factors that are difficult to control for. Although subjects do not interact they enter the room in which the experiment takes place together (in treatment *Social*), and thereby, see the other participant of their session before taking a seat behind their private cubicle. While it is difficult to assess what subjects learn from this short visual impression of their reference subject, it seems plausible that subjects at least get to know the gender of the other participant. Thus, when we think of gender as one dimension in which subjects can be more or less similar we can expect that subjects grouped with another participant of the same gender (more similar subjects) experience a stronger comparison effect on risk taking than subjects grouped with another participant of a different gender (less similar subjects). Furthermore, we can also exploit our data to investigate a general gender effect. Many studies in the experimental literature find that risk taking exhibits a gender related component, and thus, social comparison related risk taking behavior might also differ along the gender dimension,

[46]In specification (4) the sum of coefficients of variables $Social_i$ and $Social_i \times I_{upside}$ is insignificant (p-value 0.245) using an F-test. Thus, while we find evidence in line with Prediction 4.2 for lotteries with more downside probability mass, for lotteries with more upside probability mass we find no statistically significant effect, consistent with our findings above.[47]

[48]Festinger (1954) refers to situations when subjects compare their own ability to the ability of others while we are interested in an extension of his idea to income comparisons.

independently of Festinger's similarity idea.[49,50]

In specifications (1) and (2) of Table 4.3 we estimate model (4.9) as above but focus only on subsamples of subjects that are grouped with another subject of the same gender in treatment *Social*.[51] In specification (1) we focus on the treatment effect for female-female pairs. We find that the coefficient of *Social* is larger in absolute terms (−0.191) and more significant (p-value 0.000) compared to our results reported above. An F-test shows that the sum of the coefficients of *Social* and $Social \times I_{upside}$ is not significantly different from zero. Thus, focusing on female-female pairs provides similar but stronger results than the general specification estimated above with a significant negative effect in case of lotteries with more downside probability weight but no significant effect for lotteries with more upside probability weight. In specification (2) we focus on a subsample of male-male pairs. We find an in absolute terms slightly larger coefficient for *Social* than in the complete sample (−0.133) that is significant at the 5 percent level (p-value 0.046). However, in situations of lotteries with more downside probability weight this negative effect is smaller in absolute terms compared to female-female pairs. For lotteries with more upside probability weight we also find in the male-male subsampel a statistical zero effect.[52,53] Finally, in specifications (3) and (4) we plainly focus on the gender of the deciding subject without considering the gender of the reference subject. In specification (3), using a subsample of female subjects, we find a sizable negative effect for lotteries with more downside probability mass (coefficient of *Social*: −0.133) that is significant at the 1 percent level (p-value

[49] Many studies in the experimental literature find that women take less risk than men do (e.g., see Charness and Gneezy 2012). However, some studies argue that gender differences are small and context specific (e.g., Schubert et al., 1999).

[50] We find that male subjects invest on averge 4.7 percent more of their endowment into the risky lottery compared to female subjects in our experiment. This difference, however, is insignificant.

[51] More precisely, in specification (1) we compare female subjects in treatment *Alone* to female-female pairs in treatment *Social*. Analog, in specification (2) we compare male subjects in treatment *Alone* to male-male pairs in treatment *Social*.

[52] In the male-male subsample the sum of the coefficients of *Social* and $Social * I_{upside}$ is indistinguishable from zero using an F-test (p-value 0.24).

[53] Note, that same-gender pairs show a significantly stronger treatment effect to social comparisons for lotteries with more downside probability mass. In a formal test we first create a dummy variable that takes a value of one for different-gender pairs and zero for same-gender pairs. We interact this variable with the treatment dummy that takes a value of one for observations in treatment *Social*. This interaction model is separately estimated for lotteries with more downside and upside probability weight, in each case on observations from treatments *Alone* and *Social*. The estimate of the interaction term is significantly different from zero at the 5 percent level for lotteries with more downside probability weight. Results are reported in Table 4.8 of Appendix 4.A.

	Female with Female (1) risk share	Male with Male (2) risk share	Female (3) risk share	Male (4) risk share
Social	-0.191***	-0.133**	-0.133***	-0.046
	(0.043)	(0.066)	(0.050)	(0.081)
I_{upside}	0.186***	0.276***	0.186***	0.276***
	(0.064)	(0.063)	(0.064)	(0.063)
Social $\times I_{upside}$	0.165**	0.041	0.115	-0.033
	(0.079)	(0.095)	(0.078)	(0.094)
Constant	0.389***	0.379***	0.389***	0.379***
	(0.035)	(0.058)	(0.035)	(0.058)
Individual controls	No	No	No	No
Time and sequence fixed effects	No	No	No	No
N	280	272	344	328
Clusters	35	34	43	41

Note: Random effects panel regression model. Dependent variable: share invested into the risky lottery. Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. Gender-specific subsamples. Specifications (1) and (2) apply subsamples in which grouped subjects are of the same gender in treatment *Social* and of the corresponding same gender in treatment *Alone* (female-female compared to female and male-male compared to male). Specifications (3) and (4) focus simply on gender without considering the gender of the other (reference) subject in treatment *Social*.

Table 4.3: Subjects of the same gender

0.008). In situations of lotteries with more upside probability mass we find no significant effect.[54] Focusing on male subjects in specification (4) we find neither a significant effect for lotteries with more downside (p-value 0.575) nor more upside (p-value 0.28) probability mass. Thus, in line with the idea of Festinger (1954) we find larger estimated effects of comparison concerns on risk taking for more similar subjects compared to the full sample. Robust to our findings above, comparison considerations affect risk taking in situations of lotteries with more downside probability mass but not for lotteries with more upside probability mass. Finally, our results indicate that effects of comparisons on risk taking appear to be more pronounced for female than for male subjects although a formal test does not show a significant gender difference.[55]

Result 4.3 *Comparison considerations affect risk taking more when the reference subject is of the same gender.*

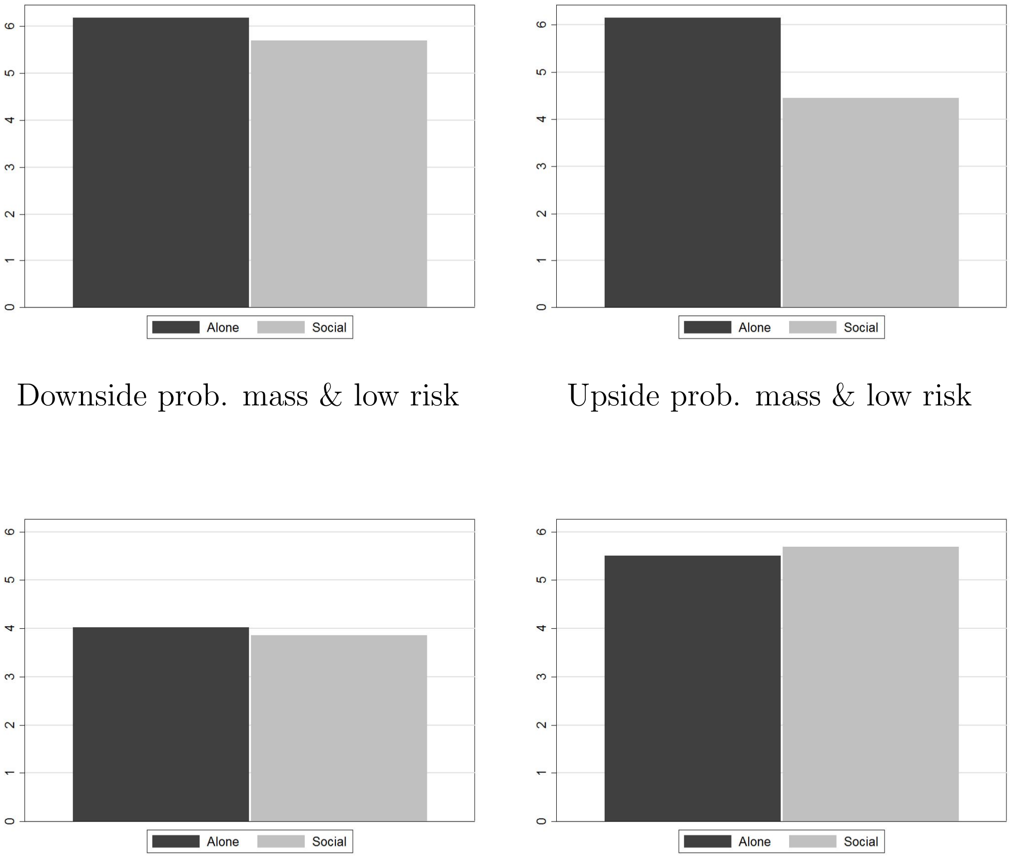
4.3.2 Evidence from satisfaction

We can make use of observations of passive subjects by investigating measured satisfaction. In a first step, we consider simple means to investigate Prediction 4.3. Figure 4.2 shows mean satisfaction of passive subjects in situations of lotteries with more downside and lotteries with more upside probability mass for low-risk and high-risk groups.[56] Recall that in situations of lotteries with more upside probability mass and low risk share and in situations of lotteries with more downside probability mass and high risk share passive subjects are likely to be behind in rank compared to the active reference subject. Consequently, in these situations theory predicts passive subjects to be less satisfied in treatment *Social* compared to treatment *Alone Passive*. We find that subjects are indeed less satisfied for lotteries with more upside probability mass and low risk shares (see Figure 4.2, upper right corner), reporting by 1.70 points lower satisfaction in treatment

[54] An F-test for the sum of *Social* and $Social * I_{upside}$ shows again a statistical zero effect in situations of lotteries with more upside probability mass (p-value, 0.76) for the subsample of female subjects.

[55] We interact a dummy variable, that takes the value of one for male subjects and zero for female subjects, with a treatment dummy that takes a value of one for observations in treatment *Social*. We estimate this interaction model separately for lotteries with more downside and upside probability mass, in each case on observations from treatments *Alone* and *Social*. We cannot reject that the coefficient for the interaction term is equal to zero in neither of the two specifications.

[56] Recall that we implement two groups: one that receives high and one that receives low risk shares. See Section 4.2.4 for more details.



Downside prob. mass & low risk Upside prob. mass & low risk

Downside prob. mass & high risk Upside prob. mass & high risk

Figure 4.2: Average satisfaction of passive subjects

Social compared to treatment *Alone Passive*. This difference is significant at the 5 percent level when we apply a non-parametric Wilcoxon rank-sum test (p-value 0.012) and significant at the 1 percent level for a two-sided t-test assuming unequal variances (p-value. 0.0002). In all other situations, including situations of lotteries with more downside probability mass and high risk shares (Figure 4.2, lower left corner) we do not find a significant difference in satisfaction between treatment *Alone Passive* and treatment *Social*. Thus, we find evidence in line with Prediction 4.3 in situations of lotteries with more upside probability mass and low risk shares but no significant effect in case of lotteries with more downside probability mass and high risk shares.

To elaborate further on these findings we run a random effects linear panel regression model on the observations of treatment *Alone Passive* and passive subjects of treatment *Social*.[57] We estimate the following model:

$$(4.10)\quad satisfaction_{i,n} = \beta_0 + \beta_1 Social + \beta_2 I_{High\_Risk} + \beta_3 Social \times I_{High\_Risk} + \varepsilon_{i,n}$$

The dependent variable $satisfaction_{i,n}$ is reported satisfaction of passive subject i facing lottery n. I_{High_Risk} is an indicator variable that takes a value of 1 for subjects receiving a high risk share from the computer (high-risk group) and 0 for subjects receiving a low risk share (low-risk group). Our variables of main interest are variables *Social*, a treatment dummy that takes a value of 1 for observations of treatment *Social*, and the interaction term $Social \times I_{High\_Risk}$. Coefficient β_1 measures the treatment effect on satisfaction for subjects receiving low risk shares ($I_{High\_Risk} = 0$) while the sum of β_1 and β_3 measures the treatment effect for high risk share subjects ($I_{High\_Risk} = 1$).

In specification (1) of Table 4.4 we estimate model (4.10) on the set of lotteries with more upside probability mass (lotteries L_2, L_4, L_6, L_8 of Table 4.1). The coefficient of *Social* is -1.702 and significant at the 1 percent level (p-value 0.000). Thus, subjects are significantly less satisfied in treatment *Social* compared to *Alone Passive* when they receive a low risk share and face more probability mass on the upside (when they are more likely to be behind). In situations of high risk shares we find a positive but insignificant treatment effect.[58]

In specification (2) we estimte the identical model on the set of lotteries with more downside probability mass (lotteries L_1, L_3, L_5, L_7 of Table 4.1). We find neither a significant treatment effect for high nor for low risk shares.[59] The negative and significant coefficient of I_{High_Risk} (-2.163, p-value 0.000) shows a strong negative reaction in satisfaction comparing high to low risk shares independent of the treatments *Social* and *Alone*. This strong effect is only present in situations of lotteries with more downside probability mass as these lotteries exhibit by construction a considerably higher riskiness (standard deviation) compared to lotteries with more upside probability mass in our experiment (also see Table 4.1).

Result 4.4 *Comparison considerations adversely affect satisfaction of subjects when subjects are likely to be behind in income compared to another subject in*

[57] Results are robust to using a pooled OLS model with clustered standard errors on subject level. Since satisfaction is an ordinal concept we also apply a random effects ordered probit model and find that results are robust, in line with Ferrer-i-Carbonell and Frijters (2004).

[58] The sum of the coefficients of *Social* and $Social \times I_{High\_Risk}$ is positive (0.187) and not significantly different from zero using an F-test (p-value 0.775)

[59] Neither the coefficient of *Social* (-0.491, p-value 0.294) nor the sum of coefficients of *Social* and $Social \times I_{High\_Risk}$ (-0.171, p-value 0.809) are statistically distinguishable from zero.

	Upside probability mass lotteries (1) satisfaction	Downside probability mass lotteries (2) satisfaction
Social	-1.702***	-0.491
	(0.400)	(0.467)
I_{High_Risk}	-0.656	-2.163***
	(0.553)	(0.516)
$Social \times I_{High\_Risk}$	1.889**	0.320
	(0.768)	(0.848)
Constant	6.156***	6.188***
	(0.228)	(0.296)
Individual controls	No	No
Time and sequence fixed effects	No	No
N	252	252
$Clusters$	63	63

Note: Random effects panel regression model. Dependent variable: satisfaction. Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. Dependent variable: reported satisfaction of passive subjects. The sample includes observations from treatment *Alone Passive* and passive subjects from treatment *Social*. Specification (1) incorporates the subsample of lotteries with more upside probability mass (lotteries L_2, L_4, L_6, L_8 of Table 4.1). Specification (2) incorporates the subsample of lotteries with more downside probability mass (lotteries L_1, L_3, L_5, L_7 of Table 4.1).

Table 4.4: Satisfaction of passive subjects with high and low risk shares

situations of lotteries with more upside probability mass.

It is notable that we only find an effect for being behind in expected income-rank in situations of lotteries with more upside probability mass. One possible explanation for this result is that passive subjects might find it more difficult to assess the behavior of active subjects in case of lotteries with more downside probability mass compared to lotteries with more upside probability mass. The latter lotteries exhibit by construction comparably little risk, and thus, it might appear straightforward to predict that most active participants invest a larger share of their endowment into the risky lottery. Thus, "low-risk group" passive subjects might find it straightforward to predict to be behind in expected rank. On the other hand, lotteries with more downside probability mass are considerably more risky by construction but offer consequently more extreme returns (a "long shot"). Passive subjects might anticipate that the extreme (low probability) returns are potentially attractive for some active participants, and thus, passive subjects might find it less straightforward to predict the behavior of active subjects.[60] Consequently, passive subjects might experience a higher strategic uncertainty for the lotteries with more downside probability mass than for lotteries with more upside probability mass that could explain the weaker results.[61]

4.3.3 Higher risk for the relative income position: Uncorrelated lottery returns

In Section 4.3.1 we find that comparison concerns induce less risk taking for lotteries with more downside probability weight while we find no statistically significant effect for lotteries with more upside probability weight. What explains this asymmetric result? One possible candidate for an explanation is that subjects do not

[60]Note that the low probability, high return of some of the lotteries with more downside probability mass can be considered to be substantial for a student participating in a laboratory experiment (in the best case more than 100 EUR).

[61]For instance, Kahneman and Tversky (1979) suggest that individuals might overweight low probabilities. Camerer and Kunreuther (1989) discuss that individuals face problems to handle low probabilities and either dismiss or overestimate these. Furthermore, evidence shows that some individuals are attracted by the "long shot" properties that are common to the lotteries with more downside probability weight. Golec and Tamarkin (1998) and Garrett and Sobel (1999) find that individuals are willing to forgo expected return for lotteries exhibiting such "long shot" properties. Related to this work, evidence by Grossman and Eckel (2015) shows that individuals take more risk in case of positively skewed lotteries, a typical property of "long shots". Also note that the lotteries with more downside probability mass are positively skewed. Summing up, assessing how other subjects respond to low probability events with rather "extreme" returns (and positively skewed probability distributions) might be particularly difficult for subjects.

care so much about their income rank but rather about the distance in income relative to others for their risk taking decisions, as first theoretically discussed by Konrad and Lommerud (1993). To see why this might explain the asymmetric result, first consider lotteries with more upside and lotteries with more downside probability mass (see Table 4.1). By construction, lotteries with more downside probability weight exhibit higher risk (i.e. higher standard deviations) than lotteries with more upside probability weight. Thus, one could also read the results of Section 4.3.1 as follows: Comparison considerations lead to less risk taking in situations of higher risk. This interpretation would be in line with Konrad and Lommerud (1993) who show that comparison considerations lead to less risk taking when individuals are relatively more risk averse with regard to their relative income position than with regard to their pure consumption.[62]

To investigate whether the individuals' preferences for the distance in income can explain our results we run a control treatment in which we increase the risk for the relative income position while, at the same time, we keep the risk for the personal income unchanged (treatment "*Social Uncorr*").[63] The idea of this treatment is inspired by Konrad and Lommerud (1993) who show that the correlation structure of risk, e.g. correlated or uncorrelated income risks, can matter. *Social Uncorr* is identical to treatment *Social* except for that the returns of the risky lottery are uncorrelated for subjects. More precisely, while in treatment *Social* the identical dice rolls determine the return of the risky lottery for both subjects, in treatment *Social Uncorr* for each subject dice are independently rolled, and thus, the return of the risky lottery is independently determined.[64] Thus, while in treatment *Social* both subjects of a session receive either a positive or a zero payoff from the risky lottery, in treatment *Social Uncorr* subjects can also end up with a positive (zero) payoff from the risky lottery while the other participant receives a zero (positive) payoff. Consequently, subjects can be relatively further ahead or behind in income in treatment *Social Uncorr* than in *Social*, leading to

[62]This holds for the case of non-systematic (uncorrelated) risk in Konrad's and Lommerud's (1993) theoretical study. In our main treatment the lottery payoffs are perfectly correlated for subjects. However, different to the assumption of a symmetric investment decision in Konrad and Lommerud (1993), risk shares for the reference (passive) subjects are randomly drawn in our study. This introduces additional risk for the relative income position of active subjects and effectively leads to a situation comparable to "non-systematic risk" in Konrad and Lommerud (1993).

[63]We run additional 44 sessions with 88 participants. Also see Table 4.5 in Appendix 4.A.

[64]Subjects learn about the procedure (one-for-all or independent dice rolls) from the instructions before the experiment starts (e.g., see Appendix 4.C for the instructions of treatment *Social*).

higher risk in the social dimension.

When the distance in incomes of subjects causes our asymmetric results of Section 4.3.1 we would expect that subjects respond to the higher risk in the social dimension accordingly. Thus, we would expect that subjects reduce risk taking comparing treatment *Social Uncorr* to treatment *Social* (i.e. a decrease in a_1). On the other hand, when income-rank comparisons are at work (as formalized in Section 4.2.1) we should observe similar responses for risk taking comparing the control treatment *Social Uncorr* to treatment *Social* and Predictions 4.1 and 4.2 should still hold.[65,66] Thus, comparing treatment *Social Uncorr* to *Social*, we expect no significant difference in a_1.

We estimate a similar model as presented in equation (4.9) of Section 4.3.1 on observations from treatment *Social* and *Social Uncorr*. In the first step, we do not separate situations of lotteries with more upside and lotteries with more downside probability mass (no interaction term) and focus plainly on the treatment effect. We find no significant difference in risk taking between the treatments, suggesting that the distance in incomes does not explain our results (see specification (1) in Table 4.9 in Appendix 4.A.1).[67] We can further investigate the treatment effect of social comparisons by comparing the control treatment *Social Uncorr* to treatment *Alone*. We apply similar specifications as presented in Table 4.2 in Section 4.3.1. Compared to the outcomes in Section 4.3.1, the effects are of the same sign, however, of lower magnitude and indistinguishable from zero (see Table 4.10 in Appendix 4.A.1). Furthermore, when we run estimations on subsamples based on gender (more or less similar subjects) we find again similar results. We estimate similar specifications as presented in Section 4.3.1 on observations from treatments *Social Uncorr* and *Alone*. For female subjects and female-female pairs the coefficients of *SocialUncorr* and $SocialUncorr \times I_{LeftS}$ are of the same sign but of slightly smaller magnitude compared to Section 4.3.1 (see Table 4.11 in Appendix 4.A.1). Comparison considerations lead to significantly lower risk taking for lotteries with more downside probability mass, whereas we find no significant treatment effect in case of lotteries with more upside probability mass. In contrast to Section 4.3.1, we generally find no significant treatment effect for male-male gender

[65]See Appendix 4.B.2 for a derivation of this claim.

[66]This prediction holds for all cases but the corner-case $a_1 = a_2 = 0$. Recall that a_2 is a continuous random variable, and thus, $a_2 = 0$ is a zero-probability event.

[67]The coefficient *SocialUncorr* is insignificant (specification (1), Table 4.9). Similarly, we find no significant difference between the treatments *Social Uncorr* and *Social* when we estimate all other specifications as presented in Table 4.2 in Section 4.3.1 (results are reported in Table 4.9 in Appendix 4.A.1).

pairs anymore, neither for lotteries with more downside nor for lotteries with more upside probability mass. In summary, estimations based on the control treatment *Social Uncorr* provide no evidence for other forms of comparison concerns explaining the asymmetric findings of Section 4.3.1. Generally, results appear to be in line but weaker compared to Section 4.3.1.

Result 4.5 *In situations of uncorrelated risky returns:*
the effect of comparison considerations on risk taking is weaker. For female subjects we find similar effects of lower magnitude compared to the situation of correlated risky returns that are stronger when the reference subject is also female (female-female pairs). For male subjects we find no significant comparison effect on risk taking.

Overall, the control treatment provides results supporting the findings in Section 4.3.1: Social comparisons induce subjects to take less risk in case of lotteries with more downside probability mass but we find no significant effect in situations of lotteries with more upside probability mass. Preferences for the distance of incomes, and thus, also social loss aversion, seem not to be driving the asymmetric responses for different lotteries. What else could explain the asymmetric result? One possible interpretation is that subjects perceive payoffs of a similar range as "almost" identical. As by construction possible payoff differences are considerably smaller for lotteries with more upside probability mass compared to lotteries with more downside probability mass, subjects might perceive these payoff differences as negligible, and thus, we do not measure comparison effects. Put differently, to trigger an empirically measurable reaction larger payoff differences are necessary (as it is the case in situations of lotteries with more downside probability mass). This interpretation is in its nature related to the idea of income-distance based preferences in that there might be an implicit threshold in the distance of income so that subjects feel to be ahead or behind in rank.

4.4 Conclusion

We investigate the effect of social comparisons on risk taking inspired by the theoretical contributions of Robson (1992) and Konrad and Lommerud (1993). Our model predicts that income-rank-dependent preferences lead to less (more) risk taking in situations of lotteries with more downside (upside) probability mass. We find evidence of income comparisons affecting risk taking decisions. In line with our theoretical predictions, individuals take significantly less risk in situations of lotteries with more downside probability mass. We do not find a significant effect for lotteries with more upside probability mass. Individuals who face a reference subject of the same gender exhibit a larger comparison effect on risk taking. We interpret this finding as evidence for Festinger's (1954) idea that individuals prefer to compare themselves to more rather than to less similar subjects. Independent of the reference subject, the measured effects appear to be strongest for female individuals.

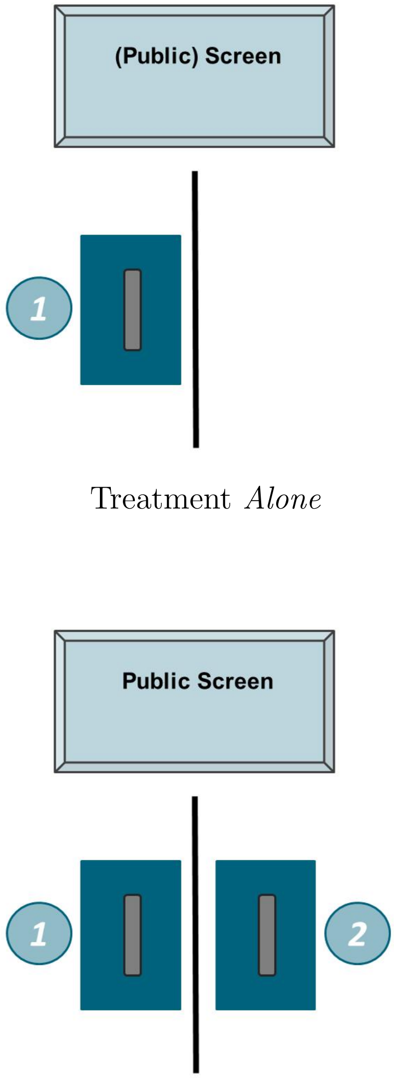
The asymmetric finding for lotteries with more downside and lotteries with more upside probability mass comes as a surprise. A further control treatment provides no evidence that other forms of income comparisons, such as the distance in incomes or social loss aversion, can explain the asymmetric effect. Our favored explanation is that subjects might perceive situations in which expected income of others falls not far apart from their own expected income as "almost equal". In our setup, lotteries with more upside probability mass offer by construction no large difference in the expected income of subjects, contrary to lotteries with more downside probability mass. This comparably small difference might be too small to trigger an empirically measurable response.

Overall, we find that social comparisons affect risk taking behavior. Our results give guidance where we should expect social comparison effects on risk taking: Comparison effects particularly emerge for lotteries with more downside than upside probability mass that are commonly positively skewed. Skewed distributions are applicable in various situations, such as in financial and insurance markets, a natural environment for risk taking decisions.[68] Furthermore, our finding of reinforced comparison effects for more similar individuals implies that some industries and occupational fields are more prone to comparison effects on risk taking than others. Effects can be expected to be stronger when managers and employees are predominantly of the same gender, such as in the financial sector or in human resources related positions.

[68] For an overview see Adcock et al. (2015).

Appendix

4.A Appendix



Treatment *Social*

Figure 4.3: Laboratory setup

Note: In treatment *Alone* participates one, in treatment *Social* two subjects in each session. Subjects in treatment *Social* are separated by a room divider to ensure privacy, while both subjects observe the identical “public screen” that shows the payoff-relevant portfolio choice (i.e. subject (1) looks to her left, subject (2) to her right).

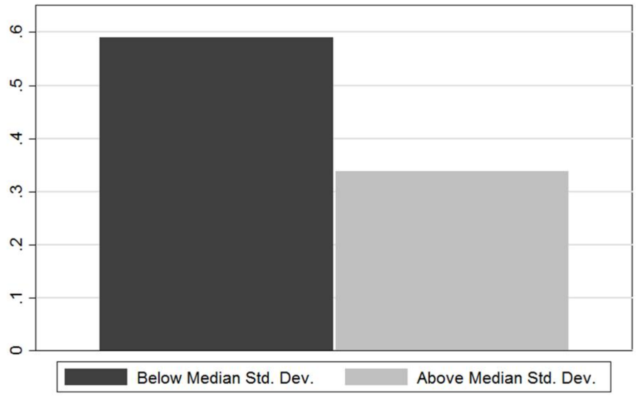
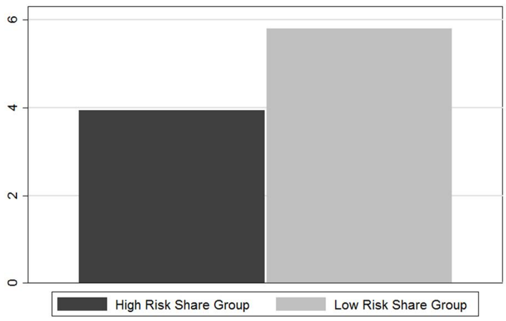
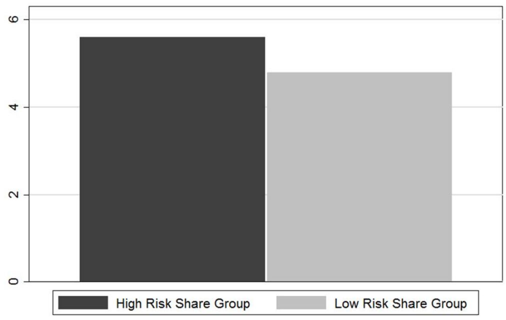


Figure 4.4: Risk share: Above and below median std. dev. lotteries

Note: Risk share refers to the share of the endowment that subjects invest into the risky lottery, taking values between 0 (0%) and 1 (100%).



Above median std. dev. lotteries



Below median std. dev. lotteries

Figure 4.5: Satisfaction of passive subjects in situations of more or less risk

Note: Self-reported satisfaction is measured on a scale 0-10. Passive subjects are randomly assigned into two groups: receiving high risk shares (high-risk group) or low risk shares (low-risk group).

Treatments	*Alone*	*Social*	*Alone Passive*	*Social Uncorr*
# Sessions	45	45	18	44
Participants				
# Active	45	45	-	44
# Passive	-	45	18	44
Observations per participant[a]				
# Downside prob. lotteries	4	4	4	4
# Upside prob. lotteries	4	4	4	4

[a] Subjects face additionally one symmetric (non-skewed) lottery that we use for a plausibility check.

Note: In treatments *Alone* and *Alone Passive* participates one subject in each session. In treatments *Social* and *Social Uncorr* participate simultaneously one active and one passive subject in each session. Treatments *Alone, Social, Alone Passive* are explained in Section 4.3.1, details on the control treatment *Social Uncorr* are presented in Section 4.3.3.

Table 4.5: Summary of the experimental treatments

	Alone	*Social*	*Social Uncorr*	*Total*			
	Mean	Mean	Mean	Mean	S.D.	Min	Max
Risk share	0.50	0.44	0.52	0.48	0.32	0	1
Downside m.	0.39	0.30	0.38	0.61	0.31	0	1
Upside m.	0.62	0.57	0.64	0.36	0.30	0	1
Satisfaction	6.7	7.0	7.0	6.9	2.2	0	10
Downside m.	6.3	6.8	6.8	6.6	2.2	0	10
Upside m.	7.1	7.0	7.1	7.0	2.1	0	10
Male	0.49	0.45	0.64	0.53	0.50	0	1
Age	23.8	24.3	25.3	24.5	6.2	18	60
Econ	0.13	0.25	0.18	0.19	0.39	0	1

Note: Descriptive statistics for all 134 subjects that actively choose their investment. “Risk shar” refers to the share of endowment that subjects invest into the risky lottery. It that takes values between 0 and 1. “Male” takes a value of 1 for male subjects. Econ takes a value of 1 for subjects that study in business related fields such as economics. “Downside m.” refers to lotteries with more downside probability mass while “upside m.” refers to lotteries with more upside probability mass.

Table 4.6: Summary statistics of actively investing subjects

	Alone Passive	*Social Passive*	*Social Uncorr Passive*	*Total*			
	Mean	Mean	Mean	Mean	S.D.	Min	Max
High-risk group							
Risk share	0.81	0.81	0.81	0.81	0.09	0.62	0.95
Downside m.	0.78	0.78	0.78	0.78	0.12	0.62	0.95
Upside m.	0.82	0.82	0.82	0.82	0.05	0.77	0.89
Satisfaction	4.48	4.89	4.42	4.75	2.59	0	10
Downside m.	4.03	3.85	2.09	3.44	2.40	0	9
Upside m.	5.50	5.69	6.41	5.82	2.32	1	10
Low-risk group							
Risk share	0.21	0.21	0.21	0.21	0.10	0.05	0.39
Downside m.	0.22	0.22	0.22	0.22	0.07	0.13	0.30
Upside m.	0.22	0.22	0.22	0.22	0.13	0.05	0.39
Satisfaction	6.11	4.99	5.04	5.12	2.85	0	10
Downside m.	6.19	5.70	5.87	5.83	2.70	0	10
Upside m.	6.16	4.46	4.48	4.64	2.87	0	10
Male	0.78	0.50	0.59	0.58	0.49	0	1
Age	22.4	23.8	23.1	23.3	3.8	18	39
Econ	0.11	0.16	0.18	0.16	0.37	0	1

Note: Descriptive statistics for all 107 passive subjects. "Risk share" refers to the share of endowment that is invested into the risky project (lottery). It takes values beteween 0 and 1. "Male" takes a value of 1 for male subjects. Econ takes a value of 1 for subjects that study in business related fields such as economics. "Downside m." refers to lotteries with more downside probability mass while "Upside m." refers to lotteries with more upside probability mass. "Passive" refers to the share of risky investment being randomly drawn from a distribution. Two groups are implemented with different distributions applying for the randomly drawn risk share. In a first step, we record the empirical distribution of risk shares for each lottery of the first 10 active subjects. We apply this random draws of this empirical distribution as the passive risk shares. These are *identical* for subjects of the same group of passive subjects across all treatments. "High-risk group" refers to a situation where the passive risk share is a random draw from right end outside the 95 percent confidence interval of the empirical distribution of risk shares of each lottery. "Low-risk group" refers to a situation where the passive risk share is a random draw from the left end outside the 95 percent confidence interval of the empirical distribution of risk shares of each lottery.

Table 4.7: Summary statistics for passive subjects

	Downside probability mass lotteries (1) risk share	Upside probability mass lotteries (2) risk share
Social	-0.191***	-0.059
	(0.039)	(0.053)
Social $\times I_{different\_gender}$	0.191**	0.027
	(0.074)	(0.069)
Constant	0.384***	0.615***
	(0.034)	(0.032)
Individual controls	No	No
Time and sequence fixed effects	No	No
N	336	336
Clusters	84	84

Note: Random effects panel regression model. Dependent variable: Share of endowment invested into the risky lottery. Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. Specification (1) applies a subsample of lotteries with more downside probability mass while specification (2) a subsample of lotteries with more upside probability mass. $I_{different\_gender}$ is an indicator variable that takes a value of 1 for participants grouped to another participant of a different gender in treatment *Social* and a value of 0 for a pair of participants of the same gender.

Table 4.8: Same-gender and different-gender pairs

4.A.1 Results for uncorrelated lottery returns

	All asymmetric lotteries (1) risk share	All asymmetric lotteries (2) risk share	More asymmetric lotteries (3) risk share	Less asymmetric lotteries (4) risk share
$SocialUncorr$	0.052	0.057	0.072	0.042
	(0.034)	(0.050)	(0.064)	(0.049)
I_{upside}		0.258***	0.407***	0.190***
		(0.038)	(0.064)	(0.038)
$SocialUncorr$		-0.010	0.013	-0.034
$\times I_{upside}$		(0.060)	(0.086)	(0.062)
$risk\ aversion_i$	-0.034***	-0.034***	-0.038***	-0.030***
	(0.011)	(0.011)	(0.013)	(0.008)
$Constant$	0.542***	0.358***	0.358**	0.336***
	(0.123)	(0.127)	(0.170)	(0.126
Individual controls	Yes	Yes	Yes	Yes
Time and sequence fixed effects	Yes	Yes	Yes	Yes
N	648	648	324	324
$Clusters$	81	81	81	81

Note: Random effects panel regression model. Dependent variable: Share of endowment invested into the risky lottery. Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. I_{upside} is an indicator variable that takes a value of 1 for lotteries with more upside probability mass and zero otherwise. $SocialUncorr$ is an indicator variable that takes a value of 1 for observations in treatment *Social Uncorr* and zero otherwise. Specifications (1) and (2) include all observations of asymmetric lotteries (more downside or upside probability mass) from treatments *Social* and *Social Uncorr*. Specification (3) includes the subsample of more asymmetric lotteries (L_5, L_6, L_7, L_8 in Table (4.1)). Specification (4) includes the subsample of less asymmetric lotteries (L_1, L_2, L_3, L_4 in Table (4.1)). "Individual controls" include gender, age, whether the field of study is business related, risk aversion, impulsivness and patience, enviousness, a dummy whether subjects participted in laboratory expermints before. "Time fixed effects" include round fixed effects and a dummy for the sequence in which lotteries are shown.

Table 4.9: Comparisons and risk taking for correlated vs. uncorrelated lotteries

	All asymmetric lotteries	All asymmetric lotteries	More asymmetric lotteries	Less asymmetric lotteries
	(1)	(2)	(3)	(4)
	risk share	risk share	risk share	risk share
$SocialUncorr$	-0.010	-0.036	-0.052	-0.020
	(0.054)	(0.054)	(0.067)	(0.058)
I_{upside}	0.231***	0.216***	0.324***	0.173***
	(0.045)	(0.044)	(0.068)	(0.042)
$SocialUncorr \times I_{upside}$	0.031	0.033	0.080	-0.015
	(0.063)	(0.064)	(0.089)	(0.064)
$risk\ aversion_i$		-0.019	-0.014	-0.023*
		(0.012)	(0.014)	(0.013)
Constant	0.384***	0.404***	0.522***	0.307***
	(0.034)	(0.099)	(0.141)	(0.104)
Individual controls	No	Yes	Yes	Yes
Time and sequence fixed effects	No	Yes	Yes	Yes
N	680	680	340	340
$Clusters$	85	85	85	85

Note: Random effects panel regression model. Dependent variable: Share of endowment invested into the risky lottery. Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. I_{upside} is an indicator variable that takes a value of 1 for lotteries with more upside probability mass and 0 otherwise. $SocialUncorr$ is an indicator variable that takes a value of 1 for observations in treatment *Social Uncorr* and 0 otherwise. Specifications (1) and (2) include all observations of asymmetric lotteries from treatments *Alone* and *Social Uncorr* (i.e. lotteries with relatively more downside or upside probability mass). Specification (3) includes the subsample of more asymmetric lotteries (L_5, L_6, L_7, L_8 in Table 4.1). Specification (4) includes the subsample of less asymmetric lotteries (L_1, L_2, L_3, L_4 in Table 4.1). "Individual controls" include gender, age, whether the field of study is business related, risk aversion, impulsivness and patience, enviousness, a dummy whether subjects participted in laboratory expermints before. "Time and sequence fixed effects" include round fixed effects and a dummy for the sequence in which lotteries are shown.

Table 4.10: Comparisons and risk taking for uncorrelated lotteries

	Female with Female	Male with Male	Female	Male
	(1)	(2)	(3)	(4)
	risk share	risk share	risk share	risk share
$SocialUncorr$	-0.155**	0.128	-0.130**	0.061
	(0.076)	(0.095)	(0.058)	(0.082)
I_{upside}	0.186***	0.276***	0.186***	0.276***
	(0.065)	(0.063)	(0.064)	(0.063)
$SocialUncorr$ $\times I_{upside}$	0.150	-0.076	0.107	-0.032
	(0.168)	(0.0956)	(0.093)	(0.086)
Constant	0.389***	0.379***	0.389***	0.379***
	(0.035)	(0.058)	(0.035)	(0.058)
Individual controls	No	No	No	No
Time and sequence fixed effects	No	No	No	No
N	216	280	296	384
$Clusters$	27	35	37	48

Note: Random effects panel regression model. Dependent variable: Share of endowment invested into the risky lottery. Standard errors in parentheses, *p<0.10, **p<0.05, ***p<0.01. I_{upside} is an indicator variable that takes a value of 1 for lotteries with more upside probability mass and zero otherwise. $SocialUncorr$ is an indicator variable that takes a value of 1 for observations in treatment Social Uncorr and zero otherwise. Gender-specific subsamples. Specifiations (1) and (2) focus on subsamples in which grouped subjects are of the same gender in treatment *Social Uncorr* and of the corresponding same gender in treatment *Alone* (female-female compared to female and male-male compared to male). Specifications (3) and (4) focus on gender without considering the gender of the other (reference) subject in treatment *Social Uncorr*.

Table 4.11: Subjects of the same gender and uncorrelated lottery returns

4.B Mathematical appendix

4.B.1 Costs of deviating from the optimal investment without income comparisons

Given an optimal investment allocation a_1^* for the consumption part of the utility function $u(y_1(a_1))$, we can define the constant $\overline{U} = u(y_1(a_1^*))$. Thus the cost function defined in (4.5) of Section 4.2.1 can be stated as

$$\begin{aligned} C(a_1) &= \overline{U} - E[u(y_1(a_1))] \\ &= \overline{U} - [p_G \times u(\theta_G a_1 + (1-a_1)) + (1-p_G) \times u(1-a_1)] . \end{aligned}$$

Taking the first derivative with resepct to a_1 yields

$$\frac{\partial C(a_1)}{\partial a_1} = -p_G(\theta_G - 1) \times u'(\theta_G a_1 + (1-a_1)) + (1-p_G) \times u'(1-a_1) . \tag{4.11}$$

Considering the second derivative with respect to a_1:

$$\frac{\partial^2 C(a_1)}{\partial a_1^2} = -p_G(\theta_G - 1)^2 \times u''(\theta_G a_1 + (1-a_1)) - (1-p_G) \times u''(1-a_1) . \tag{4.12}$$

From (4.12) we see that

$$\frac{\partial^2 C(a_1)}{\partial a_1^2} > 0 \quad if \quad u''(\cdot) < 0,$$

and thus, assuming that subjects are risk averse for the standard part of the utility function (i.e. $u(y_1(a_1))$ is strictly concave) is sufficient to ensure that the cost function $C(a_1)$ is convex.

4.B.2 Uncorrelated lotteries and income-rank comparisons

The following holds for all a_1 and a_2 but the corner case $a_1 = a_2 = 0$. Analog to equation (4.6) in Section 4.2.1, the optimization problem for the situations when returns of risky lotteries are uncorrelated becomes

$$
\begin{aligned}
\text{(4.13)} \qquad \max_{a_1} \pi_1 \;=\; & p_G^2 \times [F(a_1)W + (1 - F(a_1))L] \\
& + (1 - p_G)^2 \times [(1 - F(a_1))W + F(a_1)L] - C(a_1) \\
& + p_G\,(1 - p_G)\,W + p_G\,(1 - p_G)\,L.
\end{aligned}
$$

The last two terms of equation (4.13) refer to the comparison utility gain (loss) when the lottery of one subject pays the “good-state” return θ_G while the lottery of the other subjects pays 0 in the “bad-state”. In these situations, independent of the subjects’ investment decisions, the subject receiving θ_G (0) will be ahead (behind), and thus, will receive utility from comparisons equal to W (L). Solving problem (4.13) yields the first-order condition

$$
\text{(4.14)} \qquad f(\widetilde{a}_1^s)[p_G^2\,(W - L) + (1 - p_G)^2\,(L - W)] = C'(\widetilde{a}_1^s),
$$

in which we define $\widetilde{a}_1^s$ as optimal investment when we incorporate social comparisons in case of uncorrelated lottery returns. We can further simplify (4.14) to

$$
\text{(4.15)} \qquad f(\widetilde{a}_1^s)[W - L][2p_G - 1] = C'(\widetilde{a}_1^s).
$$

Equation (4.15) is identical to equation (4.7) in Section 4.2.1 and it follows directly that $\widetilde{a}_1^s = a_1^s$.

4.C Experimental instructions

Welcome to the experiment![1]

Please read this instruction carefully and completely. Thoroughly understanding this instruction will help you to earn more money. In the experiment, your earnings are measured in Taler. At the end of the experiment we will convert the Talers you earned into Euros and pay you accordingly. The conversion rate is: **25 Talers = 1 Euro**. Additionally, each participant receives a show-up fee of 6 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 comply with this rule you will be asked to leave the laboratory without getting paid. In case you have a question before the experiment begins please raise your hand. Whenever you have a question at any time during the experiment please press the "help button" and we will help you.

Your task:

You will observe **Project A** and **Project B** on a common screen. Each participant receives 175 Taler that are fully invested into Project A and Project B. One participant decides for himself how he splits his endowment between Project A and Project B (0 to 175 Talers can be invested into a project). A random draw of the computer determines the split of the second participant's endowment between Project A and B. The roles of actively investing or of having the computer to decide are randomly assigned before the experiment starts and remain unchanged for the duration of the experiment.

Your earnings in the experiment depend on the investment decisions: each project exhibits multipliers (i.e. returns) multiplying the invested Talers. The two projects differ as follows:

- **Project A** offers two different multipliers of which just one will be chosen at the end of the experiment. One of the two multipliers leads to a higher payoff than the other. For each investment decision the common screen shows the probabilities that the high or the low multiplier is chosen.(See a stylized example for how project A works attached at the end of the instructions.)
- **Project B** offers a single multiplier of one; i.e. your investment into Project B will be paid to you one-to-one.

[1] The experiment was conducted in German. This appendix contains a translated version of the instructions for the treatment *Social*.

The realization of Project A (that is, whether the high or the low multiplier applies) will be simultaneously decided for all participants by the same dice rolls at the end of the experiment.
After the investment decision (the random investment by the computer, respectively) each participant will answer the following question on their private screen:

How satisfied are you with your current Project A/ Project B-combination on a scale 0 (very dissatisfied) to 10 (highly satisfied)?

Procedure
Overall, you will repeat this task several times, and thus, invest several times in a Project A and Project B. At each point in time the active participant will decide on the investment anew and independent of the pervious decisions while the computer randomly draws a new investment decision for the passive participant. In order to proceed to the next Project A / Project B combination, a "Done" button appears on your screen after one minute. You do not need to click and proceed immediately. It is important to take your time for each decision.
At the end of the experiment the computer will randomly select one Project A / Project B combination for the payoff.

- The randomly selected Project A/ Project B combination will appear on the common screen.
- The experimenter will roll dice clearly visible for all using a dice cup under ample shaking. This dice roll determines the investment outcome for both participants. The experimenter will enter the result into the common screen.
- The computer calculates the payoff of Project A according to your investment decision and to the outcome of the dice rolls. Your final result for Project A and Project B of the selected investment decision will be summed up and displayed on your private screen.

Consequently, your earnings will be determined by the investment into Project A and Project B and by the dice rolls.
After the experiment we ask you to provide some further information. As a matter of course, all of your provided information will be treated anonymously.

Thank you very much for showing up and good luck!

Practice round

Before the experiment starts you will participate in a practice round that will help you to better understand the experiment. The practice round is not relevant for the disbursement.

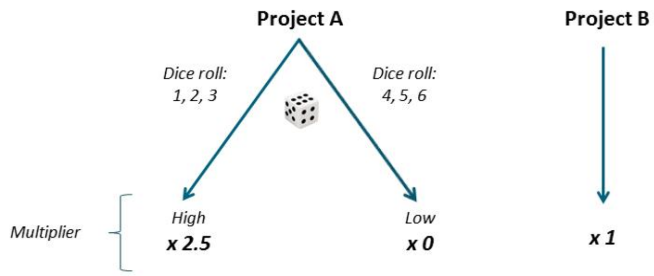


Figure 4.6: Example for illustration

Chapter 5

Concluding remarks

The evidence of social comparisons under uncertainty and the behavioral implications for risk taking is remarkably sparse in the field of economics. The present thesis contributes to filling this gap by applying experimental and empirical methods to investigate informational and comparison effects in chapters 2 and 3. Chapter 4 investigates behavioral implications of social comparisons under uncertainty and provides evidence of the effects of social comparisons on risk taking applying an experimental analysis.

The experimental results of Chapter 2 provide causal evidence of the informational and comparison effects when income is uncertain. Informational effects countervail and neutralize comparison effects if and only if uncertainty about future income is large enough. The evidence on informational effects is established in an environment when the informational value of signals from the income of other individuals is unknown. Thus, stronger informational effects should be expected when the relationship between the incomes of individuals is less ambiguous. Unexpectedly, social comparison and informational effects are asymmetric. While individuals react significantly to being behind in income and to observing bad signals from the income development of others, I find neither a significant effect for being ahead nor for good signals. I interpret this finding as individuals being more reactive to "bad news" than to "good news." An interesting implication of both simultaneously working effects is that individuals may (ceteris paribus) experience lower acceptance of income inequality. Catching up to richer individuals will be more important than the possible disutility resulting from other individuals catching up to oneself. At the same time, a significant response to bad signals from the income development of others may reinforce Varian's (1980) argument of "redistributive taxation as a social insurance" in an uncertain environment.

From an international perspective, the present thesis finds evidence in line with social comparison and informational effects between residents of different countries. Individual subjective well-being relates to the readily observable economic growth of the reference countries. The results of this analysis should be taken with a pinch of salt with respect to the limited data that is available on a global scale. An analysis with (unavailable) panel survey data, instead of the applied repeated cross-sectional data, would certainly improve the identification of effects. Nonetheless, I make use of the data that is available to derive relevant findings. The evidence on international comparison effects suggests that the theoretical investigations of Aronsson and Johansson-Stenman (2014, 2015) on the provision of global public goods and international tax coordination are relevant. What is more, informational effects that have not been considered in this branch of literature turn out to be important and to even dominate comparison effects when countries exhibit close economic ties. Not least, while social comparison effects have the potential to impede international cooperation between countries, countervailing informational effects might act as facilitating factors. Thus, the evidence suggests that advances in economic integration between countries have the potential to align the interests of these countries and might be socially beneficial beyond the standard international-trade arguments.

The theory-guided evidence of Chapter 4 shows that social comparisons affect risk taking behavior. While a possible interpretation of the evidence is that the income-rank is relevant to individuals, I find no evidence of other forms of social comparisons, such as an extension of the concept of loss aversion to the social dimension. Furthermore, the results of Chapter 4 give guidance as to where we should expect social comparison effects on risk taking: Comparison effects particularly emerge for lotteries with more downside than upside probability mass that are commonly positively skewed. Skewed distributions are applicable in various situations, such as in financial and insurance markets, a natural environment for risk taking decisions. The findings of Chapter 4 further show that comparison effects on risk taking are reinforced if individuals are of the same gender, and thus, are more similar. This finding implies that, ceteris paribus, some industries and occupational fields may be more prone to comparison effects on risk taking than others. Effects can be expected to be stronger when managers and employees are predominantly of the same gender such as in the financial sector or in human-resources-related positions.

The results of the present thesis underline that social comparison and informational effects are empirically relevant factors in uncertain environments. While social comparisons are widely analyzed and understood, informational effects should not be neglected. The countervailing nature of informational effects in various situations needs to be considered to understand the implications of social comparisons under uncertainty. Finally, I hope that the findings with respect to risk taking behavior not only improve our understanding of the matter but also stimulate further work in this far from comprehensively researched branch of the literature.

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