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ESSAYS IN BEHAVIORAL ECONOMICS

by

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CENTRAL EUROPEAN UNIVERSITY
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DISCLOSURE OF CO-AUTHORS CONTRIBUTION

Title of paper: Holding a Portfolio and Wishful Thinking

Co-authors: Balázs Krusper

The nature of the cooperation and the roles of the individual co-authors and approximate share of each co-author in the joint work: The paper was developed in close cooperation with Balázs Krupser throughout all stages. We designed both the main question and the design together. We shared the tasks during the literature review, programming, regression analysis and writing as well.

Title of paper: The Effect of Managers' Health Shocks on their Future Employment

Co-authors: Kinga Marczell

The nature of the cooperation and the roles of the individual co-authors and approximate share of each co-author in the joint work: The paper was developed in close cooperation with Kinga Marczell throughout all stages. We designed both the main question and the empirical strategy together. We shared the tasks during the literature review, programming, regression analysis and writing as well.

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Abstract

Two of the three essays are experimental studies on motivated beliefs, while the third chapter is a health economics paper on the labor market consequences of a health shock. The first chapter shows that people distort beliefs about third parties to excuse self-interested behavior. The second chapter (joint work with Balázs Krusper) demonstrates inflated beliefs as a result of holding a portfolio. The third chapter (joint work with Kinga Marczell) uses a Hungarian administrative data set and shows how managers' career path are differentially affected by a health shock compared to the career path of non-managers.

Chapter 1 - I Gain, You Mitigate, He Keeps

Motivated Beliefs about the Success of Third Parties to Excuse Self-interested Behavior

This paper examines whether people distort beliefs about third parties – such as the ability of scientists to offset one's environmental impact – to excuse self interested behavior. I set up a lab experiment in which dictators decide how much money to take, with the success of a third party in solving a puzzle determining whether the money comes from passive participants or another source. The experiment exogenously varies whether it is the success or the failure of the third party that results in taking the chosen amount from passive participants. After participants decide the amount, they report their beliefs about the success of the third party. I find that the proportion of participants believing in the success of the third party is 13 percentage points higher when the success of the third party results in taking the money from a different source. With monetary incentives for correct beliefs, this effect goes down to 6 percentage points and becomes insignificant. This means that the presence of a third party might result in even more self-interested behavior than it has been previously thought.

Chapter 2 - Holding a Portfolio and Wishful Thinking

with Balázs Krusper

This paper investigates whether people distort their beliefs about ambiguous outcomes of products as a result of owning them. We set up a lab experiment where people have to form beliefs about portfolios' payoff probabilities. The experiment exogenously varies whether the good, or the bad portfolio is assigned to the subject. We find when subjects hold the portfolio they have a 2.75 pp higher belief about its payoff probabilities than when they don't hold the portfolio. This effect is significant on a one-sided test. The study also tests for asymmetric belief updating, effects of changing payoff size, and changes in incentives for correct guessing. We find neither sign of differential belief updating, nor an effect of changes in incentives for guessing correctly. When the amount the portfolio pays off is increased, there is less of an effect of owning it. This is hard to reconcile with the current theories, however, disappointment aversion might be a plausible explanation.

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Chapter 3 - The Effect of Managers' Health Shocks on their Future Employment

with Kinga Marczell

Using Hungarian administrative data set, this study analyzes the effect of a health shock on employment outcomes – such as wage and employment. Distinguishing managers, we estimate interaction effects as well. A health shock results on average 45pp permanent drop in the likelihood of employment starting one year after the event, and a temporary drop in wages. The likelihood of employment for managers, however, are affected 5.5pp less and their wages experience a moderate drop lasting even after the health shock. These differences can not be explained by neither observables, nor differential trends prior to the health shock.

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Foremost, I would like to express the deepest appreciation to my supervisor, Botond Kőszegi. He was my mentor and providing wise and gentle support guided by my long term interests even in times when I felt unmotivated and hopeless. He was always taking my ideas seriously that gave me the courage to pursue even brave ideas that looked ridiculous at times. I am grateful to Marc Kaufmann for being a partner in brainstorming and being critical about my writing and communication in general. His dedication to cleanly conveying ideas gives me enormous motivation to raise my standards and develop myself in this direction. I would also like to thank Alessandro de Chiara and Ester Manna who always gave me useful feedback and encouragements. They further convinced me that one can do excellent research without being too stressed about it and I am grateful for that.

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I am really grateful to my parents, Gábor and Bernadett, who supported me throughout my graduate years and never questioned my decision of doing graduate studies. I am thankful to my father Gábor Hajdu, who was always available to listen to my research ideas and – going around with an open mind – he told his ideas and observations to me. With the years I have leaned how to listen and use his input. I thank my sister Kinga and my brother-in-law Balázs, who were always happy to listen to my ideas and proved to be a tough audience to convince about the relevance of the questions I pursue. Your critical view sharpened my arguments and made me a better researcher and I am really grateful for that. I would like to thank to my girlfriend, Rebeka Honti, who were standing by my side before the finish line and providing everything to me to be in the best form. This work benefited a lot from her great writing skills. She was never too tired to listen certain paragraphs and gave her critical opinion wrapped in love.

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Chapter 1

I Gain, You Mitigate, He Keeps

Motivated Beliefs about the Success of Third Parties to Excuse Self-interested Behavior![1]

1.1 Introduction

A growing literature in experimental economics documents how individuals use excuses to act self-interestedly, presumably to maintain positive beliefs about themselves, while at the same time taking money. For instance, subjects behave more self-interestedly when they can avoid learning how their decisions affect others[2]; if they can distort their beliefs in a self-serving way[3]; and if they can rely on the possibility that their decision doesn't

[1]I am grateful to Botond Kőszegi, for his support and financing of this project. Thanks also to Alessandro De Chiara, Alex Imas, Alexander Cappelen, Anna Sódor, András Molnár, Armin Falk, Bertil Tungodden, Christine Exley, Christopher Heintz, Erik Sørensen, Gary Charness, Katherine Coffman, Marc Kaufmann, Mia Karabegović, Philipp Albert, Sevgi Yuksel, Václav Korbel, and seminar participants at UCSB, CEU and the 13th Nordic Conference on Behavioural and Experimental Economics for their helpful comments and suggestions. I am appreciative of the hospitality and assistance from Tomas Miklanek, Jan Vávra and the Laboratory of Experimental Economics in Prague. Any remaining errors are my own.

[2]Dana et al. (2007), Bartling et al. (2014), Grossman (2014)

[3]Konow (2000), Haisley and Weber (2010), Di Tella et al. (2015)

influence the outcome[4].

Past work in experimental economics suggests that people use a few types of excuses depending on the specific context they are in. This paper investigates a novel type of excuse that is likely to be available to individuals in many, if not most real-life social decisions: a type of excuse based on what outsiders to the specific interaction do or can do. Consider the following example: When a typical person ponders whether to use plastic bags — a convenient choice but one that hurts the environment and therefore other people — he presumably takes into account, at least with some weight, the environmental impact. But that environmental impact, in turn, depends on whether scientists figured out a way to fully recycle plastic bags. If the person believes that scientists have figured or will soon figure this out, then it is more acceptable to make the convenient choice of using plastic bags. To make himself feel better, he could convince himself that scientists are excellent at advancing the technology of recycling. Whether such biased beliefs arise is the focus of this study.

In a lab experiment, I find that people distort their beliefs about third parties – such as the ability of scientists to offset one's environmental impact – to excuse self-interested behavior. Specifically, people are up to 13 percentage points more likely to believe that the third party succeeds when success leads to offsetting the negative impact. With monetary incentives for correct beliefs, this effect goes down to 6 percentage points and becomes insignificant. The first consequence is, the presence of a third party might result in even more self-interested behavior than it has been previously assumed. Secondly, any policy that decreases the scope for belief distortion, such as giving information that is considered extremely reliable by receivers, results in less self-interested behavior. Thirdly, interested parties, who have a stake in the role of the third party might have different beliefs driven solely by excuse-making, than those who have no immediate personal interest in the outcome. This can lead to opposing views in policy debates about the need for regulation.

After the literature review, Section 1.3 describes the experimental design. It is a

[4] Dana et al. (2007), Andreoni and Bernheim (2009), Falk and Szech (2013b)

modification and extension of the classical dictator game, where one person, called the *Choice Maker*, decides how much to take from a Passive Participant, by adding a third player who can influence the outcome. Just like in a dictator game, the *Choice Maker* decides how much to take for himself. This amount, however, might not be taken from the Passive Participant depending on the success of the third player (called the *Riddle Taker*) in solving a puzzle. To cleanly identify the excuse motive, there are two treatments: In the Friend-treatment the chosen amount is not taken from the Passive-Participant if the *Riddle Taker* succeeds, but taken otherwise. So, the *Riddle Taker* is basically working for the *Choice Maker*, hence the name, Friend treatment. In the Enemy treatment the amount is taken if the *Riddle Taker* succeeds, that is, *Riddle Taker* is working against the *Choice Maker*, hence the name, Enemy treatment.

Importantly, the *Riddle Taker* doesn't know in which treatment he is in and this is common knowledge. As a consequence, in the absence of excuse-making, we would predict no difference in *Choice Maker*'s belief about Riddle Takers' success across treatments. However, *Choice Maker* might want to believe that the chosen amount is not taken from the *Passive Participant*. This would result in *Choice Maker*s' belief about the *Riddle Takers*' success being higher in the Friend treatment than in the Enemy treatment.

So, that is exactly what is elicited. After *Choice Maker*s decide how much to take, they made two types of guesses regarding *Riddle Takers*' success in solving the puzzle. First, they had to make a yes/no guess whether their own assigned *Riddle Taker* solved the riddle (*Individual beliefs*). Second, they had to guess the average success rate of the *Riddle Takers* by choosing a 10-percentage-point wide interval going from 0 - 10, 10 - 20 etc. (*Population beliefs*).

In Section 1.4, I use a simple model to highlight the mechanism of excuse-making and derive predictions. I generalize the Charness and Rabin (2002) model of other-regarding preferences to non-linear utility with uncertain consequences and allowing for motivated beliefs, focusing on the **Friend treatment**. In the model, each *Choice Maker* chooses a sure amount $a \in [0, a_{max}]$ that he receives himself, and also chooses a belief about whether the *Riddle Taker* succeeds, where deviations from the prior are costly. Given weak functional form assumptions, if the *Choice Maker* puts more weight on her material utility than on the *Passive Participant*'s material utility, her optimal chosen amount is positive (Proposition 1). More to the point, if belief distortion has a sufficiently low cost, then belief distortion is optimal (Proposition 2) and results in *Choice Makers* having more optimistic beliefs than their prior. Once people exhibit belief distortion, the marginal cost of choosing a higher amount is lower. Therefore, people choose a higher amount than they would with an unchanged prior (Proposition 3). For illustration, taking this to the extreme: if people can believe that their self-interested actions have no negative consequences, the best they can do is to take as much as possible.

Section 1.5 present the results from 2 experiments. One, where only the *Individual beliefs* were monetarily incentivized, and one, where only the *Population beliefs* were. I find that *Choice Makers* distort their beliefs about their own *Riddle Taker* in a self-serving way. Specifically, in the case of no monetary incentives for correct guesses about the own assigned *Riddle Taker*, 96% of *Choice Makers* in the **Friend treatment** say that their directly assigned *Riddle Taker* was able to solve the puzzle, while this number in the **Enemy treatment** is only 83%. This difference, the previously mentioned 13pp, is statistically significant. [5]

To test whether the effect for *Individual beliefs* holds up when there is an actual monetary cost of belief distortion, I ran the second experiment where only *Individual beliefs* were monetarily incentivized. In this run, the point estimate decreases and loses statistical significance ($\Delta = 6$pp), making the evidence for the effect of monetary incentives on belief distortion inconclusive. In both runs, the treatment effect estimates for *Population beliefs* point to the right direction, however, not significant with the current sample size. In case, one would consider pooling samples from the two runs, the treatment effect for *Population beliefs* is marginally significant ($\Delta = 4.41$pp, p = .06) on a one-sided t -test.

[5] Based on a pre-experimental survey measuring social preferences (see Murphy et al. (2011) for details) I conducted heterogeneity analysis and found that, while there is no significant difference in the incentivized *Population beliefs* across treatments on average, there is a high and significant treatment effect for less prosocial *Choice Makers* ($\Delta = 13.6$pp; $p < .05$). However, this was an ex-post analysis and did not replicate in a follow-up experiment ran to cleanly test this effect. At the same time, this evidence is inconclusive, as the experiment ex-post seems to be underpowered.

In conclusion, the experiment does not provide enough evidence to reject the hypothesis that the effect decreases, when participants are paid for correct guesses. However, the results indicate that people distort their beliefs in a self-serving manner to believe that the negative impact of their actions is mitigated, and this effect seems to be stronger for *Individual beliefs*, which potentially serve as a more direct excuse, than *Population beliefs*.

1.2 Related Literature

All previous work on excuses and self-interested behavior is fundamentally about two-person economic exchange. The few papers discussing shared responsibility situations show that the presence of third parties might result in self-interested behavior, but they don't discuss the mechanism at play. My research both goes beyond the two-person setting and pins down a mechanism of excuse-making in a social context.

Just as this research looks at beliefs, there is previous work showing that people may think about counterpart's action in a self-serving way. Di Tella et al. (2015) look at a two-player reciprocal setting where one player, the Allocator, can decide how many tokens he wants out of the 20 tokens they earned together on a real-effort task. Simultaneously, the other player, called the Seller, has to decide whether to sell the tokens to the experimenter for a price of $1/token - in which case Seller receives an additional bribe of $10, or to sell the tokens for a price of $2/token. They find that in a treatment where less tokens are blocked, hence, the Allocator can take more from the Seller, the Allocator is more likely to believe that the Seller accepted the bribe and the tokens are sold at a lower price. This result is consistent with the interpretation that in a reciprocal situation, people avoid altruistic actions by distorting beliefs about others' altruism. Schwardmann and van der Weele (2016) also provide evidence of self-serving belief distortion. In their work subjects have to guess their own relative performance on an intelligence task. In one treatment, however, they also have to convince others about their high relative performance. They show that in case subjects have to convince others – and there is a monetary reward in case they succeed – they are more overconfident about their own relative ability than in case when they do not have to convince anybody. They also show that subjects believe that ability matters for being convincing to others. This result is consistent with the interpretation that people distort their belief about their own ability in order to be perceived as more able by others. There is also some recent research on norms, conformism and motivated beliefs (Bicchieria and Dimanta (2018); Charness et al. (2017)). My work contributes to this literature in two ways. First, it provides evidence of motivated beliefs in a broader context when the impact of one's action on others depends on other actors as well. Second, it presents evidence that people can distort beliefs about the ability of others and this is a domain that has not been investigated so far.

My research speaks to other moral wiggle room related papers where the exact consequences of one's action are unknown (Dana et al. (2007); Bartling et al. (2014); Grossman (2014)). These papers show that people sometimes try to avoid information related to the consequences of their decision and those who are not informed act in a more self-centered way. One interpretation is that people can maintain optimistic beliefs about the consequences of their decisions as long as there is no conflicting information, therefore information avoidance can be beneficial to them.

This paper also relates to the literature on morals where subjects had to decide over life and death. It has been shown for instance that people value a life of a mouse less if it is evaluated through a bilateral double auction market instead of a binary choice between a certain amount and the life of the mouse (Falk and Szech (2013a)). Even without the market situation people are more likely to sacrifice lives of mice for monetary gains if the responsibility is shared with other people (Falk and Szech (2013b)). My research offers an alternative mechanism that works through motivated beliefs. With the treatment it might get easier for people to act self-interestedly and still think morally of themselves by believing that others also act in a self-interested way.

Finally, my research also relates to the research on excusing selfishness (Konow (2000); Andreoni and Bernheim (2009); Haisley and Weber (2010); Exley (2015); Exley and Kessler (2017, 2018)), where people use the ambiguity of the exact consequences of giving, or the degree of freedom in the interpretation of what is fair in a self-serving manner. I propose an excuse based on what outsiders to a specific interaction do, or can do.

Table 1.1: Time Line

	Roles			
Stages	*Choice Maker*	*Passive Participant*	*Riddle Taker*	Spectator
Pre-survey	Allocation choices	Allocation choices	Allocation choices	Allocation choices
	Proxy questions	Proxy questions	Proxy questions	Proxy questions
	Justice sensitivity	Justice sensitivity	Justice sensitivity	Justice sensitivity
Main Stage	Puzzle and solution	Puzzle and solution	Solving the puzzle	Puzzle and solution
	Task explanation	**Population beliefs**		**Population beliefs**
	Control questions	Reading CM's instruction		
	Deciding the amount			
	Individual belief			
	Population beliefs			

1.3 Design

In April 2018, I recruited all together 320 participants in the Prague Laboratory in Experimental Economics to participate in one of the 10 sessions using ORSEE (Greiner (2015)). The experiment was conducted in Czech with participants mostly from the University of Economics Prague studying Economics, who speak Czech as their mother tongue. The average earning was around 300 CZK (about 12 EUR) and subjects received a 100 CZK for participating. The experiment consisted of 2 main parts, a pre-experimental survey and the main stage (see the detailed timeline in Table 2.1). A pre-experimental survey was part of the experiment that participants had to complete before the main stage. The invitation letter stated that a pre-experimental survey is a prerequisite to participate and upon registering to any sessions another e-mail was sent out to participants with the link to the pre-experimental survey.

In the main stage participants draw tokens from a sack and occupy the boxes in the lab with the number on the token. The main stage is programmed in zTree (Fischbacher (2007)). Each participant is randomly assigned one of the following roles: *Choice Maker* (CM), *Riddle Taker* (R), *Passive Participant* (PP) or Spectator (S). The first session contains the same number of *Choice Makers*, *Riddle Takers* and *Passive Participants.*

From the second session on, however, there is only one *Riddle Taker* on each session. *Choice Makers* and *Passive Participants* are randomly paired and each *Choice Maker* is told that a *Passive Participant* is randomly assigned to him and only him and a *Riddle Taker* is assigned to him as well. If a participant is not assigned to any of these roles he gets assigned to be a Spectator. Spectators don't have a substantive role in the experiment. The role was created, so everybody who shows up can participate.

To most reliably detect belief distortion, I have designed the experiment so that *Choice Makers* are likely to prefer to increase the payoffs of Passive Participants. Existing research on social preferences suggests that this may not be the case when a *Choice Maker* is behind the *Passive Participant* in terms of payoffs. To make sure that a *Choice Maker* is never behind, *Choice Maker* and Passive Participant start the experiment with the same endowment. This way the only case when lower payoff for the counterpart is strictly preferred is when the participant has competitive preferences, which is relatively uncommon. As the final parametrization the *Choice Maker* and the PP are endowed with 150 CZK each. The *Choice Maker* has to choose an amount he receives between 0 and 150 CZK over her initial endowment. The *Choice Maker* always receives the chosen amount, however, whether this amount is taken from the *Passive Participant*, or provided from the experimenter's budget depends entirely on the performance of the *Riddle Taker*. *Riddle Taker*'s task is to try to solve a specific puzzle taken from Loewenstein et al. (2006) within 10 minutes (see Figure 1.1 for the puzzle and the instruction).

The main variable of interest is *Choice Makers*' beliefs about the Riddle Takers' success. The treatment variation is whether *Riddle Taker*'s success, or failure results in *Choice Maker's* chosen amount not taken from the PP. Specifically, in the **Friend Treatment**, if the *Riddle Taker* is able to solve the puzzle, the chosen amount is NOT taken from the *Passive Participant*, otherwise, it is. In the **Enemy Treatment** it is vice versa (see Table 1.2). This setup, besides having a larger treatment variation, also guarantees a natural comparison group – where the stages across treatments are the same – as opposed to just estimating an average prior belief – or non-distorted belief – by using subjects without a stake in the decision with the only task to guess the success rate.

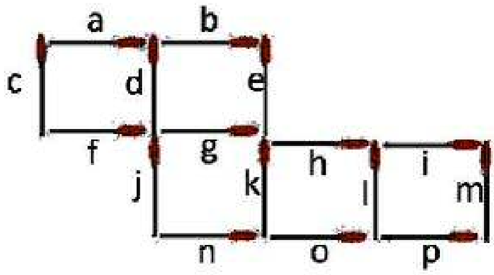

Figure 1.1: Puzzle

Instruction: Look at the figure! It consists of matches from *a* to *p*. By repositioning only two of the matches, how would you create four squares instead of five? Remember that the squares may be repositioned, but the new squares have to be the same size as the old ones. Just as in the figure, all matches must be used and have to end up as sides of squares! Which of the 2 matches have to be repositioned?

Table 1.2: Treatment variation

Friend Treatment		
Riddle Taker succeeds	→	no money is taken from the PP
Riddle Taker does not succeed	→	money is taken from the PP
Enemy Treatment		
Riddle Taker succeeds	→	money is taken from the PP
Riddle Taker does not succeed	→	no money is taken from the PP

To make sure that *Choice Makers* understand the possible consequences of the Riddle Taker's performance, an example is provided with a random chosen amount explaining the payoffs to both the *Choice Maker* and the PP in case the Riddle Taker succeeds and in case he does not. This is followed by many control questions asking the Choice Maker's and PP's payoffs for different scenarios. In case of no mistake the Choice Maker earns 50 CZK. The fraction of Choice Makers answering all questions correct is approx. 80%. After the answers are submitted a feedback is received about each answer determining whether it is correct, or not, together with the task explanation, hence, subjects can learn from their mistakes.

The information structure is such that the *Riddle Taker* doesn't know which treatment he is in, but knows that he gets randomly assigned to one at the end of his stage. This information is common knowledge. The purpose of this specific information structure is to ensures that in the absence of motivated beliefs *Choice Makers* have the same beliefs about the *Riddle Taker*'s ability in the two treatments. In the presence of motivated beliefs, however, *Choice Makers* in the Friend (Enemy) Treatment are motivated to inflate (deflate) their beliefs about the ability of the Riddle Taker.

A possible threat to identify the effect of motivated beliefs is the projection of information about the solution of the puzzle. If the effect of information projection is sizable and there is a difference, due to sampling, across treatments in the subjective belief of being able to solve the puzzle a difference in success rate guesses across treatments may purely come from having an unbalanced sample with respect to subjective beliefs about knowing the solution. In order to get around this problem – at the expense of reducing uncertainty and leaving less space for motivated beliefs – the solution is given out to *Choice Makers* together with the puzzle.

After the *Choice Maker* chooses the amount for himself he has to guess if her assigned *Riddle Taker* was able to solve the puzzle (*Individual beliefs*). This is a simple yes, or no question. As the success of the own assigned *Riddle Taker* serves as the most direct excuse the *Choice Maker* can have, this question makes the role of the *Riddle Taker* more salient. The final stage is the belief elicitation about the success rate (*Population beliefs*) and it is implemented as a surprise stage. This way there are less hedging concerns. That is, if the *Choice Maker* had known that he could earn money at the end for correct beliefs he might have taken more and report pessimistically about the consequences. As a consequence, he either takes a lot from the *Passive Participant*, but earn money for correct beliefs, or do not earn for correct beliefs, but not taking from the *Passive Participant* either. In order to have a belief elicitation method that is easy to understand *Choice Makers* have to guess the success rate by choosing a 10 percentage point wide intervals where intervals go as 0 - 10%, 10 - 20%,...,90 - 100%. If the interval contains the real success rate, Choice Maker receives 200 CZK. This belief elicitation method is purposefully simple, so, subjects can easily understand without allocating all of their attention to comprehend some more complex payoff scheme, hence, still keeping the benefits of motivated beliefs salient.

1.4 An illustrative model

In this section I use a simple model to motivate the setting I study and to describe some predictions that give insight for the mechanism. For simplicity the model describes excuse in case of the **Friend treatment**. I generalize the Charness and Rabin (2002) social preference model to non-liner utilities and endogenous beliefs, using a similar setting as in the actual design. I formalize social preferences when outcomes for a third-party is uncertain and the Choice Maker can choose his belief about the consequences. Then I describe the properties of the optimal belief.

Consider a Choice Maker, who can choose an amount for himself from an interval $a \in [0, a_{max}]$ that he receives for sure. However, whether this same amount is taken from his counterpart (called *Passive Participant* (PP)) depends on an external factor. The Choice Maker has a prior belief of success p_0 that is of the external factor acting in a way that the amount is not taken from the PP. Both the Choice Maker and the PP start with an endowment of e. The Choice Maker's actual payoff is $e + a$, but he is prosocial and cares about the PP in the following manner:

$$U_{ChoiceMaker}(a) = \sigma u_{ChoiceMaker} + (1 - \sigma)E_{p_0}(u_{PP}), \tag{1.1}$$

where $u_{ChoiceMaker} = u(e+a)$ and $E_{p_0}(u_{PP}) = p_0 u(e) + (1-p_0)u(e-a)$. Hence, the Choice Maker cares about the utility derived from his outcome and the expected utility of the Passive Participant derived from his outcome. The Choice Maker aggregates the two in a linearly separable way putting a relative weight of σ on the own utility term, where σ can be interpreted as a measure of selfishness. For simplicity I assume that the Choice Maker has a standard utility function over payoffs, and that he uses the same utility function when thinking about the PP's expected utility.[6]

[6] In the actual experiment people decide over money. This case the motivation for applying a standard utility function over the possible outcomes may seem questionable from the theoretical point of view. However, people don't behave in a purely outcome based manner in the lab even when they decide over money. There can be many reasons why this is the case, but what is important is that a model with risk aversion is likely to be a better approximation of how people behave in this setting than a purely outcome based one.

Assuming that $u' > 0$, $u'' < 0$ and that we have an interior solution the Choice Maker's optimal decision, a_0^* has to satisfy the following first-order condition:

$$\frac{\sigma}{(1-\sigma)}\frac{1}{1-p_0} = \frac{u'(e-a_0^*)}{u'(e+a_0^*)}. \tag{1.2}$$

Observe that the right-hand side is increasing in a_0. Hence, the amount people choose is decreasing in prosociality and increasing in the belief of success.

Now, assume that the Choice Maker can choose his belief that is his subjective probability p about the success with a cost proportional to $C(p - p_0)$. Her preferences over the amount to choose and his belief is the following:

$$U_{ChoiceMaker}(a,p) = \sigma u_{ChoiceMaker} + (1-\sigma)E_p(u_{PP}) - \eta(\sigma)C(p-p_0), \tag{1.3}$$

where $E_p(u_{PP}) = pu(e) + (1-p)u(e-a)$ and $C'(0) = C''(0) = 0$, $C'_{>0} > 0$, $C''_{>0} > 0$. $\eta(\sigma)$ represent the possibility that the cost of belief distortion might depend on the level of prosociality. As the Choice Maker's belief about PP's expected payoff directly depends on Choice Maker's subjective probability that the amount he chooses is not taken from the PP, his beliefs are motivated by prosocial considerations. By inflating p, the Choice Maker can believe that the bad outcome for PP is happening with a lower probability.

The Choice Maker's optimal decision is a monetary amount-belief pair (a^*, p^*) that has to satisfy the following first order conditions:

$$\frac{\sigma}{(1-\sigma)}\frac{1}{1-p^*} = \frac{u'(e-a^*)}{u'(e+a^*)} \tag{1.4}$$

$$u(e) - u(e-a^*) = \frac{\eta(\sigma)}{1-\sigma}C'(p^*-p_0), \tag{1.5}$$

The following proposition immediately follows from Equation 1.4.

Proposition 1 *(Chosen Amount) $\sigma > .5$ is a sufficient condition to have $a^* > 0$.*

Intuitively, if one has a higher relative weight on his own utility than on the counterpart's utility, he is better off by taking from the counterpart starting with the same endowment.

Proposition 2 *(Optimal Belief) $p^* > p_0$, that is, belief distortion is optimal, assuming $\sigma > .5$.*

The intuition is the following: Choice Maker can increase his utility by increasing his expectation over *Passive Participants* material utility through increasing his subjective probability that the chosen amount is not taken from the Passive Participant, that is, *Riddle Taker* succeeds. The following prediction is not directly tested by the current design, but it is an important implication of the belief distortion.

Proposition 3 *(Optimal Action) $a^* > a_0^*$, that is, the opportunity of belief distortion makes people choosing higher amounts.*

As Choice Makers engage in belief distortion – coming from Equation 1.4 – the optimally chosen amount is actually higher than what they would choose without belief distortion. If people believe that there is less chance of taking the chosen amount from the PP and additional increase in the chosen amount now hurts the *Passive Participant* less in expectation as without the belief distortion. As a result, it is optimal to choose a higher amount once the subjective belief about the likelihood of taking from the *Passive Participant* is smaller. To have predictions related to the extent of prosociality one has to specify $\eta(\sigma)$. Unfortunately, at this point we don't know much about the nature of the cost of belief distortion. However, we have good reasons to think that these costs are partly psychological and that people with different prosociality traits differ in how easy it is for them to distort beliefs. If the distortion would be only constrained by making sub-optimal decisions in the future then one would have ridiculously positive beliefs about things that are not relevant for future outcomes (e.g. having unrealistic beliefs about one's past achievements, or personality). There are certainly people, who have ridiculously positive view of certain things, however, most people seem to be constrained by what they know and what they consider plausible. The argument for why the psychological cost for people with different prosociality might differ is twofold. First, because less prosocial people care less about the counterparts' outcome they focus less on the consequences of their decisions in relation to others and likely attend less to information about these consequences. Second, less prosocial people might be able to maintain their self-image exactly by having a lower cost of belief distortion, hence, distorting to a greater extent. Both of these plausible mechanisms act in the way that if $\eta(\sigma)$ is not constant across types, it is lower for less prosocial types, such that $\eta'(\sigma) < 0$.

Proposition 4 *(Optimal Action and Prosociality) If $\eta(\sigma)$ is such that $\eta'(\sigma) \leq \frac{-\eta(\sigma)}{1-\sigma}$, then for any σ, σ', where $\sigma < \sigma'$ it is true that $a^*_\sigma < a^*_{\sigma'}$ and $p^*_\sigma < p^*_{\sigma'}$. That is, less prosocial types (people with higher σ) take more and distort more.*

1.5 Results

I present the results from 2 experiments. One, where only the *Individual beliefs* were monetarily incentivized, and one, where only the *Population beliefs* were.

In the first experiment – where only the *Individual Beliefs* are incentivized –, there were 10 sessions conducted with 119 *Choice Makers* all together in April 2018. The average earning for the 45 minutes for Choice Makers was 415 CZK. The fraction of correct answers for the control questions were relatively high: 92 out of 119 Choice Makers completed the hypothetical scenarios without any mistake (see Figure 1.2).

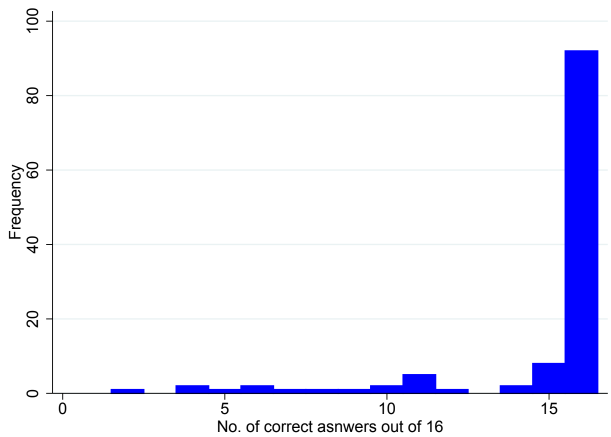

Figure 1.2: The distribution of correct answers for hypothetical scenarios about chosen amounts and *Riddle Taker*'s success.

Figure 1.3 shows the distribution of chosen amounts by the Choice Makers. As predicted by Proposition 1 almost every Choice Maker chooses a positive amount.[7]

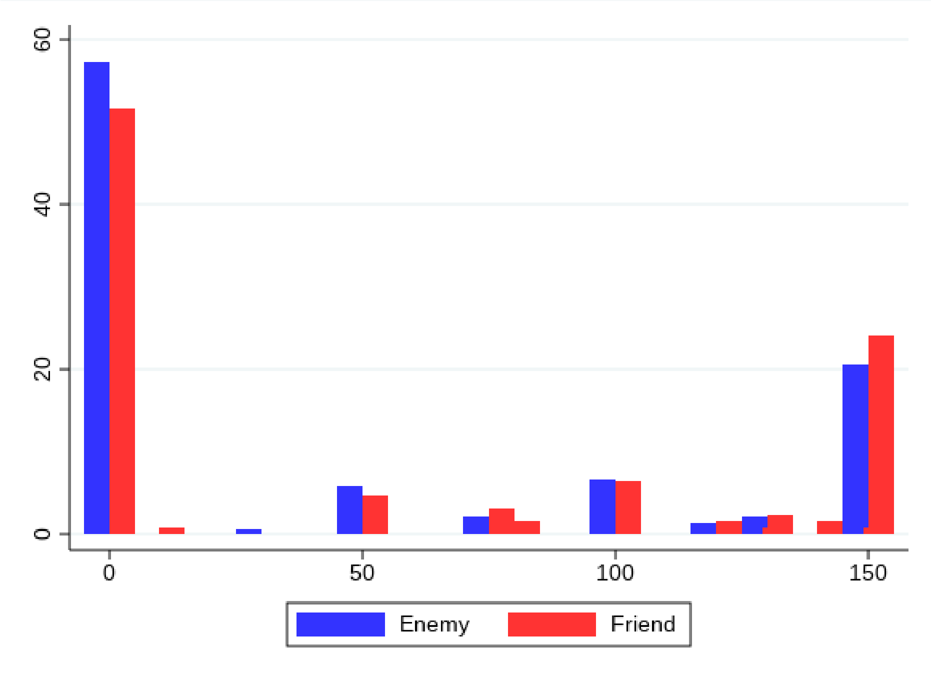

Figure 1.3: The empirical distribution of the chosen amount by the Choice Maker in the Enemy and in the Friend treatment.

After Choice Makers decided how much they want for themselves they are shown the possible payoffs in case the assigned *Riddle Taker* succeeds and in case he does not. On the next stage subjects were asked if they think their assigned Riddle Taker was able to solve the puzzle within the allocated 10 minutes (*Individual beliefs*). This question is binary and not incentivized. The purpose of this question is twofold. First, it allows to test whether Choice Makers report in a self-serving way when there is no actual monetary consequence of belief distortion. Second, it makes the role of the *Riddle Taker* more

[7]There is only one subject choosing 0. This subject has a $\hat{\sigma}_i = -.4$ and his success rate guess is between 90%-100% being in the Enemy treatment. This means according to the pre-experimental survey he is really prosocial and believes that he is certainly taking the chosen amount from the Passive Participant. Hence, him choosing 0 is consistent with the theoretical predictions. The average amount is 119 CZK. Subjects' choices are bunching on round numbers, even though they were allowed to choose any integer. One reason for choosing round numbers may come from the example amounts used in the experiment. This is a random sum that is a multiple of 10 between 0 and 150 CZK used to describe the payoffs for the *Choice Maker* and for the *Passive Participant* in case the *Riddle Taker* succeeds and in case he is not. The random sum in the example has no effect on the Choice Maker's chosen amount (the correlation is ρ = -0.0064, p=.9448).

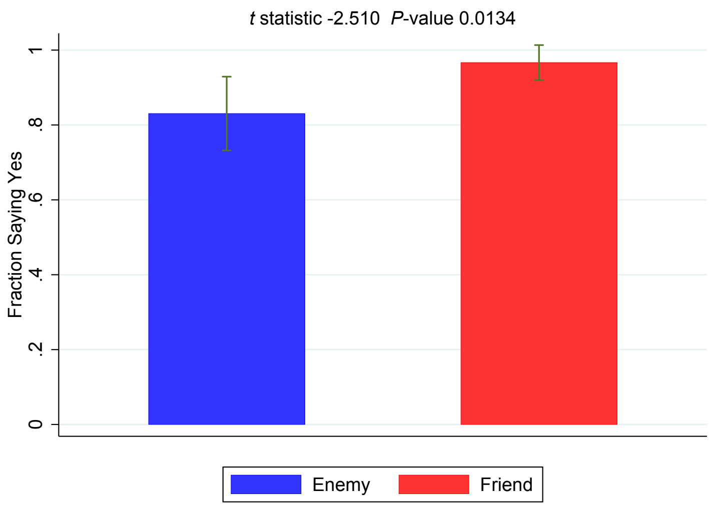

Figure 1.4: Fraction of subjects saying that their assigned *Riddle Taker* was able to solve the puzzle. The error bars show the 95% confidence interval of the point estimates. The difference is statistically significant (p = 0.0134).

salient. Figure 1.4 shows the fraction of subjects saying that their assigned *Riddle Taker* was able to solve the puzzle separately for the Enemy and for the Friend treatment. Subjects in the Friend treatment are on average more optimistic about their own *Riddle Taker* (96.7 %) than subjects in the Enemy treatment (83.1 %) and this difference is statistically significant (p = .013). This result is consistent with Proposition 2 and shows that subjects report in a self-serving way distorting their beliefs to the direction that allows them to believe that the consequence of their decision is not that severe. The design is set up in a way that in both treatments subjects have incentives to distort beliefs, hence, the previous result only confirms that there is belief distortion, but doesn't compare it to a prior. To gain some information about to what extent the difference in beliefs comes from each of the two treatments one can use the beliefs of the *Passive Participants*. *Passive Participants* were elicited the *Individual beliefs* and *Population beliefs* as well before they knew anything about their role in the experiment, hence, one can consider their beliefs as it was Choice Maker's prior belief.[8] The fraction of Passive Participants saying that

[8]The belief elicitation for *Passive Participants* were always unincentivized. The reason for that, is to provide only one excuse to the Choice Maker, which is the Riddle-take's performance. If the *Passive Participant* could earn money on the belief elicitation he would be also responsible for him outcome.

their directly assigned *Riddle Taker* was able to solve the puzzle is also 96% just as in the **Friend treatment**, therefore, the difference in *Individual beliefs* across treatments potentially comes from Choice Makers in the **Enemy treatment** being pessimistic about their own *Riddle Taker*.

Let's consider the results related to the *Population beliefs*. The true success rate is very low, only 3 out of 17 (17.6%) *Riddle Takers* could solve the puzzle within the allocated 10 minutes. Choice Makers and PPs received the solution as well together with the puzze, hence, one would expect that even PPs overestimate the real success rate, and indeed the average *Population beliefs* by PPs is 61.26 %. In the Friend treatment subjects are expected to inflate their beliefs, while in the Enemy treatment it is the other way around. Column (2) in Table 1.3 presents the point estimate of being in the Friend treatment as opposed to the Enemy treatment. On average, subjects in the Friend treatment have higher *Population beliefs* (62.8%) by 4.36 pp, which is 24% of the standard deviation ($p = .12$ on a one-sided test) than subjects in the Enemy treatment (58.5%). Consistent with Proposition 2 PPs' *Population beliefs* lies between the *Population beliefs* in the Enemy and in the Friend treatment.

Observe that in all groups the average *Individual beliefs* is much higher than the *Population beliefs*. The first measure is binary and the later is about the average success rate, hence, the difference in the two beliefs are not inconsistent with subjects having the same prior about the average success rate and the likelihood that the directly assigned *Riddle Taker* succeeds. Still, the difference suggests that subjects are likely to be more optimistic about their own *Riddle Taker* than about the *Riddle Takers* on average. It might be the case that they consider the directly assigned *Riddle Taker* as a kind of in-group member and would like to see him perform well, or the *Choice Maker* inflates his expectation over the *Riddle Taker*'s utility as well, but only for the directly assigned one as it is more salient. In any case, it seems like an interesting and consistent pattern that would require further investigation to understand.

Based on a pre-experimental survey measuring social preferences (see Murphy et al. (2011) for details), I conducted heterogeneity analysis and found that, while there is no

Table 1.3: Testing the difference in the average success rate guesses between the Friend and the Enemy treatment. The *Friend* dummy shows the effect of being in the Friend treatment as opposed to the Enemy treatment on the average success rate guess.

	(1)	(2)	(3)	(4)
	Individual beliefs	Population beliefs	Individual beliefs	Population beliefs
CM in Friend	0.136***	0.0436	0.0645	0.0452
	(0.003)	(0.223)	(0.428)	(0.379)
PP	0.136***	0.0279	0.0645	0.0161
	(0.001)	(0.369)	(0.360)	(0.717)
Constant	0.831	0.585	0.839	0.571
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	238	238	124	124
No. of Choice-makers	119	119	62	62
Incentivized	Population b.	Population b.	Individual b.	Individual b.

p-values in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

significant difference in the incentivised *Population beliefs* across treatments on average, there is a high and significant treatment effect for less prosocial *Choice Makers* ($\Delta =$ 13.6pp; p < .05). However, this was an ex-post analysis and did not replicate in a follow-up experiment ran to cleanly test this effect. At the same time, this evidence is inconclusive, as the experiment ex-post seems to be underpowered.

To test whether the effect for *Individual beliefs* holds up when there is an actual monetary cost of belief distortion I ran another experiment, with monetary incentives only for guessing correctly the success of the directly assigned *Riddle Taker*. This time guessing correctly the average success rate is not incentivized. By paying for correct guesses about the directly assigned *Riddle Taker*, the subject knows that he will be able to infer from the payoffs whether the chosen amount is taken from the *Passive Participant*, or not. That is, if he earns the money for guessing correctly, he knows the payoff for the *Passive Participant* with certainty. To attenuate this possible confound the payoff scheme is the following: Choice Makers were told that their is a 50% chance that they are paid based on their guess, in which case if they are correct they receive 30 CZK and 0 otherwise. If they are not paid based on their guess they receive 30 CZK with 50% chance and 0 otherwise. Therefore, the payoff doesn't tell unambiguously whether the chosen amount was taken from the *Passive Participant*.

With this incentivization the effect decreases and looses statistical significance (see Table 1.3 Column (3), $\Delta = .06$), making the evidence of monetary incentives on belief distortion inconclusive. However, one would expect a smaller effect with monetary incentives for at least two reasons: First, subjects might trade off monetary incentives against the benefit of inflated beliefs that they are not taking from the *Passive Participant*. Second, the information the expected payoff provides might make belief distortion harder. Since subjects know, that they will soon get to know the true outcome, they might engage in less belief distortion to avoid the emotional cost of getting to know the truth. In any case, the effect is unfortunately not significantly different from the previously find 13 pp treatment effect either to serve as evidence for any of the two mechanisms.

Given the somewhat different results, the question arises whether the incentivized or the unincentivized beliefs are more relevant. Unlike in many or most economic contexts, where incentivization captures more a real life scenario, in the current context unincentivized *Individual beliefs* might be more relevant than the incentivized one, as in reality it is rare that there is direct monetary consequence of holding the wrong belief and a decision-maker might care less about other types of costs. This mechanism of distorting *Individual beliefs* is relevant in any setting, where there is a difference in how much stake certain parties have in the decisions.

The two runs used no preselection of subjects and exhibited a meaningful, but not significant, treatment effect for *Population beliefs*. In case, one would consider to pool the samples from the two runs the treatment effect for *Population beliefs* is marginally significant ($p = .06$) in a one-sided t-test.

1.6 Conclusion

In case of decisions in social contexts we usually don't know how our actions affect others and this can give rise to distorted beliefs. If one is motivated to believe that others are not hurt much, he is motivated to distort beliefs about those who can mitigate or enhance these consequences. Decision-makers will view their behavior less harmful, than those who are outsiders. In policy debates, for instance, this leads to different views on how harmful one's behavior is and whether it has to be regulated.

A second relevant consequence that one should think about is the following: If a decision-maker ends up with optimistic beliefs, he can share this information with others and as a result, other decision-makers will have an inflated prior to start with. In a social learning context this can have a multiplicative effect. Moreover, these beliefs are likely to spread, as it provides information that allows one to act self-interestedly, while thinking good of himself.

I show suggestive evidence that people can create their own excuses by changing their beliefs about others and as a result, likely choosing actions with higher negative impact on others believing that their actions actually don't cause much harm. In this design Choice Makers experienced no uncertainty in what tasks others actually work on, and what the solution is. In real life scenarios, however, people usually don't know how the consequences of their actions can be mitigated. This uncertainty in what others might be working on is likely to give more space for belief distortion. Another important factor that people may use for belief distortion is narratives. If one can pick, or weigh arguments in a way that makes the favored outcome seem more likely, belief distortion might be easier. As a further step one might want to investigate the role of narratives and the level of uncertainty in relation to belief distortion.

10.14754/CEU.2019.06

Chapter 2

Holding a Portfolio and Wishful Thinking[1]

Co-author: Balázs Krusper

2.1 Introduction

Most economically relevant choices are made under uncertainty that makes belief formation a central part of the choice process. Behavioral economics and psychology have accumulated ample evidence that people make various mistakes when forming their beliefs and that these mistakes affect their choice. Among other reasons, people can distort their beliefs to maintain a positive self-image[2], or to act in a self-interested way.[3]

In this study, using a lab experiment, we focus on a specific mechanism for distorting beliefs called wishful-thinking. To understand this mechanism better, consider the following example: A person is owning a car, where the quality of the car determines after how

[1]We are grateful to Botond Kőszegi, for his support and financing of this project.

[2]see Eil and Rao (2011), Möbius et al. (2014), Exley and Kessler (2018), Zimmermann (n.d.)

[3]see Konow (2000), Haisley and Weber (2010), Di Tella et al. (2015)

many miles the car breaks down. There is objective uncertainty in the cars' longevity and this uncertainty are unknown to the owner. When thinking about the car's value the owner will base her evaluation on objective characteristics of the car and her belief about its longevity. Her belief might be motivated. Belief about the longevity of the car enters into her utility function making her better off, believing that she can use the car for a long time on the expense of possibly having non-appropriate maintenance. That is, she inflates her beliefs about longevity compared to her beliefs had she not owned the car. Whether people distort their beliefs about uncertain outcomes – such as the longevity of the car – simply as a result of owning a product is the focus of this paper. In a lab experiment we ask people to guess the probability that a portfolio pays off, where a portfolio is a compound lottery, consisting of 5 firms. A portfolio pays off if there are at least three firms that make a profit. Firms come from two different industries. One of the industries is more profitable and subjects can figure out which one it is. Subjects are shown the industry composition of two portfolios and they have to guess the payoff probabilities. The experiment exogenously varies which of the two portfolios gets assigned to the subject. That is, which of the two portfolios' success determine their payoffs in the experiment.

We find when subjects hold the portfolio they have a 2.75 pp higher belief about its payoff probabilities than when they don't hold the portfolio. This effect is significant on a one-sided test. We also show that payoff amounts and monetary incentives for correct beliefs are unlikely to matter for belief distortion and we present no sign that people would weight signals about portfolios depending on whether they hold the portfolio, or not.

We base our hypotheses on the previous literature showing evidence of wishful-thinking in various environments (see Bénabou and Tirole (2016) for a review on the topic). However, this study is the first to cleanly measure the effect of wishful-thinking in a financial setting and to conduct a thorough analysis of how incentives affect the extent of belief distortion in this context.

In Section 2.2 we discuss the four hypotheses that are being tested. In our main hypothesis (Hypothesis 1) we posit that holding a portfolio makes people optimistic about the uncertain probability that the portfolio pays off compared to their beliefs had they not owned the portfolio. To identify such belief distortion, the experiment exogenously varies which of the two portfolios the subject receives. Then, subjects have to report their beliefs about the probabilities that each of the two portfolios (owned and not owned) pays off. Subjects are incentivized to report truthfully (Karni, 2009).

The asymmetric updating hypothesis (Hypothesis 2) tells that people weighting positive signals about the portfolio paying off relative to negative signals is greater if the financial product is held. The literature provides mixed results concerning how belief updating plays a role in having distorted beliefs. Möbius et al. (2014) argues that an important factor is an asymmetric weight on signals. That is people overweight news favoring desired outcomes relative to the news that is going against. While their study provides evidence in favor of this explanation, there are other experiments not finding similar patterns (e.g. Barron (2018)). This paper adds to the debate by – following Möbius et al. (2014) – analyzing the belief updating process and how it is affected by holding the financial product. We show that asymmetric belief updating is unlikely to drive belief distortion in this context.

The Effect of payoff amounts hypothesis (Hypothesis 3) says, when someone is holding a financial product, her level belief distortion is contingent on the payoff. Specifically, a larger payoff makes the distortion larger. The last hypothesis is the Incentives for correct guesses (Hypotheses 4). It tells that – in case one is holding the financial product – the higher the monetary incentive for guessing correctly the smaller the belief distortion is. In the case of the last two hypotheses, again, the literature is providing mixed results: On the one hand, for instance, Mijović-Prelec and Prelec (2010) exogenously varies the incentive for the precision of guesses and the amount of the monetary prize and find, that people bias beliefs more favorably in case of high monetary prizes and less incentives for accuracy. On the other hand, Mayraz (2011) don't find any effect of the size of monetary incentives for accurate guesses on belief distortion. Our results support the view that the amount of monetary incentives for correct guesses has no effect on belief distortion. Additionally, increasing the amount a portfolio pays in case it actually pays off makes the belief distortion rather more modest as opposed to more extreme, which is hard to reconcile with the current theories.

Section 2.3 describes the experimental design. We exogenously vary which of the two portfolios gets assigned to the subject. In case the assigned portfolio pays off the subject receives a payment, hence, in case the assigned portfolio pays off the subject is better off. This is not true for the portfolio that is not assigned to the subject. This created the experimental variation having portfolios that are held and portfolios that are not held. Hence, each subject is the same in holding one out of the two portfolios, the only difference is in which of the two portfolios they hold. Then, subjects have to guess the probabilities that each of the two portfolios is paying off and they are rewarded for correct guesses. After the baseline guesses subjects are shown a noisy signal about the portfolios' performance. Now, subjects have a chance to re-evaluate their earlier guesses. The experiment ends with a questionnaire eliciting various characteristics of the subjects. The experiment also varies the payoff amount of the portfolio and the monetary incentive for correct guesses. These help to identify the effect of monetary incentives on belief distortion.

Section 2.4 discusses the procedure. The experiment used 403 subjects from Amazon Mechanical Turk (MTurk) receiving \$3.8 on average for an average time of 17 minutes spent on the experiment. The group is filtered for experienced participants from the US. The experiment was programmed in oTree (Chen et al., 2016) and participants completed it online using the MTurk platform. A low payoff portfolio paid \$3 in case it actually paid off and a high payoff portfolio paid \$7. The variation in the incentives for guessing correctly was more modest, being \$0.5 and \$1.5.

Section 2.5 presents the empirical strategy to test the four hypotheses and interpret the results in detail.

2.2 Hypotheses

The paper tests four hypotheses concerning belief distortion as a result of wishful-thinking. Three of the four hypotheses follows from Brunnermeier and Parker (2005), while one the fourth hypothesis – concerning a self-serving belief updating – is proposed by Möbius et al. (2014) to explain optimistic beliefs about own intelligence. Many theories (Bénabou and Tirole, 2016) suggest that – on the top of the available information – people also change their beliefs, as a result of favoring a certain outcome. The first hypothesis says that beliefs about payoff probabilities get inflated as a result of making subjects payoff dependent on the payoff of the portfolio. Specifically:

Hypothesis 1 *(Main Hypothesis) Assigning the portfolio makes beliefs about payoff probabilities larger.*

The intuition is that expecting the non-zero payoff has in itself an intrinsic value. People don't have stakes – besides the incentives for correct guesses – in whether a non-held portfolio is paying off. However, they do have a financial stake in the portfolio they hold. This might make people inflate their beliefs about the probability that the portfolio they hold pays off and with that, increase the intrinsic value from those beliefs, but don't do so when forming beliefs about the portfolio they don't hold.

Möbius et al. (2014) in their famous study show that people overweight positive, but noisy signals compared to negative ones when it comes to forming beliefs about their relative rank on an intelligence test. The second hypothesis is about the belief updating process when the portfolio is held as opposed to not being held. It is saying, having a stake in payoffs changes how people weight positive signals about the probability that the portfolio pays off relative to negative ones. If the portfolio is held the relative weight on positive signals is higher.

Hypothesis 2 *(Asymmetric Updating) Assigning the portfolio makes the reaction of beliefs to a positive news – relative to a negative one – larger.*

The intuition is a kind of self-serving belief updating. That is, people can have a narrative about why they become more optimistic after receiving a positive signal as the signal serves a basis of the narrative, while the actual updating process is probably unconscious and not salient to them making it less of a constraint.

The third hypothesis is saying that – in case the portfolio is held – the higher the payoff is the more distorted the beliefs are. This would be a prediction of theoretical models (such as Brunnermeier and Parker (2005)) assuming that beliefs about expected outcomes have an intrinsic value.

Hypothesis 3 *(Effect of payoff amounts) Increasing the payoff amount increases the belief distortion.*

The intuition is simple. If the payoff is higher it simply feels better to think that the amount will be received. More specifically, if people have risk-averse preference over their belief of the probability that they receive the amount, then a higher amount increases the marginal utility from inflating beliefs, making optimal beliefs higher.

The monetary costs from distorting beliefs manifest in a lower likelihood of being paid for guessing. Hence, if the opportunity cost of belief distortion is higher, the belief distortion is expected to be smaller. Our last hypothesis is concerned with this mechanism.

Hypothesis 4 *(Incentives for correct guesses) People inflate their beliefs less if monetary incentives for correct guesses are higher.*

The next section introduces the experimental design in detail to test these hypotheses.

Table 2.1: Timeline

1st Stage	Examples and questions
2nd Stage	News Puzzle
3rd Stage	One of the two portfolios is randomly assigned
4th Stage	Guessing payoff probabilities
5th Stage	First firm is revealed
6th Stage	Updating guesses
7th Stage	Questionnaire

2.3 Design

2.3.1 The main treatment

To test our main hypothesis (Hypothesis 1) we need two things. First, we need a data set that contains subjective beliefs about the payoffs of financial products. Second, we need an exogenous variation in what people own. Therefore, in a lab experiment, we ask participants to evaluate imaginary financial products. In particular, they observe two portfolios – one of them being randomly assigned to them – and estimate the probability that each of the two portfolios pays off. The experiment consists of 7 Stages (see Table 2.1).

Subjects are taught about the set up through examples and test questions on the 1th Stage. They have to solve these control questions correctly to proceed in the experiment, however, they can have as many attempts as they want (for details consult the instruction in Section B.4 in the appendix).

On the 2nd Stage subjects are told that there are two imaginary industries, the Eclipse and the Rosepaw industry. One of them contains a higher fraction of firms that makes profit. The exact fractions are unknown, but subjects can figure out which of the two industries are more profitable by reading little news excerpts about the industries (2nd Stage). The objective probabilities are 0.5 and 0.26. In fact, the role of the Eclipse and Rosepaw names are randomized. That is, once the Eclipse, once the Rosepaw is the more profitable industry. This strategy helps to make sure that names doesn't contain any information and the data reassured us that this is the case.

After subjects guessing which of the two industries are more profitable they are provided with the information about two portfolios (3rd Stage, see Figure 2.1). A portfolio consists of 5 different firms, each of them coming from either the Eclipse, or the Rosepaw industry. Subjects are told, they get paid after one of the two randomly assigned portfolio. Then, the portfolios are revealed together with the information of which of the two got assigned to the subject. There is always a Good portfolio and a Bad portfolio. The Good portfolio contains 4 firms, while the Bad portfolio only contains 2 firms from the Eclipse industry. It is known by the subjects, a portfolio pays off if at least 3 of the 5 firms make profit. [4]

On the 4th Stage subjects have to guess the probability that each of the two portfolios, the Good and the Bad portfolio, pays off. They are paid using an incentive compatible payoff scheme.

One of the arguments of why people can maintain optimistic beliefs about desired outcomes, is overweighting positive news (Möbius et al., 2014). To test for such asymmetric updating (Hypothesis 2), on the 5th Stage the outcome of the first firm (profit vs. loss) is revealed to the subjects.

On the 6th Stage, knowing the performance of the first firms, subjects can re-evaluate their earlier guesses about the payoff probabilities. The guesses are incentivised the same way as the earlier guesses.

The experiment closes with a survey (7th Stage) to capture personal characteristics, such as relationship status, wage, risk aversion and many others (see Section B.4 in the appendix)

2.3.2 Varying monetary incentives

To test Hypothesis 3 and 4, we exogenously vary the payoff that the portfolio pays in case it actually pays off, and the monetary incentives for correct beliefs. Of course, these variations are introduced in a way, that it is orthogonal to all other variations and to each other.

[4]Whether Portfolio_1, or Portfolio_2 is the Good portfolio is randomized to make sure that the estimates are not contaminated with order effects. By the randomization we can test for order effect and we can reject that order matters.

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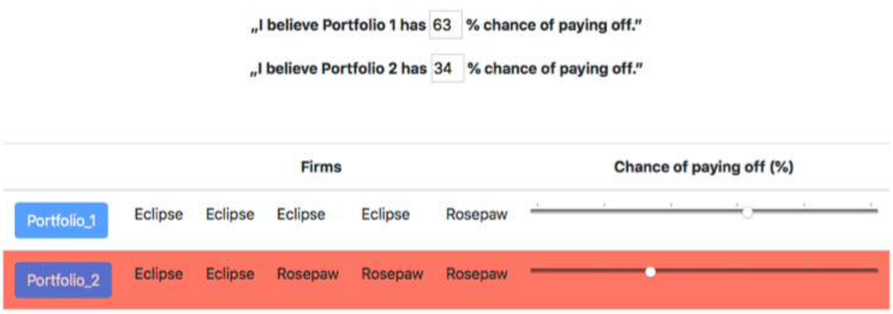

Figure 2.1: Good and Bad portfolio

Note: The slider and the cells are tied together, hence, changing one changes the other.

The portfolio's payoff amount appears twice during the experiment and only the number is changed to avoid any framing effect. The amount paid for correct guesses only appears once, however, it is put at a relatively central place in the instruction to make sure that subjects are aware that they can earn money by guessing correctly.

2.4 Procedure

The experiment was run on Amazon Mechanical Turk (MTurk) filtering for subjects who are from the US and has a high experience point using MTurk. The experiment was programmed using oTree (Chen et al., 2016). There are 403 participants who completed the experiment online. The average time spent is 17 minutes and the average payoff is \$3.8, payoffs ranging from only a completion payment of \$1.5 to \$10. The whole experiment took less than two hours and people got assigned randomly to a treatment after they started the experiment, hence, their is no within day effect that would differ across treatments.

Participants final payoffs come from three different sources. Firstly, there is a \$1.5 paid for each subjects for completion. Secondly, subjects can earn money by the portfolio that got assigned to them (either \$3, or \$7) in case it pays off. Thirdly, there is a monetary incentive (either \$0.5, or \$1.5) belief elicitation questions. Subjects are told that they will get paid after one of their randomly drawn guesses and it pays to report honestly. However, if participants want to know the exact method how their payoff is calculated

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when choosing that specific question, they could click on a link, that opens a window explaining the method.[5]

In terms of structure, participants see examples of portfolios and what it means that firms can come from two different industries, where industries have different fractions of profit making firms. Then, participants are shown little made up news excerpts about the Eclipse and the Rosepaw industry, and they have to decide which industry is more profitable based on the news. They also have to report how confident they are in their answers. Participants are incentivised to report their confidence truthfully. They are told, that they have to evaluate two portfolios, where each firm can come from the Eclipse, or the Rosepaw industry. They are also told that one of the two portfolios is randomly assigned to them. When evaluating the portfolios, subjects have to choose the probability that the given portfolio is paying off by moving a slider or typing in a percentage. At the same screen, subjects can also see which of the two portfolios got assigned to them, that is, contributing to their final payoffs (see Figure 2.1).

Figure 2.2: Eliciting Updated Beliefs

Note: The slider and the cells are tied together, hence, changing one changes the other.

To analyze the belief updating process, subjects are shown whether the first firm in the portfolio makes profit, or loss (see Figure 3.1). Based on this information, subjects can re-evaluate their beliefs. With this, the main part of the experiment finished and a questionnaire starts, that measures various characteristics of the participants and measures

[5]We thank Katherine Coffman for suggesting this method.

their understanding of the different parts of the experiment.

2.5 Empirical Strategy and Results

Out of the 403 subjects, not everyone could solve the news puzzle (2nd Stage) and figure out which industry is the more profitable one (see Table 2.2 for the frequencies). If one mistakes the good industry she will perceive the portfolios as having different compositions of good- and bad-industry firms, than those, who could figure out the more profitable industry. To account for this, most of the estimations are replicated only for subjects guessing the more profitable industry correctly and for all the subjects, using the more profitable industry as an IV for what the subject guessed.

Table 2.2: Distribution of subjects according to whether they were able to pinpoint the profitable industry based on the news provided

Puzzle Solution	No.
Incorrect	66
Correct	337
Total	403

2.5.1 Main Effect

Subjects in both treatments have to guess the good and the bad portfolios' payoff probabilities. Figure 2.3 shows the mean estimates of the subjective probabilities that a certain portfolio pays off separately for the two treatment groups. The figure shows that subjects receiving the good (bad) portfolio have a higher belief that the good (bad) portfolio pays off, than subjects receiving the bad (good) portfolio. The difference, however, in beliefs about the bad portfolio is more modest. The figure also shows the objective probabilities that the portfolio pays off. About both portfolios and in both treatments subjects way overestimate the objective probabilities that the portfolio pays off. As not all subjects figured out which of the two industries contain more firms that make profit the figure is replicated using only the subjects with correct solutions (see Figure B.1 in the appendix). The directions and magnitudes are virtually the same.

To check whether the differences in means are not coming from some outliers, Figure 2.4 shows the distribution of beliefs and the cumulative distribution of beliefs that the good (bad) portfolio pays off separately for each treatment group, that is, subjects who received the good portfolio and subjects who received the bad portfolio. The probability distributions (left column) show that there is a smaller probability mass on lower beliefs and a higher probability mass on higher beliefs when the portfolio is owned, compared to beliefs when the given portfolio is not owned. The cumulative distributions (right column) shows a fist order stochastic dominance in the expected direction in case of beliefs about the good portfolio. This difference is very modest in case of beliefs about the bad portfolio.

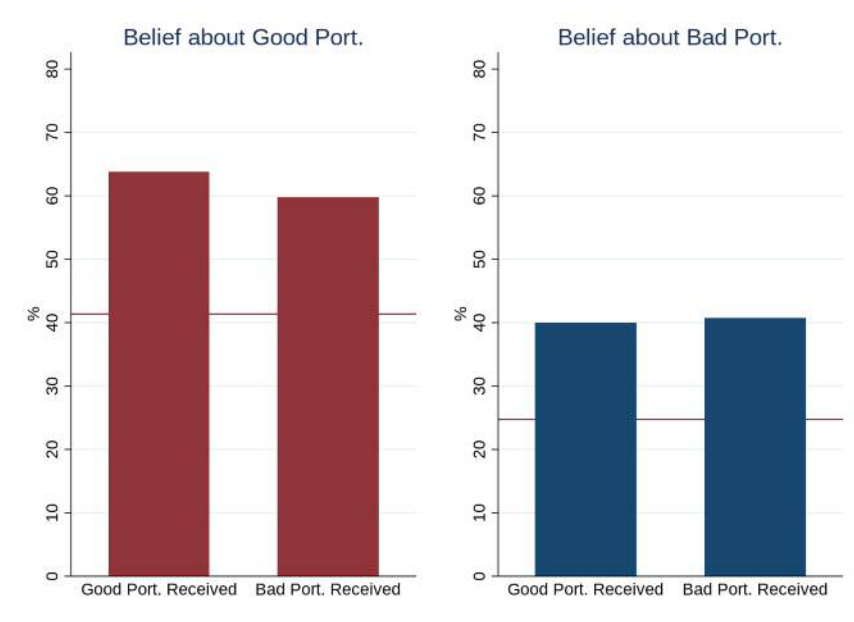

Figure 2.3: Mean estimates of the subjective probabilities that the good portfolio (left panel) and the bad portfolio (right panel) pays off

Note: The horizontal red line represents the true objective probabilities that the given portfolio pays off. The mean beliefs about the two portfolios are decomposed based on which of the two portfolios (good vs. bad) the subject received.

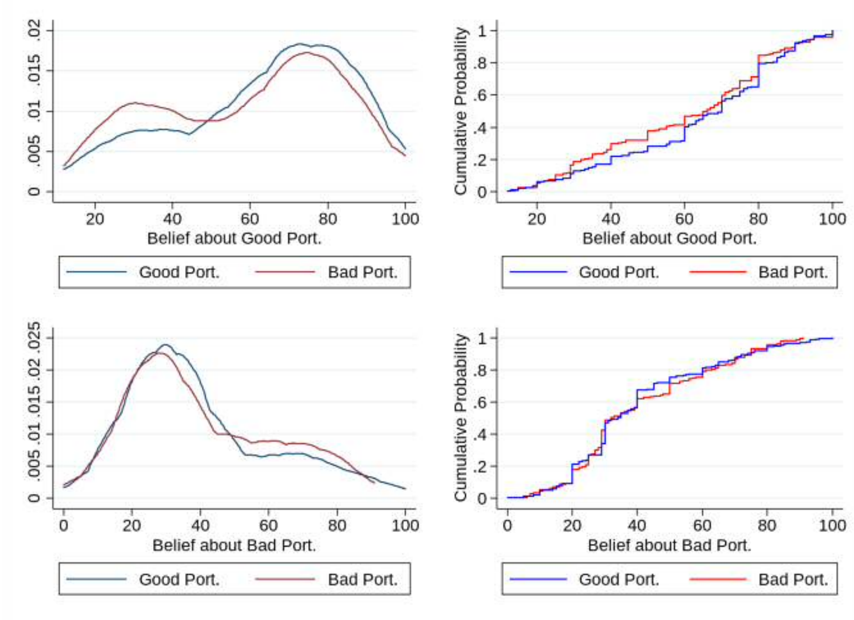

Figure 2.4: Probability distributions and cumulative distributions of the beliefs that the good portfolio (first row) and the bad portfolio (second row) pays off

Note: Each panel depicts the distribution separately for the group who received the good portfolio and the group who received the bad portfolio

In order to test our main hypothesis (Hypothesis 1), – that is, people inflate their beliefs about the portfolio they hold compared to their beliefs had they not been holding it – we estimate the following regression:

$$Belief_{ij} = \alpha + \beta \cdot Treated_{ij} + \xi i + \epsilon_{ij} \tag{2.1}$$

Where $Belief_{ij}$ is the belief of subject i about the probability that portfolio j is paying off. j can be either the good or the bad portfolio. $Treated_{ij}$ is 1 if subject i is holding the portfolio j and 0 otherwise. ξ_j is a portfolio fixed effect. The estimates for this baseline specification can be found in Table 2.3. The coefficient on the treatment dummy shows that holding the portfolio makes someone on average 2.3 pp more optimistic that the portfolio pays off. However, as some subjects mistakes which is the profitable industry, this estimate is just an intent to treat effect. To get closer to the actual treatment effect we want to measure we use two other specifications. First, we estimate the treatment effect for those, who could solve the news puzzle correctly (Column 2). Second, exploiting the treatment variation within portfolio and puzzle solution cells, we use a Portfolio x Puzzle fixed effect. That is, we estimate the average treatment effect of holding a portfolio on beliefs by including those as well, who mistake the profitable industry. This is our preferred estimate and it shows that holding the portfolio – controlling for whether you can give a correct solution on a news puzzle, or not – makes someone on average 2.75 pp more optimistic that the portfolio pays off. If not mentioned otherwise, these 3 specifications are estimated for all of the further analyses.

Table 2.3: The effect of owning a portfolio on beliefs that the portfolio pays off

	(1) Belief	(2) Belief	(3) Belief
Treated	2.337	1.977	2.753*
	(0.140)	(0.204)	(0.062)
Constant	49.92***	49.95***	63.90***
	(0.000)	(0.000)	(0.000)
Observations	806	674	806
Portfolio FE	Portfolio	Portfolio	Port. x Puzzle
Resticted to correct solutions		X	

p-values in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: The unit of observation is a subject x portfolio pair. The omitted group is a good portfolio which is not owned. The table contains two-sided p-values. If one accepts that the alternative hypothesis is an effect greater than zero – instead of non-zero – the theoretically correct p-values are halves of those in the table. Column (2) is a restricted sample, contains only those who were able to choose the profitable industry correctly. Column (3) uses the whole sample and an interaction fixed effect of the portfolio and whether the subject could solve the news puzzle.

To investigate whether the treatment effect is different for the bad portfolio than for the good portfolio, we use the following diff-in-diff specification:

$$Belief_{ij} = \alpha + \beta \cdot Treated_{ij} + \gamma \cdot Bad_{ij} + \delta \cdot Treated_{ij} \cdot Bad_{ij} + \epsilon_{ij} \tag{2.2}$$

Here, the omitted group is the good portfolio that is no held. The Bad_{ij} dummy is 1 if the portfolio is the bad one. The coefficient on $Treated_{ij}$ measures the treatment effect for the good portfolio. Table 2.4 presents the estimates. Looking at Column (3), our preferred specification, the treatment effect is 4.3 pp for the good portfolio and only around 1 pp for the bad portfolio. However, looking at the estimate of the interaction

Table 2.4: The effect of owning a portfolio on beliefs that the portfolio pays off, separately for the good and for the bad portfolio

	(1)	(2)	(3)
	Belief	Belief	Belief
Treated	3.956*	3.779*	4.356**
	(0.077)	(0.086)	(0.037)
Bad P.	-19.81***	-26.84***	-4.156
	(0.000)	(0.000)	(0.188)
Treated x Bad P.	-3.238	-3.605	-3.206
	(0.306)	(0.247)	(0.277)
Constant	59.85***	63.41***	63.13***
	(0.000)	(0.000)	(0.000)
Observations	806	674	806
Fixed Effect			Puzzle
Resticted to correct solutions		X	

p-values in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: The unit of observation is a subject x portfolio pair. The omitted group is a good portfolio which is not owned. The table contains two-sided p-values. If one accepts that the alternative hypothesis is an effect greater than zero – instead of non-zero – the theoretically correct p-values are halves of those in the table. Column (2) is a restricted sample, contains only those who were able to choose the profitable industry correctly. Column (3) uses the whole sample and an interaction fixed effect of the portfolio and whether the subject could solve the news puzzle.

effect, these two effects are not significantly different from each other.

2.5.2 Belief Updating

To see whether our experimental manipulation of revealing the first firm's performance has any effect on beliefs we plot subjects posterior beliefs against their prior separately for positive and for negative signals (see Figure 2.5). The 45 degree line shows no updating and dots above (below) the line represent an increase (decrease) in beliefs. Our manipulation seems to work. When subjects are informed that the first firm makes a profit, the overwhelming majority increases their beliefs, while the opposite is true having the first firm making loss.

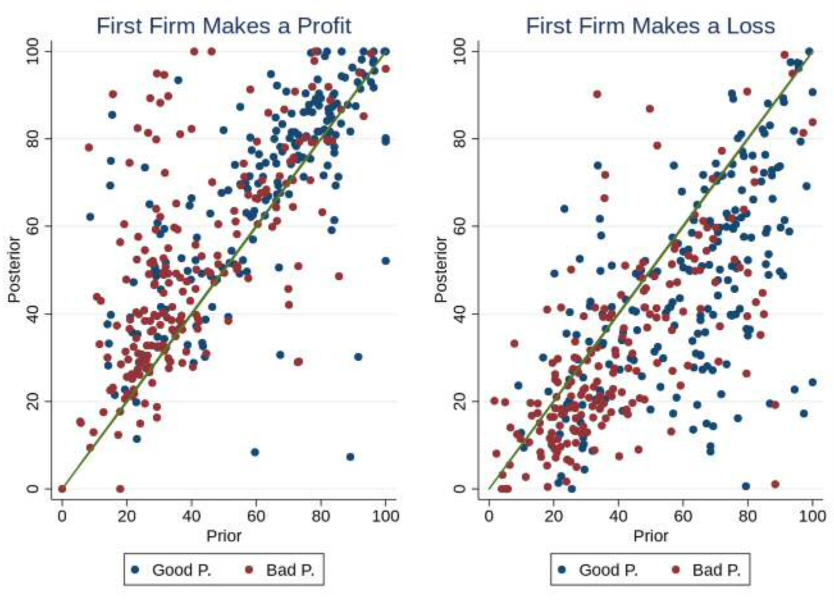

Figure 2.5: Individual priors and posteriors grouped by the first signal (profit vs. loss) and the type of the portfolio (good vs. bad)

Notes: The plot contains a minimal jitter to avoid overlaps on round numbers and better present the distribution of beliefs.

In order to test Hypothesis 2, we follow the empirical strategy of Möbius et al. (2014). Using Bayesian updating as a baseline, one can derive the following updating rule (see Section ?? in the appendix):

$$\mu_{ij}^1 = \mu_{ij}^0 + I(s_{ij} = Profit)\lambda_{Profit} + I(s_{ij} = Loss)\lambda_{Loss} \tag{2.3}$$

Where μ_{ij}^0 is the prior belief, λ_{Profit} and λ_{Loss} are log-odds ratios for the signals, and

Table 2.5: Belief Updating

	(1) Posterior	(2) Posterior	(3) Posterior
δ	0.722***	0.724***	0.721***
	(0.000)	(0.000)	(0.000)
β_G	0.398***	0.439***	0.913***
	(0.000)	(0.000)	(0.006)
β_B	0.669***	0.670***	0.0724
	(0.000)	(0.000)	(0.863)
T x β_B		-0.00167	-0.00239
		(0.986)	(0.980)
T x β_G		-0.0778	-0.0773
		(0.330)	(0.334)
Observations	768	768	768
Controls			X
P($\delta = 1$)	0.000		
P($\beta_G = 1$)	0.000		
P($\beta_B = 1$)	0.000		
P($\beta_G = \beta_B$)	0.000		

p-values in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: The unit of observation is a subject x portfolio pair. The omitted group is a good portfolio which is not owned. The table contains two-sided p-values. If one accepts that the alternative hypothesis is an effect greater than zero – instead of non-zero – the theoretically correct p-values are halves of those in the table. Column (2) is a restricted sample, contains only those who were able to choose the profitable industry correctly. Column (3) uses the whole sample and an interaction fixed effect of the portfolio and whether the subject could solve the news puzzle.

μ^1_{ij} is the log-odds posterior. That is, Bayesian updating is linearly separable in log-odds ratios. Using this property, we estimate the following specification:

$$\mu^1_{ij} = \delta\mu^0_{ij} + \beta_G I(s_{ij} = Profit)\lambda_{Profit} + \beta_B I(s_{ij} = Loss)\lambda_{Loss} + \epsilon_{ij} \tag{2.4}$$

Hence, we keep the separability, but allow for non-stability $\delta <> 1$ and arbitrary weights on signals, and use the data to estimate these parameters assuming this specific functional form. The estimates are shown in the first column of Table 2.5. We can reject the stability hypothesis and the symmetric updating. Subjects seem to put a higher weight on bad signals compared to good signals, but in general, both weight are strictly less than one, making the updating more conservative compared to the Bayesian updating. To test our hypothesis concerning the treatment effect, we use the following specification:[6]

$$\mu_{ij}^{1} = \delta\mu_{ij}^{0} + \beta_G I(s_{ij} = Profit)\lambda_{Profit} + \beta_B I(s_{ij} = Loss)\lambda_{Loss} +$$
$$+ T \times \beta_G \cdot Treated_{ij} I(s_{ij} = Profit)\lambda_{Profit} + T \times \beta_B \cdot Treated_{ij} I(s_{ij} = Loss)\lambda_{Loss} + \epsilon_{ij} \tag{2.5}$$

Where $T \times \beta_G$ denotes the coefficient on the interaction, measuring how a positive signal is weighted differently if the portfolio is held compared to the weighting had the signal not been held. Similarly, $T \times \beta_B$ measures the differential treatment effect for bad signals in the updating process. Column (2) in Table 2.5 presents the estimates. The point estimates are virtually zero, meaning, that our data don't support the self-serving updating hypothesis.

2.5.3 The effect of incentives

Lastly, for testing the effect of monetary incentives on belief distortion (Hypothesis 3 and 4), we estimate the following specification:

$$Belief_{ij} = \alpha + \beta \cdot Treated_{ij} + \gamma \cdot Treated_{ij} \cdot High\ Payoff_{ij} + \xi_i + \epsilon_{ij} \tag{2.6}$$

Where $High\ Payoff_{ij}$ is 1 if the portfolio has a high payoff (\$7) in case it pays off and 0 otherwise. The omitted group is a portfolio that is not held. The coefficient on the $Treated_{ij}$ dummy shows the belief distortion in case the payoffs are low (\$3), while γ shows how different the treatment effect is when payoffs are high. The estimates are presented in Table 2.6. Interestingly, the point estimate for the interaction effect is negative. That is, our data don't support the hypothesis that belief distortion is higher, when payoffs are

[6] We use the same notation as Möbius et al. (2014).

higher.[7] A moderating effect on optimism (not usually considered much in the literature) is disappointment aversion. Since the subject will soon find out whether the portfolio pays off, having inflated beliefs increases imminent disappointment. This can moderate the optimism ("defensive pessimism"), and might contribute to the negative effect of payoffs.

Table 2.6: Treatment effect estimates for High and Low payoffs

	(1)	(2)	(3)
	Belief	Belief	Belief
Treated	3.358*	3.331*	4.345**
	(0.083)	(0.083)	(0.016)
Treated x High Payoff	-2.037	-2.648	-3.169
	(0.362)	(0.229)	(0.129)
Constant	49.92***	49.95***	63.94***
	(0.000)	(0.000)	(0.000)
Observations	806	674	806
Portfolio FE	Portfolio	Portfolio	Port. x Puzzle
Resticted to correct solutions		X	

p-values in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: The unit of observation is a subject x portfolio pair. The omitted group is a portfolio that is not owned and has a low payoff. Column (2) is a restricted sample, contains only those who were able to choose the profitable industry correctly. Column (3) uses the whole sample and an interaction fixed effect of the portfolio and whether the subject could solve the news puzzle.

Finally, we test Hypothesis 4 using the following specification:

$$Belief_{ij} = \alpha + \beta \cdot Treated_{ij} + \gamma \cdot Treated_{ij} \cdot High\ Incentive_{ij} + \xi_i + \epsilon_{ij} \tag{2.7}$$

Where $High\ Incentive_{ij}$ is 1 if the subject can receive \$1.5 for her guesses and 0 otherwise. The parameter of interest is γ, showing how a higher incentive changes the level of the treatment effect. Based on the estimates in Table 2.7, our data don't support the hypothesis, that a higher incentive for correct guesses decreases the belief distortion.[8]

[7] The differential treatment effect of varying the payoffs separately for good and bad portfolios can be

Table 2.7: Treatment effect for High and Low monetary incentives for correct guesses

	(1) Belief	(2) Belief	(3) Belief
Treated	1.905	2.218	2.334
	(0.322)	(0.240)	(0.193)
Treated x High Incentive	0.885	-0.499	0.858
	(0.692)	(0.821)	(0.681)
Constant	49.92***	49.95***	63.91***
	(0.000)	(0.000)	(0.000)
Observations	806	674	806
Portfolio FE	Portfolio	Portfolio	Port. x Puzzle
Resticted to correct solutions		X	

p-values in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: The unit of observation is a subject x portfolio pair. The omitted group is a portfolio that is not owned and subject has a low monetary incentive for correct guesses. Column (2) is a restricted sample, contains only those who were able to choose the profitable industry correctly. (3) uses the whole sample and an interaction fixed effect of the portfolio and whether the subject could solve the news puzzle.

2.6 Concluding Remarks

People inflate their beliefs about the probability that a portfolio pays off if the portfolio gets assigned to them. Although, the current context might seem limiting, – as portfolios are very different from other products – financial decisions have direct, and many times enormous, monetary consequences, making mistakes in this domain especially relevant. A very interesting finding is the relatively strong evidence against the hypothesis that a higher payoff results in a larger belief distortion. Given the negative point estimates, it is unlikely that the true effect is actually positive, or even if it is, it is likely to be very modest.

In terms or generalizability, one should be cautious, as the nature of the ambiguity in our setting and the information acquisition is relatively specific. Another important factor might come from the MTurk pool we use, having limited knowledge about financial

found in the appendix in Table B.3. There seem to be no differential effect for the two type of portfolios.

[8]The decomposition of the effect by types of portfolios can be found in the appendix in Table B.4, showing similar patterns.

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products and not much experience in thinking about probabilities. It could be the case that professionals are less prone to such wishful-thinking. These kind of limitations offer many possibilities for further investigation.

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Chapter 3

The Effect of Managers' Health Shocks on their Future Employment

Co-author: Kinga Marczell

3.1 Introduction

How managers are different from non-managers has been a longstanding question in the literature. We contribute to this enquire, by analyzing the labor market outcomes for managers and non-managers before and after a health shock. Specifically, we ask what is the effect of a health shock on employment outcomes, such as wage and employment. Further, we ask whether there is a differential effect for managers.

One of the limited evidence in the literature comes from Riphahn (1999) showing – using the German Socio-Economic Panel – that a health shock doubles the unemployment risk. We find that a health shock results on average around a 45pp permanent drop in the likelihood of employment starting one year after the event and a temporary drop in wages. For managers, the drop in the likelihood of employment is 5.5pp smaller, however, they experience a significant wage decrease which is 15% of the wage difference between managers and non-managers before health shock. We conduct this exercise using a Hungarian administrative panel dataset linking employers and employees, that contains, alongside with labor market variables, inpatient health care costs. Using employment-related measures as the outcome variables, we estimate the average treatment effect on the treated of an illness episode — which is defined as a sudden peak in a manager's inpatient health cost history. Compared to Judiesch and Lyness (1999), who use data from a single firm for exploring this effect, we have the advantage of using a large database including managers from all sectors of the economy.

The remainder of the paper is organized as follows. Section 3.2 describes the dataset, Section 3.3 presents the framework for the analysis and provides definitions of the variables used during the estimation. Section 3.4 shows descriptive statistics, Section 3.5 presents and discusses the estimation results. Section 3.6 concludes and points out directions for future research.

3.2 Data and institutional settings[1]

We use a large, longitudinal dataset linking administrative data from the Hungarian National Pension Insurance, the National Tax and Customs Administration, and the National Health Insurance Fund, originally compiled for the Centre for Economic and Regional Studies of the Hungarian Academy of Sciences. The original dataset contains a 50% random sample of Hungarian citizens of age 15-73 in 2003 covering the period 2003-2011. The database contains information about the date of birth, gender, and the 2003 region of residence of individuals, alongside with monthly information about their employment status and labor income. In case an individual was employed in a given month, we observe an identifier of the company he or she worked at, allowing us to identify coworkers.

The National Health Insurance Fund provides information about public health care spending corresponding to individuals at a yearly frequency. In- and outpatient care and

[1] This data description is taken from a thesis chapter (Marczell, 2019).

medication costs are recorded separately, medication costs are split to an out-of-pocket and a publicly financed part. This information should represent total health expenditures fairly accurately, as co-payments are not important in Hungary, and private health care providers only represented a meaningful market share in a handful of areas, such as gynecology or dentistry during the time period covered by the dataset. According to the calculations of the Hungarian Statistical Office[2] the share of government expenditures in total health expenditures was 69% in 2008.[3] Even out of the remaining 31%, paid by households and NGO's, we do observe out-of-pocket medication costs, amounting to at least 6 percentage points out of the 31%[4]. The remaining (maximum of) 25% of expenditures that we do not observe, contain estimated values of gratuities, which are tightly linked to state-financed health care interventions. (Gratuities are informal — and often large — payments made by patients to doctors and other health care employees when receiving state-financed health care services, a phenomenon widely present in the Hungarian health care system.) All in all, the public health care costs recorded in the database should constitute a close proxy for overall health expenditures.

3.2.1 Subsample of managers

We identify managers based on workers occupation codes. The International Standard Classification of Occupations (ISCO) distinguishes heads of unites and other managers from subordinates. This allows us to consider middle manager positions, such as e.g. accounting and service branch manager, or department managers in construction. If a person shows up in the data at least 6 consecutive months in a manager position that calendar year is considered to be a manager year and the person is considered to be one

[2]in line with international statistical methodologies developed by WHO, OECD and Eurostat

[3]Data source: https://www.ksh.hu/docs/hun/xftp/stattukor/eukiadasok1015.pdf, downloaded on 30 Oct, 2018.

[4]Calculated by adding up all out-of-pocket medication costs in the sample, and doubling it, as my sample is a 50% random sample. Note, that this is an underestimation, as children are not covered by the dataset.

with manager experience. In order to capture positions where a manager is responsible for a number of people within the company, manager positions in a company with less than 10 employees are not counted towards the manager experience.

3.2.2 Defining health shocks

Health shock is defined as a sudden surge from 0 to an in the top percentile of those who have a positive in that given year. More than 50% of the person-year observations have 0 inpatient health expenditures, hence, with our definition of the health shock we capture those people, who had some serious incident, such as a life saving surgery. The mean expenditures and the cutoffs are presented in Table 3.1.

Table 3.1: Inpatient health expenditures in each year

Year	Mean Inpatient expenditure	P95 Inpatient expenditure
2003	119,705	351,955
2004	154,192	477,515
2005	172,143	555,582
2006	182,353	587,628
2007	200,405	662,389
2008	220,787	771,049
2009	202,359	704,455
2010	212,604	758,363
2011	231,734	841,823

Note: The first column shows the mean inpatient health expenditure of those who had a non-zero expenditure in that given calendar year in HUF. Column two shows the 95^{th} percentiles that are used as cutoffs for determining the state of a health shock. As a frame of reference, the mean expenditure in 2003 is $550, while the cutoff is $1600.

3.3 Empirical Strategy

3.3.1 The Effect of a Health Shock

As a first step for the exploration, we will analyze how job outcomes evolve for healthy and non-healthy people. Importantly, we define the following groups for this exercise:

- *Non-healthy:* Having a health shock during the observed period.
- *Healthy:* Having zero inpatient health expenditure throughout the whole observed period.

To have a balanced panel, so we have the same people in our database each year, we separately handle people with health shocks in different years. This procedure takes care of selection into- and out of the sample across time. We also provide regression analysis showing the same effect, but controlling for observed characteristics. This tells whether there is a difference that is not explained by observables between job outcomes for people with and without health shock.

To do this, we define a set of year dummies D_i^t taking the value 1 in calendar year t. Using these dummies we are able to capture, how much larger or smaller the job outcomes are in the treatment group, who experienced a health shock in a given calendar year t compared to people who are not experiencing health shocks. We estimate the models with the inclusion of people with health shocks in different years separately to always have a balanced sample across time to estimate the different pre-history year differences from the same people. To highlight the importance of having a balanced sample in this context consider the following example: There are people who experienced a health shock in 2005. They have two pre-history years that can be observed in the dataset, that is 2003 and 2004. Using this group of people and those who never experienced a health shock we can calculate the differences in job outcomes in 2003 and 2004 between those who experienced a health shock in 2005 and those who never experienced a health shock. Now assume that we add to the analysis the group of people who experienced the health shock in 2006. This case the estimated difference in job outcomes one year prior to the health shock would come from 2 different calendar years. However, what is more problematic, the estimated difference in job outcomes 3 years prior to the health shock would only come from people experiencing the health shock in 2006, as those who have the health shock in 2005 are not observed by limitations of the data 3 years prior to their health shock. To separate the effect of getting further back from the health shock on job outcomes from having a different composition of the sample we report the estimates using different, but balanced panels in respect to the possibility of observing the person in the database. Specifically, we estimate the following equation:

$$y_{it} = \sum_{2003<t\leq 2011} D_{it}^k \delta_k + \sum_{2003<t<\leq 2011} T_i \cdot D_{it}^k \omega_k + \beta X_{it} + \epsilon_{it} \tag{3.1}$$

In Equation 3.1, y_{it} is job outcomes of person i in year t. $T_i = 1$ for those people, who experienced a health shock and 0 for others. ω parameters capture how different people with later health shocks are in 2004, ..., 2011 years. X_{it} are control variables, including age, age squared and gender.

3.3.2 Differential Effect of a Health Shock on Managers

This part concentrates on how differently job outcomes change after a health shock for managers. To be able to measure this we define 2 non-exhaustive groups within those who experienced a health shock during the observed 2003 - 2011 period. We introduce the following definition:

- *Manager experiencing a health shock:* having a manager position at any point before the first observed health shock.
- *Non-manager experiencing a health shock:* having no manager position at all, but being observed in a non-manager position at any point before the first observed health shock.

As a first comparison, we plot the job outcomes against event years, where time 0 is the year, when people experience the health shock. To have comparable event years, we work with deflated outcomes by each year and restrict out sample to those who can be observed at least two years before and at least two years after the health shock, hence, making our sample a balanced panel within event years -2 to 2. We chose the 2 years before and after criteria, so we have some years to observe, but not loosing too many observations.

Just as in the previous exercise, we estimate the effects with controls using regressions with event year dummies.

$$y_{it} = \delta M_{it} + \omega After_{it} + \beta M_{it} After_{it} + \gamma X_{it} + \epsilon_{it} \tag{3.2}$$

In Equation 3.2, y_{it} is job outcomes of person i in year t. $M_i = 1$ for those people, who had a manager position prior to the health shock and 0 for others. $After_{it}$ takes the value 1 for everyone after their heath shock. β parameter captures how differently managers' job outcomes affected by a heath shock compared to non-managers. X_{it} are additional controls.

3.3.3 Variables

The outcome variables used are wage and employment rate. The health expenditures are only observed with a yearly resolution. Accordingly, we choose the same one calendar month each year to be our time measurement, hence, the monthly seasonality is fully controlled for, moreover the overlapping assumption - that is, different parameters are not estimated from observations in different months - is by construction satisfied.

3.4 Descriptives Statistics

Table 3.2: Distribution of first health shock years separately for managers and non-managers

	Seen in Manager Position		
First Health Shock	No	Yes	Total
2003	3,982	440	4,422
2004	3,747	382	4,129
2005	3,549	376	3,925
2006	2,970	290	3,260
2007	2,314	234	2,548
2008	2,363	247	2,610
2009	2,543	241	2,784
2010	2,449	240	2,689
2011	2,630	270	2,900

After merging all the necessary information for our analysis, we end up with around 2.7 million people followed for 9 years from 2003 to 2011. Out of them, there are 230k who have at least one manager year during the 9 years. Focusing only on those, who had a health shock, there are 29k people with a health shock and 2.700 of them has at least one manager year. 18k people can be observed with workplace before their first health shock having no manager positions in that period. Around 1.900 of them had a manager year before their first health shock. Table 3.2 shows the number of non-healthy managers and non-managers by their first health shock year.

Table 3.3: Comparing managers and non-managers on observables

	Non-managers	Managers	Difference	p
Wage	103068	219064	-115996***	0.0000
Male	0.5016	0.5930	-0.0915***	0.0000
Age	33	38	-5***	0.0000
Out-patient	7716	7992	-276***	0.0000
In-patient	14212	13151	1060***	0.0000
Prescription (gov.)	11546	14278	-2732***	0.0000
Prescription (personal)	5645	8381	-2735***	0.0000

People having manager positions during their career are likely to be different from others. Table 3.3 compares the two groups based on their 2003 characteristics. It shows that future managers tend to be older with a much higher wage, they are more likely to be males and – interestingly – have a higher prescription expenditure.

3.5 Results

3.5.1 The Effect of a Health Shock

As a first step, we focus on the healthy/non-healthy comparison. The event-study plots – Section C.3 in the appendix – show that a health shock results in a permanent drop of 30 to 40pp in the likelihood of being employed. In general, people experiencing a health shock are more likely to work in the pre-history compared to those, who are always healthy. They also experience a drop in their employment prior to the event. In terms of wages, there is a significant drop for people experiencing a health shock in the event year. Interestingly, the average wage catches up quickly, but another drop seem to occur a few years later. Table C.5 and Table C.4 presents the regression results using the specification described in Equation 3.1 strengthening the previous conclusion.

3.5.2 Differential Effect on Managers

Figure 3.1 plots the effect of health shock separately for non-managers and managers on job outcomes. As expected, managers earn more even during the pre-history, however, they also more likely to be employed. After the health shock managers seem to experience a drop in their wages and the regression results in Table 3.4 confirms this differential effect. Specifically, while non-managers don't experience a drop in their wages in the two-year average after the health shock, managers earn around 29.000 HUF less. Although, this effect is statistically significant, compared to the baseline difference in earnings between managers and non-managers, it decreases the wage gap by modest 15 percent. Observe, that the level of the health expenditure does not explain the differential effect, hence, it might be that managers are facing different types of health problems than non-managers, based on the estimates it is unlikely that this would explain the whole differential effect.

There is also a differential effect on wages. Managers are more likely to be employed

Figure 3.1: The effect of a health shock on job outcomes, separately for managers and non-managers

Note: Evolution of job outcomes for people with health shock.

before the health shock. They also seem to experience a smaller drop in the likelihood of being employed. Table C.8 confirms these differences. Managers are 11.4pp more likely to be employed prior to the event and they experience a 5.5pp smaller drop in the employment likelihood than non-managers. These differences are significant and can not be explained by observables. Again, health cost doesn't explain this difference either.

Table 3.4: Comparing the effect of a health shock on managers' and non-managers' wage

VARIABLES	(1) Demeaned wage	(2) Demeaned wage	(3) Demeaned wage	(4) Demeaned wage
After	-5,155*	-4,661	-2,167	-2,167
	(2,990)	(2,991)	(3,008)	(3,008)
Manager	192,946***	191,672***	189,095***	189,095***
	(4,708)	(4,716)	(4,725)	(4,725)
Manager x After	-27,072***	-27,596***	-29,063***	-29,063***
	(8,401)	(8,399)	(8,393)	(8,393)
Health Cost	-0.00120	-0.000841	-0.000285	-0.000285
	(0.000965)	(0.000969)	(0.000971)	(0.000971)
Male		10,264***	9,159***	9,159***
		(2,485)	(2,514)	(2,514)
Age in 2003			1,496	1,496
			(922.9)	(922.9)
Age Square in 2003			-6.341	-6.341
			(11.05)	(11.05)
Constant	-16,078***	-22,552***	-77,423***	-77,423***
	(2,603)	(3,037)	(19,254)	(19,254)
Observations	25,481	25,481	25,481	25,481
R-squared	0.081	0.082	0.084	0.084
Health Emp.	Yes	Yes	Yes	Yes
Controls	No	Male	Age Male	Age Male
Prior wage	No	No	No	Yes

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

The identification relies on the parallel trend assumption. There is an apparent trend before the event, hence, one might worry about differential trends explaining the differential treatment effect. That is, managers wage/employment trajectory has a steeper downward/upward trend compared to non-managers. To show, this is not the case, we estimate the differential change in employment outcomes before and after the event (see Table 3.6). It shows, that the employment changes (decrease) after the event compared to the pre-history is smaller for managers, but the opposite is true for the wages. Hence, the differential effect of a health shock is not fully driven by differential trends.

Table 3.5: Comparing the effect of a health shock on managers' and non-managers' wage

VARIABLES	(1) Employed	(2) Employed	(3) Employed	(4) Employed
Manager	0.0912***	0.0963***	0.114***	0.114***
	(0.00951)	(0.00951)	(0.00939)	(0.00939)
After	-0.491***	-0.491***	-0.491***	-0.491***
	(0.00408)	(0.00407)	(0.00401)	(0.00401)
Manager x After	0.0557***	0.0557***	0.0557***	0.0557***
	(0.0134)	(0.0134)	(0.0132)	(0.0132)
Health Cost	-1.27e-08***	-1.41e-08***	-1.59e-08***	-1.59e-08***
	(1.48e-09)	(1.49e-09)	(1.47e-09)	(1.47e-09)
Male		-0.0443***	-0.0229***	-0.0229***
		(0.00394)	(0.00394)	(0.00394)
Age in 2003			0.0249***	0.0249***
			(0.00139)	(0.00139)
Age Square in 2003			-0.000370***	-0.000370***
			(1.64e-05)	(1.64e-05)
Constant	0.770***	0.798***	0.460***	0.460***
	(0.00438)	(0.00504)	(0.0294)	(0.0294)
Observations	50,244	50,244	50,244	50,244
R-squared	0.242	0.244	0.265	0.265
Health Emp.	Yes	Yes	Yes	Yes
Controls	No	Male	Age Male	Age Male
Prior wage	No	No	No	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Change

VARIABLES	(1) DD. demeaned wage	(2) DD. employed
Manager	-44,915**	-0.0369*
	(19,114)	(0.0196)
Constant	10,312	-0.0160***
	(7,572)	(0.00593)
Observations	1,950	12,561
R-squared	0.003	0.000

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

3.6 Concluding Remark

Using a Hungarian administrative data set, we find that a health shock results in a 45pp permanent drop in the likelihood of employment and a small temporary drop in wages. The likelihood of employment for managers, however, are affected 5.5pp less and their experience a moderate drop lasting even after the health shock. These differences can not be explained by neither observables, nor differential trends before the health shock.

An intuitive, and plausible explanation for the findings is grit. Manager types are likely to pursue even when facing adversities. Having a higher wage and employment ratio even before the health shock is consistent with this explanation. Of course, one can not fully refute other explanations, such as managers facing different type of shocks than non-managers. It would be useful to use a data set that contains information about the exact type of health shocks. This would allow to concentrate more on shocks that are really exogenous and would also help to investigate differential effects within the same type of shock between managers and non-managers.

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Appendix A

Appendix for Chapter 1

A.1 Proof of Proposition 4

The first order conditions are the following:

$$\frac{\sigma}{(1-\sigma)}\frac{1}{1-p^*} = \frac{u'(e-a^*)}{u'(e+a^*)} \tag{A.1}$$

$$u(e) - u(e-a^*) = \frac{\eta(\sigma)}{1-\sigma}C'(p^* - p_0), \tag{A.2}$$

If σ is higher the optimal action is going to be higher, hence, $u(e) - u(e-a)$ is going to be higher. For p^* to be increasing in σ it is a sufficient condition to have $\frac{d\frac{\eta(\sigma)}{1-\sigma}}{d\sigma} \leq 0$. That is $\eta'(\sigma) \leq \frac{-\eta(\sigma)}{1-\sigma}$.

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Table A.1: Testing the difference in the average success rate guesses between the Friend and the Enemy treatment. The *Friend* dummy shows the effect of being in the Friend treatment as opposed to the Enemy treatment on the average success rate guess. Standard errors are clustered on the session level.

	(1) Individual beliefs	(2) Population beliefs	(3) Individual beliefs	(4) Population beliefs
CM in Friend	0.136*	0.0436	0.0645	0.0452
	(0.075)	(0.310)	(0.168)	(0.339)
PP	0.136**	0.0279	0.0645*	0.0161
	(0.025)	(0.209)	(0.058)	(0.587)
Constant	0.831	0.585	0.839	0.571
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	238	238	124	124
No. of Choice-makers	119	119	62	62
Incentivized	Population b.	Population b.	Individual b.	Individual b.

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

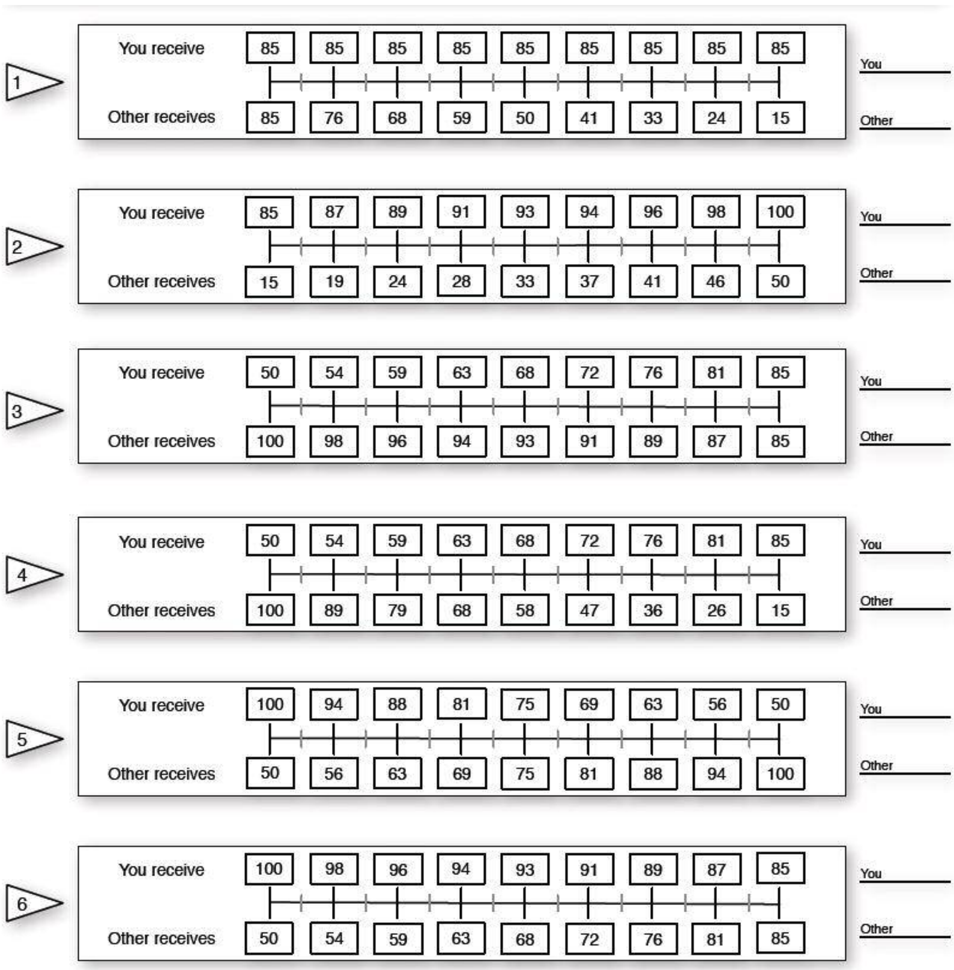

Figure A.1: The first 6 items from the original study of Murphy et al. (2011)

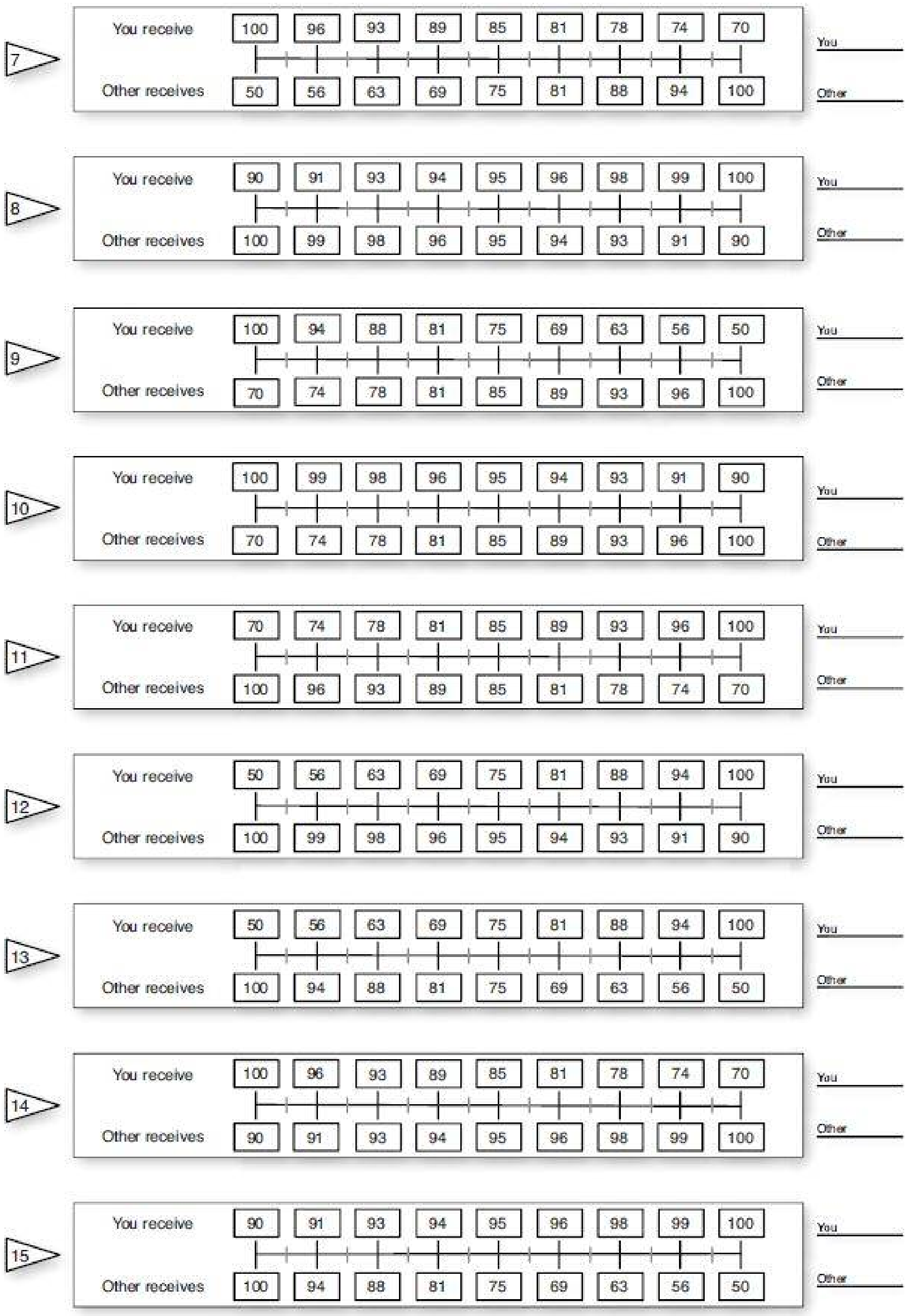

Figure A.2: Items from 7 to 15 from the original study of Murphy et al. (2011)

A.2 Instruction[1]

A.2.1 Common Instruction

Good morning / afternoon, if you are here for the economics experiment, please come in now. Please have your photo ID ready!

Welcome everybody! Thank you for participating on today's session! I am ... and I will run this experiment on decision-making. At this point you get 100 CZK for showing up. Everything you earn during the experiment is additional to this. Please pay careful attention to the instructions as a considerable amount of money is at stake. The entire experiment should last at most an hour. At the end of the experiment you will be paid privately. We ask everyone to remain silent until the end of the experiment. If you have any question please, always raise your hand! Some of you can earn money on some of the control questions, make sure you know the answers, or ask if at any point you are uncertain/confused.

[Start the program!]

From the next screen on you will see your instructions and you have to follow the instructions by your own. You can proceed in your own pace.

[Once, they finished the treatment you can start the questionnaire]

[Subjects are told to go to the front desk to collect earnings, once they are ready]

[1]The original language of the instruction is Czech.

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A.2.2 Choice Maker's Instruction

The following riddle – called Boxes Riddle – has an important role in today's experiment. Please, inspect carefully the riddle together with its solution provided!

Look at the figure below! It consists of matches from a to p. By repositioning exactly two of the matches, how would you create four squares instead of five? The squares may be repositioned, but the new squares have to be the same size as the old ones. Just as in the figure, all matches must be used and have to end up as sides of squares!

Which of the 2 matches have to be repositioned?

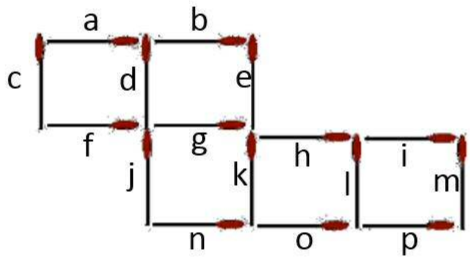
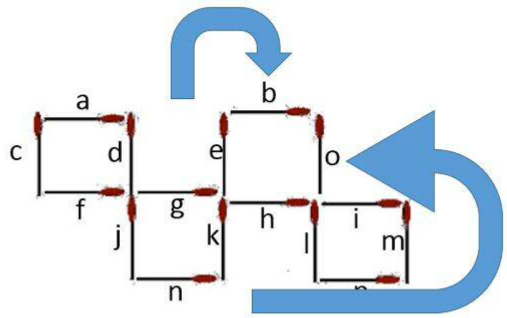


Figure A.3: Puzzle

The solution to the Boxes Riddle involves repositioning the right match in the top row (match b) and the middle match in the bottom row (match o) to form a new box in the top row, third column (this leaves two boxes in the top row (1st and 3rd columns) and two in the bottom row (2nd and 4th columns)) as shown in the figure below.

I have read and understood the solution to the Boxes Riddle.



Introducing the roles

Your task in this experiment will be to make ONE decision. This decision is in regard to the distribution of money.

In today's experiment 3 participants are relevant for the actions and payoffs to be implemented: you (called "the **Choice Maker**") will interact with the "**Riddle Taker**" and the "**Passive Participant**".

The **Riddle Taker** and the **Passive Participant** are randomly selected from the other participants in today's session. Both you and the **Riddle Taker** must do a single task. The payments will be distributed according to some rules we'll explain to you in a moment.

All decisions you make today will be implemented and will be payoff relevant. This experiment is completely anonymous: neither the other Participants, nor the organizer will be able to link your decision to your identity. Also, you will not be able to find out the identity of the people you're interacting with. The identity of both the **Riddle Taker** and the **Passive Participant** will be hidden from you. You will only get feedback about your own payoff in the experiment and **will not receive any information about the performance and earnings of the people you are interacting with!**

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Please, answer the control questions!

What is your assigned role?

Passive Participant

Riddle Taker

Choice Maker

Will you be able to find out the identity of participants you play with?

Yes

No

Click "Continue" to proceed!

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Task Explanation[2]

(Friend treatment)

You are the **Choice Maker**. You start the experiment with 150 CZK.

Another participant has been randomly assigned the role of **Passive Participant**.

This participant has also received 150 CZK, separately from your 150 CZK. The **Passive Participant** has been randomly assigned to you and only you. Your task is to decide the amount you want for yourself over your initial 150 CZK.

This amount should be between 0 and 150 CZK, including 0 and 150.

At the end of the experiment you will receive your initial 150 CZK PLUS the amount you choose.

However, your choice might hurt the **Passive Participant** according to the rules described below:

A third participant has been randomly assigned the role of the **Riddle Taker**.

The **Riddle Taker** has been randomly assigned to you.

The task of a ***Riddle Taker*** is to solve the same riddle with the matches that you have seen.

The **Riddle Taker** has **10 minutes** to solve the riddle.

There are two outcomes, based on whether the **Riddle Taker** succeeds in solving the match riddle:

a, If the **Riddle Taker** CAN **SOLVE the riddle**, the additional amount you choose is taken from a different source to be given to you, and the **Passive Participant can keep all of their money.**

[2]Only for the reader, treatment variation is highlighted with blue.

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b, However, if the **Riddle Taker CAN NOT SOLVE the riddle**, the additional amount you choose above your initial 150 CZK is **taken from the Passive Participant** and given to you.

Therefore: whether the *Passive Participant* is affected by your choice (in case the amount you choose is above zero) depends entirely on whether the *Riddle Taker* is able to solve the match riddle. The *Passive Participant* has no other task to make money. Hence, your choice and the performance of the *Riddle Taker* will determine how much the *Passive Participant* earns in this experiment.

You receive your initial 150 CZK PLUS the amount you choose IN ANY CASE, regardless of whether the **Riddle Taker** can solve the match riddle, or not. Your money is therefore not affected by the success, or failure of the **Riddle Taker**.

Proceed to Control Questions

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Control Questions

You earn an additional 50 CZK in case all of your answers to the following questions are correct! (Please only indicate an integer value!)

What is the maximum amount of money you can choose for yourself (not including your initial 150 CZK)?

How much money does the *Passive Participant* start with?

If the *Riddle Taker* CAN solve the riddle – and you were to choose a non-zero amount –, can the *Passive Participant* keep his initial 150 CZK?

Yes

No

If the *Riddle Taker* CAN'T solve the riddle – and you were to choose a non-zero amount –, can the *Passive Participant* keep his initial 150 CZK?

Yes

No

Let's see an example with a randomly drawn amount:

If you were to choose 20 CZK[3] for yourself and the Riddle Taker CAN SOLVE the riddle:

You earn (including your initial money): 170 CZK and

the *Passive Participant* earns 130 CZK.

However, if the *Riddle Taker* CAN NOT SOLVE the riddle:

You earn (including your initial money): 170 CZK and

the *Passive Participant* earns 150 CZK.

[3]The amount is randomized.

How much do you earn (including your initial 150 CZK) if you were to choose 130 CZK for yourself and the *Riddle Taker* CAN'T SOLVE the riddle? ☐

How much do you earn (including your initial 150 CZK) if you were to choose 130 CZK for yourself and the *Riddle Taker* CAN SOLVE the riddle? ☐

How much money does the *Passive Participant* earn if you were to choose 130 CZK for yourself and the *Riddle Taker* CAN'T SOLVE the riddle? ☐

How much money does the *Passive Participant* earn if you were to choose 130 CZK for yourself and the *Riddle Taker* CAN'T SOLVE the riddle? ☐

How much money does the *Passive Participant* earn if you were to choose 90 CZK for yourself and the *Riddle Taker* CAN'T SOLVE the riddle? ☐

How much money does the *Passive Participant* earn if you were to choose 90 CZK for yourself and the *Riddle Taker* CAN'T SOLVE the riddle? ☐

How much money does the *Passive Participant* earn if you were to choose 150 CZK for yourself and the *Riddle Taker* CAN'T SOLVE the riddle? ☐

How much money does the *Passive Participant* earn if you were to choose 150 CZK for yourself and the *Riddle Taker* CAN'T SOLVE the riddle? ☐

How much money does the *Passive Participant* earn if you were to choose 40 CZK for yourself and the *Riddle Taker* CAN'T SOLVE the riddle? ☐

How much money does the *Passive Participant* earn if you were to choose 40 CZK for yourself and the *Riddle Taker* CAN'T SOLVE the riddle? ☐

How much money does the *Passive Participant* earn if you were to choose 20 CZK for yourself and the *Riddle Taker* CAN'T SOLVE the riddle? ☐

How much money does the *Passive Participant* earn if you were to choose 20 CZK for yourself and the *Riddle Taker* CAN'T SOLVE the riddle? ☐

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About the *Riddle Taker*

The task of a **Riddle Taker** is to solve the same riddle with the matches that you have seen. The **Riddle Taker** didn't get the solution and the information you did. The **Riddle Taker** doesn't know about the **Choice Maker** and the **Passive Participant**.

Specifically, the **Riddle Taker** only receives the following information besides the instruction for the match riddle:

"You have only ONE task in this experiment. You receive 150 CZK for sure for doing the following task. You have 10 min. to solve the following riddle. In case you can solve the riddle your reward is 100 CZK over your initial 150 CZK. Whether you can solve the riddle, or not may be better for other participants, but which one has payoff consequences is randomized."

The **Riddle Taker** doesn't know about your task, or the amount you choose for yourself!

As you already know:

a, If the **Riddle Taker** **CAN SOLVE the riddle**, the additional amount you choose is taken from a different source to be given to you, and the **Passive Participant can keep all of their money.**

b, However, if the **Riddle Taker** **CAN NOT SOLVE the riddle**, the additional amount you choose above your initial 150 CZK is **taken from the Passive Participant** and given to you.

Click "Continue" to proceed!

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About the Passive Participant

The **Passive Participant** got all the information that you did, therefore, they know everything that you might base your decision on.
The **Passive Participant** will make NO CHOICE that would influence the outcome.
They will only see their final payoff.

Whether the *Passive Participant* is affected by your choice (in case the amount you choose is above zero) depends entirely on whether the *Riddle Taker* is able to solve the match riddle. The *Passive Participant* has no other task to make money. Hence, your choice and the performance of the *Riddle Taker* will determine how much the *Passive Participant* earns in this experiment.

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Control Questions

Does the *Riddle Taker* know about the other roles in the experiment?

Yes

No

Does the *Riddle Taker* know your task?

Yes

No

Does the *Passive Participant* know that you may take money from him/her?

Yes

No

Click "Continue" to proceed!

Decide the amount you want for yourself between 0 and 150 CZK (including 0 and 150) over your initial 150 CZK!

(Please only indicate integer value between 0 and 150!)

100

Click "Continue" to proceed!

In case the *Riddle Taker* CAN SOLVE the riddle the Passive Participant earns 150 CZK and you earn 250 CZK.

In case the *Riddle Taker* CAN NOT SOLVE the riddle the Passive Participant earns 50 CZK and you earn 250 CZK.

Click "Continue" to proceed!

Do you think the *Riddle Taker* could solve the riddle within the allocated 10 minutes?

Yes

No

Click "Continue" to proceed!

Bonus question:

Several Participants in this experiment have been assigned the role of Riddle Taker. We are going to calculate the percentage of *Riddle Takers* who could solve the riddle. Guess what percentage of the *Riddle Takers* were able to solve the Riddle!

If your guess is correct you will receive an additional 200 CZK.

0 - 10%

10 - 20%

20 - 30%

30 - 40%

40 - 50%

50 - 60%

60 - 70%

70 - 80%

80 - 90%

90 - 100%

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Appendix B

Appendix for Chapter 2

B.1 Tables and Figures

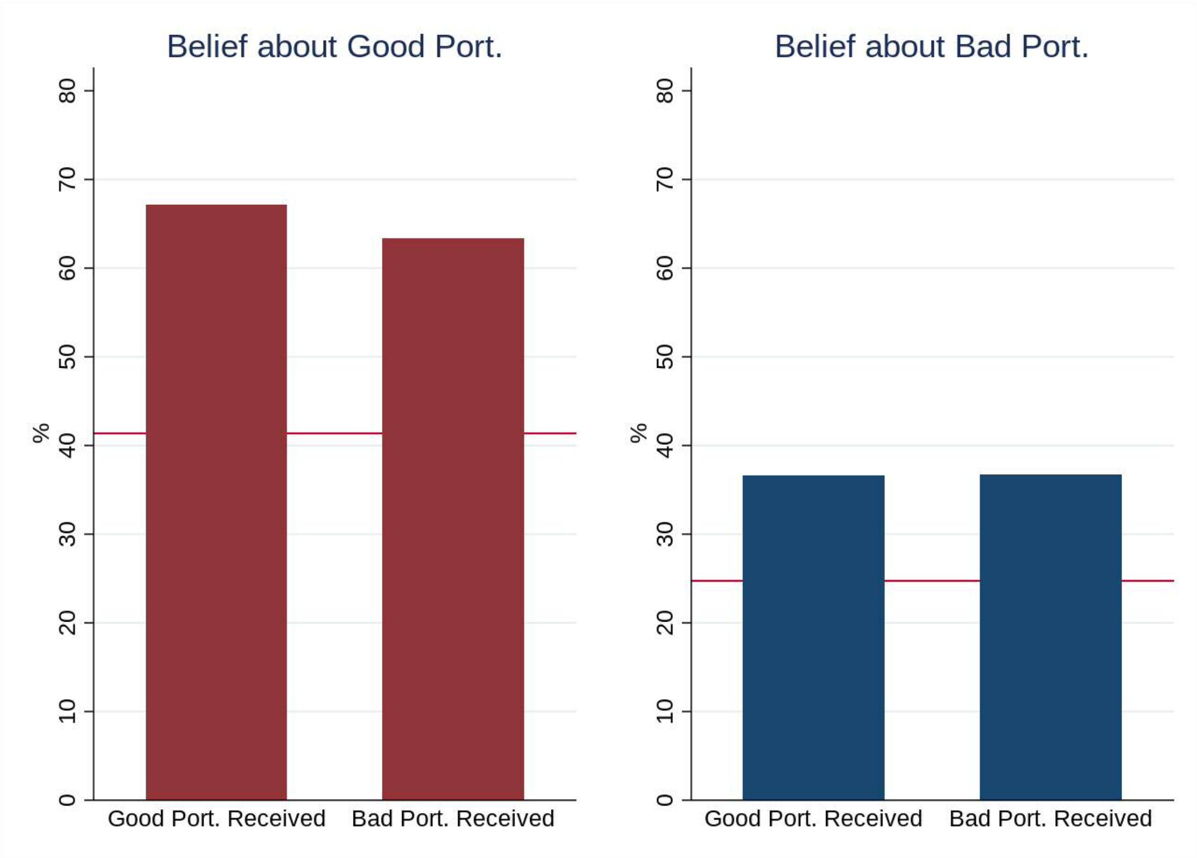

Figure B.1: Mean estimates of the subjective probabilities that the good portfolio (left panel) and the bad portfolio (right panel) pays off

Note: The horizontal red line represents the true objective probabilities that the given portfolio pays off. The mean beliefs about the two portfolios are decomposed based on which of the two portfolios (good vs. bad) the subject received.

Table B.1: The effect of owning a portfolio on beliefs that the portfolio pays off including controls

	(1) Belief	(2) Belief	(3) Belief
Treated	2.337	1.977	2.753*
	(0.136)	(0.201)	(0.059)
q_is_married	4.553***	3.171*	4.611***
	(0.006)	(0.052)	(0.003)
q_is_risklover	3.538**	3.258**	3.611**
	(0.034)	(0.047)	(0.022)
q_age	-0.238	-0.404	-0.251
	(0.589)	(0.347)	(0.542)
q_age2	0.00223	0.00422	0.00235
	(0.652)	(0.379)	(0.611)
q_math	0.443	0.415	0.446
	(0.211)	(0.232)	(0.176)
q_finance_involved	0.284	0.348	0.272
	(0.445)	(0.355)	(0.434)
Constant	46.01***	49.89***	60.56***
	(0.000)	(0.000)	(0.000)
Observations	806	674	806
Portfolio FE	Portfolio	Portfolio	Port. x Puzzle
Resticted to correct solutions		X	

p-values in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: The unit of observation is a subject x portfolio pair. The omitted group is a good portfolio which is not owned. The table contains two-sided p-values. If one accepts that the alternative hypothesis is an effect greater than zero – instead of non-zero – the theoretically correct p-values are halves of those in the table. Column (2) is a restricted sample, contains only those who were able to choose the profitable industry correctly. (3) uses the whole sample and an interaction fixed effect of the portfolio and whether the subject could solve the news puzzle.

Table B.2: The effect of owning a portfolio on beliefs that the portfolio pays off, separately for the good and for the bad portfolio, including controls

	(1) Belief	(2) Belief	(3) Belief
Treated	3.581	3.422	4.006*
	(0.108)	(0.119)	(0.053)
Bad P.	-20.19***	-27.20***	-6.085*
	(0.000)	(0.000)	(0.056)
Treated x Bad P.	-2.488	-2.889	-2.506
	(0.431)	(0.354)	(0.394)
Constant	56.38***	63.63***	60.24***
	(0.000)	(0.000)	(0.000)
Observations	806	674	806
Fixed Effect			Puzzle
Resticted to correct solutions		X	

p-values in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: The unit of observation is a subject x portfolio pair. The omitted group is a good portfolio which is not owned. The table contains two-sided p-values. If one accepts that the alternative hypothesis is an effect greater than zero – instead of non-zero – the theoretically correct p-values are halves of those in the table. Column (2) is a restricted sample, contains only those who were able to choose the profitable industry correctly. Column (3) uses the whole sample and an interaction fixed effect of the portfolio and whether the subject could solve the news puzzle.

B.2 Belief Updating

- State space: $\Omega = \{H, L\}$
- Signal space: $S = \{s_H, s_L\}$
- Prior-belief: $p_0 = P(\omega = H)$
- Posterior-belief: $p_1 = P(\omega = H|s)$
- Conditional signal distribution: $P(s = i|\omega = i) = q$, for $i \in \{H, L\}$

Using Bayes-rule we can write the posteriors conditional on the signals in the following

Table B.3: Treatment effect estimates for High and Low payoffs separately for Good and Bad portfolio

	(1) Belief	(2) Belief	(3) Belief
Treated x Good	4.558*	5.247*	5.981**
	(0.098)	(0.058)	(0.020)
Treated x Bad	1.828	1.511	2.457
	(0.499)	(0.569)	(0.329)
Good P.	20.19***	27.20***	6.057*
	(0.000)	(0.000)	(0.057)
Treated x High Payoff x Good P.	-1.930	-3.448	-3.892
	(0.546)	(0.277)	(0.191)
Treated x High Payoff x Bad P.	-1.483	-1.979	-1.926
	(0.632)	(0.514)	(0.503)
Constant	36.09***	35.94***	54.01***
	(0.000)	(0.000)	(0.000)
Observations	806	674	806
Fixed Effect			Puzzle
Resticted to correct solutions		X	

p-values in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: The unit of observation is a subject x portfolio pair. The omitted group is a portfolio that is not owned and has a low payoff. Column (2) is a restricted sample, contains only those who were able to choose the profitable industry correctly. Column (3) uses the whole sample and an interaction fixed effect of the portfolio and whether the subject could solve the news puzzle.

Table B.4: Treatment effect for High and Low monetary incentives for correct guesses separately for Good and Bad portfolio

	(1) Belief	(2) Belief	(3) Belief
Treated x Good	4.314	5.251**	4.543*
	(0.110)	(0.048)	(0.071)
Treated x Bad	-0.509	-0.922	0.0919
	(0.851)	(0.729)	(0.971)
Good P.	20.20***	27.20***	6.117*
	(0.000)	(0.000)	(0.055)
Treated x High Incentive x Good P.	-1.561	-3.914	-1.149
	(0.625)	(0.216)	(0.699)
Treated x High Incentive x Bad P.	3.204	2.939	2.818
	(0.303)	(0.335)	(0.330)
Constant	36.16***	36.25***	54.10***
	(0.000)	(0.000)	(0.000)
Observations	806	674	806
Fixed Effect			Puzzle
Resticted to correct solutions		X	

p-values in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: The unit of observation is a subject x portfolio pair. The omitted group is a Bad portfolio that is not owned and subject has a low monetary incentive for correct guesses. Column (2) is a restricted sample, contains only those who were able to choose the profitable industry correctly. Column (3) uses the whole sample and an interaction fixed effect of the portfolio and whether the subject could solve the news puzzle.

way:

$$P(\omega = H|s = H) = \frac{P(s = H|\omega = H)P(\omega = H)}{P(s = H|\omega = H)P(\omega = H) + P(s = H|\omega = L)P(\omega = L)}$$
$$= \frac{qp_0}{qp_0 + (1-q)(1-p_0)}$$

$$P(\omega = L|s = H) = \frac{P(s = H|\omega = L)P(\omega = L)}{P(s = H|\omega = H)P(\omega = H) + P(s = H|\omega = L)P(\omega = L)}$$
$$= \frac{(1-q)(1-p_0)}{qp_0 + (1-q)(1-p_0)}$$

Dividing the two and then taking the log,

$$\frac{P(\omega = H|s = H)}{P(\omega = L|s = H)} = \frac{p_0}{1-p_0}\frac{q}{1-q}$$
$$log(\frac{p_1}{1-p_1}) = log(\frac{p_0}{1-p_0}) + log(\frac{q}{1-q})$$

Similarly,

$$\frac{P(\omega = H|s = L)}{P(\omega = L|s = L)} = \frac{p_0}{1-p_0}\frac{1-q}{q}$$
$$log(\frac{p_1}{1-p_1}) = log(\frac{p_0}{1-p_0}) + log(\frac{1-q}{q})$$

B.3 Parametrization

P($goodportfoliopays$ — $firstfirmprofit$) = 0.6115

P($goodportfoliopays$ — $firstfirmloss$) = 0.2493

P($badportfoliopays$ — $firstfirmprofit$) = 0.4428

P($badportfoliopays$ — $firstfirmloss$) = 0.1363

P($firstfirmprofit$ — $goodportfoliopays$) = 0.7389

P($firstfirmloss$ — $goodportfoliopays$) = 0.3012

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$\mathrm{P}(firstfirmloss — goodportfoliodoesn'tpay) = 0.2069$

$\mathrm{P}(firstfirmprofit — badportfoliopays) = 0.8954$

B.4 Instruction

General overview

This study includes problem-solving and a post-completion questionnaire. After completing the study, you will earn a completion payment plus you may earn some bonus payment. The bonus scheme is designed in a way that you always benefit from telling the truth. For each question, we will explain clearly how you can earn bonus payments.

We will ask you to evaluate various financial investments. The following screens will explain the task and walk you through a few examples. The role of these examples is to make sure you understand the financial investments and how your payoff will depend on them. You have to answer all example questions correctly to continue to the main part. (There is no restriction on how many times you can try.)

Nem vagyok robot

reCAPTCHA

Adatvédelem - Általános Szerződési Feltételek

Click on the Next button to get started!

Next

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Example 1/5

Firms
Imagine that there are a lot of firms in the economy. Some firms make a profit while other firms make a loss.

Portfolios
Consider a portfolio that contains 5 randomly selected firms from a pool of firms. The return of the portfolio depends on the number of firms that make a profit. If at least 3 firms make a profit then the portfolio is successful and it pays off. If less then 3 firms make a profit then the portfolio is unsuccessful and it pays nothing.

Example
The table below presents all firms. $ represent firms that make a profit and ⊘ represent firms that make a loss. By clicking on the "Show" buttons you can see which firms are selected into the portfolio.

We ask you to do two things:

1. Reveal all firms whether they make a profit by clicking on the Show button.
2. Indicate whether the portfolio pays off.

Firms	Firm 1	Firm 2	Firm 3	Firm 4	Firm 5	Does the portfolio pay off?
	Show	Show	Show	Show	Show	---------

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Example 2/5

In this example you can see another portfolio.

1. Reveal all firms.
2. Indicate whether the portfolio pays off.

If you need you can find the summary of the rules here (click to expand).

Firms	Firm 1	Firm 2	Firm 3	Firm 4	Firm 5	Does the portfolio pay off?
	Show	Show	Show	Show	Show	---------

Example 3/5

Industries
Now imagine that there are two industries, both having the same number of firms.

Example
First, you can see the firms in the two industries. Note that in this example, Industry A has less firms that make a profit than Industry B.

Then, we show you a portfolio. You can also see the industries that firms belong to. Firms are randomly selected from the given industries.

Here we ask you to do three things:

1. Indicate the number of Industry A firms in the portfolio.
2. Reveal all firms.
3. Indicate whether the portfolio pays off.

If you need you can find the summary of the rules here (click to expand).

Industry A	Industry B
	

	Firm 1	Firm 2	Firm 3	Firm 4	Firm 5	Number of Industry A firms	Does the portfolio pay off?
Industry	A	A	A	A	B		---------
Profit	Show	Show	Show	Show	Show		

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Example 4/5

Similarly to the previous example, you can see the firms in the two industries and a portfolio. Note that in this example, Industry A has more firms that make a profit than Industry B.

1. Indicate the number of Industry A firms in the portfolio.
2. Reveal all firms.
3. Indicate whether the portfolio pays off.

If you need you can find the summary of the rules here (click to expand).

Industry A	Industry B

	Firm 1	Firm 2	Firm 3	Firm 4	Firm 5	Number of Industry A firms	Does the portfolio pay off?
Industry	A	A	B	B	B		---------
Profit	Show	Show	Show	Show	Show		

Example 5/5

Similarly to the previous example, you can see the firms in the two industries and a portfolio. Note that in this example, Industry A and Industry B have the same number of firms that make a profit.

1. Indicate the number of Industry A firms in the portfolio.
2. Reveal all firms.
3. Indicate whether the portfolio pays off.

If you need you can find the summary of the rules here (click to expand).

Industry A	Industry B
	

	Firm 1	Firm 2	Firm 3	Firm 4	Firm 5	Number of Industry A firms	Does the portfolio pay off?
Industry	A	B	B	B	B		---------
Profit	Show	Show	Show	Show	Show		

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Examples finished

You answered all example questions correctly. Now you can continue with the main part of the study.

Please pay attention to all questions. At the end of the study, one question will be randomly selected and you may earn an additional bonus of $1.50 depending on your answer to that question.

Keep in mind that the bonus scheme is designed in a way that you benefit from telling the truth. For each question, we will explain clearly how you can earn bonus payments.

Click on the Next button to get started!

Next

Industries

Rosepaw and **Eclipse** are made up industries for the purpose of this task. One of them has a better outlook than the other one.

Below provide you 4-4 pieces of news related to the **Rosepaw** and the **Eclipse** industries. Read the news below carefully to find out which industry has a better outlook.

Rosepaw	Eclipse
Tax reform generally helped consumers keep more of their income, and that helped spur both new and used demand last year on the **Rosepaw** industry. Experts predict that the surge continues in the upcoming years.	Industry experts say: "The production shutdown at some of the leading plants in the **Eclipse** industry suggests a long-term plummeting of the market that might exceed the Easter plunge in the previous year."
Threats of imposing tariffs on imports negatively affect profit outlook for companies in the **Rosepaw** industry for most parts of the world. European producers, most notably the Boar Productions, are still exposed to the possibility of import tariffs in a key market and could well be caught in the crossfire between the US and China.	Spherecords Inc., the largest company on the **Eclipse** market, on Tuesday said it remains positive for the year ahead as it reported double digit profit growth for next year.
Along with other major companies in the **Rosepaw** industry, Berrycorp can prepare for weeks of disruption, as it uses 25 separate suppliers; most of them suffering from significant delays in production.	Analysts continue to report a steady improvement in business in the **Eclipse** industry, according to the **Eclipse** Alliance for Productivity and Innovation (EAPI) quarterly business outlook survey released today.

Along with other major companies in the **Rosepaw** industry, Berrycorp can prepare for weeks of disruption, as it uses 25 separate suppliers; most of them suffering from significant delays in production.

Analysts continue to report a steady improvement in business in the **Eclipse** industry, according to the **Eclipse** Alliance for Productivity and Innovation (EAPI) quarterly business outlook survey released today.

Many small firms in the **Rosepaw** industry have collapsed and many more to follow. Stocks sink after profit outlook is below expectations, negative cash flow view is confirmed.

After the volatile end of the previous year, tentative stability has returned to **Eclipse** market at the start of the new year, with investors seeing some reversal of the losses experienced in December. Growth momentum has slowed, but the deceleration phase should end before midyear with supportive and flexible policy actions.

Questions

1. **Which industry has a better outlook?**

----

2. Use the slider to report how confident you are in your answer. Note that you can also directly type in your answer into the boxes.

"I believe I have ___ % chance of answering the previous question correctly".

Recall, it pays to honestly report your estimates. In case you are interested in the details, click here.

Next

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Portfolios

Next, we will ask you to evaluate two portfolios: how likely they are to pay off? The portfolios will differ in their industry composition: the number of Eclipse firms and Rosepaw firms will be different.

In addition to that, the computer will randomly select one of the portfolios and you will **earn an additional $7.00 if it turns out to pay off.**

Next

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Portfolios

You can see two portfolios in the table below. As before, each portfolio contains 5 firms and pays off if at least 3 of them make a profit.

It is unknown whether these firms make a profit, but you can see which industries they belong to. As before, industries contain the same number of firms. However, **the industry with better outlook has a higher share of firms that make a profit.** Firms are randomly selected from the given industries.

Selection

The computer randomly selected Portfolio 2 for you. Remember, you earn **$7.00** if your portfolio pays off.

Questions

Use the sliders to report your estimates of each portfolio's chance of paying off. Note that you can also directly type in your answer into the boxes.

Before answering this question, consider the following piece of information: **A portfolio where each firm is randomly picked from the Eclipse or the Rosepaw industry, would pay off with 29% chance.**

„I believe Portfolio 1 has 63 **% chance of paying off."**

„I believe Portfolio 2 has 34 **% chance of paying off."**

	Firms					Chance of paying off (%)
Portfolio_1	Eclipse	Eclipse	Eclipse	Eclipse	Rosepaw	
Portfolio_2	Eclipse	Eclipse	Rosepaw	Rosepaw	Rosepaw	

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News

Now you will get some news about the portfolios: we reveal whether some firms make a profit or loss. Here you find a little explanation about how to interpret these news.

As an example, consider what happens if the first firm is revealed. How many of the remaining firms are needed to make a profit in order the portfolio to pay off? There are two cases:

- **Case 1**: The first firm makes a profit, therefore only 2 of the remaining 4 firms are needed to make a profit.
- **Case 2**: The first firm makes a loss, therefore still 3 of the remaining 4 firms are needed to make a profit.

Recall, firms are randomly selected from the given industries. It means that you don't know which of the above cases apply until the first firm is revealed.

- **If you learn that the firm makes a profit**, you know for sure that Case 1 applies. Thus, it is **good news** and increases the portfolio's chance of paying off.
- **If you learn that the firm makes a loss**, you know for sure that Case 2 applies. Thus, it is **bad news** and decreases the portfolio's chance of paying off.

Next

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News

News

Now we reveal the first firm in both portfolios.

- **Portfolio 1**: The first firm makes a **profit**.
- **Portfolio 2**: The first firm makes a **loss**.

Questions

How would you change your estimates in light of these information? Use the sliders to report your estimates of each portfolio's chance of paying off. Note that you can also directly type in your answer into the boxes.

„I believe Portfolio 1 has 72 **% chance of paying off."**

„I believe Portfolio 2 has 27 **% chance of paying off."**

		Firms				Previous answer	Chance of paying off (%)
Portfolio_1		Eclipse	Eclipse	Eclipse	Rosepaw	63%	
Portfolio_2		Eclipse	Rosepaw	Rosepaw	Rosepaw	34%	

Recall, it pays to honestly report your estimates. In case you are interested in the details, click here.

Next

You are finished with the main part of the study. Now we would like to ask a few questions about your demographics and about your way of thinking in this task.

Please click on the Next button to continue to the questionnaire.

Next

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Questionnaire 1/4

Age:

Gender:

Ethnic origin:

Is English your first language?

Highest degree or level of school (if currently enrolled, highest degree received):

Did you attend high school in the United States?

Marital status:

Number of children:

Employment status (besides working on Mturk):

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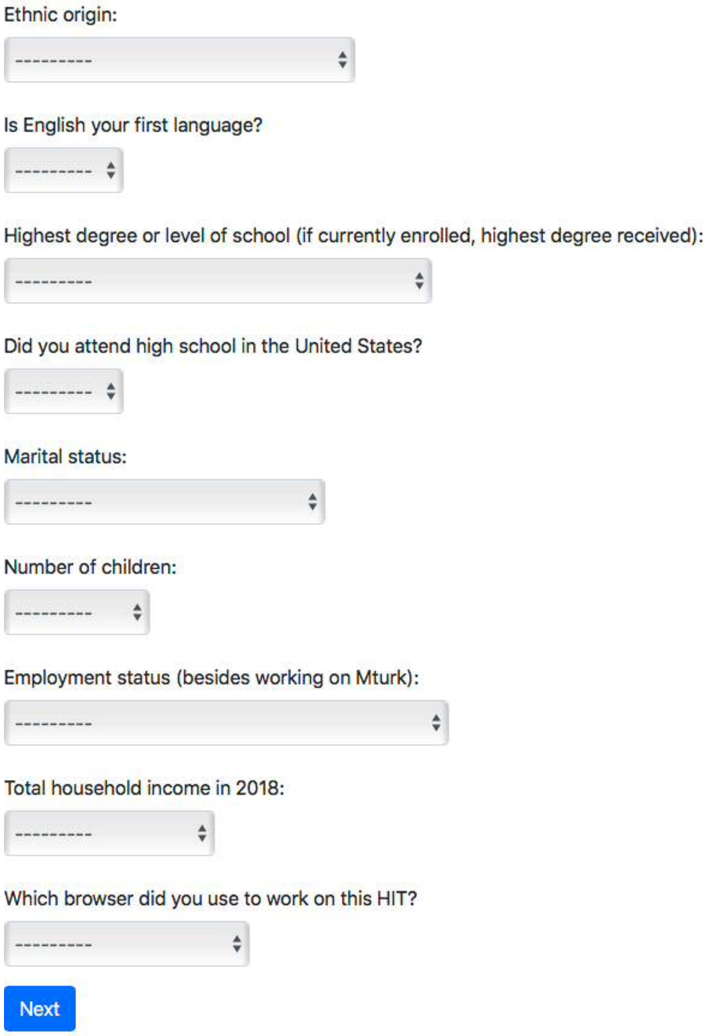
Ethnic origin:

Is English your first language?

Highest degree or level of school (if currently enrolled, highest degree received):

Did you attend high school in the United States?

Marital status:

Number of children:

Employment status (besides working on Mturk):

Total household income in 2018:

Which browser did you use to work on this HIT?

Next

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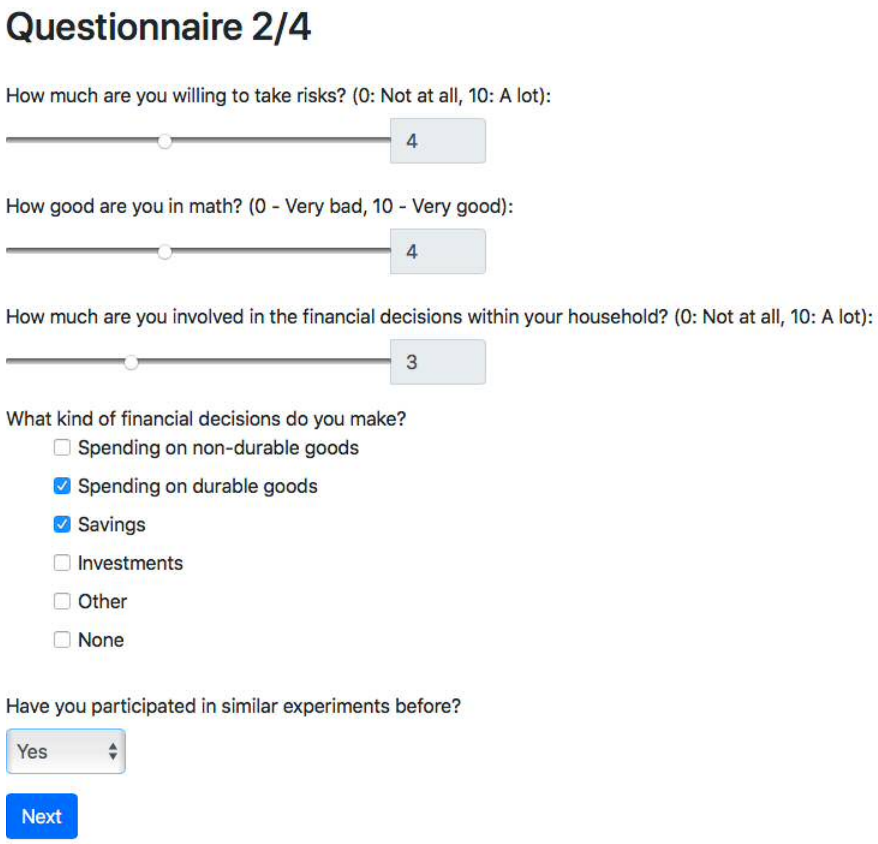

Questionnaire 2/4

How much are you willing to take risks? (0: Not at all, 10: A lot):

4

How good are you in math? (0 - Very bad, 10 - Very good):

4

How much are you involved in the financial decisions within your household? (0: Not at all, 10: A lot):

3

What kind of financial decisions do you make?

- [ ] Spending on non-durable goods
- [x] Spending on durable goods
- [x] Savings
- [ ] Investments
- [ ] Other
- [ ] None

Have you participated in similar experiments before?

Yes

Next

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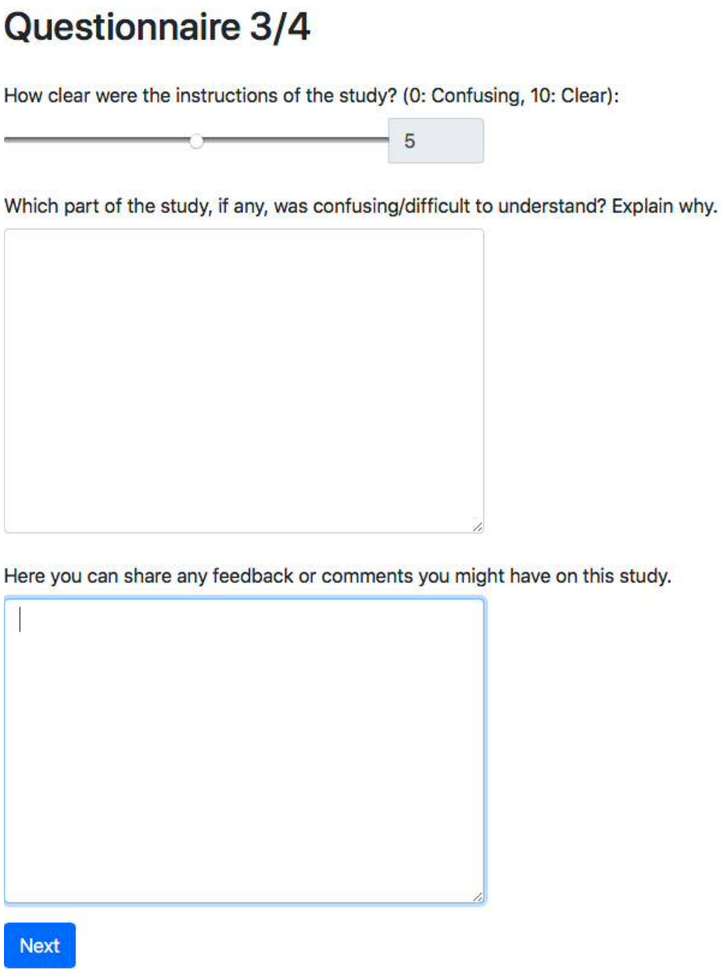

Questionnaire 3/4

How clear were the instructions of the study? (0: Confusing, 10: Clear):

5

Which part of the study, if any, was confusing/difficult to understand? Explain why.

Here you can share any feedback or comments you might have on this study.

Next

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Questionnaire 4/4

How difficult was to decide which industry has better outlook? (0: Easy, 10: Difficult):

5

How difficult was to estimate the portfolios' chances of paying off? (0: Easy, 10: Difficult):

3

Do you think that you should have been able to calculate the answer exactly from the given information?

No

How much did you think about the industry outlooks when you were estimating the portfolios' chances of paying off? (0: Not at all, 10: A lot):

7

Did you think that chances are likely to be low because the experimenter set them to minimize payoffs?

Yes

How much did you feel disappointed/lucky about the portfolio that was selected for you? (0: Disappointed, 10: Lucky):

4

How difficult was to determine the payoff at which it is worth switching to the other portfolio? (0: Easy, 10: Difficult):

5

Walk us through your thought process: How did you estimate the portfolios' chances of paying off? :

Payoffs

Thanks for participating in the study! Below you can see how your final payment is determined.

Bonus

It turns out that your portfolio does not pay off.

Based on your answer to a randomly selected question, you earned an additional $1.50.

Total payoff

Your total payoff (including the participation fee and the bonuses) is $3.00.

Finish

Please click on the Next button to submit your answers.

Next

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Appendix C

Appendix for Chapter 3

C.1 Technical details

In our dataset, occupations are coded using the Hungarian Standard Classification of Occupations, which is almost identical to the International Standard Classification of Occupations. [1]

C.2 Health Costs

[1]For a full comparison of the two classification schemes, see https://www.ksh.hu/docs/osztalyozasok/feor/fordkulcs_feor_isco_hu.pdf

Table C.1: Medication expenditures (out-of-pocket) in each year

Ev	Mean gyszer beteg	P95 gyszer beteg
2003	7,923.8	27,467.0
2004	9,334.4	33,160.0
2005	11,369.4	40,110.1
2006	12,566.8	44,058.0
2007	13,796.0	50,367.0
2008	13,104.9	49,361.0
2009	13,438.3	51,632.0
2010	13,858.4	53,830.0
2011	14,615.4	56,354.7
Total	12,158.0	45,417.6

Note: The first column shows the mean expenditure of those who had a non-zero expenditure in that given calendar year in HUF. Column two shows the 95^{th} percentiles.

Table C.2: Medication expenditure (state provided) in each year

Ev	Mean gyszer tb	P95 gyszer tb
2003	16,286.5	68,895.0
2004	20,760.3	85,200.2
2005	26,942.6	109,246.7
2006	33,542.8	130,647.2
2007	32,333.9	118,779.0
2008	36,019.8	127,439.6
2009	39,345.6	136,778.1
2010	45,149.0	154,351.0
2011	48,711.2	165,814.0
Total	32,956.0	118,851.8

Note: The first column shows the mean expenditure of those who had a non-zero expenditure in that given calendar year in HUF. Column two shows the 95^{th} percentiles.

Table C.3: Outpatient health expenditures in each year

Ev	Mean jarobeteg	P95 jarobeteg
2003	9,797.1	36,054.4
2004	12,265.6	44,802.5
2005	13,677.8	49,849.4
2006	14,098.3	51,679.0
2007	15,262.4	56,042.0
2008	17,052.9	61,702.0
2009	16,046.4	57,582.0
2010	16,392.5	59,142.0
2011	17,612.8	63,799.0
Total	14,604.4	53,349.4

Note: The first column shows the mean expenditure of those who had a non-zero expenditure in that given calendar year in HUF. Column two shows the 95^{th} percentiles.

C.3 Event-study graphs for people with health shock, compared to always healthy

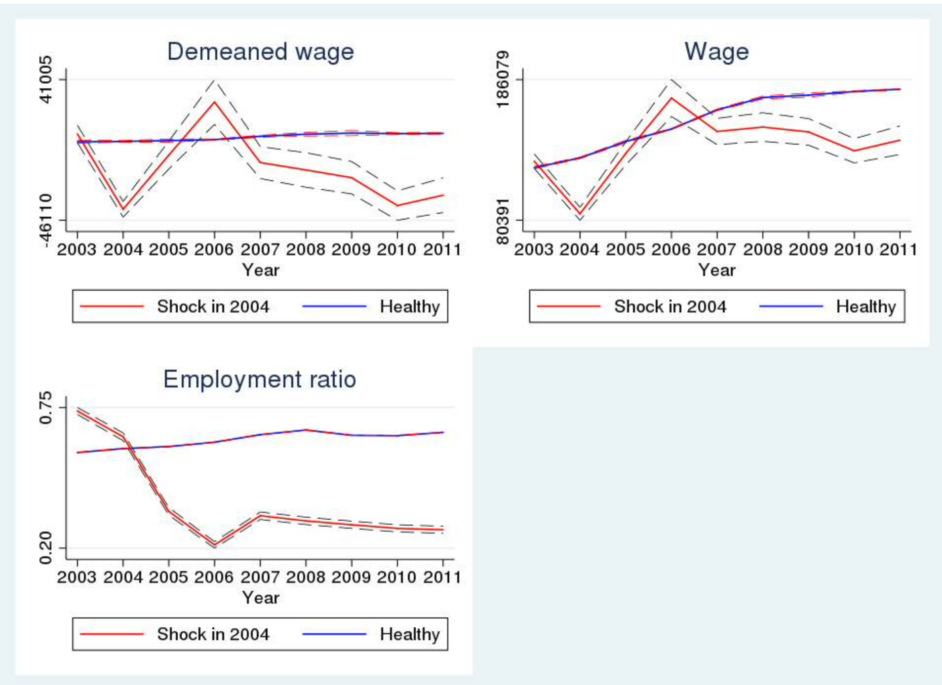

Figure C.1: The evolution of job outcomes for always healthy and people with health shock in 2004 having a healthy pre-history

Table C.4: The effect of a health shock on employment

VARIABLES	(1) employed	(2) employed	(3) employed	(4) employed	(5) employed	(6) employed	(7) employed	(8) employed
Have shock x Year 2003	0.145***	0.145***	0.121***	0.0842***	0.0578***	0.0778***	0.0979***	0.0739***
	(0.00846)	(0.00950)	(0.0114)	(0.0134)	(0.0144)	(0.0148)	(0.0155)	(0.0152)
Have shock x Year 2004	0.0278***	0.0957***	0.0680***	0.0607***	0.0446***	0.0535***	0.0432***	0.0231
	(0.00846)	(0.00950)	(0.0114)	(0.0134)	(0.0144)	(0.0148)	(0.0155)	(0.0152)
Have shock x Year 2005	-0.275***	-0.0319***	0.0265**	0.0178	0.0165	0.0174	-0.00184	0.000777
	(0.00846)	(0.00950)	(0.0114)	(0.0134)	(0.0144)	(0.0148)	(0.0155)	(0.0152)
Have shock x Year 2006	-0.424***	-0.336***	-0.0801***	0.000600	-0.0196	0.00694	-0.0288*	-0.0270*
	(0.00846)	(0.00950)	(0.0114)	(0.0134)	(0.0144)	(0.0148)	(0.0155)	(0.0152)
Have shock x Year 2007	-0.339***	-0.359***	-0.310***	-0.0585***	0.0126	0.0141	0.0139	-0.0202
	(0.00846)	(0.00950)	(0.0114)	(0.0134)	(0.0144)	(0.0148)	(0.0155)	(0.0152)
Have shock x Year 2008	-0.378***	-0.396***	-0.423***	-0.374***	-0.112***	-0.0321**	-0.0154	-0.0727***
	(0.00846)	(0.00950)	(0.0114)	(0.0134)	(0.0144)	(0.0148)	(0.0155)	(0.0152)
Have shock x Year 2009	-0.372***	-0.386***	-0.424***	-0.429***	-0.368***	-0.139***	-0.0389**	-0.104***
	(0.00846)	(0.00950)	(0.0114)	(0.0134)	(0.0144)	(0.0148)	(0.0155)	(0.0152)
Have shock x Year 2010	-0.385***	-0.399***	-0.437***	-0.447***	-0.462***	-0.371***	-0.111***	-0.123***
	(0.00846)	(0.00950)	(0.0114)	(0.0134)	(0.0144)	(0.0148)	(0.0155)	(0.0152)
Have shock x Year 2011	-0.403***	-0.412***	-0.463***	-0.456***	-0.473***	-0.452***	-0.376***	-0.196***
	(0.00846)	(0.00950)	(0.0114)	(0.0134)	(0.0144)	(0.0148)	(0.0155)	(0.0152)
Year 2003	-0.685***	-0.685***	-0.686***	-0.686***	-0.686***	-0.686***	-0.686***	-0.686***
	(0.000950)	(0.000950)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)
Year 2004	-0.670***	-0.670***	-0.670***	-0.671***	-0.671***	-0.671***	-0.671***	-0.671***
	(0.000950)	(0.000950)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)
Year 2005	-0.662***	-0.662***	-0.662***	-0.663***	-0.663***	-0.663***	-0.663***	-0.663***
	(0.000950)	(0.000950)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)
Year 2006	-0.645***	-0.645***	-0.645***	-0.645***	-0.645***	-0.645***	-0.646***	-0.646***
	(0.000950)	(0.000950)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)
Year 2007	-0.615***	-0.615***	-0.615***	-0.616***	-0.616***	-0.616***	-0.616***	-0.616***
	(0.000950)	(0.000950)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)
Year 2008	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***	-0.597***	-0.598***	-0.598***
	(0.000950)	(0.000950)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)
Year 2009	-0.617***	-0.618***	-0.618***	-0.618***	-0.618***	-0.618***	-0.618***	-0.618***
	(0.000950)	(0.000950)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)
Year 2010	-0.619***	-0.619***	-0.619***	-0.619***	-0.619***	-0.619***	-0.620***	-0.620***
	(0.000950)	(0.000950)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)
Year 2011	-0.606***	-0.606***	-0.606***	-0.607***	-0.607***	-0.607***	-0.607***	-0.607***
	(0.000950)	(0.000950)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)	(0.000951)
Male	0.00315***	0.00309***	0.00309***	0.00304***	0.00308***	0.00304***	0.00305***	0.00302***
	(0.000268)	(0.000268)	(0.000268)	(0.000268)	(0.000268)	(0.000268)	(0.000268)	(0.000268)
Age in 2003	0.0754***	0.0754***	0.0755***	0.0755***	0.0755***	0.0755***	0.0755***	0.0755***
	(5.47e-05)	(5.47e-05)	(5.47e-05)	(5.47e-05)	(5.47e-05)	(5.47e-05)	(5.47e-05)	(5.47e-05)
Age Square in 2003	-0.000981***	-0.000981***	-0.000982***	-0.000982***	-0.000982***	-0.000982***	-0.000982***	-0.000982***
	(7.86e-07)	(7.86e-07)	(7.87e-07)	(7.87e-07)	(7.87e-07)	(7.87e-07)	(7.87e-07)	(7.87e-07)
Observations	11,827,431	11,822,301	11,816,199	11,812,545	11,811,276	11,810,745	11,810,034	11,810,331
R-squared	0.687	0.687	0.687	0.687	0.687	0.688	0.688	0.688
Healthy Shock	2004	2005	2006	2007	2008	2009	2010	2011

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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Table C.5: The effect of a health shock on wage

VARIABLES	(1) Demeaned wage	(2) Demeaned wage	(3) Demeaned wage	(4) Demeaned wage	(5) Demeaned wage	(6) Demeaned wage	(7) Demeaned wage	(8) Demeaned wage
Have shock x Year 2003	275.7	7,484	5,079	-8,229	-5,585	3,014	-8,017	-2,438
	(10,670)	(11,951)	(14,572)	(17,451)	(19,077)	(19,369)	(19,909)	(19,723)
Have shock x Year 2004	-47,204***	6,822	5,198	-10,165	-8,346	4,766	-11,885	-2,252
	(11,485)	(12,228)	(14,961)	(17,555)	(19,047)	(19,495)	(20,478)	(20,241)
Have shock x Year 2005	-14,607	-44,000***	7,642	-9,964	-12,480	4,446	-13,129	-3,059
	(15,637)	(13,404)	(15,336)	(18,017)	(19,337)	(19,909)	(21,059)	(20,459)
Have shock x Year 2006	17,243	-4,422	-57,967***	-12,342	-18,358	-8,384	-10,169	-5,807
	(19,920)	(18,681)	(16,500)	(18,017)	(19,625)	(19,807)	(21,222)	(20,628)
Have shock x Year 2007	-22,862	7,465	-11,092	-61,188***	-23,627	-11,814	-14,368	-5,910
	(16,040)	(18,459)	(20,495)	(18,435)	(18,726)	(19,275)	(20,098)	(20,063)
Have shock x Year 2008	-30,115*	-6,645	-28,735	-41,651	-81,951***	-16,507	-20,803	-17,644
	(16,564)	(19,058)	(23,823)	(25,509)	(20,349)	(19,673)	(20,258)	(20,589)
Have shock x Year 2009	-36,114**	-25,002	-29,615	-38,226	-39,314	-76,402***	-22,072	-19,500
	(16,993)	(19,412)	(24,851)	(29,087)	(27,982)	(21,871)	(20,938)	(21,474)
Have shock x Year 2010	-54,355***	-39,528**	-57,830**	-39,589	-33,849	-32,439	-82,269***	-21,102
	(17,422)	(19,922)	(25,543)	(30,275)	(33,647)	(28,981)	(22,242)	(21,848)
Have shock x Year 2011	-48,995***	-41,320**	-48,062*	-47,422	-25,781	-26,279	-49,749*	-83,902***
	(17,596)	(19,939)	(26,336)	(30,037)	(33,483)	(32,851)	(29,691)	(23,098)
Year 2003	-172,621***	-172,630***	-172,593***	-172,676***	-172,688***	-172,762***	-172,662***	-172,699***
	(1,849)	(1,850)	(1,850)	(1,851)	(1,851)	(1,850)	(1,850)	(1,850)
Year 2004	-171,544***	-171,553***	-171,516***	-171,600***	-171,612***	-171,686***	-171,586***	-171,623***
	(1,845)	(1,846)	(1,846)	(1,846)	(1,846)	(1,846)	(1,846)	(1,846)
Year 2005	-169,947***	-169,956***	-169,919***	-170,003***	-170,014***	-170,089***	-169,989***	-170,026***
	(1,841)	(1,841)	(1,842)	(1,842)	(1,842)	(1,842)	(1,842)	(1,842)
Year 2006	-168,059***	-168,067***	-168,031***	-168,115***	-168,126***	-168,201***	-168,101***	-168,139***
	(1,832)	(1,832)	(1,833)	(1,833)	(1,833)	(1,833)	(1,833)	(1,833)
Year 2007	-164,467***	-164,475***	-164,439***	-164,522***	-164,533***	-164,607***	-164,508***	-164,545***
	(1,810)	(1,810)	(1,811)	(1,811)	(1,811)	(1,811)	(1,811)	(1,811)
Year 2008	-161,626***	-161,634***	-161,598***	-161,681***	-161,692***	-161,766***	-161,667***	-161,704***
	(1,798)	(1,799)	(1,799)	(1,800)	(1,800)	(1,800)	(1,800)	(1,800)
Year 2009	-159,841***	-159,850***	-159,814***	-159,897***	-159,908***	-159,982***	-159,883***	-159,920***
	(1,795)	(1,795)	(1,795)	(1,796)	(1,796)	(1,796)	(1,796)	(1,796)
Year 2010	-158,890***	-158,899***	-158,863***	-158,946***	-158,957***	-159,030***	-158,932***	-158,969***
	(1,786)	(1,786)	(1,786)	(1,787)	(1,787)	(1,787)	(1,787)	(1,787)
Year 2011	-156,869***	-156,878***	-156,842***	-156,925***	-156,935***	-157,009***	-156,911***	-156,948***
	(1,772)	(1,772)	(1,773)	(1,773)	(1,773)	(1,773)	(1,773)	(1,773)
Male	22,342***	22,337***	22,329***	22,340***	22,340***	22,347***	22,331***	22,338***
	(369.2)	(369.3)	(369.4)	(369.5)	(369.5)	(369.5)	(369.5)	(369.4)
Age in 2003	8,020***	8,021***	8,019***	8,025***	8,025***	8,030***	8,024***	8,026***
	(101.2)	(101.3)	(101.3)	(101.3)	(101.3)	(101.3)	(101.3)	(101.3)
Age Square in 2003	-92.57***	-92.58***	-92.56***	-92.63***	-92.64***	-92.70***	-92.63***	-92.66***
	(1.401)	(1.401)	(1.402)	(1.402)	(1.402)	(1.402)	(1.402)	(1.402)
Observations	7,408,101	7,406,946	7,404,712	7,403,489	7,403,152	7,403,327	7,403,299	7,403,530
R-squared	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002
Healthy Shock	2004	2005	2006	2007	2008	2009	2010	2011

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C.6: Comparing the effect of a health shock on managers' and non-managers' wage

VARIABLES	(1) Demeaned wage
After	-7,076**
	(3,395)
Manager	202,800***
	(5,634)
Manager x After	-18,772*
	(9,671)
Constant	-22,276***
	(1,789)
Observations	19,054
R-squared	0.088
Controls Included	No

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C.7: Comparing the effect of a health shock on managers' and non-managers' employment

VARIABLES	(1) Employed
Manager	0.108***
	(0.0113)
After	-0.460***
	(0.00482)
Manager x After	0.0720***
	(0.0159)
Male	-0.0223***
	(0.00472)
Age in 2003	0.0225***
	(0.00166)
Age Square in 2003	-0.000348***
	(1.96e-05)
Constant	0.493***
	(0.0347)
Observations	36,056
R-squared	0.236
Controls Included	No

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

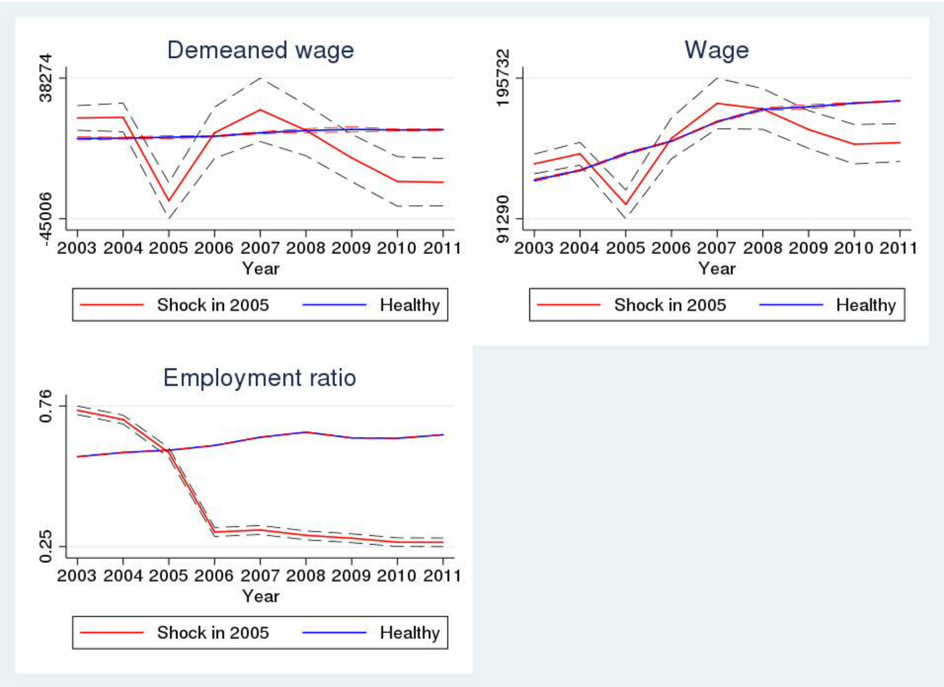

Figure C.2: The evolution of job outcomes for always healthy and people with health shock in 2005 having a healthy pre-history

Table C.8: Comparing managers and non-managers with health shock

VARIABLES	(1) Employed	(2) Employed	(3) Employed	(4) Employed
Manager	0.0912***	0.0963***	0.114***	0.114***
	(0.00951)	(0.00951)	(0.00939)	(0.00939)
After	-0.491***	-0.491***	-0.491***	-0.491***
	(0.00408)	(0.00407)	(0.00401)	(0.00401)
Manager x After	0.0557***	0.0557***	0.0557***	0.0557***
	(0.0134)	(0.0134)	(0.0132)	(0.0132)
Health Cost	-1.27e-08***	-1.41e-08***	-1.59e-08***	-1.59e-08***
	(1.48e-09)	(1.49e-09)	(1.47e-09)	(1.47e-09)
Male		-0.0443***	-0.0229***	-0.0229***
		(0.00394)	(0.00394)	(0.00394)
Age in 2003			0.0249***	0.0249***
			(0.00139)	(0.00139)
Age Square in 2003			-0.000370***	-0.000370***
			(1.64e-05)	(1.64e-05)
Constant	0.770***	0.798***	0.460***	0.460***
	(0.00438)	(0.00504)	(0.0294)	(0.0294)
Observations	50,244	50,244	50,244	50,244
R-squared	0.242	0.244	0.265	0.265
Health Emp.	Yes	Yes	Yes	Yes
Controls	No	Male	Age Male	Age Male
Prior wage	No	No	No	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

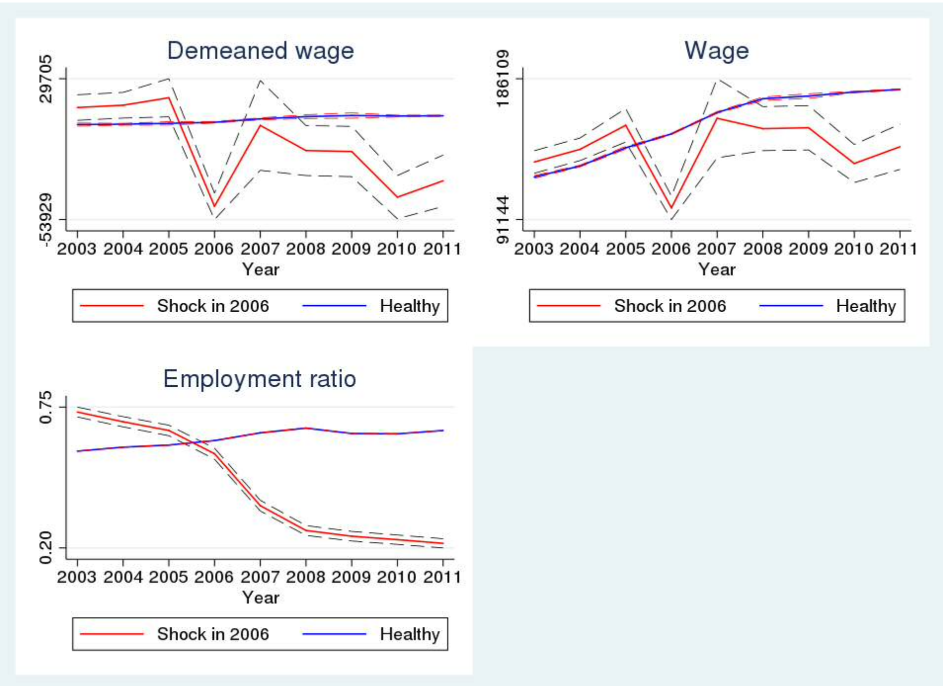

Figure C.3: The evolution of job outcomes for always healthy and people with health shock in 2006 having a healthy pre-history

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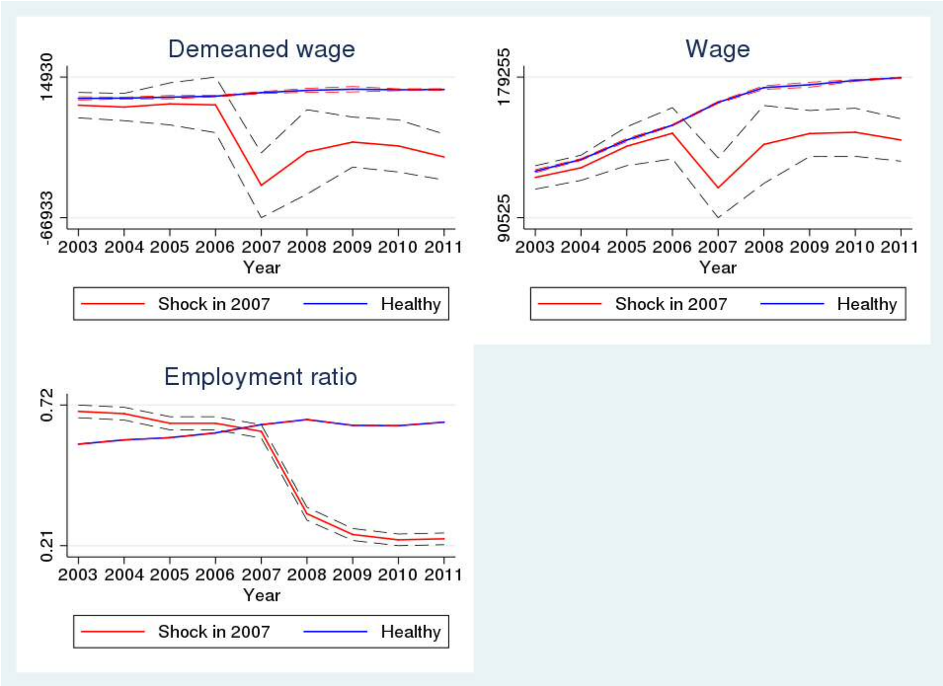

Figure C.4: The evolution of job outcomes for always healthy and people with health shock in 2007 having a healthy pre-history

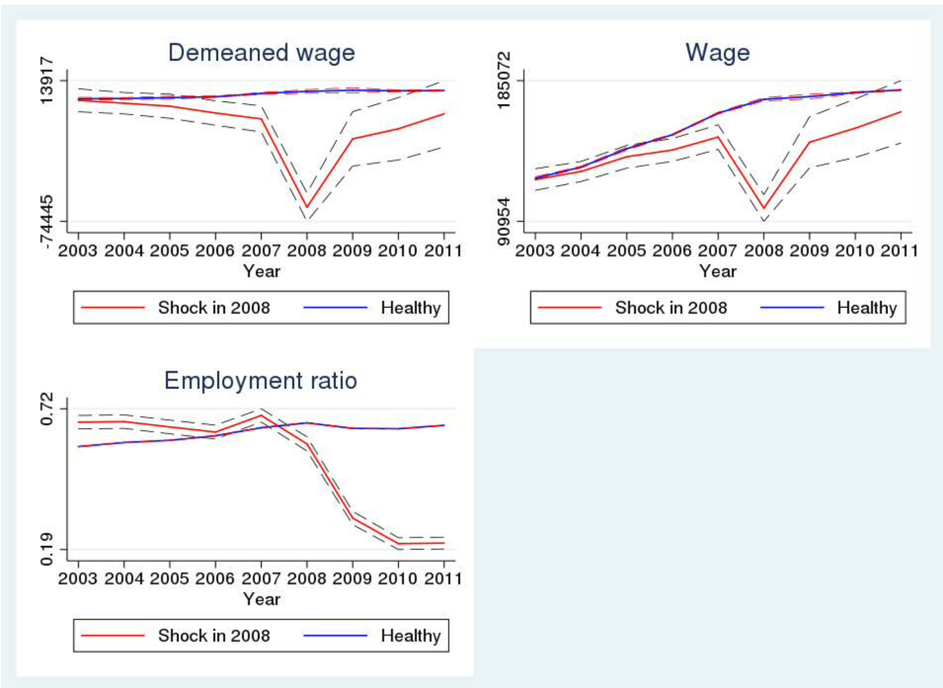

Figure C.5: The evolution of job outcomes for always healthy and people with health shock in 2008 having a healthy pre-history

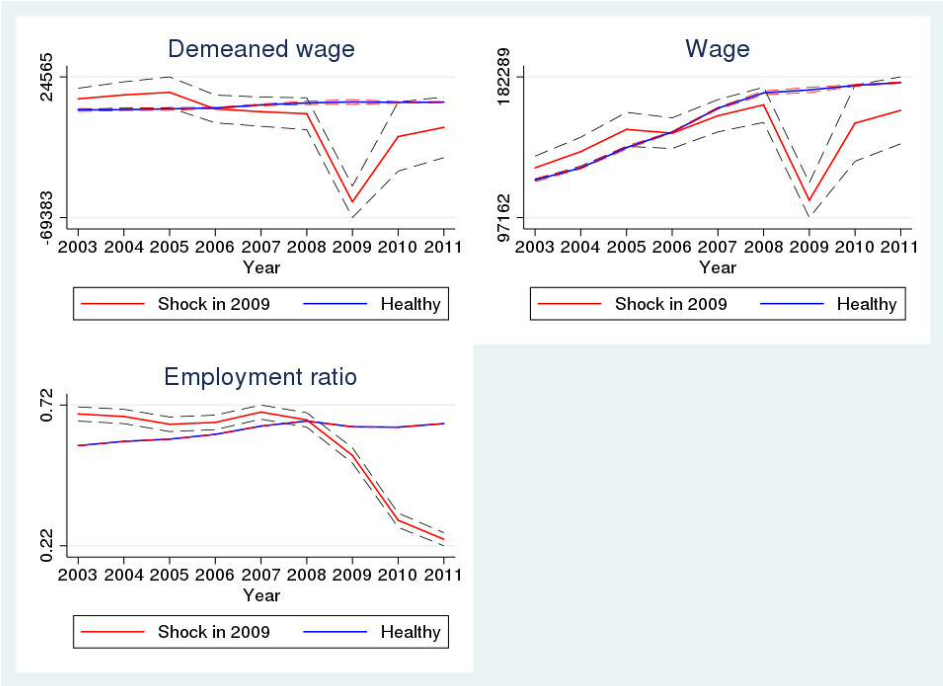

Figure C.6: The evolution of job outcomes for always healthy and people with health shock in 2009 having a healthy pre-history

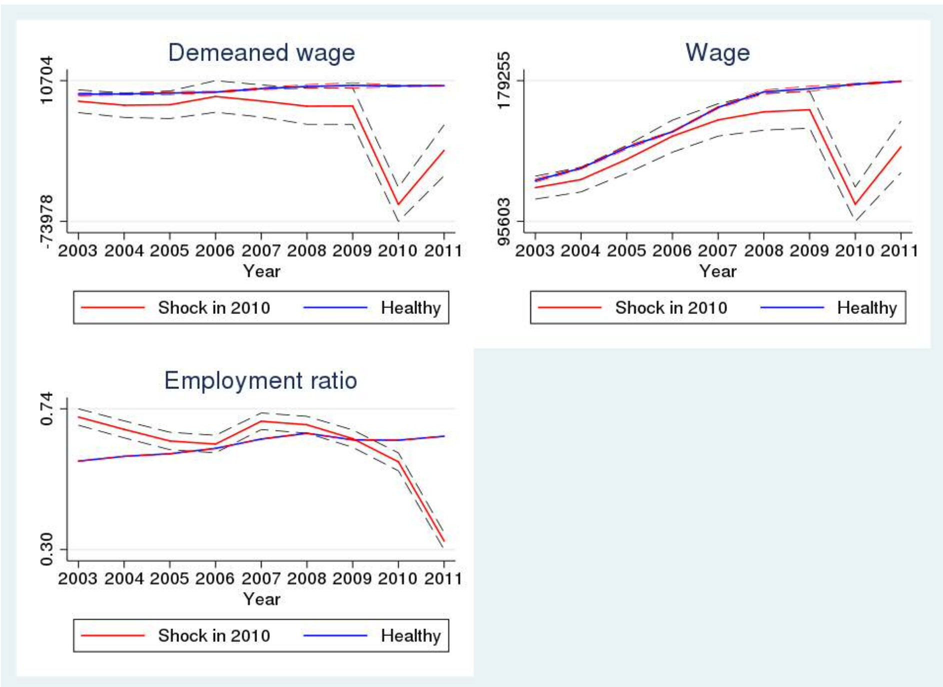

Figure C.7: The evolution of job outcomes for always healthy and people with health shock in 2010 having a healthy pre-history

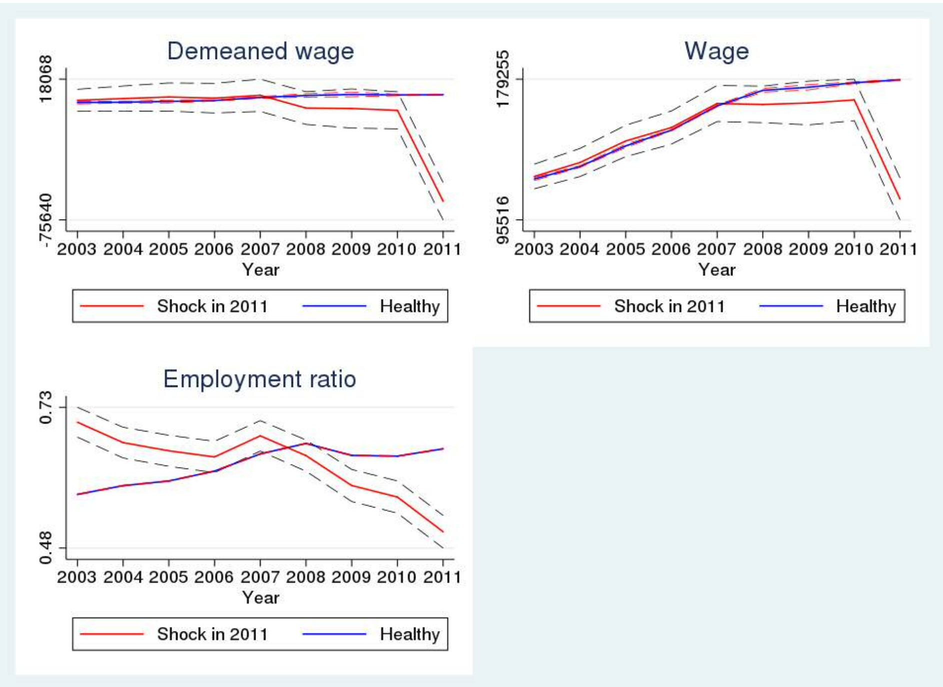

Figure C.8: The evolution of job outcomes for always healthy and people with health shock in 2011 having a healthy pre-history

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