The Effect of Feedback Content and Timing on Self-other Gap in Risk-taking

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Abstract

Previous experiments on delegated decision making find seemingly contradictory results: some experiments find that people take greater risks when they decide for others than for themselves, while other experiments find the opposite. I hypothesize that these mixed conclusions are the result of the type and the timing of feedback provided to subjects and conduct an experiment to identify these causes. In a choice between two binary lotteries, subjects either learn the outcome of only the chosen lottery or the outcome for both the chosen and the unchosen lottery. Feedback is provided immediately after each decision or after a sequence of ten decisions. I find that when subjects receive immediate feedback on the outcome of both the chosen and the unchosen options, they make a risky shift. That is, subjects take greater risks for others than for themselves. If I alter either the timing or the content of feedback, the risky shift disappears. If I alter both the timing and the content of feedback so that the feedback is given at the end, only on the outcome of the chosen option, I find a risky shift again. These findings reconcile the contradictory results in the previous studies. I present a theoretical model and analyze how subjects' risk-taking behavior evolves as they make more decisions.

1 Introduction

Many important decisions are delegated: We entrust the investment managers, congressmen, and physicians with our financial, political and health-related decisions respectively. Often delegated decisions are about how much risks to take. The empirical evidence suggests that people making decisions for others do not always take the same amount of risks as they would take for themselves. This so-called self-other gap in risk-taking has been extensively studied in the experimental literature, and now we better understand some aspects of it. For example, some experiments have looked that incentivized delegations, in which the delegator and the delegated get the same or proportional payoffs, and therefore their incentives are aligned. These experiments find that people take risks as if they weigh between their own risk preference and their believed risk preference of the other person.

On the other hand, previous experiments on unincentivized delegations have reported seemingly contradictory results. In unincentivized delegations, the incentives of the delegator and the delegated are misaligned because the delegated decision maker's choice only determines the payoff of the other person, but not his own. Some experiments on unincetivized delegation find that delegated decision-makers take greater risks for others than for themselves, while other experiments find the opposite. Since these experiments differ in many aspects of their experiment designs, their results are not directly comparable.

Although incentivized delegations have been studied more widely for its application in the financial market, unincentivized delegations are also pervasive in our society: parents representing their children, high school counselor advising on college admission, doctors and families who make medical decisions for patients, and appointed government officials are less likely to be driven by the pecuniary payoffs from their decisions. Their decisions depend more on their concern for the people people for whom they make decisions.

My experiment on unincentivized delegation aims to reconcile the mixed results in the previous literature. First, by analyzing past experiments, I identify their feedback content and timing as the potential driving force of the results. In particular, it matters whether the subjects get some feedback on the outcome of the unchosen option (feedback content) and when the feedback is given (feedback timing). The feedback aspect of experimental design has been relatively neglected in previous studies on delegated decision making, although all experiments make some conscious choices on feedback. In some previous experiments, subjects only learn the outcome of their chosen options. In other experiments, subjects learn the outcome of both their chosen and unchosen options, which enables them to evaluate their choices counterfactually. The previous experiments also differ in their timing of feedback. In some experiments, subjects learn the outcome immediately after each decision. In other experiments, they learn the outcome only at the end of a sequence of decisions.

When previous experiments are sorted by feedback content and timing as in Table 1, a four-fold pattern emerges. In Table 1, the self-other gaps found in each study is color-coded. The light gray shade means that the corresponding study finds a cautious shift (Subjects take smaller risks for others than for themselves). The black shade means a risky shift (Subjects take greater or the same risks for others than for themselves). No shade means that the study finds no significant self-other gap. When the feedback is given immediately after each decision, people who do not anticipate counterfactual feedback make a cautious shift, while people who receive given counterfactual feedback make a weakly risky shift. When the feedback is given only at the end, the direction of shifts reverse. People make a weakly risky shift when they do not get counterfactual feedback while they make a cautious shift when they do get counterfactual feedback.

Table 1

Summary of previous experiments

Contents \ Timing	Immediate	At the end
No Counterfactual Feedback	Reynolds et al.(2009)	Chakravarty et al.(2011) Eriksen et al.(2017)
Counterfactual Feedback	Pollmann et al.(2014) Eriksen & Kvaløy(2010)	Füllbrunn & Luhan(2015) Eriksen & Kvaløy(2010)

My experiment directly tests the hypothesis that the feedback content and timing drive the self-other gaps in risk-taking as summarized in Table 1. Subjects choose between a risky and a safe lottery either for themselves or for another person. The decision problem remains constant as the feedback content and timing vary across treatments. Table 2 summarizes the results from the experiment. The table shows that the self-other gaps found in my experiment are at least weakly consistent with the pattern observed in the previous studies: A risky shift was found only under the conditions where previous experiments found risky shifts. Under the conditions where previous experiments found cautious shifts, my experiment finds no significant cautious or risky shift.

Table 2
Summary of experimental results in this study

Contents \ Timing	Immediate	At the end
No counterfactual evaluation	None	Risky
Counterfactual evaluation	Risky	None

2 Literature

This study is related to two groups of studies on risk preference. The first group of studies compares the amount of risks people take when they make decisions for themselves and when they make decisions for other people. The second group of studies focuses on the effect of counterfactual evaluation on risk preference. In this literature chapter, I discuss the two groups of studies in more detail.

2.1 Self-other gap in risk preference

Numerous experiments have studied delegated decision making under risks using various decision problems, incentives, design features, and therefore, have yielded different results. Some experiments find that people take greater risks for others than for themselves (risky shift), while others find that people take smaller risks for others than for themselves (cautious shift).

There have been attempts to develop a unified framework that can reconcile these contradictory findings. Pahlke et al. (2015) find that a delegated decision maker's risk attitude depends on the domain of payoffs – whether the payoffs are in the gain, loss, or gain/loss mixed region. Delegated decision making accentuates the four-fold pattern of risk attitude as predicted by prospect theory.The accentuation can drive a risky shift or a cautious shift depending on the domain of payoffs. Füllbrunn and Luhan (2015) make the first distinction between incentivized delegations (with perfectly aligned payoffs) and unincentivized ones (with fixed payment). Their experiment directly compares the risk-taking behavior under the two incentive conditions. Under both the incentivized and the unincentivized delegations, they find cautious shifts of similar sizes.

The findings from other experiments on incentivized delegation are consistent with Füllbrunn and Luhan (2015)'s result: people make a cautious shift when deciding for others (Bolton and Ockenfels, 2010; Bolton et al., 2015; Pahlke et al., 2015; Eijkelenboom and Vostroknutov 2016). However, when it comes to unincentivized delegation, the experimental findings are mixed. Table 3 lists seven experimental studies on unincentivized delegation.[1] The studies use a variety of decision problems, with the Gneezy & Potters (1997) investment game being the most popular. The column named Feedback Content shows that the experiments differ in whether the subjects learn the outcome of unchosen options or not. FC means that the subjects in the study learn the outcome of both the chosen and the unchosen option. F means that subjects learn only the outcome of the unchosen option. The experiments also vary in the timing of feedback. Some studies provide feedback immediately after each round of decision whereas other studies provide it less frequently.

The column named Result shows the findings from the experiments. Cautious means that the study finds a cautious shift in risk-taking, Risky means that the study finds a risky shift in risk-taking, and none means that the study finds no statistically significant shift. Two studies report risky shifts, three others report cautious shifts, and the remaining two studies report none.[2]

My study attempts to reconcile these seemingly contradictory results by highlighting the differences in feedback content and timing across these studies. The literature on delegated decision making has been mostly neglecting the feedback aspect of experimental design. The only study that directly tests the effect of feedback is Eriksen and Kvaløy (2010). Their experiment varies the frequency of feedback across treatments to find that subjects make a cautious shift only when the feedback is infrequent (every three rounds of decisions). When the feedback is given after every round, there is no significant self-other gap in risk-taking.

[1] Two treatments from Eriksen and Kvaløy (2010) count as two separate studies here because the treatments differ in feedback timing.

[2] Eriksen & Kvaløy (2010) do not report the self-other gap in risk-taking separately for the immediate feedback treatment and the infrequent feedback treatment. The results reported here were obtained from the raw data the authors kindly provided. I ran Mann-Whitney U tests following the definitions and procedures described in their study. In the immediate feedback treatment, the z-value and the two-sided p-value for the self-other gap in risk-taking were respectively 0.705 and 0.481. In the infrequent feedback treatment, the z-value and the two-sided p-value were respectively 1.715 and 0.086.

Table 3

Previous experiments on unincentivized delegated decision making under risks

Study	Decision Problem	Design	Result	Feedback Content	Feedback Timing	Remarks
Eriksen and Kvaløy (2010)	GP	BS	none	FC	after each round	
Eriksen and Kvaløy (2010)	GP	BS	Cautious	FC	after every 3 rounds	
Füllbrunn and Luhan (2015)	GP	WS	Cautious	FC	at the end	
Pollmann et al. (2014)	GP	BS	Risky	FC	after each round	
Eriksen et al. (2017)	EG	WS	none	F	at the end	50/50 gambles with different payoff spread
Chakravarty et al. (2011)	MPL	WS	Risky	F	at the end	2 gambles with same prob and different payoff spread
Reynolds et al. (2009)	Binary prospects	WS	Cautious	F	after each task	sure thing vs. risky

- GP = Gneezy & Potters (1997) investment game
- EG = Eckel & Grossman gamble choice
- MPL = Multiple Price Choice
- WS = Within-subject design
- BS = Between-subject design
- Cautious = cautious shift
- Risky = risky shift
- FC = feedback provided on outcomes from both chosen and unchosen options
- F = feedback provided on the outcome of the chosen option only

2.2 Counterfactual evaluation

My experiment also connects to the literature on the effect of counterfactual evaluation on risk-taking. A decision maker faces a counterfactual evaluation if he expects his decision to be evaluated (by himself or others) in comparison to the option he did not choose. There is ample empirical evidence that counterfactual evaluation is an important factor in risk-taking behavior.

If an agent choses for himself, he might expect to regret a choice that turns out to ex-post suboptimal. Fioretti et al.(2017) cite a wide range of examples in which anticipated regret affects risk-taking behavior: heart disease prevention (Boeri et al., 2013), auctions (Filiz-Ozbay and Ozbay, 2007; Hayashi and Yoshimoto, 2016), financial markets (Fogel and Berry, 2006; Frydman et al., 2017; Frydman and Camerer, 2016, Michenaud and Solnik, 2008) and portfolio and pension choices (Muermann et al., 2006; Hazan and Kale, 2015). Loomes & Sugden (1981) construct a theoretical model of regret and test it experimentally in a sequence of studies.[3] They develop a representation of an agent whose expected utility incorporates regret triggered by a counterfactual evaluation. Experimentally they demonstrate that agents can exhibit preference reversals between two lotteries if they behave as a regret-averse agent. Loomes & Sugden's model only considers an environment where the agent always learns the outcome of the unchosen options. In contrast, the experiments by Zeelenberg et al. (1996) and Ritov (1996) consider an agent who might not always learn the outcome of the unchosen option. Their results are consistent with an agent who is regret-averse as in Loomes & Sugden's, except that the regret is triggered only if he learns the outcome of the unchosen option.

If an agent chooses for someone else, he might also expect to be blamed for an ex-post suboptimal choice. Gurdal et al. (2013) find a significant effect of the anticipated blame on decision making when the blame reduces rewards to the decision maker.

Alex Imas (2017) is similar to this study in its methodology. His experiment is also motivated by the seemingly contradictory results in previous experiments: Some experiments find that people take greater risks after experiencing a loss while other experiments find that people take smaller risks after a loss. Imas shows that the mixed results can be reconciled by distinguishing paper loss and realized loss. His experiment finds that a realized loss reduces risk-taking in a subsequent decision whereas a paper-loss increases risk-taking. The findings match the observations from previous studies. Although his study and my study answer different questions, the

[3] Loomes & Sugden (1982); Loomes & Sugden (1983); Loomes & Sugden (1986); Loomes (1987); Loomes & Sugden (1987); Loomes (1988); Loomes, Starmer & Sugden (1991)

two studies have two similarities in their approach: First, we test hypotheses inspired by observations from previous experiments. Second, we both identify the feedback aspect of experimental design, which has been largely ignored so far, as an important determinant of risk-taking behavior.

3 Hypotheses

I design and implement an experiment to test the following hypotheses formulated from the findings in the previous experiments.

1. When the feedback is immediate:

   (a) When people do not learn the outcome of the unchosen project, they are less likely to take risks for others than for themselves. (cautious shift)

   (b) When people learn something about the outcome of the unchosen project, they are more likely to take risks for others than for themselves. (risky shift)

2. When the feedback is provided only at the end, the self-other gaps described in hypothesis 1 reverse. In other words,

   (a) When people do not learn the outcome of the unchosen project, they are more likely to take risks for others than for themselves. (risky shift)

   (b) When people learn something about the outcome of the unchosen project, they are less likely to take risks for others than for themselves. (cautious shift)

4 Experimental Design

4.1 Decision problem

Subjects face a choice between a safe project and a risky project for a hypothetical firm. The outcome of each project is binary: success or failure.

Table 4
Choice of Projects

Project	Payoff	Probability
R(Risky)	130	.35
	0	.65
S(Safe)	70	.65
	0	.35

Table 4 shows the possible payoffs of the two projects and the probability of each payoff. The risky project R succeeds with 35% probability and yields a payoff of 130 ECU(experimental currency unit)s in that case. The project fails with the remaining 65% probability and yields a zero payoff in that case. The safe project S succeeds with 65% probability and yields a payoff of 70 ECUs in that case. Project S fails with the remaining 35% probability and yields a zero payoff in that case. Success and failure of the two projects are independent.

Table 5
Choice of Projects in Zeelenberg(1996)

Projects	Payoff	Probability
R(Risky)	130	.35
	0	.65
S(Safe)	X	.65
	0	.35

The decision problem is a modification of the one used by Zeelenberg et al. (1996). Their decision problem specifies the same payoffs and probabilities for the risky project as in Table 4. They also assume the same probability of success for the safe project. The only difference between their decision problem and the one used here is that they left the payoff of the safe project in the case of success as a variable X, as in Table 5. The first part of their experiment elicits X that makes each subject indifferent between the risky project and the safe project. In the second part, each subject is asked to choose between the risky and the safe project, assuming that the safe project has the chosen X as the success payoff.

The mean of X elicited in the first part of Zeelenberg et al. (1996) is 69.05, which suggests that subjects are on average close to risk neutral.[4] For my experiment, I round up that mean and specify the safe project's success payoff as 70. There are three reasons for the payoff specification. First, the risky project becomes a mean-preserving spread of the safe project. Therefore any shift in the choice between the two projects reflects the subject's risk attitude. Second, by choosing a number close to the mean, I expect that roughly half of the subjects would choose the risky project while the other half chooses the safe project. In this case, the fraction of risk-taking subjects would have a plenty room to shift upward or downward depending on the treatment. Lastly, rounding up the number enables a simpler presentation of the decision problem to the subjects.

The decision problem differs from the Gneezy & Potters(1997) investment game, a popular choice for experiments on delegated decision making(Eriksen & Kvaløy 2010; Füllbrunn & Luhan 2015; Pollmann et al. 2014). In a Gneezy & Potters investment game, a subject decides how much of a given fund he would invest in a risky project. The amount of investment measures his risk preference. Since not investing yields a sure thing, subjects always know the outcome of not investing. On the other hand, if a subject chooses not to invest at all, it depends on the experiment design whether the subjects would see the outcome of the investment or not. Therefore, a decision to invest is always subject to a counterfactual evaluation regardless of the experiment design, while investing nothing might not be so if the experimenter chooses not to show the counterfactual outcome to the subjects.

In the decision problem I present to the subjects, they face a choice between two binary lotteries. Neither of the lotteries is a sure thing. The decision problem allows the experimenter to manipulate what the subjects learn and don't learn about the outcome of the lotteries in a way that is impossible in a Gneezy & Potters investment game. For example, subjects might learn only the outcome of the projects they choose. On the other extreme, subjects might learn both the outcome of the chosen and unchosen projects. Alternatively, subjects might learn the outcome of the project they chose and also learn the outcome of the risky project regardless of choice. In this case, only safe project would be subject to a counterfactual evaluation. Such manipulations lead to various treatments I will discuss in the next section.

[4] $X = 70$ makes the subjects perfectly risk neutral.

4.2 Treatments

The experiment features a $2 \times 3 \times 2$, between-subjects design, as summarized in Table 6. The first dimension of the design is whether subjects choose for themselves or another person. In the baseline (SELF) treatments, each subject chooses one of the two projects for his own (hypothetical) firm as an owner. He chooses from the same two projects for 10 rounds. In OTHER treatments, half of the subjects are randomly assigned a role of an agent while the other half a role of an owner. The roles are fixed, and all subjects remain anonymous throughout the experiment. The agent is asked to choose a project on behalf of his owner. The owner gets the realized outcome of the project chosen by the agent, while the agent gets a fixed payment regardless of his choice. The owner never makes a decision. The agent chooses between the same two projects for 10 rounds, each time for a different owner (turnpike design). Both the owner and the agent see the decision problem and what the agent chose in each round.

Table 6
Treatments

Feedback Timing	Feedback Content	Decisions for	
		SELF	OTHER
IMMEDIATE	SAFE		
	CHOSEN		
	RISKY		
AT THE END	SAFE		
	CHOSEN		
	RISKY		

In the second dimension of the design, I manipulate what subjects learn about the outcome of the unchosen option, as in Zeelenberg(1996). In CHOSEN treatments, all subjects learn only the realized outcome of the chosen project. In SAFE treatments, all subjects learn the outcome of the safe project regardless of which project was chosen. That is, subjects learn only the outcome of the safe project if the safe lottery is chosen, and learn the outcome of both projects if the risky lottery is chosen. This treatment is intended to make the risky project more regret/blame-prone than it is in the CHOSEN treatment. A subject who chooses the safe project does not see the outcome of the unchosen lottery, while a subject who chooses the risky project might have to see that he could have done better by choosing the safe project. The last treatment, RISKY, mirrors the SAFE treatment in that subjects learn the outcome of the risky project regardless of which project was chosen. This makes the choice of the safe project more prone to regret or blame.

The third dimension of the design varies the timing of feedback provision. In IMMEDIATE treatments, subjects get feedback immediately after each round of decision. In AT THE END treatments, subject get feedback only after they make all 10 rounds of decisions. Moreover, they learn only the outcome in the round that is randomly selected for payments.

4.3 Payment

In all the treatments described above, subjects make decisions for ten rounds. At the end of the experiment, only one of the 10 rounds is randomly selected for payments. I convert the payoff from the selected round into a dollar amount at the rate of $0.10 per ECU. For example, 70 ECUs translates into $7. In SELF treatments, each subject gets the payoff from their chosen project in the selected round, in addition to a show-up fee of $10. In OTHER treatments, both owners and agents are paid a show-up fee of $10. The owners are also paid the payoff from the project their agent chose in the selected round. The agents are paid a fixed fee of $5 regardless of their decisions.

5 Results

5.1 Session Characteristics

The experiment was conducted at the Center for Experimental Social Science at New York University. Subjects were recruited from undergraduate students. Table 5 summarizes the session characteristics.

Table 7

Session Characteristics

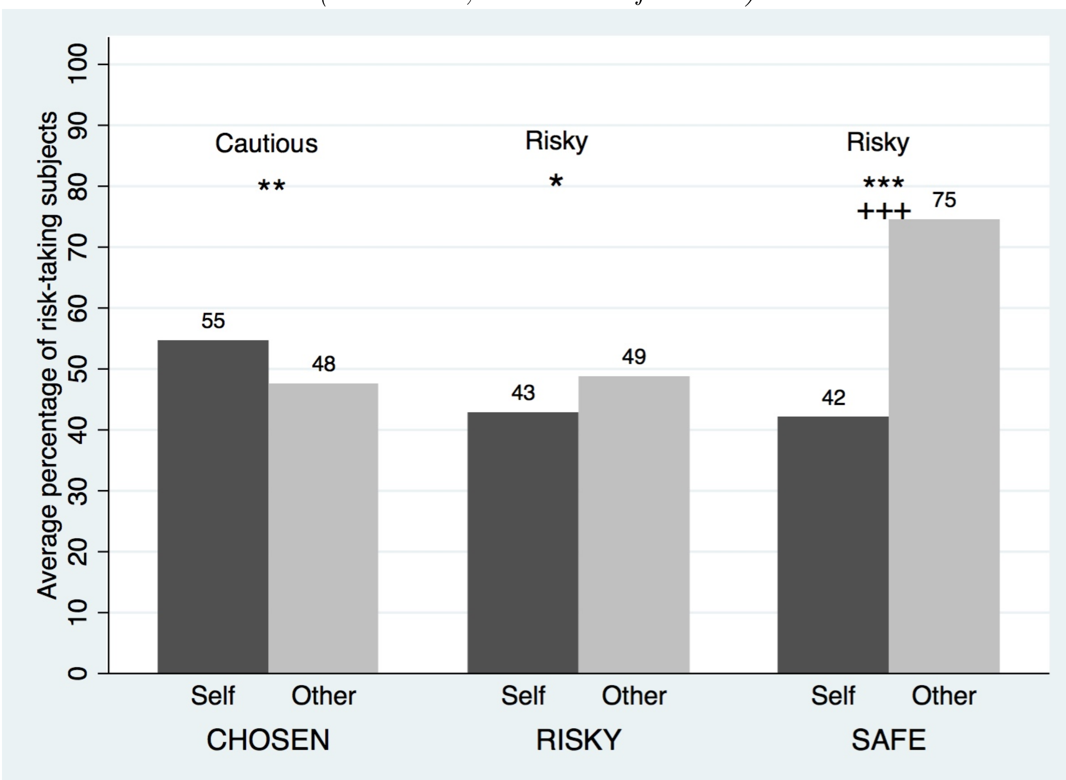
Feedback Timing	Decision for	Feedback Content	Session	Subjects	Total
IMMEDIATE	SELF	SAFE	1	12	30
			2	18	
		CHOSEN	1	18	28
			2	10	
		RISKY	1	25	25
	OTHER	SAFE	1	18	44
			2	26	
		CHOSEN	1	18	58
			2	16	
			3	24	
		RISKY	1	28	48
			2	20	
AT THE END	SELF	SAFE	1	7	29
			2	7	
			3	15	
		CHOSEN	1	6	31
			2	8	
			3	17	
		RISKY	1	16	32
			2	16	
	OTHER	SAFE	1	28	58
			2	30	
		CHOSEN	1	30	60
			2	30	
		RISKY	1	30	54
			2	24	
Total			26	497	497

5.2 Immediate Feedback

The self-other gap in risk-taking

Graph 1

The average fraction of subjects who choose the risky project (all rounds, immediate feedback)



* significant at the 10% level
** significant at the 5% level
*** significant at the 1% level
\+ significant at the 10% level (clustering)
++ significant at the 5% level (clustering)
+++ significant at the 1% level (clustering)

Each bar in Graph 1 shows the percentage of subjects choosing the risky project in each treatment where subjects get feedback immediately. The horizontal axis shows the name of the treatment. I average the percentages across 10 rounds. For example, in the CHOSEN treatment, on average 55% of subjects who choose for themselves

(Self, dark gray bar) and 48% of subjects who choose for another subject (Other, light gray bar) select the risky project. The asterisks and crosses above the bars show the significance of the self-other gap in risk-taking. The asterisks show the significance estimated by the Mann-Whitney U-test. Significance levels and the corresponding numbers of asterisks are shown below the graph. No asterisk means that the difference between the bars is not significant at 10% level.[5] In addition to the Mann-Whitney U-test, I also compute Somers' D (or Kendall's τ), a rank-order statistic that is similar to the Mann-Whitney statistic that allows for clustering.[6] Since I observe 10 rounds of decisions from each individual, the data are likely to be clustered at the individual level. The crosses show the significance of the self-other gap estimated by this second test. No cross means that the self-other gap is not significant at the 10% level. In this section, I discuss both the results from the Mann-Whitney U-test and the Somers' D test. However, in other sections, I focus on the conclusions from Somers' D test to be more conservative and concise.

In the CHOSEN treatment, neither project is subject to a counterfactual evaluation. Subjects in this treatment make an insignificant cautious shift in risk-taking. People are less likely to take a risk when choosing for others than for themselves (55% vs. 48%), but the difference is not significant at the 10% level when accounting for clustering. In contrast, when either project is subject to a counterfactual evaluation (SAFE and RISKY treatments), subjects make a risky shift (42% vs. 75% in SAFE treatment; 43% vs. 49% in RISKY treatment). The risky shift in SAFE treatment remains significant at 1% level when accounting for clustering.

The self-other gaps observed here help us understand the mixed results in the previous delegated decision-making experiments. In a previous experiment in which subjects learn only the outcome of the chosen option immediately after each decision, subjects make a cautious shift (Reynolds et al. 2009). On the other hand, in experiments where subjects also learn the outcome of the unchosen option, they make a risky shift (Pollmann et al. 2014), or no significant shift in risk-taking (Eriksen & Kvaløy 2010).

The result from the experiment demonstrates that varying the feedback content can sway subjects' risk preference in experiments. In particular, whether people make a cautious or a risky shift in delegated decisions crucially depends on what they learn about the outcomes of the chosen and the unchosen alternatives. Even though many studies have shown that feedback content can affect risk attitude in

[5] Given that we are testing one-sided alternative hypotheses in this experiment, the comparison is based on the one-sided p-value of the test. I compute the p-value by dividing the p-value of the usual-two sided test by two.

[6] Without clustering, Kendall's τ is equal to Mann-Whitney.

individual decision-making (Loomes & Sugden 1981; Zeelenberg et al. 1996; Ritov 1996), studies on delegated decision-making have mostly neglected the feedback in the decision-making environment.

What can the experimental results suggest on the delegated decision making under risks in the real world? As an example, consider the empirical evidence that physicians tend to make a cautious shift when they choose a procedure for their patients (Garcia-Retamero & Galesic (2012); Ubel et al. (2011)). If physicians respond to feedback in a similar way to the laboratory subjects, the cautious shift hinges on the assumption that patients will not learn about the outcome of other procedures they could have chosen. What if patients start to learn the outcome of alternative procedures or medications from online forums or websites such as WebMD and Mayo Clinic?[7] My finding suggests that doctors might now prescribe risky procedures more often, possibly more often than they would prescribe for themselves.

Decisions for self

Graph 2 compares the risk-taking rates across three treatments in which subjects make decisions for themselves. When subjects choose for themselves, they are more likely to take risks if they do not learn the outcome of the unchosen project. The results are inconsistent with the prediction of regret aversion theory as posited by Loomes & Sugden (1981).

Loomes & Sugden (1981) constructed the regret aversion model which predicts how the counterfactual outcome feedback affects individuals' decisions under risks. In a sequence of studies, they develop a representation of an agent who incorporates rejoice and regret triggered by the knowledge of the counterfactual outcome into his expected utility.[8] They demonstrate that experimental subjects can exhibit preference reversals between two lotteries if they behave as regret averse agents.

The regret aversion model by Loomes and Sugden only considers an agent who always learns the outcome of both chosen unchosen options. Some subsequent studies make an additional assumption that the agent can experience regret or rejoice only if the agent learns the realized outcome of the unchosen option. Under this assumption, an option can be more attractive to a regret averse agent if the agent would not learn the counterfactual outcome after choosing that option. Zeelenberg et al. (1996) experimentally tests this prediction by comparing three treatments I use in my

[7] http://www.webmd.com; https://www.mayoclinic.org

[8] Loomes & Sugden (1982); Loomes & Sugden (1983); Loomes & Sugden (1986); Loomes (1987); Loomes & Sugden (1987); Loomes (1988); Loomes, Starmer & Sugden (1991)

experiment as well: removing the counterfactual outcome feedback from the safe project (SAFE treatment), removing the counterfactual outcome feedback from the risky project (RISKY treatment), and removing the counterfactual outcome feedback from both projects (CHOSEN treatment). The decision problem stays the same across treatments: choosing between the risky and the safe project. Only the feedback on the counterfactual outcome differs across treatments. The regret aversion model makes the following predictions. In comparison to the safe project, the risky project would be the most attractive in the RISKY treatment, less so in the CHOSEN treatment, and the least attractive in the SAFE treatment. Zeelenberg et al. (1996) show that the fraction of subjects choosing the risky project is indeed consistent with this prediction.

In this experiment, subjects in the SAFE treatment take risks less often than the subjects in the CHOSEN treatment (55% vs. 43%), as seen in Zeelenberg et al. (1996). However, as opposed to Zeelenberg et al. (1996), subjects in the RISKY treatment also take risks less often than the subjects in the CHOSEN treatment as well (55% vs. 43%). Subjects respond to the feedback on the counterfactual outcome, but not exactly in a way predicted by the regret aversion model.

The discrepancy between the result of this experiment and that of Zeelenberg et al. (1996) is puzzling, given the close similarity of the decision problems used in the two studies. A critical difference between the two experiments is that Zeelenberg et al. (1996) uses a two-stage procedure. From each subject, the experimenter first elicits the payoff of the safe project (X in Table 5) that would make the subject indifferent between the safe project and the risky project. Then the subjects are presented with a feedback condition (SAFE or RISKY) and asked to choose between the two projects. This feature makes the experiment closer to a within-subject design since the subjects are exposed to two different feedback conditions in the elicitation stage and the project choice stage. The design might have a demand effect by inducing subjects to change their behavior when they face a new feedback condition they never thought about in the elicitation stage. The result from this study suggests that Zeelenberg et al. (1996) findings that are consistent with the regret aversion theory might be sensitive to details of the experimental procedure.

Table 4
Choice of Projects

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R(Risky)	130	.35
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S(Safe)	70	.65
	0	.35

Table 5
Choice of Projects in Zeelenberg (1996)

Projects	Payoff	Probability
R(Risky)	130	.35
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S(Safe)	X	.65
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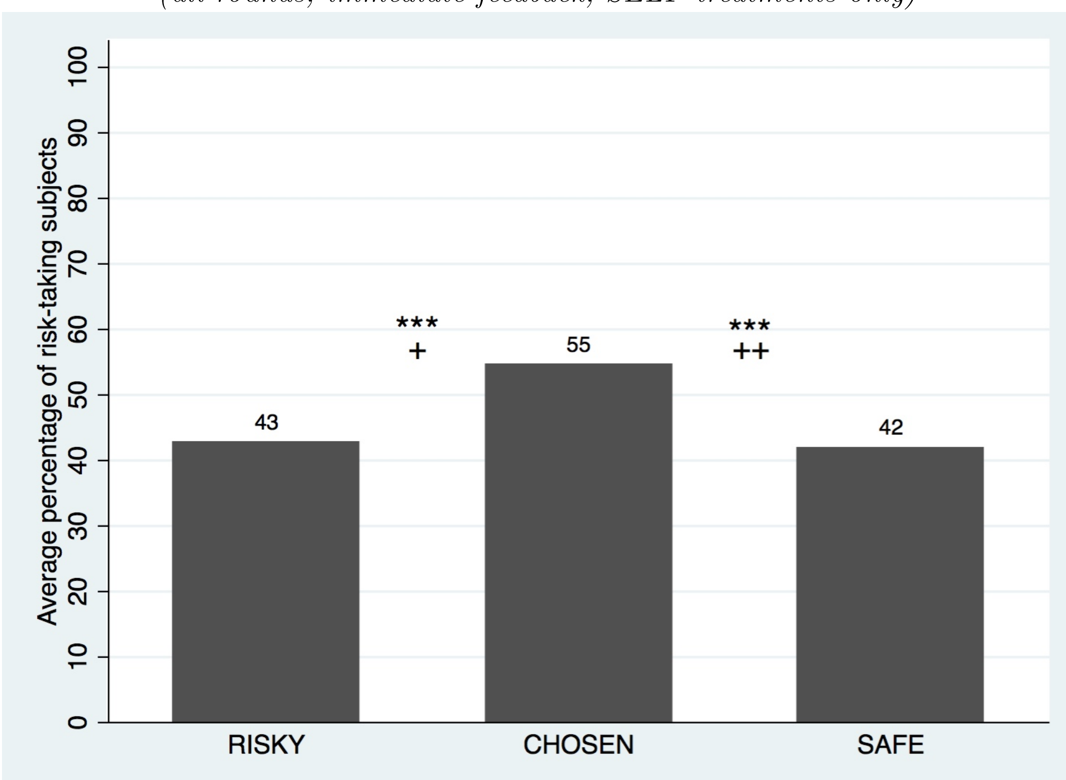
Graph 2
The average fraction of subjects who choose the risky project (all rounds, immediate feedback, SELF treatments only)



* significant at the 10% level
** significant at the 5% level
*** significant at the 1% level

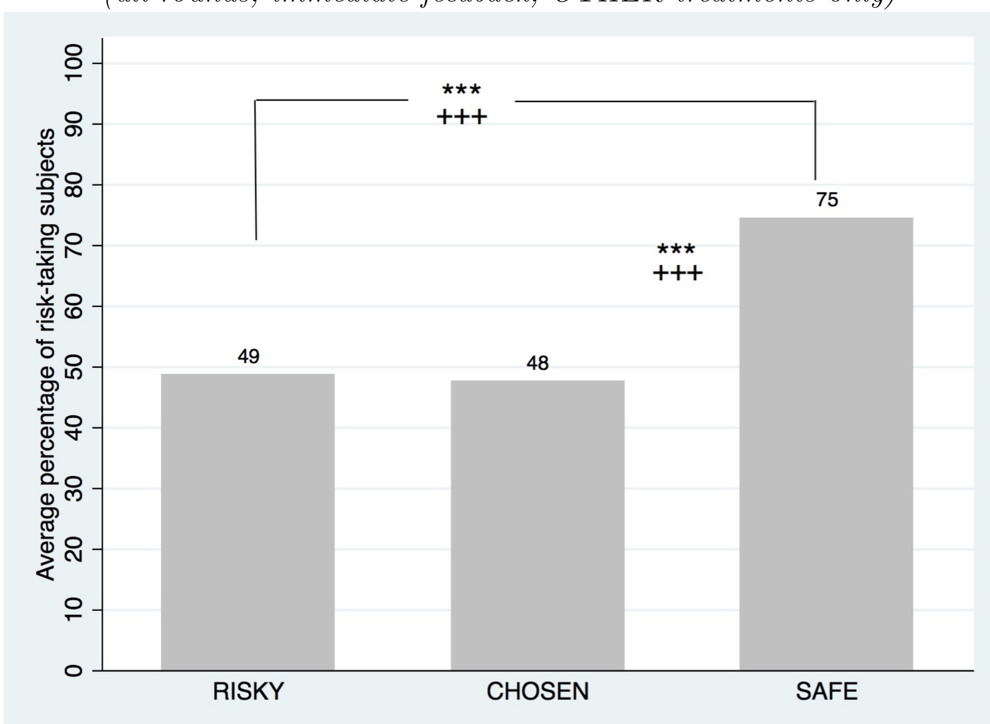
\+ significant at the 10% level (clustering)
++ significant at the 5% level (clustering)
+++ significant at the 1% level (clustering)

Decisions for others

Graph 3 displays the average percentage of risk-taking subjects when they choose as an agent on behalf of an owner. The agent's choice is unincentivized: whereas the owner gets the payoff from the chosen project, the agent gets a fixed payoff that does not depend on his choice. The graph shows that anticipating feedback on the counterfactual outcome makes people weakly more risk-taking when they choose on behalf of others (OTHER treatment). This result contrasts to the one in the SELF treatment where anticipating counterfactual outcome feedback makes people more cautious. In OTHER treatments, subjects respond asymmetrically in RISKY and SAFE treatments. Subjects are far less risk-averse in the SAFE treatment than in the CHOSEN treatment (75% vs. 48%), while they are just as risk-averse in the RISKY treatment as in the CHOSEN treatment (49% vs. 48%).

Graph 3

The average fraction of subjects who choose the risky project
(all rounds, immediate feedback, OTHER treatments only)



* significant at the 10% level
** significant at the 5% level
*** significant at the 1% level

+ significant at the 10% level (clustering)
++ significant at the 5% level (clustering)
+++ significant at the 1% level (clustering)

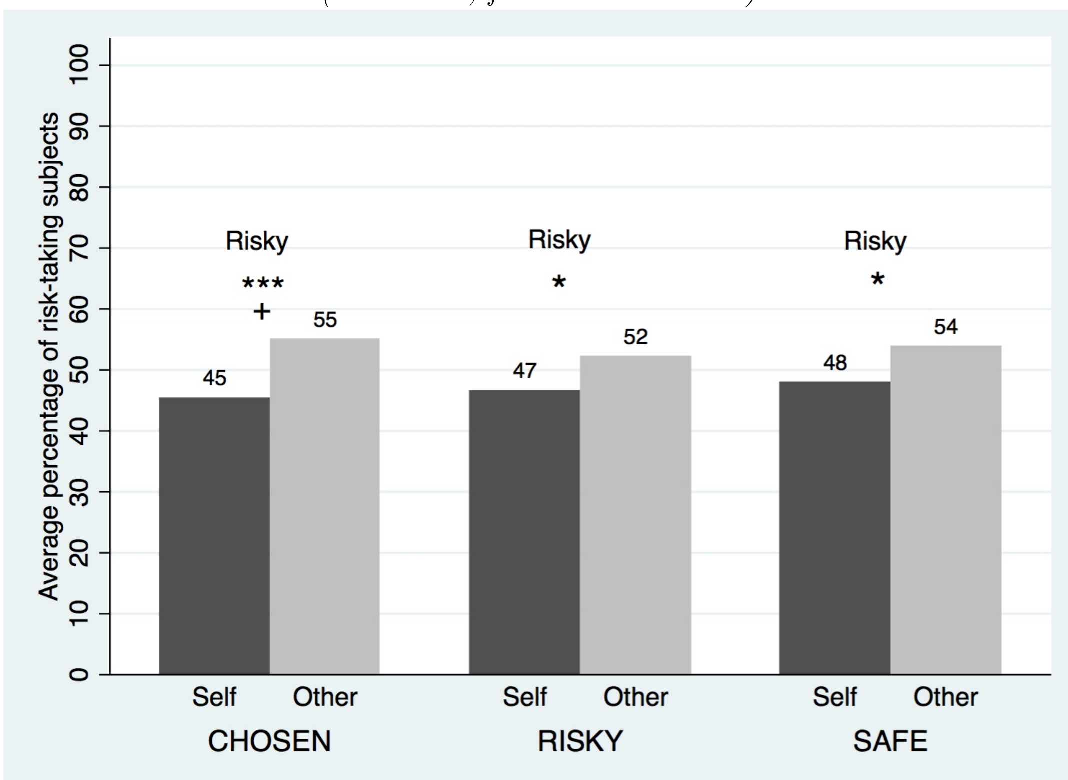
5.3 Feedback at the end

Comparison between decisions for self and decisions for others

Now we focus on the treatments in which subjects receive the feedback only after they make all ten rounds of decisions. Graph 4 shows the percentage of subjects choosing the risky project in each treatment.

Graph 4

The average fraction of subjects who choose the risky project (all rounds, feedback at the end)



* significant at the 10% level
** significant at the 5% level
*** significant at the 1% level
\+ significant at the 10% level (clustering)
++ significant at the 5% level (clustering)
+++ significant at the 1% level (clustering)

When the feedback is given at the end, subjects make a statistically significant risky shift only in the CHOSEN treatment.[9] Subjects do not show a self-other gap that is

[9] p-value=0.098, Somers' D test

significant at 10% level when a project faces a counterfactual evaluation. (RISKY and SAFE treatments)[10]

The results here show that the self-other gap in risk-taking depends on not only the content but also the timing of feedback. They also help us understand the contradictory findings in the previous studies. Table 2 replicated below summarizes the self-other gaps found in all treatments so far. Risky in the entry means that subjects make a risky shift in the corresponding treatment. None means that they did not show a significant self-other gap. Table 1 categorizes the seven previous studies previously introduced in Table 3 by the feedback content and timing in the experiments. The self-other gap found in each study is color-coded. The black shade means that the corresponding study finds a risky shift – subjects take greater risks for others than for themselves. The lighter gray shade means that the study finds a cautious shift – subjects take smaller risks for others than for themselves. No shade means that the study does not find a self-other gap significant at 10%.

Tables 1 and 2 show that my experiment finds risky shifts under the same feedback content/timing conditions under which other experiments found risky shifts (Charavarty et al. 2011; Pollmann et al. 2014). Under the other feedback content/timing conditions, other studies find cautious shifts whereas my experiment finds no significant self-other gap (Füllbrunn & Luhan 2015; Eriksen & Kvaløy 2010; Reynolds et al. 2009). The self-other gaps found in my study only weakly match those of previous studies. However, the results show that the counterfactual evaluation can have an opposite effect on the self-other gap in risk-taking depending on when the feedback is given.

Table 2

Summary of experiment results in this study

Content \ Timing	Immediate	At the end
No counterfactual evaluation	None	Risky
Counterfactual evaluation	Risky	None

[10]p-value=0.174 and 0.241 respectively, Somers' D test

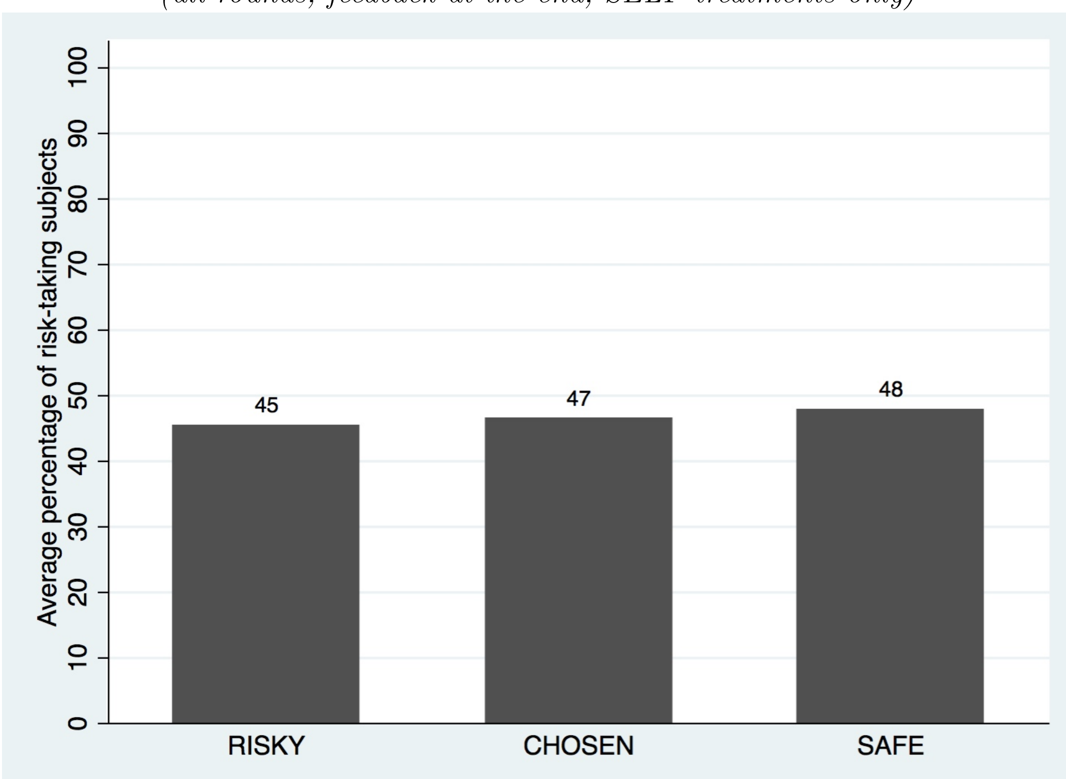
Table 1
Summary of previous experiments

Content \ Timing	Immediate	At the end
No Counterfactual Evaluation	Reynolds et al.(2009)	Chakravarty et al.(2011) Eriksen et al.(2017)
Counterfactual Evaluation	Pollmann et al.(2014) Eriksen & Kvaløy(2010)	Füllbrunn & Luhan(2015) Eriksen & Kvaløy(2010)

Decisions for self

As shown in Graphs 5 below, the subjects' risk-taking behavior is not affected by the feedback on the counterfactual outcome. None of the differences is significant at the 10% level.

Graph 5
The average fraction of subjects who choose the risky project
(all rounds, feedback at the end, SELF treatments only)

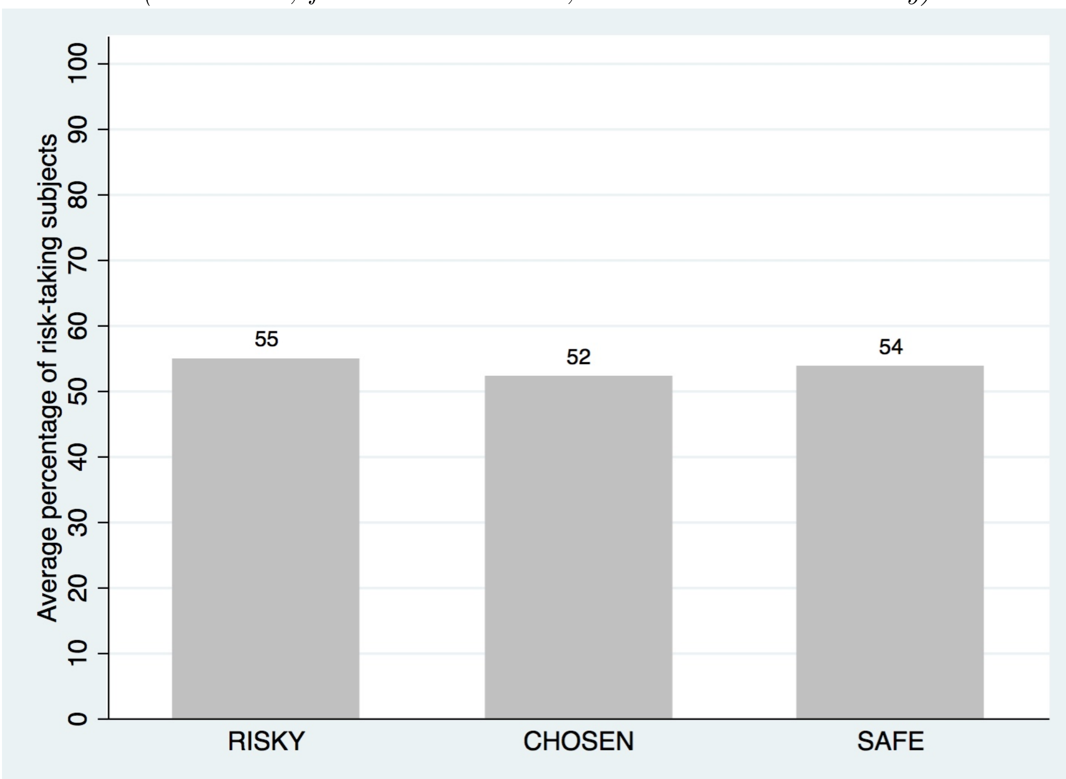


Decisions for others

Subjects making decisions for others also do not respond to the feedback on the counterfactual outcome when the feedback is given only at the end.

Graph 6

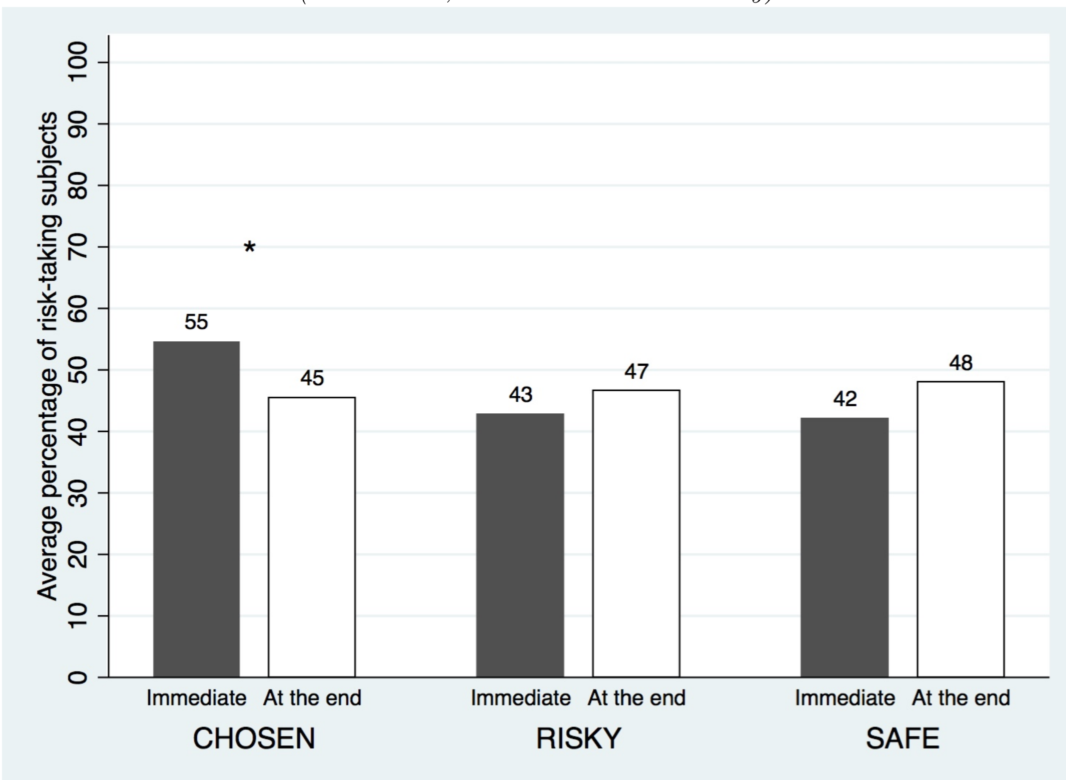
The average fraction of subjects who choose the risky project (all rounds, feedback at the end, OTHER treatments only)



5.4 Timing of Feedback

Now we take a look at how feedback timing affects risk-taking behavior when subjects make decisions for themselves. In CHOSEN treatment, subjects are more risk-averse when the feedback is given only at the end, although the difference is not significant at 10% level when controlling for the clustering at the individual level. Subjects in SAFE and RISKY treatments show somewhat smaller risk aversion when the feedback is given at the end, but again the differences are not significant.

Graph 7
The average fraction of subjects who choose the risky project (all rounds, SELF treatments only)



* significant at the 5% level
** significant at the 1% level
*** significant at the 0.1% level
+ significant at the 10% level (clustering)
++ significant at the 5% level (clustering)
+++ significant at the 1% level (clustering)

The most prominent model that connects feedback frequency and risk-taking behavior is the model of myopic loss aversion by Benartzi & Thaler (1995). Myopic loss aversion posits that infrequent feedback reduces loss-aversion (and therefore increases risk-taking) if agents evaluate their decisions by aggregating the gains and losses between feedback periods. Benartzi & Thaler (1995) and the subsequent experiment by Thaler et al. (1997)'s consider a decision-making environment that is similar to the RISKY and SAFE treatments in my experiment: the subjects learn the outcome of their unchosen options. For example, the portfolio managers, a key example in the myopic loss aversion, can monitor the returns of not only their chosen assets but also the unchosen assets. I want to note that my experiment and theirs are not directly comparable. In my experiment, subjects are paid only for one randomly selected round. If the subjects learn the outcome after each round, the feedback from a given round is payoff-relevant with only one-tenth probability. On the other hand, Benartzi & Thaler's study considers agents whose payoff accumulates every round. Therefore any feedback subjects get is relevant to their final payoff. With this difference in mind, notice that subjects in SAFE and RISKY treatments are slightly more risk-taking when the feedback is infrequent, although the difference is not statistically significant at 10% level. This result is weakly consistent with the findings by Benartzi and Thaler(1995). The result from Eriksen and Kvaløy (2010)'s experiment echoes this finding.

5.5 Gender difference in risk-taking

The gender difference in risk-taking behavior has been extensively studied. As Niderle (2014) summarizes in her survey, some previous experimental studies find no gender difference, whereas other studies find women being more risk-averse than men. The found differences are often small and heavily dependent on elicitation methods. In this section, I briefly discuss the gender difference in risk-taking behavior observed in this experiment.

I collected the gender information as a part of the subject recruitment process. When subjects create a user account on the recruitment system, they self-report their gender together with other personal information such as date of birth, major, last degree earned, email address and phone number. Then subjects sign up for experiments they are eligible to participate. The gender information is never brought up to the subjects again before, during and after this experiment. All participants in this experiment report themselves to be either a male or a female. Table 8 shows the number and the percentage of female subjects in each treatment. Given the small sample size within each gender group, the results to be discussed should be taken only as suggestive.

The experimental data support the previous findings that women are in general more risk-averse than men. Graphs 8 and 9 show that female subjects are never more likely than the male subjects to choose the risky project in any treatment (at 10% significance level). Female subjects are significantly less likely to choose the risky project in some, but not all treatments. When we aggregate the data to IMMEDIATE treatments and to AT THE END treatments, female subjects are less likely than the male subjects to choose the risky project in both IMMEDIATE and AT THE END treatments (at the 10% significance level).

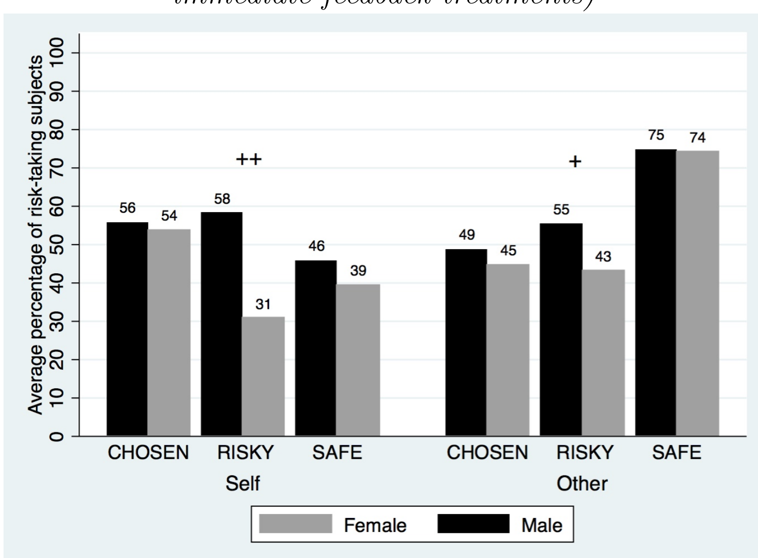
Table 8

Number and percentage of female subjects by treatment

Feedback timing	Decision for	Feedback contents	Subjects	# of female subjects	% of female subjects
IMMEDIATE	SELF	SAFE	30	18	60
		CHOSEN	28	16	57
		RISKY	25	14	56
	OTHER	SAFE	22	10	45
		CHOSEN	29	9	31
		RISKY	24	13	54
AT THE END	SELF	SAFE	29	20	69
		CHOSEN	31	17	55
		RISKY	32	15	47
	OTHER	SAFE	29	14	48
		CHOSEN	30	12	40
		RISKY	27	12	44
Total			336	179	51

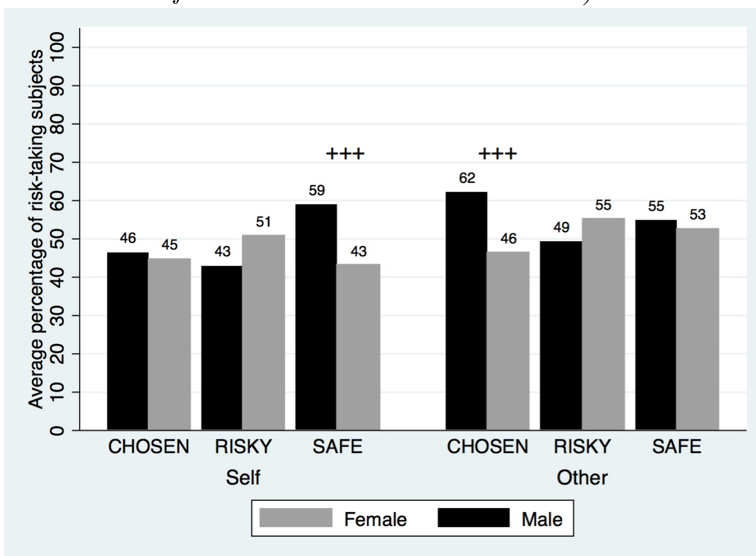
Graph 8

The average fraction of subjects who choose the risky project, by gender (all rounds, immediate feedback treatments)



Graph 9

The average fraction of subjects who choose the risky project, by gender (all rounds, feedback at the end treatments)



6 Model

Here I present a model that leads to predictions that are consistent with the key findings so far. In this model, an agent is assumed to derive utility from two aspects of the projects: the monetary payoffs and the psychological payoffs. Anticipated regret or blame is assumed to lower the psychological payoff. The agent's utility function is simply a weighted sum of the monetary and the psychological payoffs. I add two key assumptions to generate predictions that are consistent with findings from the experiment: (1) People are altruistic, but they care more about their own utility than about others'. (2) People are more impatient with their own utility than with others'.

Let U_R denote the utility derived from the monetary payoff of the risky project; U_S denote the utility derived from the monetary payoff of the safe project. Similarly, let V_R denote the utility derived from the psychological payoff of the risky project, and let V_S denote the utility derived from the psychological payoff of the safe project. Also, suppose that the agent weights the monetary payoff and the psychological payoff differently when deciding for self and when deciding for others. When the agent decides for self, the monetary payoff has a weight of $\alpha_1 > 0$, and the psychological payoff has a weight of $\alpha_2 > 0$. When the agent decides for others, the monetary payoff has a weight of $\beta_1 > 0$, and the psychological payoff has a weight of $\beta_2 > 0$. The positive weights β_1 and β_2 imply that the agent is altruistic in both monetary and psychological dimensions. Suppose that the agent is altruistic, but he cares more about his own monetary and psychological payoffs than about the payoffs to the other person for whom they make the decisions. That is, $\alpha_1 > \beta_1$ and $\alpha_2 > \beta_2$.

6.1 Immediate feedback

For now, focus on IMMEDIATE feedback treatments where the agent gets feedback right after each decision. Later we introduce a time-discounted version of this model for AT THE END feedback treatments. Let $W_{self}(X)$ denote the utility the agent gets when he chooses project X for himself. Similarly, let W_{other} denote the utility the agent gets when he chooses project X for another person. Then the utility functions W_{self} and W_{other} can be written as follows:

$$W_{self}(X) = \alpha_1 U_X + \alpha_2 V_X$$
$$W_{other}(X) = \beta_1 U_X + \beta_2 V_X$$

where $X \in \{R, S\}$.
The psychological payoff V_X represents the anticipated regret or blame that results from a counterfactual evaluation. Here I make an important assumption that the regret or blame is relevant only when the agents receive some feedback on the counterfactual outcome. In particular, if they receive no feedback on the counterfactual outcome, an agent never anticipates regret or blame. That is, $V_R = 0$ and $V_S = 0$ in CHOSEN treatments. If counterfactual feedback is given, the agent anticipates potential regret or blame. The agent feels regret if he decides for himself and feels blamed if they decide for another person. Anticipated regret and blame undermine the utility derived from the chosen project ($V_R \leq 0$ and $V_S \leq 0$). To accommodate the findings from the experiment, I make an additional assumption that the agent anticipates greater psychological disutility from the risky project than from the safe project ($V_R < V_S \leq 0$).

For example, if an agent cares only about the probability of regret/blame in each project, such preference might induce the additional assumption $V_R < V_S$. Suppose that you feel regret or blame when your chosen project fails, but the regret/blame disappears if you also learn that the unchosen option did not do any better. That is, an agent whose chosen project fails would regret if he does not learn the outcome of the unchosen project. If he learns the outcome of the unchosen project, he feels regret/blame only if the unchosen project strictly outperformed the chosen one. In this case, under the SAFE treatment, the risky project induces regret/blame with a probability $(0.65)(0.65) = 0.4225$, whereas the safe project induces regret/blame with a probability 0.35. Under the RISKY treatment, the risky project induces regret/blame with a probability 0.65, whereas the safe project induces regret/blame with a probability $(0.35)(0.35) = 0.1275$. In either treatment, the risky project induces regret/blame with a higher probability.[11]

Now suppose that an agent is more likely to choose the risky project if the advantage the risky option has over the safe option increases. The size of the risky project's advantage is defined as the difference between the utilities from the two projects:

$$Adv^R_{self} = W_{self}(R) - W_{self}(S) = \alpha_1(U_R - U_S) + \alpha_2(V_R - V_S)$$
$$Adv^R_{other} = W_{other}(R) - W_{other}(S) = \beta_1(U_R - U_S) + \beta_2(V_R - V_S)$$

Without counterfactual feedback, the psychological payoffs are never activated and thus the second terms $\alpha_2(V_R - V_S)$ and $\beta_2(V_R - V_S)$ are zero. Since we assumed $\alpha_1 > \beta_1$, additionally assuming $U_R \geq U_S$ would imply $Adv^R_{self} \geq Adv^R_{other}$. The agent is less likely to take the risky project when deciding for others, resulting in a cautious or no shift in risk-taking. To support the additional assumption $U_R \geq U_S$, we look for evidence in the data. Subjects could be heterogeneous in their risk preference in monetary payoffs, so the assumption might hold for some agents and not for others. 55% of subjects choose the risky project when choosing for themselves in the CHOSEN treatment, which suggests that $U_R \geq U_S$ holds for a significant fraction of subjects.

Unlike in treatments without counterfactual feedback, we assume $V_R < V_S$ in treatments with counterfactual evaluations, which implies that there will be less risk-taking when counterfactual evaluations are available. This prediction holds when subjects choose for themselves, although it does not hold when subjects choose for others. The assumption $\alpha_2 > \beta_2$ further predicts that the risky project's advantage is more adversely affected by the psychological disutility when the agent makes a decision for self than for others. If the relative magnitudes of $(V_R - V_S)$ or α_2/β_2 are sufficiently

[11] An alternative assumption would be that an agent who does not learn the outcome of the unchosen project uses the probability of success for each project to compute an "imagined" probability of regret/blame. For example, an agent who chooses the safe project never learns the outcome of the risky project, but he can "imagine" that the risky project succeeds with probability 0.35, so his imagined probability of regret/blame is $(0.35)(0.35) = 0.1275$. This assumption does not change the fact that the risky project has a higher chance of regret/blame in both SAFE and RISKY treatments.

large, the psychological disutility could override the monetary utility, resulting in a risky shift. This prediction is consistent with the experimental results.

6.2 Feedback at the end

Now we move onto the treatments in which the feedback is provided only at the end of the experiment. Unlike in the immediate feedback treatments, subjects in these treatments learn the outcome of the projects after some time. As a simple way to model this, I assume that an agent time-discounts his utility from a decision if the feedback on the decision is provided only after some time. I also add another crucial assumption that an agent is more impatient with his own payoffs than with other people's payoffs. That is,

$$Adv^R_{self} = \delta[W_{self}(R) - W_{self}(S)] = \delta[\alpha_1(U_R - U_S) + \alpha_2(V_R - V_S)]$$
$$Adv^R_{other} = \gamma[W_{other}(R) - W_{other}(S)] = \gamma[\beta_1(U_R - U_S) + \beta_2(V_R - V_S)],$$

where $\delta < \gamma$. The assumption that people are more impatient with their own payoffs than with others' is empirically supported by Shapiro(2010) and Pronin et al. (2008)'s lab experiment results. If δ is sufficiently smaller than γ, $\delta\alpha_1 \leq \gamma\beta_1$ and $\delta\alpha_2 \leq \gamma\beta_2$ can hold. This last condition yields predictions that are opposite to the predictions made in the IMMEDIATE feedback treatments. First, agents make a risky shift when counterfactual feedback is not given, whereas they make a weakly cautious shift when counterfactual feedback is given. Second, without counterfactual evaluation, agents are less likely to choose the risky project when feedback is given at the end rather than immediately. Again, the experimental findings are consistent with the predictions.

7 Dynamic Analysis

7.1 The relationship between the past outcome and the present risk-taking

In this section, I report how the feedback on the past rounds' outcomes relates to the subjects' risk-taking behavior. The discussion is limited to the IMMEDIATE feedback treatments. AT THE END feedback treatments are irrelevant to this discussion because they provide no feedback until all decisions are made. Table 9 shows the results of probit regressions. The regressions estimate the relationship between the observed outcome in the previous rounds and the probability of choosing the risky project in the current round. Columns (1), (2), and (3) each consists of three sub-columns. The sub-columns labeled SELF show the results from treatments in which subjects make decisions for themselves. The sub-columns labeled OTHER show the results from treatments in which subjects make decisions for others. The sub-columns labeled Pooled show the results from both SELF and OTHER treatments. Standard errors are shown in brackets below the coefficients. The asterisks denote the significance level of the estimation: * means significance at 10% level, ** means significance at 5% level, and *** means significance at 1% level.

Column (1) shows the result of estimation with only two variables: risky success and safe success. Risky success means the risky project's observed rate of success in all previous rounds. Safe success means the safe project's observed rate of success in all previous rounds. Although each project's theoretical probability of success is known to subjects, the observed rate of success can significantly differ from the theoretical probability for two reasons. First, subjects have a limited number of observations since they choose only for ten rounds. Second, the number of observed outcomes can be further limited in treatments where subjects do not observe the outcome of the unchosen project. The regression shows that subjects who observed a high rate of success in the risky project in previous rounds are more likely to choose the risky project in the current round. Subjects who observed a high rate of success in the safe project in previous rounds are also more likely to choose the safe project.

Column(2) adds variables that are relevant to the counterfactual evaluation. For brevity, I use the terms regret and rejoice for both SELF and OTHER treatments. I say a subject experiences regret if he learns from the feedback that his chosen project failed and his unchosen project succeeded. Analogously, a subject experiences rejoice if he learns from the feedback that his chosen project succeeded and his unchosen project failed. For each subject, the variable *risky regret* denotes the fraction of previous risky choices that resulted in regret. The variable *risky rejoice* denotes the fraction of previous safe choices that resulted in rejoice. Analogous definitions apply to *safe regret* and *safe rejoice.*

Column(2) shows that not only rejoice, but also regret experienced from past safe choices predict a lower rate of risk-taking. That is, subjects behave consistently with reinforcement learning only with regard to rejoice. Subjects choosing a project for themselves respond to different variables compared to subjects choosing for others. For example, subjects choosing for others are more likely to take risks after experiencing a higher rate of success in previous rounds, whereas subjects choosing for themselves do not respond to the past success rates. Subjects choosing for others are also more

Table 9

Probit: Probability of choosing the risky project

Variable	(1) SELF	(1) OTHER	(1) Pooled	(2) SELF	(2) OTHER	(2) Pooled	(3) SELF	(3) OTHER	(3) Pooled
risky success	.299*	.473**	.364***	.290	1.657***	.858**	-.043	1.044	.285
	(.177)	(.205)	(.133)	(.541)	(.589)	(.375)	(.561)	(.682)	(.402)
safe success	-.687***	-.609***	-.671***	-.582	1.403**	.353	-.541	1.760**	.401
	(.199)	(.175)	(.131)	(.533)	(.593)	(.374)	(.562)	(.689)	(.398)
risky regret				.414	1.331***	.919***	.660	.787	.837**
				(.480)	(.502)	(.328)	(.515)	(.597)	(.363)
safe regret				-1.044***	-1.439***	-.964***	-.826**	-1.648***	-.870***
				(.399)	(.480)	(.290)	(.418)	(.554)	(.307)
risky rejoice				.745*	.295	.525*	1.03**	.148	.716**
				(.441)	(.467)	(.300)	(.473)	(.582)	(.338)
safe rejoice				-1.436***	-2.351***	-1.593***	-.735	-1.435**	-.830**
				(.418)	(.540)	(.302)	(.468)	(.599)	(.333)
mean risk-taking							2.036***	3.228***	2.681***
							(.567)	(.618)	(.393)
constant	.245	.337**	.312***	.703	-.994***	-.265	-.780	-2.589***	-1.791***
	(.151)	(.140)	(.101)	(.430)	(.384)	(.262)	(.612)	(.556)	(.371)
Observations	609	610	1,219	188	181	369	188	181	369
LR χ^2	14.09	18.36	33.87	37.85	53.70	72.35	51.20	86.86	77.29
Prob > χ^2	.001	.000	.000	.000	.000	.000	.000	.000	.000
Pseudo R^2	.017	.022	.020	.145	.214	.141	.197	.346	.151

* significant at 10% level; ** significant at 5% level; ***significant at 1% level

likely to take risks after experiencing regret from risky choices. In contrast, subjects choosing for themselves are more likely to take risks after experiencing rejoice from risky choices.

Column(3) adds a control variable mean risk-taking. As a proxy for a subject's general risk preference, the variable summarizes the fraction of risky choices the subject makes out of ten choices.[12] This addition of the new control variable is motivated by the observation that many subjects have a strong preference for one project over the other. A significant number of subjects choose one project for nine or ten rounds out of ten rounds. Table 10 below shows the fraction of such subjects in each treatment. Column (3) in Table 9 shows that the significance of past outcomes somewhat fades when the regression includes this new variable. In particular, the observed success rates are no longer significant while the regret and rejoice from risky choices remain significant. The general risk preference of a subject turns out to be a strong predictor of choices.

Table 10

The fraction of subjects who choose one project for 9 or 10 rounds

	SELF	OTHER
SAFE	$\frac{10}{30}$	$\frac{7}{22}$
CHOSEN	$\frac{6}{28}$	$\frac{11}{29}$
RISKY	$\frac{14}{25}$	$\frac{7}{24}$

The recency effect, a phenomenon that has long been documented in previous studies (Estes, 1964; Lee, 1971), means that experience affects current decisions more strongly if it was from the recent past than from the far past. To test if the general experience, not only the recent experience shape the current decision, I add last-round outcome variables to the regression. Table 11 reports the result. The estimation pools observations from both SELF and OTHER treatments. Column (4) in Table 11 adds a variable that indicates whether the subject chose the risky project in the last round. Column(5) also adds indicators for observed success in the last round. Column(6)

[12]An alternative proxy is the fraction of risky choices out of all choices but the current one. The probit regressions using this alternative proxy yield qualitatively similar results with all estimates having the same signs. The significance of the results is stronger when using the alternative proxy.

further adds variables that indicate whether the subject experienced regret or rejoice in the last round. Columns (4), (5), and (6) show that all significant results previously seen in Table 9 remain significant and the estimated coefficients preserve the signs. None of the variables newly added in columns (5) and (6) are significant at the 10% level, except for the regret from the risky project in the last round positively correlating with a higher chance of risk-taking in the current round.

Unfortunately, the analysis reported in Tables 9 and 11 potentially suffers a sampling bias due to missing values. The definitions of success, regret and rejoice rates used here inevitably generate many missing values in the data. For example, for a subject who never chooses the risky project, the observed rate of success for the risky project cannot be defined. As one way of addressing this issue, I replace the missing values with theoretical probability. In other words, when a subject has no previous observation from either the risky or the safe project, I assume that the subject uses the project's theoretical probability of success, regret and rejoice as if the theoretical probability is the observed rate. Table 12 shows the result of the probit regression performed under this new assumption. The table shows that under the new assumption, the past success rates of projects regain their significance whereas the past regret and rejoice rates somewhat lose their significance. An alternative way to impute missing values is to assume a zero rate of success, regret or rejoice for missing values. The estimation results are very similar to those reported in Table 12 and therefore are omitted.

Table 11

Probit: Probability of choosing the risky project

Variable	(4)	(5)	(6)
risky success	.412	.452	.439
	(.414)	(.435)	(.463)
safe success	.383	.379	.424
	(.410)	(.424)	(.436)
risky regret	.863**	.849 **	.739**
	(.368)	(.371)	(.377)
safe regret	-.959***	-.974***	-1.169***
	(.313)	(.317)	(.334)
risky rejoice	.710**	.715**	.652*
	(.342)	(.344)	(.348)
safe rejoice	-1.023***	-1.007***	-1.160***
	(.345)	(.351)	(.365)
mean risk-taking	2.817***	2.823***	2.869***
	(.402)	(.404)	(.410)
chose risky last round	-.486***	-.449*	-.632 *
	(.159)	(.237)	(.323)
risky success last round		-.076	.296
		(.256)	(.317)
safe success last round		.015	-.491
		(.239)	(.424)
risky regret last round			.580*
			(.304)
safe regret last round			.647
			(.422)
risky rejoice last round			(omitted)
safe rejoice last round			.781
			(.499)
constant	-1.583***	-1.607***	-1.638***
	(.378)	(.388)	(.414)
Observations	369	369	369
LR χ^2	134.45	134.54	140.86
Prob $> \chi^2$	0.000	0.000	0.000
Pseudo R^2	0.263	0.263	0.275

* significant at 10% level; ** significant at 5% level; ***significant at 1% level

Table 12

Probit: Probability of choosing the risky project; missing values for risky success and safe success filled in

Variable	(7)	(8)	(9)
risky success	.212	.467***	.467***
	(.153)	(.154)	(.161)
safe success	.123	-.255*	-.350**
	(.152)	(.147)	(.153)
risky regret	.067	.320 *	.315
	(.203)	(.182)	(.192)
safe regret	-.603***	-.849***	-.962***
	(.214)	(.198)	(.207)
risky rejoice	.333*	.598***	.544***
	(.196)	(.176)	(.189)
safe rejoice	-.217	-.303*	-.414**
	(.199)	(.179)	(.186)
mean risk-taking	3.471***	1.506***	1.500***
	(.179)	(.148)	(.149)
chose risky last round	.032	.118	.005
	(.087)	(.133)	(.146)
risky success last round		-.253**	-.356*
		(.123)	(.188)
safe success last round		.038	.107
		(.119)	(.139)
risky regret last round			.291**
			(.135)
safe regret last round			.029
			(.149)
risky rejoice last round			.357*
			(.200)
safe rejoice last round			-.055
			(.146)
constant	-1.830***	-.755***	-.654***
	(.177)	(.153)	(.158)
Observations	1422	1422	1422
LR χ^2	703.81	318.73	326.67
Prob $> \chi^2$	0.000	0.000	0.000
Pseudo R^2	0.3571	0.162	0.166

* significant at 10% level; ** significant at 5% level; ***significant at 1% level

7.2 First-round risk-taking behavior

The subjects respond to the feedback content and timing in the first round just as they would do in other rounds. First, we can limit our attention to the IMMEDIATE feedback treatments and compare the risk-taking behavior in the first round with the risk-taking behavior in all rounds. Tables 13 and 14 in the appendix show the percentage of subjects choosing the risky project in the IMMEDIATE feedback treatments. Table 13 shows the percentage averaged across all ten rounds, while Table 14 shows the percentage from the first round only. The inequality signs in Tables 13 and 14 summarize the results of Mann-Whitney U- tests and Somers? D rank order tests with clustering. Stars and crosses show how significant the difference is respectively in Mann-Whitney rank sum tests and Somers' D rank order tests. The comparison between Tables 13 and 14 reveals that subjects' risk-taking behavior responds to the feedback content and timing similarly in the first round compared to in all rounds. For example, regardless of first or all rounds, subjects are the most likely to take the risk in the CHOSEN treatment when deciding for self whereas they are the most likely to take the risk in the SAFE treatment when deciding for others. However, the differences in risk-taking rates between treatments often turn out to be insignificant in the first round data because of the small sample size.

Next, we can focus on the AT THE END feedback treatments and compare the risk-taking behavior between the first round and all rounds. Tables 15 and 16 show the percentage of subjects choosing the risky project in AT THE END feedback treatments. Table 15 shows the percentage averaged across all ten rounds, whereas Table 16 shows the percentage from the first round only. Here the subjects' risk-taking behavior does not always respond to feedback content and timing in the same way in the first round as in all rounds. However, since the differences between treatments are mostly insignificant both in the first round and in all rounds, it is hard to conclude that subjects behave differently in the first round.[13]

7.3 Time trend in risk-taking behavior

I find no time trend in risk-taking behavior. Graphs 10 and 11 show the fraction of risk-taking subjects in each round, respectively in IMMEDIATE feedback treatments and in AT THE END feedback treatments. A regression analysis reveals that subjects are neither more or less likely to take risks over time.

[13] p-value=0.174 and 0.241 respectively, Somers' D test

Some other experiments find that the risk preference of subjects changes as they make more rounds of decisions. For example, Pollmann et al. (2014) find that subjects choosing for themselves are more likely to take risks in round 5 than in round 1, while subjects choosing for others are less likely to take risks in round 5 than in round 1. Eriksen & Kvaløy (2010) also find increasing risk-taking rates.

There are two key differences in my experimental design and theirs. The first difference is whether the agent-principal matches are fixed throughout the experiment. In my experiment, agents are randomly rematched in each round with a new principal (owner) they have never been matched with before. In Pollmann et al. (2014) and Eriksen & Kvaløy (2010), agent-principal matching is fixed throughout all rounds. The other difference is the payment scheme. In my experiment, only one randomly selected round is paid. In Pollmann et al. (2014) and Eriksen & Kvaløy (2010), the subjects are paid their accumulated payoff from all rounds. One possible explanation for the increasing risk-taking rate in their experiments is that subjects realize only after some rounds that they can diversify risks across multiple rounds. Such diversification is not applicable in my experiment.

8 Conclusion and Discussion

This study was motivated by the mixed results observed in the previous experiments on the self-other gap in risk-taking: some experiments found that people take greater risks when making decisions for others as opposed to themselves, while other experiments found the opposite. I found that these previous findings seemed to depend on the type the timing of the feedback subjects received on their decision outcomes and conducted an experiment to test this hypothesis.

My experiment found that people take greater risks for others as opposed to themselves, exactly under the conditions under which the previous experiments had similar findings—when the feedback is immediately given and allows counterfactual evaluation and when the feedback is given after a sequence of decisions and does not allow counterfactual evaluation. Under the other conditions—when the feedback is immediately given and does not allow counterfactual evaluation and when the feedback is given after a sequence of decisions and allows counterfactual evaluation—my experiment found no significant self-other gap in risk-taking, whereas the previous experiments found that people take smaller risks for others than for themselves.

Although my experiment replicated the risky shifts in risk-taking (taking greater risks for others than for oneself) observed in some previous experiments, it failed to replicate the cautious shifts (taking smaller risks for others than for oneself) found in the other experiments. It is possible that an alternative decision problem would bring out a stronger effect of the feedback and induce both the risky and the cautious shifts within the same decision problem. One might consider using a decision problem that is known to induce cautious shifts and see if varying the content or the timing of feedback reserves the cautious shifts to the risk shifts. Eriksen & Kvaløy (2010)'s experiment fits the description. However, they also find a disappearance, but not a reverse of the cautious shift, suggesting that it might be somewhat difficult to find a decision problem in which the feedback has an effect that is large enough to reverse the direction of the self-other gap in risk-taking.

Other findings in my experiment suggest that the feedback on counterfactual outcomes do affect risk-taking, but not exactly in the way that the regret aversion theory predicts. For example, when making decisions for themselves, subjects were equally likely to choose the risky option in the two treatments that made different lotteries regret-free, counter to the theoretical prediction of regret aversion. It is particularly puzzling given that a previous experiment made findings supportive of regret aversion, using a decision problem similar to the one used in my experiment. It seems that numerous factors we do not fully understand affect the risk preference and seemingly unimportant differences in experimental designs might lead to opposite results. My experiment showed that the type and the timing of feedback on decision outcomes, which have been largely neglected in the literature, might be another important determinant of people's risk-taking behavior.

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A Experiment Instructions

Although the experiment consists of 12 treatments, only two representative instructions are presented here as examples. The instructions for the other treatments are written analogously and can be provided by the author at request.

A.1 Experiment Instructions for SELF - IMMEDIATE - CHOSEN treatment

Welcome

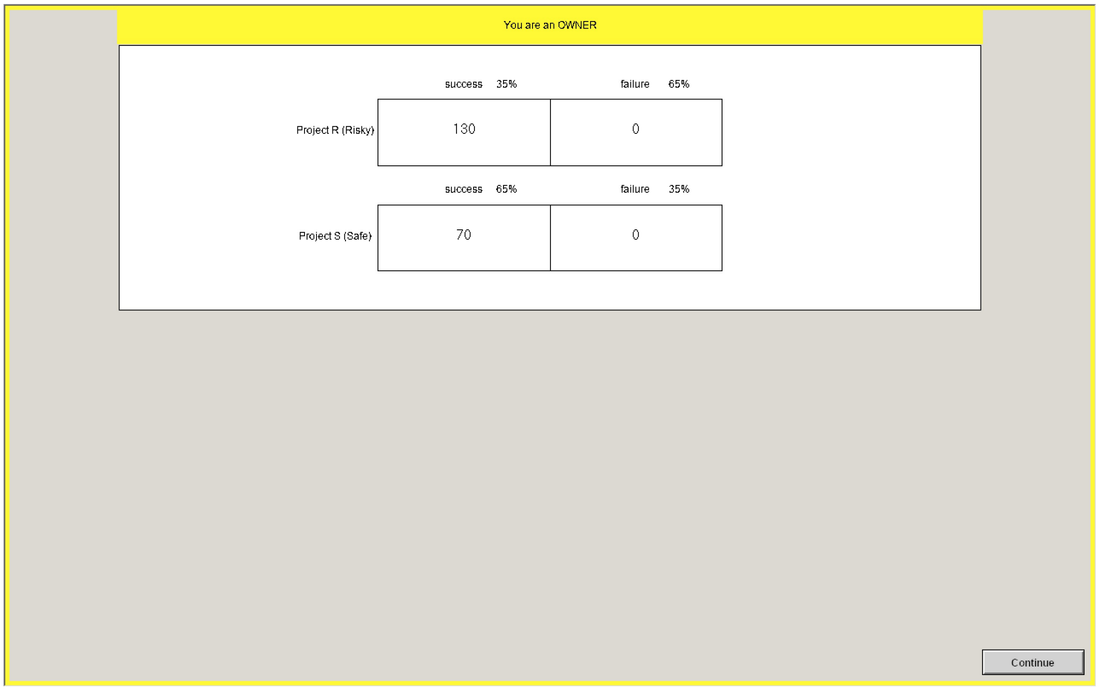
You are about to participate in an experiment on decision-making, and you will be paid for your participation with cash vouchers, privately at the end of the session. What you earn depends partly on your decisions, partly on other people's decisions, and partly on chance.

The entire session will take place through computer terminals. Please turn off pagers and cellular phones now. We will start with a brief instruction period. If you have any questions during this period, raise your hand and your question will be answered so everyone can hear.

This experiment consists of two parts. The instruction for the first part is given below. The instruction for the second part will be given to you after you have completed Part 1. At the end of the experiment, one of the two parts will be randomly selected for your payment. Each decision you take throughout the experiment will be equally likely to be selected for your payment.

Instructions for Part 1

In this part, you will be assigned a role of an owner or a role of an agent of a company. The role assignment is completely random. In the beginning, each owner will be randomly matched with one agent. The agent's task is to choose a project between the two available projects on behalf of the owner.



Both the owner and the agent will see the available projects as in the screen above. The screen shows your role (either an owner or an agent) and two available projects R(Risky) and S(safe). The table summarizes possible profits from the two projects and probability of each of them. For example, project R(Risky) is successful with 35% chance and fails with 65% chance. Project R yields 130 points of profit if the project is successful, and 0 point if it fails. Project S(Safe) is successful with 65% chance and fails with 35% chance. Project S yields 70 points if it is successful and 0 point if it fails. The outcomes of two projects are independent, i.e. it is as if we throw a 100-sided die and project R is successful only if we get 1 to 35, and we throw another 100-sided die and project S is successful only if we get 1 to 65.

If you are assigned a role of an agent:
Your task is to choose one of the two projects on behalf of your owner. Once you choose a project, both you and your owner will see which project you chose and how much profit the chosen project yielded. The profit goes to the owner. You and the owner will NOT see the profit from the unchosen project.

If you are assigned a role of an owner:
You do not make any decision. However, once your agent chooses a project, you and your agent will see the same information about the agent's choice and the outcome of projects.

Payments
Both agents and owners get a showup fee of \$10. In addition, an agent gets a fixed amount of \$5 for his decision and the owner gets the profit from the chosen project. The profits are converted to a dollar amount at the rate of \$0.10/point. For example, if the profit is 70 points, the owner gets $70 \times \$0.1 = \$7$.

In summary:

- The agent chooses one of the two projects for the owner.
- The agent and the owner will NOT see the profit from the unchosen project.

Instructions for Part 2

Part 2 is essentially 9 repetitions of Part 1. Again as an owner, you will face a choice between the same two projects for 9 rounds. The same information as in Part 1 would be shown to you, once you choose a project.

At the end of the experiment, one of the two parts of the experiment will be randomly selected for the payment. If Part 2 is selected, one of the 9 decisions you made in the part would be relevant for your payment. All decisions you made in the experiments are equally likely to be selected for the payment. Your payment in each round of Part 2 will be determined the same way as in Part 1.

A.2 Experiment Instructions for OTHER - AT THE END - RISKY treatment

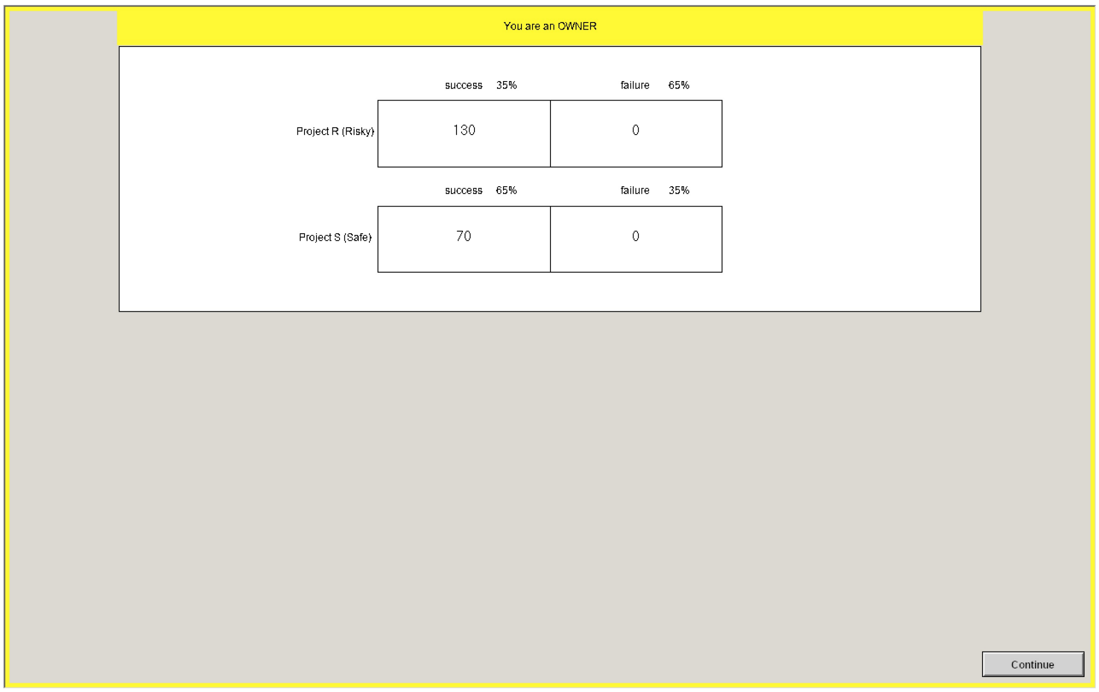
Welcome

You are about to participate in an experiment on decision-making, and you will be paid for your participation with cash vouchers, privately at the end of the session. What you earn depends partly on your decisions, partly on other people's decisions, and partly on chance.

The entire session will take place through computer terminals. Please turn off pagers and cellular phones now. We will start with a brief instruction period. If you have any questions during this period, raise your hand and your question will be answered so everyone can hear.

Instructions

In this experiment, you will be assigned a role of an owner or a role of an agent of a company. The role assignment is completely random. In the beginning, each owner will be randomly matched with one agent. The agent's task is to choose a project between the two available projects on behalf of the owner.



Both the owner and the agent will see the available projects as in the screen above. The screen shows your role (either an owner or an agent) and two available projects R(Risky) and S(safe). The table summarizes possible profits from the two projects and probability of each of them. For example, project R(Risky) is successful with 35% chance and fails with 65% chance. Project R yields 130 points of profit if the project is successful, and 0 point if it fails. Project S(Safe) is successful with 65% chance and fails with 35% chance. Project S yields 70 points if it is successful and 0 point if it fails. The outcomes of two projects are independent, i.e. it is as if we throw a 100-sided die and project R is successful only if we get 1 to 35, and we throw another 100-sided die and project S is successful only if we get 1 to 65.

<u>If you are assigned a role of an agent:</u>
Your task is to choose one of the two projects on behalf of your owner. You will face a choice between the same two projects for 10 rounds. In the beginning of each round,

each agent will be matched with a new owner he has never been matched with before. This means that each owner will be matched with a new agent he has never been matched with before.

Only after you make choices for all 10 rounds, one of the ten rounds will be randomly selected for the payment. Each round is equally likely to be selected. Then both you and your owner in the randomly selected round will see how much profit the chosen project yielded. The profit goes to the owner. You and the owner will NOT see the profit from the unchosen project in that round.

If you are assigned a role of an owner:
You do not make any decision. However, after all 10 rounds, you and your agent in the randomly selected round will see the same information about the agent's choice and the outcome of projects in that round.

Payments
Both agents and owners get a showup fee of \$10. In addition, an agent gets a fixed amount of \$5 for his decision and the owner gets the profit from the project chosen in the randomly selected round. The profits are converted to a dollar amount at the rate of \$0.10/point. For example, if the profit is 70 points, the owner gets $70 \times \$0.1 = \$7$.

In summary:

- The agent chooses one of the two projects for the owner.
- After all 10 rounds of choice, the agent and the owner will see the profit from the chosen project, but NOT the profit from the unchosen project in the randomly selected round.

B Tables

Table 13

Percentage of subjects choosing the risky project (all rounds, immediate feedback)

	Decision for self		Decision for other
SAFE	43	$<^{***}_{+++}$	75
	$\wedge^{***}_{++}$		$\vee^{***}_{+++}$
CHOSEN	55	$>^{**}$	48
	$\vee^{***}_{+}$		$\parallel$
RISKY	43	$<^{*}$	49
	$=$		$\wedge^{***}_{+++}$

= the difference is not significant at the 10% level (with and without clustering)
* significant at the 10% level
** significant at the 5% level
*** significant at the 1% level
$_{+}$ significant at the 10% level (clustering)
$_{++}$ significant at the 5% level (clustering)
$_{+++}$ significant at the 1% level (clustering)

Table 14

Percentage of subjects choosing the risky project (1st round, immediate feedback)

	Decision for self		Decision for other
SAFE	50	$<^{***}$	82
	$\parallel$		$\vee^{***}$
CHOSEN	57	$=$	48
	$\parallel$		$\parallel$
RISKY	44	$=$	54
	$\parallel$		$\wedge^{**}$

= the difference is not significant at the 10% level
* significant at the 10% level
** significant at the 5% level
*** significant at the 1% level

Table 15

Percentage of subjects choosing the risky project (all rounds): feedback at the end

	Decision for self		Decision for other
SAFE	48	$<^{*}$	54
	$\|\|$		$\|\|$
CHOSEN	45	$<^{***}_{+}$	55
	$\|\|$		$\|\|$
RISKY	47	$<^{*}$	52
	$\|\|$		$\|\|$

= the difference is not significant at the 10% level (with and without clustering)
* significant at the 10% level
** significant at the 5% level
*** significant at the 1% level
$_{+}$ significant at the 10% level (clustering)
$_{++}$ significant at the 5% level (clustering)
$_{+++}$ significant at the 1% level (clustering)

Table 16

Percentage of subjects choosing the risky project (1st round): feedback at the end

	Decision for self		Decision for other
SAFE	72	=	59
	$>^{***}$		$\|\|$
CHOSEN	35	$<^{**}$	57
	$\|\|$		$\|\|$
RISKY	50	=	59
	$<^{**}$		$\|\|$

= the difference is not significant at the 10% level
* significant at the 10% level
** significant at the 5% level
*** significant at the 1% level
$_{+}$ significant at the 10% level (clustering)
$_{++}$ significant at the 5% level (clustering)
$_{+++}$ significant at the 1% level (clustering)

Graph 8

The fraction of subjects who choose the risky project (immediate feedback)

Graph 9

The fraction of subjects who choose the risky projec (feedback at the end)