

Wearing out your shoes to prevent someone else from stepping into them: Anticipated regret and social takeover in sequential decisions

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Abstract

Comparisons with counterfactual outcomes can influence choices in sequential decisions. We examine the effect of anticipated regret, and “social takeover”—the knowledge that someone else might take over an investment one has abandoned—on persistence on an investment task. Some participants received feedback about what would have happened if they had continued investing and others did not. Some knew that another person had the opportunity to pick up their investment where they left off and others did not. Data collected from 84 dyads showed effects of both experimental manipulations. Participants invest longer, on average, when another person could take over from their previous investments, and when feedback was provided. Both anticipated regret and social takeover appear to increase the tendency to stick with an investment.

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“If you don’t take her out tonight, she’s going to change her mind, and I will take her out tonight, and I will treat her kind.”

John Lennon & Paul McCartney (“You’re Going to Lose That Girl,” 1965)

Imagine a cold winter evening. You are at a street corner, waiting for a taxi that you ordered. After a while, you call the dispatcher again, and are informed that your taxi is on its way. Shortly afterwards, another person arrives at the street corner and apparently also waits for a taxi. Ten minutes later you become restless. Should you wait any longer? Maybe you should walk to the next, bigger street corner to take your chances there? But what if you decide to walk away, and, after you leave the curbside, the taxi you ordered arrives at last and then picks up the other person? Does the presence of the other person

affect your decision? More generally, does the presence of another person who might “step in” and take over your investment increase your likelihood of sticking with it? Lennon and McCartney, apparently, believed that this was the case; otherwise why would the threat of someone else taking your “girl” out deter you from leaving her?

Quitting on an investment, of course, has a downside whether or not someone else takes it over. There is a cost to “closing the books” at a loss, as discussed in the literature on mental accounting (Thaler, 1999). In addition, there is a potential for regret if one receives information about what would have happened had one persisted. Selling a losing stock is extra painful if one learns from the news that the stock made a comeback days after you sold, which may deter one from taking the plunge. Likewise, selling a condo in a buyer’s market is especially unpleasant if the housing market revives shortly after you sell your unit, fear of which might motivate one to hold on to it for longer than one would if there were no threat of receiving such feedback (Genesove & Mayer, 2001).

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The unpleasant feelings described in these examples are primarily evoked by the information that one could have done better than one did, but might be considerably stronger if one contemplates that someone else benefited from one's decision.

We present results from an experiment that examines both of these factors—the prospect of receiving feedback on an investment one abandons, and the possibility that someone else may take over the investment—on individual decisions to either continue investing or bail out on a risky venture. In what follows, we first review the literature on sequential decisions, then review the literature on the role of counterfactual comparisons in decision-making. We hypothesize that both the prospect of receiving feedback, and the prospect of someone else taking over one's investment—“social takeover”—should deter people from abandoning an investment, and test both hypotheses in an experimental study. Finally, we discuss some specific applications and general implications of our findings.

Escalation and de-escalation of commitment in sequential decisions

Many decisions have a sequential element: whether to use a concert ticket bought a few months ago; whether to use the lift pass on the last day of a ski trip despite bad weather; whether to invest more money in a project. Normative theory prescribes that decisions of this type should be made on the margin, taking only prospective costs and benefits into account. Empirical research has shown two forms of violations of this prescription: escalation and de-escalation of commitment, i.e., continuing longer or quitting earlier as a result of earlier investments.

Escalation of commitment (Staw, 1976), also termed the ‘sunk cost effect’ (Arkes & Blumer, 1985) or ‘entrapment’ (Rubin & Brockner, 1975), occurs when irretrievable investments (sunk costs) increase the propensity of decision-makers to invest additional resources. For example, Arkes and Blumer (1985) had participants imagine that they were a company president who had to decide whether to invest \$1 million to develop a radar-blank plane, even though another firm had just begun marketing a similar but better plane. In one version, the decision was described as being preceded by \$9 million already invested into the project, while in the other version no previous investment had been made. In this and similar scenarios, more participants decide to invest in the version with previous investments than in the version without previous investment. Theoretical accounts of escalation of commitment focus on increased risk-seeking in the domain of losses (Arkes & Blumer, 1985; Thaler, 1980; Whyte, 1993), wanting to avoid the impression of being wasteful (Arkes & Blumer, 1985; Arkes & Ayton, 1999), and justification of the original decision to

‘save face’ (Brockner, 1992; Fox & Staw, 1979). Although a large body of research has accumulated (see Brockner, 1992; Camerer & Weber, 1999; Heath, 1995 for reviews), recent findings have cast doubt on the notion that sunk cost effects are as robust as suggested. In some studies, sunk costs were confounded with the degree to which the project was already completed. When these factors were manipulated orthogonally, robust effects of project completion were observed, but no effects of sunk costs (Boehne & Paese, 2000; Conlon & Garland, 1993; Garland & Conlon, 1998).

Of particular interest for the present paper are studies on social factors in escalation of commitment. Studies examined whether individuals escalate less or more than groups (e.g., Kameda & Sugimori, 1993; Moon et al., 2003; Whyte, 1993), whether escalation is imitated by observers (Brockner et al., 1984), whether escalation is more likely under competition (Brockner & Rubin, 1985; Teger, 1980), whether the presence and quality of an audience influence escalation (Brockner, Rubin, & Lang, 1981; Brockner et al., 1982), and whether accountability for the process and the outcome of decisions matter for escalation (Simonson & Staw, 1992). The notion of social takeover has not so far been examined.

De-escalation of commitment occurs when previous investments decrease the propensity to invest additional resources. Some studies have reported such an opposite sunk cost effect (Garland & Conlon, 1998, study 2; Garland, Sandefur, & Rogers, 1990). In an influential paper, Heath (1995) documented non-normative de-escalation in investment situations, and proposed a theoretical account of when it occurs. His explanation builds on *mental budgeting* (e.g., Heath & Soll, 1996; Thaler, 1980) and predicts whether and when escalation or de-escalation will occur (Heath, 1995): Escalation will occur if a budget is not set, or when it is difficult to track additional investments; however, if a budget is set, de-escalation can occur if people dislike spending past the point where expenses exceed possible returns.

Heath (1995) also pointed out that some earlier studies that had been interpreted as providing evidence of escalation of commitment actually provided evidence of the opposite—irrational de-escalation. Results in the *counter game* (e.g., Brockner & Rubin, 1985; Brockner et al., 1984; Brockner, Shaw, & Rubin, 1979), originally interpreted as showing entrapment, can actually be read the opposite way. Brockner et al. (1979) confronted participants with an electronic counter that increased from 1 to 500, and told them that at a randomly determined number they would win an additional jackpot of \$2. Participants were endowed with a \$4 budget, for each tick of the counter 1 cent was subtracted from their budget, and after every 20 ticks they were asked whether they wanted to continue or quit. In such a situation, the marginal probability of winning increases with every tick and the marginal benefit of additional investment is increasing,

so rational decision-makers who begin gambling should persist. Recently, Zikmund-Fisher (2004) provided additional evidence on de-escalation of commitment by showing that a majority of participants quit too early in a sequential task.

Effects of counterfactual comparisons on decision-making

Besides the explanations discussed above, another factor that might influence a decision-maker to pursue or abandon an investment is counterfactual comparison. Considerable research suggests that people routinely compare the outcomes of their decisions to salient counterfactuals, and that the prospect of such comparisons influences their decisions. Previous work has explored several kinds of counterfactual comparisons which we will group into non-social and social counterfactuals. Non-social counterfactuals, as discussed in the literature on regret and disappointment, are outcomes that would have occurred to oneself under different circumstances (if one had made a different choice or had been more or less lucky). Social counterfactuals are outcomes of other persons (as discussed in the literature on social comparison and social utility).

Non-social counterfactuals

Several theoretical approaches have explored the impact of alternative outcomes on decisions. *Disappointment theory* (Bell, 1985; Gul, 1991; Loomes & Sugden, 1986) posits that people compare actual outcomes against outcomes that would have occurred if uncertainty had been resolved differently. *Regret theory* (Bell, 1982; Loomes & Sugden, 1982) posits that people compare actual outcomes against outcomes of not-chosen actions in the same state of the world. Both regret theory and disappointment theory propose that the utility of the actual outcome is modified by counterfactual outcomes. General theoretical frameworks incorporating both disappointment and regret were proposed by Inman, Dyer, and Jia (1997) and by Mellers (2000; Mellers & McGraw, 2001).

Regret and disappointment describe emotions experienced after outcomes are revealed (see Connolly & Zeelenberg, 2002; Gilovich & Medvec, 1995; Zeelenberg, 1999; Mellers, Schwartz, & Ritov, 1999). From the perspective of decision-making, however, a crucial issue is whether these reactions are *anticipated*. Regret theory, as formulated by Loomes and Sugden (1982), was based on the assumption of full information—i.e., that one would learn not only about the outcome of the alternative one chose, but also of other alternatives. Information about what would have happened if one had taken a different action is clearly relevant for counterfactual comparisons, and if such information is less readily available, the proposed utility modifications are presumably less strong. This logic has

been used in empirical studies to operationalize regret via the expectation of feedback, and results indicate that decision-making is influenced by expected feedback in a way that is consistent with the idea that anticipated regret does play a role in decision-making (e.g., Zeelenberg, 1999; Zeelenberg, Beattie, van der Plight, & de Vries, 1996).

In sequential decisions where a decision-maker has to decide whether to continue investing money, time, or effort toward an uncertain goal, continuing usually is the risky option: it might lead to success and gains, but might also lead to mounting losses. On the other hand, quitting is usually a comparatively safe option: terminating an unfinished project results in clear and certain outcomes. Research on regret and risk-taking (Zeelenberg et al., 1996) shows that expectation of feedback can produce either risk-seeking or risk-aversion depending on which choice is less likely to produce regret. In a situation in which choosing the safe option prevents information about the outcomes of the unchosen risky option, participants tended to be risk-averse; however, when choosing the safe option does not preclude one from receiving that information, participants tended to be more risk-seeking. Based on these findings, we therefore predict that, in a sequential decision, expecting feedback about what would have happened after a decision to quit should increase risk-seeking and hence the likelihood of continuing.

Social counterfactuals

While approaches such as regret theory or disappointment theory are concerned with counterfactual outcomes that can be considered non-social, other approaches have pointed to the importance of social sources for counterfactual outcomes. Social comparison theory (Festinger, 1954; Suls & Wheeler, 2000) encompasses the insight that people often compare themselves to other persons. Originally formulated as a theory of self-evaluation in the areas of abilities and opinions, it has been expanded to other areas such as evaluation of happiness (e.g., Strack, Schwarz, Chassein, Kern, & Wagner, 1990) or income (e.g., Brandstaetter, 2000; Major & Forcey, 1985). Social utility research focuses more specifically on the comparison of outcomes (e.g., Loewenstein, Thompson, & Bazerman, 1989; Messick & Sentis, 1985; Ordóñez, Connolly, & Coughlan, 2000). Social utility functions have been used to explain behavior in ultimatum and dictator games, as well as a variety of other experimental games (Bolton & Ockenfels, 2000; Fehr & Schmidt, 1999).

A novel factor we examine in this paper that could affect the strength of counterfactual comparisons, and hence decisions that are influenced by such comparisons, is the possibility that another person will take over one's position. As an analogy to corporate takeovers, we use the term *social takeover* to describe such a situation. We define social takeover as a situation in which another person takes over an investment that one has abandoned.

We propose three mechanisms that should cause the potential for social takeover to influence decisions.

The first mechanism is based on the idea that social takeover makes counterfactual outcomes more salient. The findings reported above, showing that expectation of feedback induces anticipated regret, indicate that salience is important. If imagining another person taking over one's investment increases the salience of the outcomes that they experience (that one would have experienced oneself had one held on to the investment), then we should expect social takeover to either have a similar effect as feedback or to magnify the effects of feedback. The findings of Boles and Messick (1995) are consistent with this prediction: they found that alternative outcomes were more likely to be evoked as reference points when those outcomes were received by another person. Other authors have pointed out that the ease with which counterfactuals can be construed is crucial for regret (e.g., Kahneman & Miller, 1986; Kahneman & Tversky, 1982). Zeelenberg and Pieters (2004) demonstrated that people anticipate more regret (although not more envy) when imagining that their neighbors have won in the Dutch Postcode lottery than in a regular state lottery. The postcode lottery has a specific feedback structure: payoffs are determined by postal code, so if one's own district is selected it is clear that one would have won if only one had bought a ticket.

The second mechanism is that social takeover highlights the responsibility of the decision-maker. Some authors have suggested that being responsible for outcomes generates stronger regret and rejoicing (Kahneman & Tversky, 1982; Thaler, 1980), and increases loss aversion (Shefrin & Statman, 1984). Sugden (1985) noted that regret often encompasses two features: the wish to have chosen differently and the self-blame or self-recrimination of not having chosen better. This idea was recently reformulated as *decision justification theory* (Connolly & Zeelenberg, 2002), and empirical studies (Inman & Zeelenberg, 2002) showed that regret was stronger when self-blame was made easier because only weak reasons for the decision were available. Larrick (1993) argued that decision-makers care not only about outcomes, but also about maintaining and protecting a positive self-image. Social takeover encompasses the potential threat of increased self-recrimination. If the other person continues the course of action that one had oneself decided to quit, it is evident that at least one person deemed it sensible to continue. Should the other person even succeed later on, this might make it even harder to justify one's decision to quit.

The third mechanism is one of anticipated envy. Envy, or "social-comparison jealousy" (Salovey & Rodin, 1984), is a negative emotion arising from an unfavorable social comparison in a relevant domain. If decision-makers imagine a future scenario in which the other person takes over and is successful, not only can they anticipate feeling regret about their decision, but

they can also anticipate feeling envy that the other person would have something they themselves have not.

All three of the mechanisms discussed above are likely to play a role in sequential decisions, and in fact all three mechanisms drive behavior in similar directions. If social takeover works through making counterfactuals salient, then we would expect effects that are similar to the anticipation of explicit outcome feedback. If social takeover works through highlighting responsibility, we would expect the anticipated self-blame to have the same effect as anticipating regret. And, if social takeover works through the anticipation of negative social comparison emotions such as envy, we would expect decision-makers to take steps to avoid these emotions, even if those steps are risky or costly. We would therefore predict social takeover to encourage people to stick with their investments in sequential decisions.

Method

Both anticipated regret and social takeover are predicted to increase commitment in sequential decisions. We test these predictions in an experimental setting in which it is relatively easy to follow the prescription of marginal decision-making because both marginal costs and marginal benefits are well-defined. As Heath (1995) has shown, however, under these conditions de-escalation of commitment is likely—even more so when it is easy to track previous expenses. In such a situation, we would therefore expect participants to quit relatively early, in spite of increasing marginal returns. If participants expect to be confronted with counterfactual outcomes, however, for reasons discussed earlier we would expect them to quit later. Likewise, as just discussed, if they know that after quitting another person might take over their position, they should also be more likely to carry on.

Materials and procedure

Participants signed up for the study via a Carnegie Mellon University research laboratory website. They were told that they would earn \$5 in exchange for completing a questionnaire, and would be given the opportunity to participate in a gamble.

Two subjects were assigned to come to the laboratory at the same time, one of whom was assigned to be the "first player" and the other who was assigned to be the "second player," as described below. Upon arrival at the lab, they signed consent forms and completed a 12-page questionnaire dealing mainly with topics unrelated to the present study, such as ownership and use of communication devices, time use, everyday life events, and communication with friends and family. Embedded in the questionnaire was one set of items related to the current study: the social comparison orientation

scale (Gibbons & Buunk, 1999). This scale measures individual differences in the tendency to compare with other people, and consists of 11 statements (e.g., “I often compare myself with others with respect to what I have accomplished in life”; 5-point disagree–agree format). Recent research has shown that social comparison orientation, as measured by this scale, moderates the impact of social comparisons on mood and affect (Buunk & Brenninkmeijer, 2001). Completion of the whole questionnaire took about 15 min. The questionnaire was intended, in part, to distract subjects from the main point of the study, and in part to make them feel as if they had earned the money that they were later given an opportunity to invest.

After completing the questionnaire, participants received the instructions for the gamble. A simple Bingo cage—an opaque turnable globe that can be filled with marbles—was used for the experiment. With each turn of the globe, exactly one marble comes out of the globe. At the outset, the bingo cage was filled with 99 brown marbles and 1 red marble. To represent the odds in a fashion that would be meaningful to participants, these 100 marbles were laid out on a 10×10 grid and shown to them. Then the marbles were poured into the bingo cage in front of the participants to exclude any suspicions of the gamble being rigged.

The general rules of the gamble were that the red marble was worth \$7 and the brown marbles were worthless. For each draw, \$0.10 would be subtracted from the \$5 amount earned previously. Marbles drawn were not put back in the bingo cage, but placed on the 10×10 grid to indicate the amount of money already invested and the number of marbles remaining in the globe. Participants could play as long as they wanted for a maximum of 50 draws and could quit at any time.

To acquaint them with the game, and to initiate them in the process of investing, first players received an additional starting budget of \$2, and went through a trial phase of 20 draws in which they already could win. Participants who won during the trial phase were lost to the study. This trial phase ensured that participants understood the game, and also put first players in a position of deciding whether to continue to invest rather than whether to start to invest. The expected monetary value of the gamble is positive: after the trial phase, the expected value is \$5.91, taking into account the conditional probabilities to win and the necessary payment at each step.¹ Compared with a sure outcome of the original \$5 budget, not taking the gamble, therefore, indicates risk-aversion.

¹ Possible, mutually exclusive outcomes of the game are: (1) red ball at first draw ($p = 1/80$, payoff = \$11.90), (2) red ball at second draw, given that the first draw was brown ($p = 1/79 * 79/80 = 1/80$, payoff = \$11.80), (3) red ball at third draw, given that the previous draws were brown ($p = 1/78 * 78/79 * 79/80 = 1/80$, payoff = \$11.70), ... (51) red ball not within the first 50 draws ($p = 30/80$, payoff = \$0).

The expected value of \$5.91, however, is composed of lower expected value draws at the beginning (when there are still many marbles left in the cage) followed by draws that increased in expected value. The expected value of the first draw after the trial phase was approximately \$0.09—i.e., about a penny less than the cost of drawing the marble. However, if the subject got to the point of drawing 49 marbles without winning, the expected value of the next (and final) draw would be \$.23—i.e., 13 cents more than the cost. Given that the expected value of playing increased with the length of play, a person who started playing should, by economic rationality, continue to play. Although it is theoretically possible that an individual could, due to declining wealth, become more risk-averse after losing repeatedly, this would imply an absurd sensitivity of risk-aversion to wealth (see Rabin, 2000).

The experiment was a 2 (No Feedback versus Feedback) \times 2 (No Interdependence versus Interdependence) experimental design involving two players. Experimental manipulations varied the consequences of a decision to quit on the part of first players. The feedback manipulation was intended to vary anticipated regret. In the No Feedback treatment, first players knew that if they quit they would not find out what would have happened had they continued; in the Feedback treatment, they knew that they could be confronted with future counterfactual outcomes as well as outcomes of second players (if relevant). The interdependence manipulation varied social takeover. In the No Interdependence treatment, first players knew that second players would start with a newly filled bingo cage; in the Interdependence treatment, first players knew that second players would be handed over the bingo cage in exactly the state it was when they decided to quit. Table 1 illustrates the specific characteristics of the four conditions.

It should be noted that the feedback conditions include two different kinds of information. Since we were interested in the combined effect of social takeover and anticipated regret, the Feedback/Interdependence condition was set up as second players taking over the first players' position and first players witnessing the outcomes. In such a situation, however, second players' outcomes are inseparably also the potential outcomes of first players, had they continued. To control for this potential confound, we introduced both types of information also in the Feedback/No Interdependence condition. Subjects found out what would have happened if they had continued to play, and they observed the behavior and outcomes of the second player, although the second player had not taken over their bingo cage.

When participants had finished reading the instructions, a coin toss randomly assigned one person to be the first player. The trial phase was then conducted, in which 20 balls were drawn in succession. If the red ball turned up during the trial phase, the game ended. After those

Table 1
Overview of experimental conditions

	No Interdependence	Interdependence
No Feedback	When first player quits, • Second player gets a new bingo cage • First player leaves the room	When first player quits, • Second player takes over bingo cage • First player leaves the room
Feedback	When first player quits, • Second player gets new bingo cage, but first, experimenter continues to draw until red ball comes out • First player stays to watch the second player	When first player quits, • Second player takes over bingo cage • First player stays to watch the second player

first 20 draws, participants were asked whether they wanted to continue playing with the \$5 they had earned from completing the questionnaire. If they agreed, the game continued until the participants decided to quit, or ran out of money, or won by finding the red ball. The experimenter then recorded the number of draws taken, calculated the payoff for the first player, and announced it. Depending on the condition, the first person then either stayed in the room or waited outside, and the bingo cage was passed on to the second person in its current state or refilled with 80 balls. The second player was asked whether they wanted to play with the \$5 they had earned from completing the questionnaire. If they decided to play, second players then played until they decided to quit, ran out of money, or won by finding the red ball. At the end, participants completed a short questionnaire on their motives for their decisions, the perceived impact of the other person on their thoughts and actions, and their satisfaction with their actions.²

Participants

Data were collected in 84 dyad sessions. Of the 168 individuals in these pairs, 101 were male and 67 were

female. Participants were on average 22.5 years old ($SD = 5.4$, $Md = 21$), with no age difference between men and women. Thirty-eight participants reported that they were high school graduates, 69 had some college, 35 were college graduates, and 26 had completed a post-graduate degree.

Results

A number of different outcomes could occur in the experiment: winning during the trial phase or later on, quitting voluntarily immediately after the trial phase or later on, or continuing to invest until the budget was exhausted. As one would expect by chance, about one-fifth of participants won during the trial phase (17 of 84), leaving 67 participants for further analysis. After the trial phase, some participants played until they either won (10 of 67) or ran out of money (7 of 67), and other participants decided to quit voluntarily at some point (50 of 67).

The main variable of interest is the duration of play, or the timing of the decision to quit the game. The appropriate method to analyze such time-to-event data, in which subjects drop out of the sample after a critical event occurs, is survival analysis. A brief introduction to survival analysis is provided in Appendix A.

The game offers a total of 50 decision points at each of which participants can decide whether to invest or to quit. A rough impression of participants' behavior can be gained by looking at the decision to quit at the first decision point, i.e., immediately after the trial phase. Table 2 gives descriptives. In the No Interdependence/No Feedback condition, the majority (12 out of 16) decided to quit at the first decision point. In the other

² The post-game questionnaire contained free-response questions about their reasons for quitting and three items with specific response options: "To what extent did the other person figure in your thoughts during the game?"; "To what extent did the other person influence your actions during the game?," and "Regardless of the outcome, how satisfied are you with your decisions during the game?" There were no differences in these items between conditions, presumably because of a ceiling effect; everyone reported that they were not influenced by the other person. Similarly, satisfaction depended on outcome but not on condition: those who won were more satisfied than those who quit or ran out of money. From the free-response reasons for quitting or continuing given by participants, it seems that they mostly thought about the gamble in terms of the odds being too small (that's what those who did quit immediately said), and the amount of money they would have left (those who decided to quit after having taken a few draws mostly said something like "I didn't want to lose all my money," I spent too much of my money already"). None of them mentioned something like regret, only two participants mentioned that the odds improved over time, and only one person mentioned the second player. These data were not very helpful probably in part due to the difficulty of introspecting about the types of motives involved in the study and because at the time that they answered the questions, participants already knew about their own and their partner's payoff.

Table 2
Players quitting voluntarily in first round, by condition

Decision	No Interdependence		Interdependence	
	No Feedback	Feedback	No Feedback	Feedback
Quit in first round	12	7	8	6
Continue	4	10	9	11
Total	16	17	17	17

three conditions, a minority quit at the first decision point (7, 8, and 6 out of 17). A comparison between the data from the No Interdependence/No Feedback condition and the pooled data from the other three conditions shows a significant difference, $\chi^2(df=1, n=67)=5.58$, $p=.018$.

A more detailed analysis is provided by taking into account the number of rounds played by those who continued. However, these data are censored in two ways: when a person played until the budget was exhausted and when a person won by drawing the red ball. In both cases, it is not possible to know how many more draws that person would have taken; it is only known that at least a certain number of decisions to invest were made. Fig. 1 shows the non-parametric Kaplan–Meier survival functions for the four conditions; steps indicate voluntary quitting. Three characteristics seem noteworthy. First, there is a marked drop at the first decision point. Second, no voluntary quitting is observed after decision point 21. Third, all cases where participants play until their budget is exhausted are observed in only three conditions (specifically in all but the No Interdependence/No Feedback condition).

The survival functions, in the present case, describe the probability of not observing voluntary quitting before a given decision point. The survival functions suggest a different pattern of quitting over the four conditions. A log-rank test for equality of the survival functions is marginally significant, $\chi^2(3)=6.71$, $p=.08$. This test, however, omits information because it only considers the ordering of events. In particular, the information present in the long right tails introduced by participants who played until the end is neglected; the significance test would be the same if

they had won in round 21, instead of playing until round 50.

To provide a more detailed picture, a parametric survival analysis was conducted. Parametric survival analysis requires one to make specific assumptions about the functional form of the hazard function over time (i.e., the intensity with which the event under study occurs, Cleves, Gould, & Gutierrez, 2002). The simplest model assumes that hazard is constant over time. For the current data, this assumption seems not justified, given the differences in quitting between the beginning and the end of the game. Therefore, a Weibull regression model was estimated. Table 3 displays results for different models estimated in this fashion.

In model 1, the potential effects of demographic variables were tested by including age and gender. We wanted to control for these variables for two reasons: first, previous research has demonstrated quite consistent gender differences in risk-seeking, with females being more risk-averse, in particular at younger ages (see Byrnes, Miller, & Schafer, 1999 for a recent meta-analysis). Second, survival data are potentially prone to selective dropouts or “frailty effects,” and an observed survival function could be the result of a changing sample constitution; including individual-level variables can control for some of these effects. The model is significant, indicating that demographic variables influence the hazard to quit. Age has a marginally significant effect ($B=.26$, $p=.08$, hazard ratio HR = 1.29), indicating that with increasing age the hazard tends to increase. Gender significantly influences the hazard ($B=.75$, $p=.01$, HR = 2.11). Female participants are more likely to quit voluntarily, with a twofold hazard of quitting as compared with males.

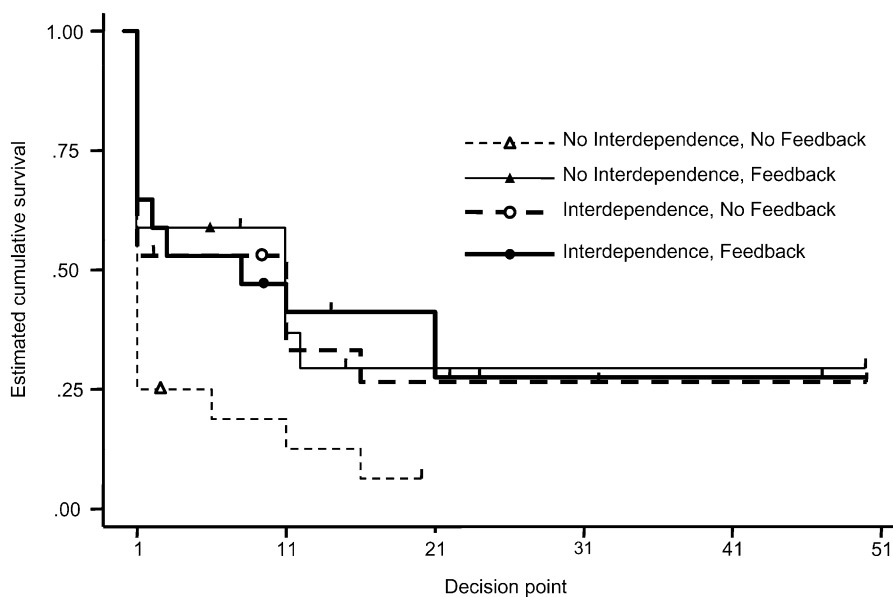


Fig. 1. Kaplan–Meier survival estimates for quitting voluntarily, by condition. Note. Steps indicate voluntary quitting, vertical marks indicate censored observations.

Table 3
Parametric survival analysis, Weibull regression for quitting voluntarily ($N = 67$)

	Model 1	Model 2	Model 3	Model 4
	LL = -123.94, $\chi^2(2) = 9.60^{**}$	LL = -121.18, $\chi^2(4) = 15.11^{**}$	LL = -118.94, $\chi^2(5) = 19.59^{**}$	LL = -109.86, $\chi^2(7) = 37.75^{**}$
Age	0.26 (0.14)	0.26 (0.14)	0.30 (0.14)*	0.22 (0.15)
Gender (female = 1)	0.75 (0.29)*	0.79 (0.31)*	0.78 (0.31)*	0.98 (0.32)**
Interdependence		-0.29 (0.30)	-0.87 (0.41)*	-0.90 (0.41)*
Feedback		-0.68 (0.30)*	-1.28 (0.41)**	-1.28 (0.42)**
Interdependence \times Feedback			1.23 (0.58)*	1.26 (0.59)*
social comparison orientation				-0.12 (0.19)
social comparison orientation \times Feedback				-0.82 (0.28)**
Constant	-1.82 (0.28)**	-1.42 (0.36)**	-1.17 (0.35)**	-1.45 (0.38)**
Weibull p	0.59 (0.07)**	0.63 (0.07)**	0.65 (0.07)**	0.76 (0.09)**
Test for inclusion		$\chi^2(2) = 5.45$	$\chi^2(1) = 4.47^*$	$\chi^2(2) = 19.70^{**}$

Note. Weibull regression parameters, standard errors in parentheses.

* $p < .05$.

** $p < .01$.

Model 2 tests for main effects of the experimental conditions, after controlling for demographics. The test for inclusion is marginally significant, $\chi^2(2) = 5.45$, $p = .07$. Only the main effect of feedback is significant ($B = -.68$, $p = .02$, HR = .51). In conditions with feedback, participants are only half as likely to quit as in conditions without feedback.

Model 3 includes the interaction term between feedback and interdependence. This step improves the model fit significantly, $\chi^2(1) = 4.47$, $p = .04$. The interaction term is significant ($B = 1.23$, $p = .03$), indicating that the hazard ratio for feedback varies with interdependence and vice versa. First, results show that the conditional parameter for interdependence is significant and negative ($B = -0.87$, $p = .03$, HR = .42) when feedback is absent. This means that there is a significant difference between the No Feedback/No Interdependence and the No Feedback/Interdependence condition: in the latter, the likelihood to quit at a given time is reduced significantly by a factor of two. Second, the conditional parameter for feedback is significant and negative ($B = -1.28$, $p < .01$, HR = .28) when interdependence is absent. This means that there is a significant difference between the No Feedback/No Interdependence condition and the Feedback/No Interdependence condition: the likelihood of quitting is lowered by about 30% relative to the former. Third, the interaction parameter has a sign that is contrary to the conditional main effect parameters, indicating a subadditive interaction. On the one hand, the parameter for interdependence is modified by the interaction term and becomes non-significant in conditions with feedback ($B = -0.87 + 1.23 = 0.36$, $p = .40$, HR = 1.43). This indicates that there is no significant difference between the Feedback/No Interdependence condition and the Feedback/Interdependence condition. On the other hand, the parameter for feedback is modified by the interaction term and becomes non-significant in conditions with interdependence ($B = -1.28 + 1.23 = -.05$, $p = .91$, HR = .95), indicating

that there is no significant difference between the No Feedback/Interdependence condition and the Feedback/Interdependence condition. Finally, when interdependence and feedback coincide, the resulting parameter is still significantly negative ($B = -.87 - 1.28 + 1.23 = -.92$, $p = .02$, HR = .40), indicating a significant difference between the No Feedback/No Interdependence condition and the Feedback/Interdependence condition. In the latter condition, the hazard to quit is about 40% that of the former.

The interaction pattern in Model 3 shows that the effects of feedback and interdependence do not simply add up, but that the combination produces about the same effect as each single element alone. This effect is illustrated in Fig. 2, showing the estimated survival functions. It is apparent that the No Feedback/No Interdependence group differs from the other three groups, and that those three groups are rather similar in terms of estimated survival. The estimated median survival time describes the time until which 50% of the sample have experienced the terminal event, and can be considered as an estimate of how long the average participant in a certain condition keeps investing before quitting voluntarily. It can be seen from Fig. 2 that the estimated median survival time is lowest for the No Feedback/No Interdependence group, with an estimate of five rounds, and higher for the other conditions, with estimates between 11 and 18 rounds.

Model 4, in addition to the experimental manipulation, includes social comparison orientation and the interaction term with feedback. Social comparison orientation measures the degree to which individuals habitually compare themselves with others. In the current context, it was assumed that this orientation interacts with feedback because, as discussed above, in the feedback conditions participants learned both about the counterfactual outcome and the outcome of the other person. The inclusion of these variables improves model fit significantly, $\chi^2(2) = 19.70$, $p < .01$. The interaction

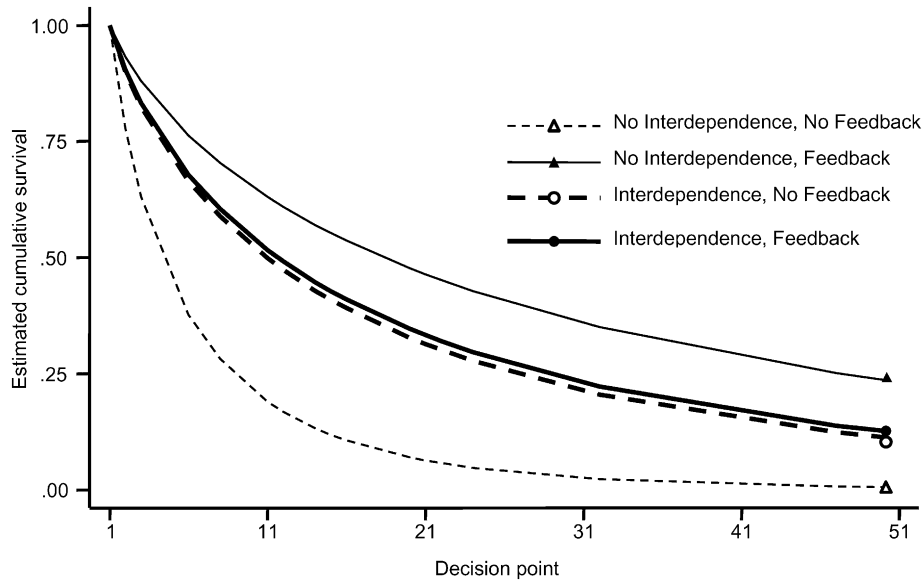


Fig. 2. Weibull survival estimates for quitting voluntarily, by condition.

term is significant and indicates that the effect of feedback depends on the level of social comparison orientation. For average levels of social comparison orientation, the effect of feedback is as described above. For participants high in social comparison orientation, the effect of feedback is enhanced: for example, a one-unit increase in standardized social comparison orientation changes the parameter for feedback by $-.82$, resulting in parameter values of $B = -2.00$ without interdependence and $B = -.87$ with interdependence. All else equal, therefore, high social comparison orientation further reduces the likelihood of quitting in conditions with feedback. On the other hand, for participants low in social comparison orientation the effects of feedback are diminished.

Discussion

We examined the impact of regret and social takeover on behavior in sequential decisions. Experimental conditions varied the consequences of a decision to quit. Results show that when participants expected to receive feedback about the outcomes that would have resulted had they continued, they were more likely to continue playing. Similarly, when they knew that after quitting another person would take over their position, participants were more likely to continue playing.

Theoretical implications

This study contributes to the literature on escalation of commitment by providing further evidence that de-escalation can occur when information about marginal costs and benefits is given and when expenses are easy to track (Heath, 1995; Zikmund-Fisher, 2004). A considerable

proportion of participants did not continue after the trial phase, although their chances were gradually increasing with each draw. Moreover, some participants started investing their “own” money and then voluntarily gave up. In line with a mental budgeting explanation for de-escalation (Heath, 1995) it is interesting that all voluntary quitting occurred before or at decision point 21. At that time, participants had lost \$2 of their “own” budget. This threshold could be a result of setting a mental budget by subtracting the initial budget of \$5 from the possible prize of \$7. Although many participants exhibit non-normative de-escalation of commitment, anticipated regret and social takeover induced participants to invest longer, on average, and counteracted the tendency to quit too early.

With regard to the literature on regret, the current work replicates earlier findings on the relation between regret and risk-aversion and extends those findings to the context of sequential decisions. As in the studies by Larrick and Boles (1995), Zeelenberg et al. (1996), and Zeelenberg and Beattie (1997), the expectation of feedback about an unchosen risky option reduced risk-aversion. In addition, individual differences seem to play a role. Participants with low tendency to compare with other people reacted less to the expectation of feedback, and by extension, to anticipated regret.

In the current paper, we introduced a new concept that we termed social takeover. It refers to the expectation that another person might take over one’s position. To the best of our knowledge, this phenomenon, and hence its impact on decision-making, has not been studied previously. Our findings indicate that, even in the absence of anticipated feedback, awareness that another person might take over one’s investment has an effect very similar to that of anticipated feedback, increasing subjects’ reluctance to quit.

We discussed three possible mechanisms through which social takeover could influence decisions. Although the current results do not allow to disentangle the three mechanisms, given that they yielded similar predictions, some speculations are possible. First, the finding that social takeover and anticipated regret show a subadditive interaction seems to weaken the first proposed mechanism of increasing the salience of counterfactuals. Second, the finding that social takeover has an impact even when decision-makers do not learn the counterfactual outcomes seems to weaken the second proposed mechanism of increasing self-blame. If one really does not know how or what the other person did, it seems a bit far-fetched to infer that one made a bad decision. The third proposed mechanism of anticipated envy seems compatible with the findings of similar commitment in both conditions where the second player took over the bingo cage. If social takeover prompts anticipation of envy by imagining a situation in which one is worse off than the other person, the tendency to invest more to avoid the realization of such a situation would be stronger under conditions of social takeover. Since the main mechanism here is one of imagining the unfavorable comparison, it would account for the findings of increased commitment even without explicit feedback. That is, just the thought of somebody else getting one's prize could be sufficient to deter from quitting. This reasoning is in line with findings on a marked reluctance to exchange lottery tickets even when the fate of one's original ticket was unknown (Bar-Hillel & Neter, 1996).

Limitations

The above discussion focused on neutral relationships between the persons involved, such as between strangers. It seems plausible that the quality of the relationship has a considerable impact on the consequences of social takeover. For hostile relationships, presumably, the inclination to avoid that the other person benefits from one's investment would be even more pronounced. For friendly relationships, on the contrary, it is conceivable that social takeover even has the opposite effect—reducing commitment below that in a solitary situation. We might be more willing to give up on investments when they go “to a good home,” to friends or family than when they vanish in the void. Self-evaluation maintenance theory (Tesser, 1986) suggests that we might bask in the reflected glory of our friends' success—provided the success is not on a dimension that is too threatening to one's self-esteem. Future studies could manipulate both the self-relevance of the decision task and the quality of the relationship.

It is certainly a limitation of the current study that it does not allow to judge the relative importance of the three mechanisms that could be driving the effects of

social takeover. Future work could proceed along the following lines to establish the roles of the proposed mechanisms:

- (a) Manipulating the salience of counterfactual outcomes in situations with and without social takeover. One approach could be to vary salience by having participants think explicitly about the counterfactual outcomes. Another approach could be to measure salience using verbal protocols, think-aloud techniques or interviews to establish whether social takeover produces more consideration of what would happen after quitting.
- (b) Manipulating the potential for self-blame. One approach would be to frame the decision problem in a way that highlights that the quality of a decision should not be judged with information that was acquired only later. In that situation, the feeling of having made a “bad decision” when the second player continues and succeeds should be decreased. In a similar vein, weak or strong arguments (as in Inman & Zeelenberg, 2002) could be provided to participants, or participants could be lead to focus on a “good decision” case (imagine you decide to quit, the second person continues but does not find the red ball) versus a “bad decision” case (imagine you decide to quit, and the second person immediately finds the red ball). Another approach would be to manipulate the feeling of competence regarding the optimal course of action, e.g., by having a third party who knows when the red ball would drop (Heath & Tversky, 1991).
- (c) Manipulating the potential for envy. One approach could be to vary the nature of the second player, assuming that envy would be more pronounced with strangers or despised persons, and less so for friends or charities. This approach, however, potentially confounds envy with other motivations, such as deliberately making it more or less difficult for the other party in the case of inheriting, and would need to consider the self-relevance of the task as discussed in self-evaluation maintenance theory. Another approach, similar to the framing intervention discussed above, would involve focusing participants on the comparison of outcomes between the two players.

Practical implications

Apart from theoretical contributions to the literature on regret and escalation of commitment, and keeping in mind the limitations of the present study, we believe that there are a number of relevant domains in which social takeover might affect a decision-maker's behavior.

In the domain of managerial decision-making, escalation of commitment has been studied extensively. One of the major motives for escalation of commitment is assumed to be self-justification. In some studies, sunk cost effects were stronger when decision-makers were responsible for the initial investment decision than when the initial decision was made by someone else (e.g., Staw, 1976). This seems to imply that placing control of losing investments in new hands could be an effective strategy for avoiding escalation, and indeed there is anecdotal evidence that some investment firms do exactly this. Our research suggests, however, that such a policy could potentially have perverse effects. If managers anticipate being replaced on losing investments, this might deter them from making the investment in the first place, or might deter them from revealing their losses.

In finance, the disposition effect (Shefrin & Statman, 1985) describes investor behavior where losing stocks are sold later and winning stocks are sold earlier than normatively prescribed, and regret was suggested as partly explaining this effect. In a similar vein, the preference for cash dividends over stock dividends has been explained to stem partly from regret (Shefrin & Statman, 1984). Such effects could be exaggerated if transactions have a social takeover component, such as when investors are selling their stocks to identifiable others.

In marketing, the idea seems to be relevant for persuasion strategies. One sales tactic induces a feeling of urgency, and the threat that another customer might take what is offered (Cialdini, 2001)—e.g., “If you don’t buy it, it’s going to get snapped up.” Such a persuasion tactic might be even more powerful if customers have the feeling that they already invested into that offer, for example, by bargaining over price. A nice example for a marketing strategy that appeals directly to social takeover was recently provided by an online casino, praising the advantage of playing slot machines online (rather than in a hall) as nobody waiting to continue playing on “your” machine.

In negotiations, settlements can be hindered by psychological barriers such as *reactive devaluation* (Ross, 1995; Ross & Ward, 1995). Reactive devaluation refers to the phenomenon that the very fact of a concession being made by the other party in a negotiation makes this concession seem less attractive. This effect also persists in the absence of strategic considerations, and one of the underlying mechanisms proposed is a change in preferences (Ross, 1995). Someone else being willing to give something up seems to devalue it even in the absence of objective reasons; social takeover could have the opposite effect and lead to unrealistic increases in valuation and pose another obstacle to conflict resolution.

Finally, as in the Beatles song, social takeover might matter in relationships and marriage. The investment model by Rusbult (1983) suggests that one’s commitment

to a relationship depends partly on the quality of alternatives oneself has. An extension of our findings would suggest that commitment might also depend on the quality of alternatives one’s partner has. Knowing that someone else might immediately step in if one discontinued a relationship could increase commitment, extend relationship duration, and discourage separation. However, it might not necessarily be the most solid basis for such a bond nor make for a happier relationship.

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Appendix A. Technical Appendix

Detailed descriptions of survival analysis are provided by Hosmer and Lemeshow (1999) and by Cleves et al. (2002). Introductions to survival analysis in applied settings are provided by Morita, Lee, and Mowday (1989) and by Singer and Willett (1991). We will give a very brief overview of the central concepts here, following Cleves et al. (2002).

Survival analysis estimates the time until an event. OLS regression, assuming a normal distribution for the residuals, is not suitable for time-to-event data (Cleves et al., 2002, p. 2). Survival analysis also handles *censored* data, which are data incomplete over time (e.g., drop-outs). Because of that missing information, classical statistical methods such as the mean or median cannot be used (p. 88). Survival analysis employs two central functions: the survivor function and the hazard function. The *survivor function* illustrates the unconditional probability of surviving (i.e., not to observe the event under study) beyond time t . The *hazard function* illustrates the probability that the event under study occurs in a given time interval, divided by the width of that time interval, provided that the subject survived until the start of that interval (p. 7). Put more simply, it describes the intensity with which the event under study occurs. A given hazard function directly translates into a survivor function. For parametric survival analysis, a plausible assumption has to be made about the hazard function, e.g., whether it can be assumed to be constant over time or not. The *Weibull hazard function* can provide various monotonically increasing or decreasing hazard functions (including constant hazard rates as a special case), and is suitable for data with monotone hazard rates (p. 207).

The specific form of the function is given by a parameter estimated from the data.

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