Commission, Omission, and Dissonance Reduction: Coping With Regret in the “Monty Hall” Problem

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Do people reduce dissonance more for their errors of commission than their errors of omission? More specifically, do people come to value a disappointing outcome obtained through a direct action more than an identical outcome obtained through a failure to act? To answer this question, the authors created a laboratory analogue of the “three doors” or “Monty Hall” problem. Subjects initially selected one box from a group of three, only one of which contained a “grand” prize. After the experimenter opened one of the two unchosen boxes and revealed a modest prize, subjects were asked to decide whether to stay with their initial selection or trade it in for the other unopened box. Regardless of the subject’s choice, a modest prize was received. Results indicated that subjects who switched boxes assigned a higher monetary value to the modest prize they received than those who stayed with their initial choice. Implications for the psychology of regret are discussed.

Consequentialist moral philosophers argue that there is no difference between actions and inactions that lead to the same outcome (Bennett, 1980, 1981, 1983; Singer, 1979). In each case, the individual makes a choice—to act or not—and it is the consequences of that choice that matter. To a consequentialist, for example, failing to avoid a loss by hanging onto a stock should hurt just as much as incurring a loss by investing unwisely in a new one.

Psychologists know better. Although there may be no logical difference between outcomes that stem from commission and omission, people’s reactions to the two cases indicate that they are vastly different psychologically. People react much more strongly to commissions than omissions. For example, actions that cause harm to others are judged to be more reprehensible than parallel failures to act (Spranca, Minsk, & Baron, 1991; Sugarman, 1986). Likewise, people experience more joy over positive outcomes and more regret over negative outcomes that stem from action as opposed to inaction (Gleicher et al., 1990; Kahneman & Miller, 1986; Landman, 1987). These latter phenomena, known as emotional amplification, have been linked to three distinct causes. First, actions are typically more vivid and salient than failures to act and thus exert more impact on information processing (Landman, 1987; McArthur, 1981; Nisbett & Ross, 1980; Taylor & Fiske, 1978). Second, people respond emotionally to events in direct proportion to how easily they can mentally “undo” the existing state of affairs and conjure up a compelling alternative reality (Kahneman & Miller, 1986; Kahneman & Tversky, 1982a; Miller, Turlbull, & McFarland, 1990). Actions are typically easier to undo because it is “easier to mentally delete an event from a chain of occurrences than it is to imagine the insertion of an event into the chain” (Kahneman & Tversky, 1982b, p. 173). Moreover, because inaction is more often the “default” or “norm” and action the exception, the alternatives to action will often seem more normative and therefore more compelling (Kahneman & Miller, 1986). Finally, because of this

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tendency to view inaction as the status quo and action as a departure from the norm, people generally feel more personally responsible for their actions than for their failures to act. A heightened sense of personal responsibility, in turn, tends to accentuate the emotional impact of events (Weiner, 1980). For all these reasons, then, outcomes that result from action typically generate more intense emotional reactions than identical outcomes brought on by inaction.

One area in which the distinction between omission and commission has not been examined is the domain of cognitive dissonance and dissonance reduction (Aronson, 1969; Cooper & Fazio, 1984; Festinger, 1957). In particular, might we expect people to experience more dissonance for a negative outcome that stems from a direct action than for an identical outcome that results from a failure to act? There are at least two reasons that we might. First, a negative outcome that stems from direct action is likely to be experienced as more aversive (emotional amplification) and hence generate more dissonance. Second, because people are inclined to feel more responsible for their actions, their actions should produce more dissonance (Cooper, 1971; Goethals, Cooper, & Naficy, 1979; Hoyt, Henley, & Collins, 1972; Scher & Cooper, 1989; see also Cooper & Fazio, 1984). With a greater sense of personal responsibility comes a greater amount of dissonance to be reduced.

Consider the phenomenon of postdecision dissonance reduction and the common dilemma of whether to keep something one has (omission) or trade it in for something else (commission). Research on the commission/omission distinction suggests that a bad decision in this situation will loom larger to the person who makes the trade than the person who keeps what he or she has. If so, will an unfortunate decision to trade create more dissonance and therefore generate more dissonance reduction than an unfortunate decision to keep what one has? It is this question that the present research was designed to address. We were interested in whether people would be more inclined to rationalize negative outcomes that stemmed from an active decision to change course than a parallel decision to stick with one's present lot.

Besides being an interesting question in its own right, we were drawn to this issue because of its implications for the experience of regret. In particular, it seemed that a tendency to reduce dissonance more for unfortunate actions than unfortunate inactions could help reconcile an apparent conflict between the results of previous research on regret and a common observation from everyday life. Experiments employing hypothetical scenarios indicate that people believe they would regret a negative outcome that stemmed from an action more than an equivalent outcome that stemmed from an inaction (Gleicher et al., 1990; Kahneman & Miller, 1986; Landman, 1987). For instance, someone who sells one stock and buys another only to lose money on the deal is predicted to feel worse than someone who considers switching stocks but stays with the original and then loses an equal amount of money (Kahneman & Tversky, 1982).

Compelling as such examples might be, however, they seem to conflict with an observation that presents itself in everyday life. In particular, when one asks people what they regret most in life, it is typically things they have not done that are mentioned. In fact, research conducted in our own lab has documented that people's biggest regrets tend to involve their failures to act (Gilovich & Medvec, in press). This suggests (among other possibilities) that there may be an interesting temporal pattern to the experience of regret due to action and inaction—namely, we may regret our unfortunate actions more in the short term but nevertheless regret our failures to act more in the long run. And, with respect to our present purposes, it may be the differential dissonance reduction induced by actions and inactions that is partly responsible for this temporal pattern. Precisely because our regrettable actions hurt more in the short term, they elicit more immediate psychological work designed to deal with the pain—more dissonance reduction. By coming to grips more quickly with our regrettable actions, it is our unfortunate inactions that will endure and seem more troublesome in the long run. (See Gilovich, Kerr, & Medvec, 1993, and Gilovich & Medvec, in press, for a discussion of additional causes of this temporal pattern to regret.)

With these issues as the theoretical backdrop for the present research, we sought to investigate whether people do indeed reduce dissonance more for their commissions than their omissions.

METHOD

Overview

Our procedure was modeled after the "three doors," or "Monty Hall," problem. Subjects were told that for participating in the experiment they would receive either a grand prize or a modest prize, depending on the decisions they made. The prizes were located in three boxes, and as on Monty's "Let's Make a Deal" television program, the subject's first task was to choose one of the boxes. Later on, the subject confronted a choice of whether to keep his or her unopened box or to trade it for one of the remaining boxes. Before the choice was made, the experimenter, like Monty, opened one of the two remaining boxes and revealed a modest prize. The subject's choice, then, was whether to keep his or her box or exchange it for the remaining one.
The experiment was arranged so that regardless of the subject's choice—to stay or switch—he or she received a modest prize. We were interested in whether subjects who lost out on the grand prize because of a direct action (those who switched boxes) would experience more dissonance than those who experienced the same fate after deciding not to act (those who kept their initial boxes). As in nearly all postdecision dissonance experiments, we did not measure dissonance itself, but dissonance reduction (cf. Brehm, 1956; Frenkel & Doob, 1976; Knox & Inkster, 1968; Regan & Kilduff, 1988; Younger, Walker, & Arrowood, 1977). We examined whether subjects who switched boxes rationalized their choices more than those who kept their original boxes by coming to view the modest prize they received as more attractive. The responses of both groups of subjects were, in turn, compared with those of subjects in a control condition whose "decisions" were determined randomly.

The Monty Hall, or three doors, problem was chosen as the backdrop of our experiment for one important reason: The choice of either decision—to stay or switch—can be made to seem compelling. The correct response to the problem is to switch. By doing so, one has a 67% chance of receiving the grand prize (and thus only a 33% chance of receiving it by sticking with one's original choice). Thus, if we experimenters want a randomly selected group of subjects to switch, we simply need to get them thinking along the lines of the correct solution to the underlying probability problem. At the same time, however, most people's intuitions tell them they should stick with their original choice. (The common view is that with only two remaining unopened boxes, there is a 50-50 chance that the grand prize is in either one. With no apparent reason to favor one over the other, most people think it best to stick with their original choice—much as students are coached to stick with their initial hunch when uncertain about an answer on a multiple choice test.) Consequently, another group of randomly selected subjects could be led to keep their original boxes by inducing them to follow their intuitions.

Subjects

The subjects were 120 students in an introductory psychology course at Cornell University who volunteered to participate in an experiment on group decision making. Subjects were recruited by telephone and told that they would receive a prize for participating in the study. Forty subjects were randomly assigned to each of three conditions—stay, switch, and control.

Procedure

When the subject reported to the laboratory, he or she was met by the experimenter and introduced to his or her "partner" for the study of group decision making. In reality, the partner was a confederate whose role was to influence the subject's choice of boxes so as to fit the randomly assigned condition. (Four persons, all female, acted as confederates.) Immediately after the introductions, the subject and confederate team was asked to choose one of three boxes. The subject and confederate were told that two of the boxes contained unnamed "modest" prizes and one box contained a "grand" prize. The confederate had been coached to hold back during this phase, and in all cases the true subject made the selection.

The confederate and subject (chosen box in hand) were then led down a hallway to a cubicle large enough for only one person. They were instructed to take turns entering the cubicle and typing into a computer some background information that was to be kept separate from the rest of the data they would provide during the experiment. While the subject was in the cubicle, the experimenter and confederate engaged in a switch of boxes if it was necessary to ensure the proper outcome.

The two team members were then led back down the hallway (with the chosen box once again in the true subject's hand) to another laboratory and seated together at a large table. The reason for all this walking back and forth with the box in the subject's hand was to create a sense of ownership of the box on the part of the subject—to get the box into the subject's psychological "endowment" (Thaler, 1980). Once in the new lab, the experimenter pointed out two tables, one labeled modest prizes containing such items as a Cornell bumper sticker, a note pad, and a novelty button, and the other labeled grand prizes containing such items as a Cornell T-shirt, tickets to a local movie theater, and a Cornell mug. The subject and confederate were then asked to make a second joint decision—whether they wanted to keep their original box or exchange it for one of the other two. As on the television show "Let's Make a Deal," the experimenter made the choice more focused by opening one of the two remaining boxes to reveal a modest prize. The choice, then, was whether to keep the original box or trade it for the other unopened box. The experimenter explained to the two team members that they had to agree on what choice to make and that they would each get the same modest or grand prize, depending on their choice. They were shown a tape recorder on the table in front of them and told that their discussion would be recorded. The experimenter then left the room while the two deliberated, asking to be summoned when they had finished.

The confederate's role in the experiment was to ensure that the choice appropriate to the randomly assigned condition—stay or switch—was made and to do so in such a way that the subject felt responsible for the
outcome. The confederate accomplished this by "hanging back" initially and ascertaining the subject's preference. If the subject's preference was consistent with the assigned choice, the confederate did nothing but ask the subject to articulate the reasons for his or her preference and then go along with it. If the subject's initial preference was inconsistent with the assigned choice condition, the confederate's task was more difficult. The confederate once again asked the subject to articulate the reasons for his or her preference, muttered a "Hmmm" to indicate that the logic was not entirely persuasive, but did not offer an alternative logic or preference of her own. The confederate then proceeded to use a variant of the Socratic method to lead the subject to the assigned choice. In particular, the confederate asked questions designed to elicit arguments consistent with the assigned choice. Questions designed to elicit common intuitions ("Isn't it 50-50 either way?") were asked to encourage subjects to stay, and queries designed to get at the correct solution to the problem ("Didn't we have a 1 in 3 chance of picking the grand prize initially?") were asked to encourage subjects to switch.

Once the subject endorsed the choice consistent with the assigned condition, the confederate asked, "Let me get this straight. Why are we staying/switching? Could you run the logic by me one more time?" This was done in a final effort to get the subject to feel responsible for the choice regardless of how much input the confederate actually had in the deliberations.

After the team made its decision, the experimenter was summoned and the choice reported. The subject was then induced to open the unchosen box, thus revealing an attractive, expensive-looking Cornell T-shirt. The subject then opened the chosen box, which contained a much less desirable Cornell bumper sticker. The confederates were coached to avoid showing any reaction when the prizes were revealed, to rule out the possibility of any systematic influence on the dependent measure.

The finality of the decision to stay or switch was emphasized by having the members of the decision team fill out a receipt with a bumper sticker in front of them. The receipt was a rather detailed one that asked subjects what choice they made, what prize they received, and what prize they would have received had they made the opposite choice. This served to reinforce our independent variable manipulation. Then, to allow sufficient time for dissonance reduction to occur, the subjects filled out two questionnaires. The first was a 10-item decision-making survey that asked such things as what courses in decision making, game theory, and so forth the subject had taken. The second questionnaire was preceded by a brief speech by the experimenter about how decision theorists have conceptualized decision making as the product of two factors, subjective probability and personal value.

Therefore, to understand the nature of the subjects' decisions, it was necessary to get a sense of how much they valued the different possible prizes in this experiment. The subject and confederate were therefore asked individually to rate all the grand and modest prizes by placing an X on a 96-mm line labeled not desirable at all at one end and very desirable at the other.

**Dependent Measure**

The last item on this questionnaire served as the dependent measure. This item required subjects to indicate the least amount of money we would have to give them to buy back the bumper sticker (which retailed for $1.10 in the campus store). It was expected that subjects who switched boxes and ended up with the modest prize would come to value it more highly than those who kept their original box.

**Control Condition**

Control subjects were run individually without a confederate and were told as they entered the lab that they were in the control condition of an experiment on decision making. They were informed that subjects in the experimental conditions made a number of choices throughout the experiment, and at each point at which a decision was made by an experimental subject, their own "choice" would be determined randomly by spinning a roulette wheel. Thus, the control subjects' initial choice of one of three boxes and their subsequent choice of whether to trade in their initial box were determined randomly. The procedure was rigged so that all the control subjects received bumper stickers as prizes and were shown the T-shirts they could have received had their "luck" been different. The control subjects then filled out the receipt and rating forms just as subjects in the stay and switch conditions did.

At the conclusion of the experiment, all subjects were thoroughly debriefed and any suspicions they had were ascertained. The subjects were allowed to keep the bumper stickers they had won.

**RESULTS**

Preliminary analyses involving sex of subject revealed no significant main effects or interactions, so this variable was ignored in all subsequent analyses.

The mean amount of money that subjects in each condition demanded in exchange for the bumper sticker conformed closely to our hypotheses. Subjects who switched boxes and ended up with the bumper sticker demanded more money from us to buy it back (M = $1.58) than those who received the same prize by sticking with their original box (M = $1.11). Both groups, in turn, demanded more money for the bumper sticker.
than subjects in the control condition ($M = \$1.00$).

These data were analyzed through a one-way analysis of variance, which revealed a significant effect of condition, $F(2, 117) = 4.05, p < .025$. Further analyses indicated that the switch condition differed significantly from both the stay and control conditions, $t(117) = 2.16, p < .05$, and $t(117) = 2.68, p < .01$, respectively. Although subjects in the stay condition assigned higher monetary values to the bumper sticker than those in the control condition, this difference was not significant, $t < 1$.

One question raised by the overall pattern of data is whether the true state of affairs is best characterized as a linear pattern from control to stay to switch (as suggested by the pattern of means) or as a simple difference between the switch condition and the other two (as suggested by the tests of significance). Is there any difference, in other words, between ending up with a disappointing prize after deciding to keep one's original box versus having the choice determined randomly? Although there was no reliable difference between the stay and control conditions, there was reason to believe that the monetary values provided by subjects in the control condition were artificially high and therefore might not offer a true test of the underlying question. In particular, the experimenter noted that subjects in the control condition appeared to have an inordinate amount of fun playing with the roulette wheel, and this led to the conjecture that these subjects' elevated mood might have inflated the monetary value they assigned to the bumper sticker (Isen et al., 1978).

Coincidentally, we were without the roulette wheel for seven of the control trials (for these subjects, the "random" decisions were based on their subject numbers and the last two digits of their local telephone numbers). A post hoc analysis of the buy-back prices from these seven subjects revealed a substantially lower mean ($M = \$ .75$) than that of the other 33 control subjects ($M = \$1.06$) who had so much fun with the roulette wheel.

This rather large difference prompted us to run a new group of 40 control subjects whose random choices were determined in the same way as the 7 previous control subjects who were run without the roulette wheel. These additional subjects were recruited from the same pool as before. As expected, the mean buy-back value from these subjects ($M = \$ .78$) was lower than that obtained in the original control condition. As Figure 1 indicates, these data lend credence to our original hypothesis that a decision to switch, a decision to stay, and a decision made randomly do indeed all have different effects on subjects' valuations. A contrast using all four conditions—switch (+2), stay (+1), original control (−2), and additional control (−2)—was highly significant, $F(1, 156) = 14.44, p < .001$. Furthermore, when the data from the control subjects who were run without the roulette wheel were compared with the data from those in the stay condition, a marginally significant difference was obtained, $t(156) = 1.74, p < .10$. Of course, because the data from the additional control subjects were collected after the other conditions were run, all these subsidiary analyses should be interpreted with caution.  

**Figure 1** Mean dollar value assigned to the bumper sticker, by condition.

**DISCUSSION**

It appears that people do indeed reduce dissonance more for their errors of commission than their errors of omission. Subjects who switched *from* a box that was subsequently found to contain a grand prize tended to value the modest prize they received more highly than those who failed to switch *to* the box containing the grand prize. Subjects in the switch condition also valued the bumper sticker more highly than subjects in a control condition whose choices were randomly determined. There was some evidence that subjects in the stay condition likewise valued the bumper sticker more highly than those in the control group.

These results both complement and extend previous findings in the counterfactual thinking literature. Previous investigators have presented data indicating that people may feel more pain over a bad outcome that stems from an action taken than an action forgone (Gleicher et al., 1990; Kahneman & Miller, 1986; Kahneman & Tversky, 1982b; Landman, 1987). What we have shown is that this initial sting of regrettable action can be undone by the process of dissonance reduction (Festinger & Walster, 1964). Because action tends to depart from the norm more than inaction, the individual is likely to feel more personally responsible for an unfortunate action. Thus, subjects who switched boxes in our experiment were more likely to experience a sense of "I brought this on myself" or "This need not have happened" than subjects who decided to keep their initial
box. Both the greater pain experienced by subjects in the switch condition and their greater sense of responsibility for that pain appear to have led them to reduce the dissonance brought on by their choice by coming to value their consolation prize more highly. Our results thus reinforce the significance of the action/inaction distinction emphasized in the counterfactual thinking literature—and, significantly, our findings were obtained in an experiment that goes beyond the hypothetical scenario methodology employed by others.

Alternative Interpretations

Several aspects of the experiment merit further discussion. One concerns the relative ease of inducing subjects to stay or switch. Because people believe there is a 50-50 chance that the grand prize is in either unopened box (Engel & Venetoulia, 1991; Tierney, 1991; vos Savant, 1990a, 1990b, 1991), and because people fear the prospect of giving up the winning box, they typically prefer to keep the box they have. Thus, it should have been easier to get subjects in the stay condition to keep their boxes than to induce those in the switch condition to trade them in. Perhaps, then, subjects in the switch condition experienced more dissonance, not because they engaged in commission rather than omission, but because, by acting at variance with their initial inclinations, they performed a more counterattitudinal or dissonant act.

We have two responses to this alternative interpretation. The first is empirical. Although it was not always possible to ascertain whether a given subject initially wanted to stay or switch, our confederates were instructed to make a note after each session of anything that might indicate the subject's original inclinations. We subsequently reviewed these comments and, although blind to the dependent measure, judged whether each subject was inclined to stay, was inclined to switch, or exhibited no preference. This analysis strongly reinforced the underlying premise of the alternative interpretation: Of the 80 subjects in the two experimental conditions, more than three times as many were clearly inclined to stay (n = 40) as were clearly inclined to switch (n = 12). Getting a subject to switch, then, did indeed require more from our confederates than getting someone to stay.

Further analyses, however, revealed that this difference was not responsible for our results. Although the 22 subjects in the switch condition who were originally inclined to stay assigned a higher value to the bumper sticker (M = $1.58) than the 8 who wanted to switch all along (M = $1.38), the difference was not large and did not approach statistical significance, t < 1. Furthermore, according to this alternative interpretation, subjects in the stay condition who originally wanted to switch should likewise have valued the bumper sticker more highly than those who wanted to stay initially. In fact, the opposite pattern was obtained. The 4 subjects who wanted to switch but were induced by the confederate to stay assigned a much lower value to the bumper sticker (M = $.38) than the 18 subjects who wanted to stay originally (M = $1.12). Clearly, then, the observed significant difference between the stay and switch conditions (i.e., our central finding) is not the result of subjects in the switch condition engaging in a more counterattitudinal behavior.

The second reason to doubt this alternative interpretation is that it is equally plausible—indeed, we believe more plausible—that the difference in inducing subjects to stay or switch would produce the opposite pattern of results. The fact that we had to exert more effort to induce subjects to switch should have made subjects in the switch condition feel less responsible for the outcome than those in the stay condition. With a lessened sense of responsibility (Cooper & Fazio, 1984; Wicklund & Brehm, 1976), there should have been less dissonance, and hence less dissonance reduction, in the switch condition—precisely the opposite of the pattern of results we obtained.

A second methodological issue we should note (one mentioned previously, in fact), is that we did not measure dissonance reduction directly but instead inferred its existence from the pattern of subjects' valuations. It is important to bear in mind, however, that our research was not undertaken in a vacuum. There are numerous demonstrations in the literature that dissonance-induced arousal does indeed occur in the precise situations specified by dissonance theory. Most of these demonstrations have employed the induced compliance paradigm (Cooper, Zanna, & Taves, 1978; Croyle & Cooper, 1983; Zanna & Cooper, 1974), but dissonance-induced arousal has been observed in postdecision contexts as well (Gerard, 1967). Our interpretation of our findings—that it is dissonance reduction we have observed—thus rests to a significant degree on these previous findings in the literature. As stated previously, we are in good company in this regard, as nearly all research on postdecision dissonance reduction has employed the same strategy (Brehm, 1956; Frenkel & Doob, 1976; Knox & Inkster, 1968; Regan & Kilduff, 1988; Younger et al., 1977).

Note also that our treatment of the omission/commission distinction conflates action versus inaction, on the one hand, with status quo versus change, on the other. In other words, when a subject in our experiment keeps his initial box, he both declines to act (omission) and keeps his original box (status quo). In contrast, when a subject switches boxes, her action (commission) results in her receiving a different box (change). These
two dimensions can be manipulated orthogonally (Ritov & Baron, 1992), albeit in ways that might seem strange to the involved subjects (e.g., if the subject does nothing, his or her chosen box will be switched; a subject’s original choice will be preserved only by taking explicit action). We chose not to disentangle these two dimensions, for the simple reason that they are typically conflated in the real-world contexts to which we want our results to generalize. We must act to get a new job, but we need not do anything different to keep our current one. We must do things to change homes (contact a real estate agent, look at other houses, etc.), but nothing special is required to remain in our present residence. We must contact a broker to trade stocks, but nothing needs to be done to maintain our current holdings.

Up to this point we have discussed our results entirely in terms of the theory of cognitive dissonance, without mention of whether self-perception processes may be involved (Bem, 1972). Postdecisional attitude change, of course, was one of the phenomena for which self-perception theory was offered as an explanatory alternative to the theory of cognitive dissonance. Can it serve as an alternative explanation of the present findings? We think not. The reason is most readily seen by putting oneself (as self-perception theory prescribes) in the role of an outside observer witnessing the behavior of our subjects. Because subjects do not know the contents of the unopened boxes, their decisions to stay or switch tell us nothing about their attitudes about the bumper sticker they received. Only if the outcome were predictable would an observer be justified in drawing any inference about the subjects’ tastes. That was clearly not the case in this experiment, and so the theory cannot predict the present results.

To check our analysis, we conducted an “interpersonal simulation” of the present experiment (Bem, 1967, 1968). We described to one group of respondents the exact procedure that subjects in the switch condition had followed, and to another group we described the details of the stay condition. Both groups of respondents (n = 25 in each) were then asked to estimate the amount of money a typical subject in the respective condition would demand to give back the bumper sticker. As expected, the interpersonal simulation did not reproduce the results obtained in the experiment proper. The responses of the two groups of simulation subjects did not differ, t < 1.

If the outcome of the decision to keep or switch boxes was not sufficiently foreseeable to support a self-directed inference process, how could it have been sufficiently foreseeable to arouse cognitive dissonance? After all, the existence of foreseeable negative consequences has been shown to be a necessary precondition for the arousal of dissonance as well (Cooper, 1971; Cooper & Worchel, 1970; Goethals et al., 1979). The answer, we submit, lies in the difference between what people believe they can foresee and what they believe others can foresee. The outcome of the decision to keep or switch boxes is objectively unforeseeable, and so subjects are unlikely to credit another person with the ability to predict it. However, given that people are subject to a host of irrationally optimistic biases (see Taylor, 1989) such as the illusion of control (Langer, 1975), they may nonetheless think that the outcome was foreseeable to them. It is not uncommon, for example, to hear people react to a perfectly random negative event with the expression “I knew it!” or “I should have known.” It is this vague, retrospective sense that the outcome was predictable that presumably enables the arousal of dissonance. Our experiment, then, may serve to identify a class of postdecisional contexts in which the theory of cognitive dissonance, but not self-perception theory, can account for attitude change.

Implications for Regret

As discussed at the outset, the present research may help to reconcile an apparent conflict between the existing literature on regret and an observation from everyday life. Research on counterfactual thinking indicates that, holding outcome constant, people regret actions taken more than actions forgone (Gleicher et al., 1990; Kahneman & Miller, 1986; Landman, 1987). In contrast, everyday observation suggests that people’s biggest regrets tend to center on things they have failed to do—an observation that has been verified empirically (Gilovich & Medvec, in press). Together, these divergent findings suggest that there may be a temporal pattern to people’s regrets over their actions taken and actions forgone. Our unfortunate actions may hurt more in the short term, but over time it may be our regrettable failures to act that stand out and elicit thoughts about “what might have been.”

The present results are certainly consistent with—indeed, could help explain—such a temporal pattern. Precisely because our regrettable actions hurt more in the short term, we tend to do more to deal with them. We are more likely to initiate remedial actions to compensate for them. If we marry the wrong person, we get divorced; if we choose the wrong line of work, we switch careers. We tend to make material changes to our lives in order to erase the unfortunate action. And what we cannot accomplish through material changes, we tend to deal with psychologically: We do more psychological work to deal with the pain of our regrettable actions. As the present results indicate, we reduce dissonance more for our regrettable actions than our regrettable inactions. Thus, some of the sting is taken out of our unfortunate actions, making our failures to act more troubling.
in the long run (see also Gilovich et al., 1993; Gilovich & Medvec, in press).

Our results reinforce the claims of those working on the subject of counterfactual thinking: Regrettable actions pack a bigger punch than regrettable inactions. Consequently, people exhibit something of an "omission bias" and are loath to engage in various actions in anticipation of future regret and recrimination (Spranca et al., 1991). At the same time, however, our results suggest that we may deal with the pain of action better than we think. As Kahneman and Snell (1990) have found, accurately predicting our own adaptations is difficult. Thus, to the extent that we base our current actions on anticipated future regrets, perhaps we should strive to resist the pull of the omission bias and be more bold and venturesome. Or, to put it in more currently popular terms, perhaps we should more often "just do it."

NOTES

1. One version of the correct solution is as follows: A person’s chances of initially picking the box with the grand prize are 33%. Because the person already knows that the "host" can only open a box containing a modest prize (uncovering the grand prize would eliminate the dilemma), nothing of significance to the subjects’ task is learned when the contents of one of the boxes are revealed. Because nothing significant has changed, the chances that the person’s original box contains the grand prize are still 33%, and therefore the person should switch to the other box, which has a 67% chance of having the grand prize.

A more compelling explanation of the correct solution can be obtained by simply listing all combinations of (a) the person’s choice of initial box, (b) the person’s choice to stay or switch, and (c) which of the two remaining boxes the "host" opens. Such a list readily reveals that one wins 67% of the time when one switches and only 33% of the time when one stays.

The reader may wish to consult Engel and Venotoulias (1991), Tierney (1991), and vos Savant (1990a, 1990b, 1991) for accounts of the rather entertaining and controversial history of this problem.

2. Various complications led us to discard the data from six additional subjects. Two subjects turned out to be acquaintances of the experimenter. Two others were suspicious of the confederate and the alleged purpose of the experiment. A final pair of subjects worked in the Cornell Campus Store and claimed that they could get our modest prize—a Cornell bumper sticker—free, thus rendering our "buy back" dependent measure meaningless.

3. Subjects in the stay condition had to be in possession of one of the two modest prizes. Subjects in the switch condition needed to possess the grand prize.

4. Although the bumper sticker was one of the items rated earlier in the questionnaire, the ratings were not part of our dependent measure. The reason is that past research suggests that endorsement effects (the increase in value of an item due to sheer personal ownership) seem to be revealed through monetary valuation, but not through scale ratings (cf. Kahneman, Kretsch, & Thaler, 1991; Loewenstein & Kahneman, 1991; but see Beggin, 1992, for an exception). Because our investigation depends so heavily on there being something of an endorsement effect (i.e., phenomenological, the choice had to be one of "staying vs. switching" rather than "one option or another"), we chose our dependent measure in light of this result from past research.

Nevertheless (of course), we felt compelled to analyze subjects’ ratings of the bumper sticker. Ratings by subjects in the switch condition were higher (M = 8.1) than in the stay condition (M = 7.4), but both were lower than in the control condition (M = 9.7). None of these differences was significant, however, p > .2. Furthermore, these ratings were not significantly correlated with our "buy back" dependent measure, r = .16.

5. The reader may be interested in whether the observed difference between the stay and switch conditions was mimicked to any extent by the responses of the control subjects who, through the luck of the wheel, ended up staying or switching. Collapsing across the two control conditions, the 35 subjects who switched boxes assigned a slightly higher value to the bumper sticker (M = $.58) than the 45 subjects who kept their initial box (M = $.86). This difference, though in the same direction as our primary results, was not statistically significant, t < 1.

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