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Outcome Bias in Decision Evaluation

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In 5 studies, undergraduate subjects were given descriptions and outcomes of decisions made by others under conditions of uncertainty. Decisions concerned either medical matters or monetary gambles. Subjects rated the quality of thinking of the decisions, the competence of the decision maker, or their willingness to let the decision maker decide on their behalf. Subjects understood that they had all relevant information available to the decision maker. Subjects rated the thinking as better, rated the decision maker as more competent, or indicated greater willingness to yield the decision when the outcome was favorable than when it was unfavorable. In monetary gambles, subjects rated the thinking as better when the outcome of the option not chosen turned out poorly than when it turned out well. Although subjects who were asked felt that they should not consider outcomes in making these evaluations, they did so. This effect of outcome knowledge on evaluation may be explained partly in terms of its effect on the salience of arguments for each side of the choice. Implications for the theory of rationality and for practical situations are discussed.

> A fault condemned but seldom avoided is the evaluation of the intention of an act in terms of the act's outcome. An agent who acted as wisely as the foreseeable circumstances permitted is censured for the ill-effects which come to pass through chance or through malicious opposition or through unforeseeable circumstances. Men desire to be fortunate as much as they desire to be wise, but yet they fail to discriminate between fortune and wisdom or between misfortune and guilt. . . . We are ingenious in 'discovering' the defect of character we believe would account for a person's misfortune. (Arnauld, 1662/1964, p. 285)

> Since good decisions can lead to bad outcomes (and vice versa) decision makers cannot infallibly be graded by their results. (Brown, Kahr, & Peterson, 1974, p. 4)

> A good decision cannot guarantee a good outcome. All real decisions are made under uncertainty. A decision is therefore a bet, and evaluating it as good or not must depend on the stakes and the odds, not on the outcome. (Edwards, 1984, p. 7)

Evaluations of decisions are made in our personal lives, in organizations, in judging the performance of elected officials, and in certain legal disputes such as malpractice suits, liability cases, and regulatory decisions. Because evaluations are made after the fact, there is often information available to the judge that was not available to the decision maker, including information about the outcome of the decision. It has often been suggested that such information is used unfairly, that reasonable decisions are criticized by Monday-morning quarterbacks who think they might have decided otherwise, and that decision makers end up being punished for their bad luck (e.g., Arnauld, 1662/1964; Berlin, 1984; Nichols, 1985).

The distinction between a good decision and a good outcome is a basic one to all decision analysts. The quotation from Edwards (1984) cited earlier is labeled by the author as "a very familiar elementary point" (p. 7). In this paper, we explore how well the distinction between decisions and outcomes is recognized outside the decision-analysis profession.

Information that is available only after a decision is made is irrelevant to the quality of the decision. Such information plays no direct role in the advice we may give decision makers ex ante or in the lessons they may learn (Baron, 1985, chapter 1). The outcome of a decision, by itself, cannot be used to improve a decision unless the decision maker is clairvoyant.

Information about possible outcomes and their probabilities falls into three relevant classes: *actor information,* known only to the decision maker at the time the decision is made; *judge information,* known only to the judge at the time the decision is evaluated; and *joint information,* known both to the decision maker at the time of decision and to the judge at the time of evaluation. (In some cases, the decision maker and the judge will be the same person, at different times.) In the cases we consider, the judge has the outcome information and the actor does not.

Although outcome information plays no direct role in the evaluation of decisions, it may play an appropriate indirect role. In particular, it may affect a judge's beliefs about actor information. A judge who does not know the decision maker's probabilities may assume that the probability was higher for an outcome that occurred than for the same outcome had it not occurred. (Note, however, that outcome information tells us nothing about the *utilities* of a decision maker, even if we have no other information about them.) In the extreme, if we have no information except outcome, it is a reasonable prima facie hypothesis that bad outcomes (e.g., space-shuttle accidents) result from

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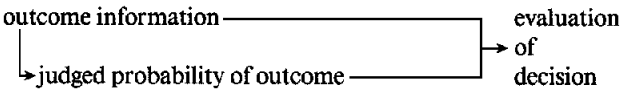
badly made decisions. We do not usually set up commissions of inquiry to delve into policy decisions that turn out well.

Another appropriate indirect role of outcome information is that it allows decision makers to modify beliefs about probabilities in similar situations. If they know nothing about the proportion of red cards in a deck, they can learn something about that proportion by drawing cards from the deck. (However, if they know that the deck is an ordinary one, sampled with replacement, they learn nothing by drawing cards.) This effect of outcome information can operate only within a sequence of similar decisions, not in a single decision.

At issue here is whether there is an *outcome bias,* in which people take outcomes into account in a way that is irrelevant to the true quality of the decision. This sort of bias is not established by showing that people take outcomes into account. As we argued earlier, outcomes are relevant when they can inform us about actor information. One way to show an outcome bias is to give the judge all relevant information about outcome probabilities known to the decision maker, plus the outcome. That is, there is only joint information and judge information (the outcome), no actor information.

Information (relevant or irrelevant) may have two effects on evaluations: (a) an effect on the judged probability of outcomes, which, in turn, affects evaluation; and (b) a direct effect on the judged quality of the decision, as shown below:



For example, we may think a decision is bad if we believe that bad outcomes were highly probable, but outcome information may also affect our evaluation even if the probability of an outcome is known.

Fischhoff (1975) demonstrated the existence of a *hindsight bias,* an effect of outcome information on the judged probability of an outcome. Subjects were given scenarios and asked to provide probabilities for different outcomes. When subjects were told the outcome and asked what probability other subjects who did not know the outcome (or they themselves if they did not know it) would give, they gave higher probabilities than those given by actual other subjects not told the outcome (or told that some other outcome had occurred). Note that these demonstrations filled our condition of eliminating actor information (where the actors were the other subjects). Subjects were asked to judge the probability for someone who had exactly the same information they had (except for outcome), no more.

Although it seems likely that the hindsight bias would lead to biased evaluations of decision quality, this has not been shown, nor is it what we seek to show here. Rather, we seek a direct effect of outcome on evaluation of decisions, an effect that does not operate through an effect of outcome knowledge on a judge's assessed probabilities of outcomes. To this end, we held probability information constant by telling subjects that probabilities were known, or by otherwise limiting probability information. Of course, in real life, the outcome bias we seek could work together with the hindsight bias (as shown in the diagram) to distort evaluations of decisions even more than either bias alone.

Zakay (1984) showed that managers counted good outcomes as one of the criteria for evaluating decisions made by other managers. However, as we have argued, it is perfectly reasonable to do this when there are facts known only to the decision maker (actor information). At issue in this article is not whether people use outcome information but whether there are conditions under which they overuse it. Thus, we look for an effect of outcome information when the subject is told everything that is relevant. In this case, outcome should play no role in our evaluations of decisions, although we hypothesize that it will.

The outcome bias we seek may be related to Walster's (1966) finding that subjects judged a driver as more "responsible" for an accident when the damage was more severe. However, questions about responsibility might be understood as concerning the appropriate degree of punishment or blame rather than rationality or quality of decision making. As a general rule, it makes sense to punish actors more severely for more severe consequences; it is usually difficult to know what the actor knew, and severity of consequences is a clue as to the degree of negligence. Even when we know what the actor knew, use of this general rule may set clearer precedents for others (as in the utilitarian rationale for "punishing the innocent"). Walster apparently intended the question about responsibility to tap subjects' beliefs about the extent to which the driver could have prevented the accident by acting differently. Walster suggested that her results were due to subjects' desire to believe that events were controllable: If bad outcomes are caused by poor decisions or bad people, we can prevent them by correcting the decision making or by punishing the people. If subjects interpreted the question this way, they would be making an error, but not the same error we seek in this study.

Similarly, studies of the effect of outcomes on children's moral judgments (e.g., Berg-Cross, 1975; Leon, 1982; Stokes & Leary, 1984; Surber, 1977) have used judgments of responsibility, deservingness of punishment, and badness, each of which could be appropriately affected by outcome. Also, in most cases no effort was made to provide the judge with all relevant information available to the actor.

Mitchell and Kalb (1981) also showed effects of outcome knowledge on judgments of both responsibility for outcomes and outcome probability. Subjects (nurses) read descriptions of poor performance by nurses (e.g., leaving a bed railing down) that either resulted in poor outcomes (e.g., the patient fell out of bed) or benign outcomes. In fact, outcome knowledge affected both probability judgments and responsibility judgments. Although the former effect might have been a hindsight bias, it might also have been an appropriate inference about actor information: Outcome information might have provided information about factors that affected outcome probability from the decision maker's viewpoint (e.g., whether the patient was alert and, if not, whether she slept fitfully). Mitchell and Kalb argued that the effect of outcome on probability did not explain the effect on responsibility judgment: The correlation between judged probability and judged responsibility, with outcome held constant, was nonsignificant across subjects. Of course, the problem still remains that the term *responsibility* need not refer only to quality of the decision.

In our experiments, instead of examining the correlation between outcome judgments and probability judgments, we fixed the outcome probabilities by telling the subjects what they were

from the decision maker's viewpoint. We also explicitly asked about the "quality of thinking." All decisions were expressed in the form of gambles. For example, an operation may lead to a cure or to death, with given probabilities. We gave the subjects probabilities of all possible outcomes and brief descriptions of each outcome. It is reasonable to assume that the quality of the decision would depend on the probabilities of the outcomes—which summarize all the information we have about uncertain states of the world that could affect the outcome—and the desirabilities or utilities of the outcomes. Although we did not provide all necessary information about desirabilities, the outcome provided no additional information on this score. In our studies an outcome bias existed when the evaluation of the decisions depended on their outcomes.

We expected to find an outcome bias because the generally useful heuristic of evaluating decisions according to their outcomes may be overgeneralized to situations in which it is inappropriate. It may be learned as a rigid rule, perhaps from seeing punishment meted out for bad outcomes resulting from reasonable decisions.

Of course, it can often be appropriate to use outcome information to evaluate decision quality, especially when actor information is substantial relative to judge information or joint information and when it is necessary to judge decisions by their outcomes (as fallible as this may be) simply because there is little other useful information. This is especially true when decision makers are motivated to deceive their evaluators about the nature of their own information.

Ordinarily, it is relatively harmless to overgeneralize the heuristic of evaluating decisions according to their outcomes. However, when severe punishments (as in malpractice suits) or consequential decisions (as in elections) are contingent on a judgment of poor decision making, insight into the possibility of overgeneralization may be warranted.

A second reason for outcome bias is that the outcome calls attention to those arguments that would make the decision good or bad. For example, when a patient dies on the operating table, this calls attention to the risk of death as an argument against the decision to perform surgery. When subjects attempt to reexamine the arguments to consider what they would have thought if they had not been told the outcome, the critical information remains salient. Fischhoff (1975) found an analogous mechanism to be operating in hindsight bias. When subjects were asked to rate the relevance to their judgment of each item in the scenario, the relevance of the items depended on the outcome subjects were given. Note that the salience of an argument based on risk or possible benefit may not be fully captured by a description of the subjective probability and utility of the outcome in question.

One type of argument for or against a decision concerns the difference between outcomes resulting from different decisions in otherwise identical states of the world. For example, a decision to buy a stock or not may compare one's feelings about buying or not buying if the stock goes up (rejoicing vs. regret), or if the stock goes down. Regret theory (Bell, 1982; Loomes & Sugden, 1982) explicitly takes such differences into account in explaining choice. Once the true state is revealed (e.g., the stock goes down), the judge may overweigh the regret associated with this state (the difference between buying and not buying in this case) when judging decision quality.

Another type of argument is that a bad outcome may be avoided by considering choices other than those considered so far, or by gathering more information about probabilities (Toda, 1984). Such arguments are equally true regardless of whether the outcome is good or bad (Baron, 1985), but a bad outcome may make them more salient. In many of our examples, there is no possibility of additional choices or information.

A third reason for outcome bias is that people may regard luck as a property of individuals. That is, people may act as if they believe that some people's decisions are influenced by unforeseeable outcomes. Such a belief might have been operating in the experiments of Langer (1975), who found that people were less willing to sell their lottery tickets when they had chosen the ticket number themselves than when the numbers had been chosen for them. Langer interpreted this finding (and others like it) in terms of a confusion between chance and skill, but the skill involved might have been the sort of clairvoyance described earlier. (The results of Lerner & Matthews, 1967, may be similarly explained.) Our experiments did not test this explanation directly, but we mention it here for completeness.

Experiment 1

Method

Materials and procedure. Subjects were given a questionnaire with a list of 15 medical decisions. They were asked to evaluate each decision on the following 7-point scale:

3 = clearly correct, and the opposite decision would be inexcusable;

2 = correct, all things considered;

1 = correct, but the opposite would be reasonable too;

0 = the decision and its opposite are equally good;

−1 = incorrect, but not unreasonable;

−2 = incorrect, all things considered;

−3 = incorrect and inexcusable.

The subjects were encouraged to use intermediate numbers if they wished and to explain answers that would not be obvious. They were reminded "to evaluate the decision itself, the quality of thinking that went into it."

The 15 cases are listed in Table 1. Case 1 read as follows:

> A 55-year-old man had a heart condition. He had to stop working because of chest pain. He enjoyed his work and did not want to stop. His pain also interfered with other things, such as travel and recreation. A type of bypass operation would relieve his pain and increase his life expectancy from age 65 to age 70. However, 8% of the people who have this operation die from the operation itself.[1] His physician decided to go ahead with the operation. The operation succeeded. Evaluate the physician's decision to go ahead with the operation.

Case 2 was the same except that the operation failed and the man died. Cases 3 and 4 paralleled Cases 1 and 2, respectively, except that

[1] The 8% figure was chosen on the basis of pilot data to make the decision appear difficult to the subjects.

Table 1
Conditions and Mean Ratings for Experiment 1

Case	Choice	Decision maker	Outcome	*M*	*SD*
1	Heart surgery	Physician	Success	0.85	1.62
2	Heart surgery	Physician	Failure	−0.05	1.77
3	Heart surgery	Patient	Success	1.00	1.05
4	Heart surgery	Patient	Failure	0.75	1.26
5	Liver surgery	Physician	Success	0.45	1.75
6	Liver surgery	Physician	Failure	−0.30	1.79
7	Liver surgery	Patient	Success	1.05	1.02
8	Liver surgery	Patient	Failure	0.35	1.24
9	Test, positive, treat	Physician	Success	1.40	1.83
10	Test, negative, treat	Physician	Success	1.15	1.75
11	Test, negative, treat	Physician	Failure	1.20	1.83
12	Test 1, Disease A	Physician	Success	−0.07	1.57
13	Test 1, Disease A	Physician	Failure	−1.30	0.71
14	Test 1, Disease B	Physician	Success	−0.22	1.69
15	Test 1, Disease B	Physician	Failure	−1.35	1.28

the man made the decision rather than the physician and the man's decision was the one that was evaluated. Cases 5 through 8 paralleled Cases 1 through 4, except that a liver ailment rather than a heart ailment was described.

Cases 9 through 11 involved a testing situation of the type studied by Baron, Beattie, and Hershey (in press). A test was described that had such poor accuracy that the best action, on normative grounds, would have been to treat the patient (for a foot infection with using an antibiotic) regardless of the test result. In Case 9, which was included for a purpose not addressed in this article, the test was positive and the disease was treated and cured. In Cases 10 and 11, the test was negative but the disease was treated anyway; it was cured in Case 10 but not in Case 11. Subjects were asked to evaluate whether the physician was correct in ordering the worthless test. A comparison of Cases 10 and 11, which differed in success versus failure, could also be used to look for an outcome bias.

Cases 12 through 15 concerned a choice between two tests in order to decide which of two diseases to treat (as studied by Baron & Hershey, in press). The two diseases, *A* and *B*, were considered equally likely. Test 1 indicated Disease A correctly in 92% of patients with *A* and Disease B correctly in 80% of patients with *B*. Test 2 indicated Disease A correctly in 86% of patients with *A* and Disease B correctly in 98% of patients with *B*. If *A* was treated (by surgery), the treatment was always successful, but if *B* was treated, the treatment was successful one third of the time. (Normatively, the two tests were equally good, because errors in detecting *A* were three times as costly as errors in detecting *B*). The physician always chose Test 1. In Cases 12 and 13, the test indicated *A*; in Cases 14 and 15, it indicated *B*. In Cases 12 and 14, the operation succeeded; in Cases 13 and 15, it failed. Subjects were asked to evaluate the physician's decision to perform Test 1.

The cases were presented in a within-subjects design. Cases to be compared were separated in the sequence as widely as possible. (The sequence used was 2, 5, 13, 10, 3, 8, 15, 9, 1, 6, 12, 11, 4, 7, and 14.) Note that a within-subjects design makes it easier to distinguish small effects from random error but at the cost of reducing the magnitude of effects because subjects may remember responses they gave to similar cases.

Subjects. Subjects were 20 undergraduates at the University of Pennsylvania, obtained through a sign placed on a prominent campus walkway and paid by the hour. Ten subjects did the cases in the order given; 10 did them in reverse order.

Results

In our analysis, we defined an outcome bias as the mean rating assigned to cases with positive outcomes minus the mean rating for cases with negative outcomes. Mean ratings of all cases are shown in Table 1. Overall, there was an outcome bias. Cases in which the outcome was success (Cases 1, 3, 5, 7, 10, 12, and 14) were rated higher than matched cases in which the outcome was failure (Cases 2, 4, 6, 8, 11, 13, and 15): mean effect = 0.70, $t(19) = 4.04$, $p < .001$, one-tailed. For the two orders, respectively, $ts(9) = 3.10$ and 2.51, both $ps < .025$. In 44.3% of the 140 pairs of cases that differed only in success or failure, higher ratings were given to the case with success; in 9.3% higher ratings were given to the case with failure, and in 46.4% equal ratings were given to the two cases. (Many subjects said that they remembered their responses to previous cases and repeated them regardless of the outcome.) For each of the 7 pairs of comparable cases (e.g., 1 vs. 2), more subjects favored the success case than the failure case, except for Cases 10 and 11, in which the numbers were equal.

Subjects might have thought that physicians were more responsible for bad outcomes, or they might have believed that the physician had information that the patient did not have (despite our instructions to the contrary). However, the outcome bias was also found for just those cases (Cases 3 and 7 vs. 4 and 8) in which the patient made the decision rather than the physician: $M = 0.48$, $t(19) = 2.59$, $p < .01$. In 17 of the 40 pairs, the success case was rated higher; in 4 cases the failure case was rated higher.[2] This issue is addressed further in Experiment 4.

The last 8 subjects run were asked after the experiment whether they thought they should have taken outcome into account in evaluating the decisions. All but 1 subject said they should not, and 1 was unsure. The outcome bias was significant for the 7 subjects who said they should not, $t(6) = 3.26$, $p < .01$; for the cases in which the patient made the decision, $t(6) = 2.50$, $p < .025$. Of these 8 subjects, 2 (including the one who was unsure) volunteered that they thought they had taken outcome into account even though they should not have, and 4 said they had not taken outcome into account. The outcome bias shown by the latter 4 subjects was 0.43, 0.29, 1.43, and 0.71, respectively. It would appear that most subjects accept the irrelevance of outcomes to judgments of rationality, they show an outcome bias even though they think they should not, and some show an outcome bias even though they think they do not. Further evidence on subjects' normative beliefs was obtained in Experiment 4.

Experiment 2

In Experiment 2, subjects were asked to rate the importance of several factors in a decision. This allowed us to test the effect

[2] However, the outcome bias appeared to be greater when the physician made the decision ($M = .80$), $t(19) = 3.56$, than when the patient made the decision; for the difference in effects, $t(19) = 2.04$, $p = .05$, two tailed.

Table 2
Conditions and Mean Ratings for Experiment 2

Case	Choice	Decision maker	Outcome	*M*	*SD*
1	Heart surgery	Physician	Success	19.9	8.3
2	Heart surgery	Physician	Failure	15.7	13.4
3	Heart surgery	Patient	Success	18.5	9.6
4	Heart surgery	Patient	Failure	15.4	13.9
5	Liver surgery	Physician	Success	18.6	7.9
6	Liver surgery	Physician	Failure	12.9	11.8
7	Liver surgery	Patient	Success	16.8	8.6
8	Liver surgery	Patient	Failure	11.5	13.3
9	Test, negative, no surgery	Physician	Cancer	11.2	15.0
10	Test, negative, no surgery	Physician	No cancer	16.8	11.6
11	No test, no surgery	Physician	Cancer	−9.3	16.8
12	No test, no surgery	Physician	No cancer	−1.0	17.2

of outcomes on the salience of various arguments for and against the decision made. We hypothesized that good outcomes would increase the salience of arguments in favor of the decision relative to that of arguments against it and that poor outcomes would do the reverse.

Method

Forty-one subjects, solicited as in Experiment 1, were given a questionnaire in a format similar to that used in Experiment 1. Subjects were asked to evaluate each decision (i.e., the quality of thinking that went into it) on a rating scale marked off from 30 to −30 (instead of 3 to −3) in order to encourage more graded evaluations.

The cases are summarized in Table 2. Cases 1 through 8 were identical in content to the corresponding cases in Experiment 1. Cases 9 through 12 concerned a testing situation in which a woman had a 5% chance of a cancer that was curable, but with more pain the longer the treatment was delayed. The woman and the physician agreed not to treat the cancer immediately unless its probability was 20% or more. An X-ray had an 80% probability of detecting cancer in those who had it and a 20% false alarm rate. (Under those conditions, the test could not have raised the probability to the threshold, so given the cost and danger of the test, which are given, the test should not be done.) In Cases 9 and 10, the test was done, was negative, and the patient was not treated. (Subjects were told that the physician would have treated the patient if the test had been positive.) In Cases 11 and 12, no test was done and the patient was not treated. In Cases 9 and 11, the woman had cancer and the treatment was more difficult than it would have been if the treatment had begun earlier. The decisions in these cases were failures. In Cases 10 and 12, there was no cancer; the decisions in these cases were successes.

After rating each decision, subjects were asked to rate the importance of various factors on the following scale:

30 = decisive, this factor alone should be sufficient, regardless of other factors;

20 = important, but must be weighed against other factors;

10 = relevant, but not important;

0 = completely irrelevant, should be ignored.

Factors were chosen to correspond to comparisons of the type made in regret theory (Bell, 1982; Loomes & Sugden, 1982), specifically, comparisons of the outcomes for the two choices within the same hypothetical state of the world. For Cases 1 through 8, the factors were of the form (using Cases 5 through 8 as an example):

1. If the operation were chosen, it might cause death, and this would be worse than living 10 more years.
2. If the operation were chosen, it might succeed, and this would be better than living 10 more years.
3. If the operation were not chosen, it might have succeeded if it had been chosen, and this would be better than living 10 years.
4. If the operation were not chosen, it might have failed if it had been chosen, and this would be worse than living 10 years.
5. Any other factor not mentioned (explain and rate).

For Cases 9 through 12, the factors were as follows:

1. If the test were done, it might be positive, the patient might have cancer, and, if so, the cancer would be treated early, which would be better than no immediate treatment.
2. If the test were done, it might be negative, the patient might have cancer, and, if so, the cost and risk of the test would be wasted, which would be worse than doing nothing.
3. If the test were done, it might be positive, the patient might have no cancer, and, if so, unnecessary testing and treatment would be done, which would be worse than doing nothing.
4. If the test were done, it might be negative, the patient might have no cancer, and, if so, the cost and risk of the test would be wasted, which would be worse than doing nothing.
5. Any other factor not mentioned (explain, and rate).

Finally, after rating the importance of these factors, subjects were asked, for Cases 1 through 8, the following: "Suppose the desirability of 'successful operation' were 100 and the desirability of 'death from surgery' were 0. On this scale, rate the desirability of 'no operation, 10 more years with pain.' "

The comparable question for Cases 9 through 12 was "Suppose the desirability of 'no test, no cancer, no treatment' were 100 and the desirability of 'negative test, cancer, no treatment' were 0. On this scale, rate the desirability of the following outcomes (using numbers below 0 or above 100 if you wish):

1. no test, cancer, no immediate treatment;
2. negative test, no cancer, no treatment;
3. positive test, cancer, immediate treatment; and
4. positive test, no cancer, unnecessary treatment.

Twenty subjects did the cases in the order 1, 6, 11, 4, 9, 2, 7, 12, 5, 10, 3, and 8; 21 did them in the reverse order. There was no effect of order. (Some subjects omitted some items. Three additional subjects, not counted as part of the 41, were omitted for apparent misunderstandings.)

Results

The mean ratings of the decisions are shown in Table 2. There was an outcome bias for each of the three sets of items: surgery judgments made by the physician (Cases 1, 2, 5, and 6), $M = 4.97$, $t(38) = 3.15$, $p < .005$; surgery judgments made by the patient (Cases 3, 4, 7, and 8), $M = 4.15$, $t(40) = 2.21$, $p < .025$); and judgments involving testing (Cases 9 through 12), $M = 3.50$, $t(40) = 3.80$, $p < .001$. That is, cases with good outcomes were given higher ratings than those with bad outcomes. Overall, the better outcome was rated higher in 49.1% of the 244 pairs of cases, lower in 16.0%, and equal in 34.8%. For all 6 pairs of cases, more subjects favored the success case than favored the failure case.

We tested the hypothesis that outcomes called attention to

arguments for or against a decision as follows by adding the importance ratings of factors that favored each decision and subtracting the ratings of factors that opposed each decision. (For example, "If the operation were chosen, it might succeed, and this would be better than living 10 more years," counted in favor of the decision to operate.) This gave an overall measure of attention to factors favoring the decision made. We subtracted these measures for bad-outcome decisions from those for good-outcome decisions. If reasons favoring the decision were given higher ratings for good-outcome decisions, these differences would be positive. This was the case only for the physician decisions, $t(38) = 3.61$, $p < .001$. The differences were not significantly above 0 for the patient decisions, $t(40) = 0.62$, or for the testing, $t(40) = 0.31$.[3]

Similarly, we examined subjects' evaluations of outcome desirabilities. In each case, each outcome rating would argue for or against the decision made depending on its level. For example, a low-desirability rating for "no operation, 10 more years with pain" would favor operating, and a high-desirability rating for "positive test, cancer, immediate treatment" would favor testing. The outcome could cause subjects to distort their desirability ratings so as to favor good-outcome decisions and oppose bad-outcome decisions. To test this, we formed a composite score as before, adding ratings that favored good-outcome decisions or opposed bad-outcome decisions and subtracting ratings that opposed the former or favored the latter. Again, the composite was significantly above 0 for the physician decisions, $t(38) = 3.13$, $p < .005$, but not for the patient decisions, $t(40) = 0.62$, or the testing, $t(40) = 0.00$.

In sum, there is evidence that the salience mechanism was at work for the physician decisions, but not for the patient decisions or the testing decisions (which were also made by the physician). We cannot explain this discrepancy.[4] What is clear, however, is that the focusing mechanism cannot fully explain the outcome bias.

Experiment 3

In Experiment 3, subjects were asked to evaluate decisions in simple monetary gambles. One reason for using such decisions was to ensure in another way that subjects believed they were given all relevant information. Medical decisions are rarely as clear-cut as we presented them, and subjects may have felt that this simplicity was implausible. In the monetary gambles we presented, there was no way to obtain additional relevant information.

Monetary gambles also allowed us to determine whether there would be an effect of foregone outcomes as well as outcomes that occur. When people decide not to make a certain investment, they often feel pleased with themselves if the market value of that investment declines or displeased if it increases; they often follow the course of investments not made as avidly as those that actually affect their fortunes. The regret theory of decision making under uncertainty (Bell, 1982; Loomes & Sugden, 1982) is based on the idea that people make decisions by comparing anticipated actual outcomes with anticipated foregone outcomes. If they do not buy the stock, they will regret it if the price goes up, and this gives them a reason to buy it. (Such intuitions were incorporated into our design of Experiment 2.) In this experiment, we told subjects what would have happened if the opposite choice had been taken. We asked whether this affected their evaluation of decision making. This would be impossible in medical contexts, because the foregone outcome is usually unknown.

Experiment 3 allowed another test of whether outcomes focus attention on arguments and outcome desirabilities that favor good-outcome decisions or oppose bad-outcome decisions.

Table 3
Conditions and Mean Ratings for Experiment 3

Case	Option 1	Option 2	Choice	Outcome	Foregone outcome	*M*	*SD*
1	\$200	\$300, .80	2	\$300	\$200	7.5	17.7
2	\$200	\$300, .80	2	\$0	\$200	−6.5	16.9
3	\$200	\$300, .80	1	\$200	\$300	9.3	13.8
4	\$200	\$300, .80	1	\$200	\$0	15.1	11.0
5	\$200, .25	\$300, .20	2	\$300	\$200	12.6	11.2
6	\$200, .25	\$300, .20	2	\$0	\$200	5.2	14.6
7	\$200, .25	\$300, .20	1	\$200	\$0	6.8	12.5
8	\$200, .25	\$300, .20	1	\$200	\$300	4.5	12.3
9	\$200, .50	\$100	1	\$0	\$100	−8.9	14.5
10	\$200, .50	\$100	1	\$200	\$100	3.0	12.9
11	\$200, .50	\$100	2	\$100	\$0	18.1	9.7
12	\$200, .50	\$100	2	\$100	\$200	12.4	12.3
13	\$200, .10	\$20	1	\$0	\$20	−4.2	18.6
14	\$200, .10	\$20	1	\$200	\$20	2.1	18.1
15	\$200, .10	\$20	2	\$20	\$0	14.6	13.7
16	\$200, .10	\$20	2	\$20	\$200	8.7	21.8

Note. \$300, .80 indicates \$300 with probability .80, \$0 otherwise.

Method

Subjects were asked to make the same evaluations as in Experiment 2 concerning a series of 16 gambles. They were told to assume that all chance outcomes were determined by the spin of a fair roulette wheel and that the person who made the decision had no more relevant information than they were given.

The cases are summarized in Table 3. Cases 1 through 4 had the following scenario:

> A 25-year-old man is unmarried and has a steady job. He receives a letter inviting him to visit Quiet Pond Cottages, where he has been considering buying some property. As a prize for visiting the property, he is given a choice between
>
> Option 1. \$200.
> Option 2. An 80% chance of winning \$300 and a 20% chance of winning nothing.

[3] In this analysis, we ignored "other factors." Only a few subjects listed such factors, which were often redundant with those in our list. Other subjects tended to explain why the decision was important rather than why it should be made one way or the other.

[4] The lack of an effect for the testing might have been due to the recognition by some subjects that testing was never appropriate, because even a positive result could not justify treatment. However, this explanation is unlikely given that the decision ratings were higher when testing was done than when it was not done. Also, this explanation cannot account for the lack of an effect for patient decisions.

> He must mail in his decision in advance, and he will be told the outcome of Option 2 whether he chooses it or not.

If a gamble was chosen, the subjects were told the outcome. If the gamble was not chosen, the subjects were told which outcome was foregone (e.g., "He chooses Option 1 and finds that he would have won $300 if he had decided on Option 2" [Case 3]).

As shown in Table 3, the cases differed in whether the more risky option, that with the higher payoff and lower probability of winning, was taken (Cases 1, 2, 5, 6, 9, 10, 13, and 14) or not taken (the remaining cases). They also differed in whether the more risky option, when taken, led to success (Cases 1, 5, 10, and 14) or failure (Cases 2, 6, 9, and 13). By comparing these sets of cases, we were able to assess the outcome bias. When the more risky option was not taken, the cases differed in whether the foregone outcome was greater (Cases 3, 8, 12, and 16) or less (Cases 4, 7, 11, and 15) than the outcome obtained. These cases can be used to look for a *foregone outcome bias* on the evaluation of decisions; decisions may be evaluated more highly when the foregone outcome is poor.

As in Experiment 2, subjects were asked to rate the importance of relevant factors, such as those in Cases 1 through 4:

1. If he chooses Option 2, winning $300 in Option 2 is a better outcome than $200 in Option 1.
2. If he chooses Option 2, winning nothing in Option 2 is a worse outcome than $200 in Option 1.
3. If he chooses Option 1, $200 in Option 1 is a worse outcome than winning $300 in Option 2.
4. If he chooses Option 1, $200 in Option 1 is a better outcome than winning nothing in Option 2.

As in Experiment 2, subjects were also asked to assign a utility to intermediate outcomes, as in the following example: "Suppose the desirability of '$300' were 100 and the desirability of 'nothing' were 0. On this scale, rate the desirability of '$200'."

Seventeen subjects did the cases in the order 1, 6, 11, 16, 5, 10, 15, 4, 9, 14, 3, 8, 13, 2, 7, and 12; 23 did them in the reverse order. There was no effect of order. (Some subjects omitted some items.)

Results

The mean ratings are shown in Table 3. There was an outcome bias for those pairs of cases that differed in the amount of money won (Cases 1 and 2, 5 and 6, 9 and 10, and 13 and 14), $M = 9.91$, $t(39) = 5.40$, $p < .001$; the better outcome was rated higher in 60.0% of the 160 pairs of cases, lower in 11.9%, and equal in 28.1%. There was also an outcome bias for the outcome that was foregone, that is, decisions were given higher ratings when the foregone outcome was less (Cases 3 and 4, 7 and 8, 11 and 12, and 15 and 16), $M = 5.03$, $t(38) = 4.15$, $p < .001$; the "better" outcome (i.e., the worse outcome for the foregone gamble) was rated higher in 46.5% of the 159 pairs of cases, lower in 13.2%, and equal in 40.3%. For all 8 pairs of cases, more subjects favored the success case than the failure case.

We tested the attention-focusing hypothesis as in Experiment 2. First, we formed a composite score from the importance ratings given to the factors favoring or opposing each decision (e.g., "If he chooses Option 2, winning $300 in Option 2 is a better outcome than $200 in Option 1" favors Option 2). This composite was significantly greater than 0 for experienced outcomes, $t(39) = 3.30$, $p < .002$, but not for foregone outcomes, $t(38) = 1.23$. Second, we formed the same sort of composite from the utility ratings (e.g., a low utility rating for $200 in the previous example would favor Option 2, the risky option). This composite was not significantly above zero either for experienced outcomes, $t(39) = 1.64$, or foregone outcomes, $t(38) = 0.67$. Again, there is some evidence for the attention-focusing explanation, but this mechanism cannot fully explain the outcome bias.[5]

Experiment 4

In Experiments 1 through 3, subjects might have construed the instruction to rate the quality of the decision as a suggestion to evaluate its outcome. Colloquially, we may use the term *bad decision* to refer to a decision with a bad outcome. (This usage in itself may be an example of the outcome bias. However, the question is whether the subjects themselves were subject to this effect or whether they attributed it to the experimenter.) One source of evidence against this is the finding that judgments were, in some conditions, predictable from arguments that were relevant only before the outcome was known. In addition, subjects showed an outcome bias even when they thought their judgments should not have been affected by outcome. That they thought outcome was irrelevant indicates that they interpreted the question we asked as we intended it. In Experiment 4, we asked subjects more systematically (in Questions A and C presented later) whether they thought that outcome was relevant, as part of a more systematic debriefing procedure. We also asked them to predict the future competence of the decision maker and to judge the quality of the decision. This provided another measure of their judgment.

Method

The cases, listed in Table 4, were identical to those used in Experiment 2, except that those in which the patient made the decision (Cases 3, 4, 7, and 8) were replaced with cases in which the physician made the decision and the probability of death from surgery was ambiguous. For example, Cases 3 and 4 stated, "The probability of death from the operation is not clear. One study at the hospital says 12%, another says 4%. There is no reason to think that one study is more valid or applicable than the other." Note that the average, 8%, is the figure given in Cases 1 and 2. This ambiguity manipulation was not relevant to the major issue under study. It may have had the function of confounding (slightly) the subjects' efforts to decipher the experiment.

In addition to the instructions given in Experiment 2, subjects were told the following:

> You will also be asked to predict the future competence of the physician as a decision maker on the following Competence scale.

[5] Cases 1–4 versus 5–8 allowed an assessment of the *certainty effect* of Kahneman and Tversky (1979). According to expected-utility theory, one should either prefer the more risky option in both sets of cases or the less risky option in both sets of cases, because the ratio of probability 1 to .8 is the same as .25 to .20. Yet, Kahneman and Tversky's results led us to expect that most subjects would favor the less risky (certain) option in Cases 1–4 and the more risky option in Cases 5–8. We compared the number of discrepancies in the expected direction (e.g., a positive rating in Case 5 and a negative or 0 rating in Case 1) with the total number of discrepancies in either direction by binomial tests. These tests were significant for Cases 5 versus 1 (9 of 10, $p < .01$), Cases 6 versus 2 (18 of 21, $p < .001$), Cases 4 versus 7 (11 of 13, $p < .01$), but not for Cases 3 versus 8 (10 of 14, $p = .06$). It was apparent that there was a certainty effect for evaluations of the others' decisions, with the outcome held constant.

Table 4
Conditions and Mean Ratings for Experiment 4

Case	Choice	Outcome	Decision		Competence	
			M	*SD*	*M*	*SD*
1	Heart surgery	Success	18.6	11.6	77.9	20.0
2	Heart surgery	Failure	15.6	12.3	75.6	21.7
3	Heart surgery[a]	Success	18.1	9.4	77.8	17.9
4	Heart surgery[a]	Failure	15.3	11.3	66.2	20.6
5	Liver surgery	Success	17.1	10.3	73.0	21.0
6	Liver surgery	Failure	14.8	9.8	73.6	16.9
7	Liver surgery[a]	Success	15.4	10.6	68.7	12.7
8	Liver surgery[a]	Failure	11.9	13.7	66.0	20.1
9	Test negative, no surgery	Cancer	13.4	12.9	75.1	18.6
10	Test negative, no surgery	No cancer	18.7	8.6	76.7	20.0
11	No test, no surgery	Cancer	−11.0	14.1	45.9	19.5
12	No test, no surgery	No cancer	−10.5	13.7	48.8	22.1

[a] Ambiguous probabilities.

Imagine that the predictions were going to be made available to prospective patients as a basis for choosing physicians. All cases involve a decision about whether some procedure should be carried out. The physician who makes the decision is never the one who carries out the procedure. The procedure is carried out by the staff of a large hospital, and the probabilities given refer to the hospital in question.

Competence scale:

100 = as competent as the most competent physician in the U.S.;

50 = in the middle; half the physicians in the U.S. are better, half are worse;

0 = as incompetent as the least competent physician in the U.S.

You need not restrict your ratings on either scale to multiples of 10, and you may go beyond the end of a scale if you wish. All cases involve a decision about whether some procedure should be carried out. You may assume the following:

1. The physician who made the decision first consulted the patient. The patient could not decide and asked the physician's advice. The physician knew that the patient would accept this advice. Hence, it is the physician who makes the decision on the patient's behalf.
2. The physician who made the decision is never the one who carries out the procedure.
3. The procedure is carried out by the staff of a large hospital, and the information given refers to the staff of this hospital.
4. The physician who made the decision has no control over which staff member carries out the procedure.
5. The physician who made the decision had no more relevant information than you are given, and there is no more relevant information that can be discovered.

At the end of the experiment, subjects answered the following questions in writing:

A. Do you think that you *should* take the outcome into account in rating the quality of the decision? Why or why not?

B. Do you think you *did* take the outcome into account in rating the quality of the decision? Why or why not?

C. Do you think that you *should* take the outcome into account in predicting the competence of the physician? Why or why not?

D. Do you think that you *did* take the outcome into account in predicting the competence of the physician? Why or why not?

E. Did you understand the second page of the instructions? (That page contained the information about the decision maker being different from the one who does the procedure, etc.) If not, what didn't you understand?

Twenty-nine subjects were solicited as in the previous experiments. Eight subjects were given the cases in the order 1, 6, 11, 2, 7, 12, 3, 8, 9, 4, 5, and 10; 21 subjects were given the reverse order. (The discrepancy in numbers was inadvertent.)

The decision ratings (the first judgment, using the scale used in previous experiments) were not used unless Question A was answered negatively, and the competence ratings (the second judgment, using the Competence scale) were not used unless Question C was answered negatively. Competence ratings were excluded for 4 subjects because of affirmative or doubtful answers to Question C. Four additional subjects (not counted as part of the 29) were excluded because they answered both Questions A and C affirmatively. Subjects were to be eliminated if Question E was not answered affirmatively, but all subjects did so.

Results

Ratings on both scales are shown in Table 4. The decision ratings replicate the results of the first three experiments. Over all items, the outcome bias was significant ($M = 2.90$), $t(28) = 2.10$, $p < .025$; the better outcome was rated higher in 31.0% of the 174 pairs of cases, lower in 12.6%, and equal in 43.7%. The competence ratings also yielded an overall outcome bias ($M = 3.41$), $t(24) = 2.26$, $p < .025$; the better outcome was rated higher in 26.7% of the 150 pairs of cases, lower in 11.3%, and equal in 62.0%. For all 6 pairs of cases and for both measures, more subjects favored the success case than the failure case. For both kinds of ratings, there were no significant effects of order of presentation. (The unequal numbers of subjects in the two orders cannot account for the results because the outcome bias was, if anything, larger for the smaller subgroup, and it was significant at $p < .025$ for both measures for the smaller group alone.) The outcome biases for the two kinds of ratings were correlated .85 across subjects. The main result, then, was that the outcome bias extended to predictions of future competence, even when subjects who thought outcome should be relevant were excluded.

There was no significant difference between ambiguous and nonambiguous cases in the size of the outcome bias, although subjects tended to give lower competence ratings for ambiguous cases regardless of the outcome, $t(24) = 2.46$, $p < .05$, two-tailed; the corresponding effect for decision ratings was not significant, $t(28) = 1.20$.

The responses of subjects who thought that they should consider outcome were informative.[6] Some subjects seemed to feel that considering outcome was inevitable and therefore appropriate (e.g., "I don't respect doctors who are responsible for people's death—and 'should' doesn't matter since respecting a doctor or not is a personal/subjective thing," and "If the deci-

[6] These examples included responses from a pilot study similar to Experiment 4, except for the explicit statement that the physician did not perform the surgery. Without this statement, subjects mentioned surgical skill as a reason for taking outcome into account in predicting competence.

sion were wrong . . . the survivors . . . would not be satisfied by an explanation based on statistics alone").

Other subjects believed that good luck is a consistent trait (verging on clairvoyance itself). Examples include the following: (a) "[A good outcome] shows that the physician has good luck with his decisions." (b) "Every doctor has some degree of intuition or experience that influences his decisions. Human decisions are rarely completely rational, so, other things equal, the more competent doctor will make the better decisions."

Other answers were based on a simple assertion that outcome was relevant: "When dealing with something as important as life the outcome is the most vital thing." "One cannot isolate the physician's skill from the patient's well-being when evaluating . . . competence."

Finally, some subjects seemed to follow the rule that "the outcome is an indication of how well the decision was made." As noted in the introduction, this is a generally good rule, but the cases given to subjects were designed to make it inapplicable.

Of course, most subjects asserted the irrelevance of outcome, and many provided articulate defenses of this view. These subjects were all included in the data analyzed. It is likely that they were influenced by the same factors mentioned by other subjects in defending the relevance of outcome. Indeed, many subjects who said they should not be influenced also said that they thought they were influenced. Most of these subjects simply admitted that they had been biased and that they had not thought much about the role of outcome until seeing the questions at the end. Many of these found the experiment instructive.

Experiment 5

The previous experiments used a within-subjects design. It is possible that this design might have suggested to subjects that the outcome was relevant. Subjects were usually aware that they were judging cases that were identical except for outcome. This experiment used a between-subjects design. No subject received identical cases differing only in outcome. In addition, we used another measure of the subject's opinion of the decision: the subject's willingness to allow the decision maker to choose on the subject's behalf.

One could also argue that subjects have little basis to judge decision quality except for outcome. In this experiment, we included a factor that subjects could use to make valid evaluations of decision quality, one that should have yielded evaluations opposite to those predicted by the outcome bias.

Method

Subjects were 111 members of a class in thinking and decision making, taught by the first author, who were present the day of the experiment. The instructions, presented in a questionnaire, were as follows:

> Suppose you are in a psychology experiment with another student (of your own sex) from this class. The other student will make two choices concerning which of two gambles to play. Whatever the other student chooses, both of you will win the same amount, which could be $20 or nothing at all. The other student will thus be deciding for both of you.
>
> Then there will be two more choices of the same sort. You can either let the other student decide on these, or you can decide yourself. Please indicate (yes or no) whether you would let the student decide under each of the following conditions.

At this point, subjects were asked a series of yes–no questions, such as "I would let him or her decide if the experimenter gave me $5 extra to do so." The monetary amounts were $5, $3, $2, $1, 0, −$1, −$3, and −$5. (When amounts were negative, the sentence read "if I had to pay the experimenter.") The number of items checked indicated the subjects' willingness to let the other student decide. This first measure served as a baseline, which indicated willingness in the absence of specific knowledge about the other student's decisions. (Two subjects were dropped for answering no to earlier questions and yes to later ones. All other subjects answered with a string of yesses followed by a string of nos.)

On the next page, the following information was given (for one condition):

> Each choice is between two decks of cards. After a deck is chosen, a card will be drawn. If the card is red, you will each win $20. If the card is black, you will each win nothing.
>
> One deck is an ordinary deck of 52 cards. Both you and the other student have made sure that there are 26 red cards and 26 black cards and that the deck is well shuffled. The other deck is the mystery deck. The proportion of red and black cards is unknown, and neither of you can inspect it.
>
> For the first choice, the other student chooses the mystery deck, the card is red, and you win $20 each. (The red card is put back, and the deck is reshuffled.) For the second choice, the other student chooses the ordinary deck, and you each win $20 again.
>
> Now, knowing how the other student chose, indicate whether you would let the student decide under each of the following conditions.

The subjects were then given the same list of yes–no questions as before. Finally, the subjects were asked, "What do you think of the other student as a decision maker?" Ratings were given on a 5-point scale, ranging from *better than most others in this class* through *average for this class* to *worse than most others in this class*. Brief explanations were requested.

There were four conditions, which were distributed in alternation among the subjects. The conditions differed in whether the outcome was $20 or nothing (the same outcome occurring both times) and in whether the mystery deck or the ordinary deck was chosen first. Whichever deck was chosen first, the student chose the other deck the second time.

Note that in the mystery-first condition when the student won, there was a good argument for not switching. Specifically, a single red card drawn from the mystery deck should have increased the student's estimate of the proportion of red cards. Given that the student chose from that deck the first time, he or she should have found it more attractive the second time. Conversely, when the student lost in the mystery-first condition, the student should have switched, and he or she did. By this argument, the decision was actually better in the lose condition than in the win condition. If the subject attended to these arguments, the outcome bias would have been reversed.

Results

The changes in willingness to let the other student decide are shown in Table 5. Putting aside the subjects who did not change, the outcome bias was significant for both the mystery-first conditions ($p < .005$, two-tailed) and the ordinary-first conditions ($p < .01$), by Fisher's exact test. Subjects were more willing to let the other student decide when the outcome was positive and less willing when it was negative. This was true even in the mystery-first conditions, in which the student was actually a better decision maker in the lose condition.

All but 23 answers to the last question—"What do you think

Table 5
Number of Subjects Who Increased, Decreased, or Did Not Change Their Willingness to Let the Other Student Decide on Their Behalf for the Four Conditions in Experiment 5

Condition	Increase	Decrease	Same
Mystery deck first			
Win	13	1	15
Lose	3	12	14
Ordinary deck first			
Win	5	0	22
Lose	2	8	16

of the other student as a decision maker?"—were average. The remaining 23 responses were analyzed as a group. For the win conditions, 6 evaluations were above average and 4 below average. For the lose condition, 2 were above average and 11 below average (including one "worse than most others"). The difference in proportion of above- and below-average evaluations was significant by a Fisher's exact test ($p < .05$).

Justifications were varied. Many asserted that it was impossible to judge because everything was luck. Many (especially in the lose conditions) referred to the lack of knowledge about the mystery deck, criticizing the student for choosing it at all. A few subjects noticed the possibility of learning from the mystery deck in their justifications. Only 1 subject who showed an outcome bias on willingness to yield the decision (in the ordinary-first win condition) referred to outcome as a justification of an evaluation ("She still beat the odds"). Some subjects explicitly denied its relevance, even when it seemed to affect them (e.g., "The success may well be random—I am not satisfied that his or her decision making is responsible"). Thus, subjects did not appear to think they were using outcome as a basis for their evaluations.

Discussion

We found consistent outcome bias in our five experiments. These effects are, at most, partly explained in terms of the focusing of attention on factors favorable to one decision or another. From subjects' justifications, it appears that a number of other factors may be at work: a kind of naturalistic fallacy in which subjects believe that bias is rational because it is a natural tendency, a belief in luck or clairvoyance as a consistent trait, and (as suggested in the introduction) the overapplication of a general rule that bad outcome is a sign of bad decision making. When outcome is relevant to the evaluation of decisions because the full context of the decision is not known to the judge, people may give it even more importance than it deserves.

It is also possible that outcomes affect the judge's beliefs about what the decision maker knew, even when the judge is told exactly what the actor knew. In other words, judges may not believe what they are told about the information available to the decision maker. This might have occurred in our experiments, and it may occur in the real world as well. In either case, it would lead to a biased judgment, so long as the judge has no good reason to disbelieve what he or she is told.

The main practical implication concerns those many cases in which people judge the decisions of others after knowing their outcomes, as occurs in law, regulation, politics, institutions, and everyday life. Our results suggest that people may confuse their evaluations of decisions with the evaluations of the consequences themselves. Mere understanding that such confusion contaminates these evaluations is not enough to eliminate it. When decisions turn out badly, it may sometimes be useful to reanalyze them from the decision maker's viewpoint at the time of the decision, both for judging the decision maker and for promulgating standards for the future (Bursztajn, Hamm, Gutheil, & Brodsky, 1984; Forst, 1974).

Our experiments did not investigate whether the outcome bias applies to decisions made by the individual who judges the decisions. However, such effects are suggested by the cognitive dissonance experiments of Sherman (1970) and Pallak, Sogin, and Van Zante (1974), in which judged enjoyment of a task (or agreement with opinions expressed in the task) was affected by consequences that could not have been foreseen at the time of the decision to do the task. People who judge their own behavior well or badly as a function of its outcome may hold themselves responsible for both good and bad luck, becoming smug in their success or self-reproachful in their failure.

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