

Avoiding Future Regret in Purchase-Timing Decisions

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When deciding when to make a purchase, people often compare their outcomes to those that would have occurred had they purchased earlier or later. In this article, we examine how pre- and postpurchase comparisons affect regret and satisfaction, and whether consumers learn to avoid decisions that result in regret. In the first two experiments, we show that information learned after the purchase has a greater impact on satisfaction than information learned before the purchase. In addition, negative price comparisons have a greater impact on satisfaction than positive comparisons. These results imply that if consumers who receive postpurchase information wish to avoid future feelings of regret, they should defer their purchases longer. Our second two experiments demonstrate this phenomenon: Subjects who were exposed to postchoice information set higher decision thresholds, consistent with the minimization of future regret. Paradoxically, providing subjects with additional postchoice information resulted in decreased average earnings, suggesting that consumers may try to avoid future regret even when doing so conflicts with expected value maximization.

Shut out all of your past except that which will help you weather your tomorrows. [Sir William Osler]

How we evaluate a purchase depends not only on the characteristics of the products we buy but also on the characteristics of products we do not buy. Our satisfaction with a recently purchased car may be lower if we learn that other cars we considered received good evaluations from *Car and Driver* or *Consumer Reports*. If we fill up our car's gas tank for \$1.30 per gallon, we may feel upset if we subsequently see gas sold at \$1.15 per gallon. We may even be displeased with our purchase of a stock that subsequently increased in value if other stocks we had considered buying increased more.

In each of the above examples, we feel regret about the purchase we made. We feel that we made a bad deci-

sion and that, given the opportunity to make the decision again, we would choose differently. Many studies have documented a relationship between regret and satisfaction; the more regret one feels following a decision, the less satisfied one tends to be with that decision (see Roese and Olson [1995b] for a recent review).

Recently, researchers have begun to explore whether anticipated regret affects actual decisions. If consumers learn what situations tend to produce the greatest regret, and if regret is sufficiently aversive, then consumers should avoid those situations. Evidence suggests that people can learn to accurately predict how much regret they will feel in particular situations (Mellers, Schwartz, and Ritov 1999; Schwartz 1998). Other work shows that when choosing between alternatives that are identical except for one alternative being associated with greater regret people will choose so as to minimize regret (Zeelenberg and Beattie 1997; Zeelenberg et al. 1996). Finally, Inman and colleagues (Inman, Dyer, and Jia 1997) present a model incorporating the effects of regret and disappointment into an expected-utility calculus.

In this article, we explore two issues: In the first two studies, we examine whether consumers feel regret in purchase-timing decisions and how that regret relates to their satisfaction. In the second two studies, we explore the degree to which people learn to avoid this regret. Purchase timing decisions offer an especially rich domain in which to investigate the role of regret because there are a variety of

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unique ways in which feelings of regret can arise. First, consumers may be sensitive to the temporal direction of the comparison. A consumer could regret having made a purchase too early and missing a better subsequent opportunity. Alternatively, he may regret having waited too long to make the purchase and passing up a better prior opportunity. Although the magnitude of these comparisons may be equivalent in some cases (e.g., when the product was purchased for \$100 but was available for \$80 in an earlier or later week), the magnitude of the consumer's regret may differ.

Second, the control that consumers have over the timing of their purchases typically varies. In some cases, consumers have no immediate need for the product and can purchase at a price or time that they desire. In other cases, consumers have a pressing need for the product and therefore have less control over the timing of their purchase. Each of these situations may lead to feelings of regret, but the regret experienced may differ depending on the degree of control available. Experiments 1A and 1B demonstrate that both consumer control and the temporal direction of price comparisons have a substantial effect on the experience of regret and the satisfaction that the consumer associates with the purchase.

In experiments 2A and 2B, we explore the degree to which people avoid regret. We construct a situation similar to our purchase-timing task in which subjects have the opportunity to learn about the outcomes of their decisions and the satisfaction that they produce. We find that subjects who are exposed to postdecision comparisons set higher purchase thresholds than subjects who are not. Differential involvement or concern about a public loss cannot explain the difference in thresholds. Results are consistent with the view that consumers can learn to anticipate and avoid the regret they will feel as a result of their decisions. This occurs despite the fact that ignoring one's feelings of regret would lead to objectively better outcomes.

REGRET AND SATISFACTION

Consumer satisfaction affects repeat purchases, product return rates, brand loyalty, and the valence of word-of-mouth communications. It is therefore important for marketers to understand how they can influence the determinants of consumer satisfaction. What factors lead to consumer satisfaction? Satisfaction certainly depends to a large extent on the performance of the chosen brand. But product experiences do not completely determine satisfaction. For example, a large body of research shows that satisfaction also depends on the level of performance that the consumer expected (Churchill and Surprenant 1982; Oliver 1980).

Satisfaction also depends on information about outcomes that were not experienced. When people evaluate their outcomes, they compare obtained outcomes to those that would have occurred had they chosen differently (e.g., Boulding et al. 1993; Kahneman and Miller 1986; Taylor 1997). These so-called counterfactual comparisons can produce either positive or negative emotions. When people compare an

obtained outcome to one that would have been superior (referred to as an "upward" comparison), they often report feelings of regret and are less satisfied with their outcome. When people compare an obtained outcome to one that would have been inferior (a "downward" comparison), they report feelings of relief and rejoicing and are more satisfied with their outcome (Roese and Olson 1993). Furthermore, research suggests that upward comparisons have greater effects on satisfaction than downward comparisons (Markman et al. 1993; Mellers et al. 1999; Roese and Olson 1995b). We predict that similar effects will be observed in purchase timing:

H1: The effects of upward price comparisons on satisfaction will be greater than the effects of downward price comparisons of the same magnitude.

This prediction is not especially controversial given the existing research on counterfactual comparisons. Nonetheless, these comparisons play a central enough role in consumer satisfaction as to warrant a separate hypothesis. We will focus on upward comparisons in the remainder of this introduction.

REGRET AND PURCHASE TIMING

Much of the work that psychologists have done linking regret and satisfaction has examined how and when people spontaneously generate particular counterfactual comparisons (Kahneman and Varey 1990). Consumer decisions, in contrast, often involve many alternatives that are provided by the purchase environment, any of which may provide information useful for evaluating one's purchase. This is especially true in the case of purchase-timing decisions.

Suppose that you have been monitoring the price of airline fares waiting for a good time to buy tickets. You finally purchase when the tickets reach \$500. There are a variety of comparisons that you could make to help you evaluate your decision. You could recall that the tickets had been \$400 two weeks ago, \$450 three weeks ago, and so forth. In each of these cases, you may feel regret for not having purchased earlier. We will refer to these prices as prepurchase prices. We expect these comparisons to affect satisfaction: as prepurchase prices decrease, you should feel more regret and report being less satisfied with a given purchase.

Purchase-timing decisions also offer consumers a second set of comparisons on which satisfaction may be based. Suppose that after you purchase your tickets, you continue to monitor prices, and you learn that prices drop to \$400 in the following week. In this case, you may regret not having waited to purchase your ticket. We will refer to these prices as postpurchase prices. Like prepurchase prices, we expect postpurchase prices to affect satisfaction: as postpurchase prices decrease, you should feel more regret and be less satisfied with a given purchase.

Although pre- and postpurchase prices may both produce feelings of regret, the manner in which they influence satisfaction may be very different. Do prepurchase or post-

purchase prices have a greater effect on satisfaction? No research has explicitly addressed this question. However, there are a number of empirical results that may give us some guidance. Perhaps the most relevant result is a study by Simonson (1992) that studied the relationship between anticipated regret and purchase timing. Simonson asked subjects to imagine that they had to purchase a wedding present in either July or August. Subjects in the regret condition were told that they would be shown comparison prices in the two months after making their choice. They were also asked to anticipate how they would feel if they (1) bought the product on sale in July and observed a lower price in August or (2) deferred until August and were forced to buy at higher prices than seen in July. Simonson found that people anticipated more regret in the second case, when a better price was passed over. Furthermore, subjects who anticipated learning August prices were significantly more likely to purchase in July than subjects who did not anticipate learning this information. Simonson argued that buying products on sale constitutes more of a subjective norm than deferring purchase, and therefore upward comparisons incurred through waiting produce greater regret (cf. Kahneman and Miller 1986).

Simonson's (1992) results suggest that prepurchase prices may have a greater influence on regret and satisfaction than postpurchase prices. However, his results may not generalize to more commonplace purchase-timing situations. First, Simonson's predictions were based on the normative nature of purchasing products on sale. Second, consumers may not be able to accurately anticipate the regret that they will feel or may be influenced by being prompted for their feelings of regret. Perhaps most important, subjects in Simonson's task were not given explicit counterfactual information. Thus, they may have imagined counterfactual prices that were, in reality, extremely unlikely.

A second literature that may yield insight is that of economic search. From an economic perspective, purchase-timing decisions are simply a variant of an economic search task (Hey 1981, 1982; Simon 1955; Stigler 1961). A price is observed in the current period and compared to the expected distribution of prices. The consumer decides to purchase in the current period if the expected returns from additional search are smaller than the costs of waiting. From this perspective, purchase-timing decisions are inherently forward looking; past prices are irrelevant unless they affect expectations (cf. Jacobson and Obermiller 1990). Of course, search theory speaks only to purchase strategies that attempt to maximize expected value and does not incorporate hedonic information into the decision calculus (Inman et al. 1997). Furthermore, none of the traditional search experiments have provided subjects with postpurchase information (Hey 1981, 1982; Stigler 1961). Nonetheless, if the results of search models generalize to satisfaction, we may find a greater effect of postpurchase prices than prepurchase prices.

Similarly, Miller and Gunasegaram (1990) offer a psychological rationale for why postpurchase prices might have a greater effect on regret. They find that later occurrences

in a sequence of events evoke counterfactual comparisons more strongly. Their results suggest that postpurchase prices, because they are the most recent price information received, may lead to especially salient counterfactual comparisons. Thus, regret and satisfaction may depend more on postpurchase prices than on prepurchase prices.

In summary, no research deals explicitly with how pre- and postpurchase prices affect purchase timing. Simonson's (1992) research, which deals with the effects of anticipated comparisons, suggests that prepurchase prices may have a greater effect. Economic search tasks, which do not incorporate hedonic values, suggest that the reverse may be true. However, if regret affects purchase timing, the effects of pre- and postpurchase prices are both likely to depend on the control that the consumer has over the decision, as discussed in the following section.

Control and Direction of Comparison

Much research suggests that regret depends on consumers' control over the situation. Zeelenberg and colleagues (Zeelenberg, van Dijk, and Manstead 1998a; Zeelenberg et al. 1998b, 1998c) show that regret is greater for those decisions over which one had control than for those decisions over which one had little control. Similarly, Mandel and Lehman (1996) find that thinking about how an outcome could have been prevented tends to generate more counterfactual reasoning than thinking about the cause of an outcome. Moreover, people tend to generate counterfactuals primarily about those outcomes over which they had control (Markman et al. 1995). Furthermore, research suggests that other emotional states, such as anger, pity, and guilt, may also depend on control or perceived control of the actor (Weiner 1986).

Consumers do not have direct control over the prices offered in stores. However, they may have control over when to purchase the product. In experiment 1, subjects experienced two different buying situations. At times, they purchased the product voluntarily based on the price at which it was offered. At other times, they were told that they had run out of the product and would have to purchase it in that period, regardless of the price at which it was offered. We will refer to the levels of consumer control as "voluntary" and "involuntary," respectively.

Based on the effects of control found in existing counterfactual literature, we believe that purchase control will moderate the effects of both pre- and postpurchase prices on regret and satisfaction. When consumers decide to purchase of their own accord during a particular shopping trip, they have presumably already integrated the preceding prices into their decision. Hence, these prices should not affect their satisfaction with the obtained outcome. However, if consumers run out of the product and are forced to buy during a particular shopping trip, they are likely to regret passing up lower prepurchase prices. Therefore, we predict that the influence of prepurchase comparisons will be greatest when consumers are forced to buy.

H2: Prepurchase prices will have a greater effect on satisfaction for involuntary purchases than for purchases in which the timing is under their control.

Postpurchase prices, however, should not effect satisfaction if consumers are forced to buy the product at that time. Prices offered subsequent to an involuntary purchase are irrelevant, as the consumer could not have waited to obtain these better prices. Since postpurchase prices are irrelevant to forced purchases, consumers should simply ignore those prices. However, when consumers do control the timing of the purchase, postpurchase prices provide meaningful comparisons, as consumers could have obtained those prices had they chosen differently. Moreover, in contrast to prepurchase prices, postpurchase prices could not yet have been incorporated in the consumer's decision. Thus, we expect postpurchase prices to have a greater effect on satisfaction for voluntary purchases.

H3: Postpurchase prices will have a greater effect on satisfaction when consumers make a purchase voluntarily than when they do not control purchase timing.

EXPERIMENT 1A

Experiment 1A examined the effects of pre- and postpurchase prices on satisfaction in a purchase-timing task. Subjects were told to monitor the price of a good over time and purchase the good at the cheapest price possible. Two goods, gourmet coffee and gasoline, were used in counterbalanced blocks. For each shopping trip, subjects had to decide whether to purchase the good in that period or defer until a later trip. However, they were also told that in some cases they would run out of the product during the intervening period. In these cases, they were required to buy during the subsequent shopping trip regardless of the price. Thus, subjects had to balance their desire to pay the lowest price against the chance of being forced to buy at the prevailing price, a trade-off typical of purchase-timing decisions. After the purchase was made, some subjects were shown the prices they would have paid on the next few shopping trips. Subjects then rated their satisfaction with the purchase and in some cases, rated how much regret they felt.

Method

Stimuli and Design. Stimuli were presented and responses collected using personal computers. Five prices were used for each of the product categories. The price for a pound of gourmet coffee was \$6.99, \$7.49, \$7.99, \$8.49, or \$8.99. The price for a tank of gasoline was \$10.50, \$11.50, \$12.50, \$13.50, or \$14.50. Each price was randomly drawn from a uniform distribution and was independent of the other prices presented. Postpurchase prices were also randomly drawn from the same distribution. Subjects saw 12 trials in each of the two product categories, and each

trial was composed of up to 16 repeated shopping trips. The order of product categories was counterbalanced. Our results did not depend on product category or order, so we collapse across these factors.

For each shopping trip there was a 20 percent chance that the subject would be required to buy at the listed price, regardless of its value. This conceivably could have produced an infinite sequence of prices. To eliminate this possibility, we artificially terminated all trials after 16 trips. Only five trials (0.2 percent) were terminated after 16 trips.

We manipulated the number of postpurchase prices shown and the dependent measures using a 3×2 between-subjects factorial design. Subjects saw the price they would have paid on either zero, one, or three trips following their purchase. Half the subjects rated the amount of regret they associated with the purchase and their satisfaction with the outcome, in that order. The other half of the subjects only rated their satisfaction with the outcome. Our results did not depend on the number of dependent measures collected.

Procedure. At the beginning of each trial, a single price was displayed. Subjects pressed a button labeled "Continue" or a button labeled "Buy." If the subject chose to continue, the price on the next shopping trip was displayed. (All previous prices remained visible.) If she chose to buy, a message was shown stating the purchased product and price (e.g., "You just bought a pound of coffee for \$6.99.") On some trials, the subject was unable to continue and was required to purchase. In these cases, a message was displayed saying that the subject had run out of the product and would therefore be forced to buy, regardless of the price. After having purchased the product, some subjects were shown the price(s) they would have paid if they had continued. When the trial had ended and all prices had been presented, dependent measures were collected. Half the subjects rated the amount of regret they associated with the purchase (using a scale of 0 = No Regret Whatsoever to 100 = A Lot of Regret) followed by their satisfaction with the outcome (using a scale of 0 = Very Dissatisfied to 100 = Very Satisfied). The other half of the subjects only rated their satisfaction with the outcome.

Subjects. Seventy-eight undergraduate students participated in this experiment in return for extra credit. For subjects making both regret and satisfaction ratings, 15, 14, and 11 subjects served in the no-future-information condition, one-future-price condition, and three-future-prices condition, respectively. There were 15, 13, and 10 subjects in the corresponding satisfaction-ratings-only conditions.

Results

Manipulation Checks. Prior to examining our hypotheses, we should first assure ourselves that the presented prices had the desired effects on regret and satisfaction. As expected, as the purchase price increased, regret ratings increased and satisfaction ratings decreased. Across all conditions, regret was positively related to price paid

TABLE 1
EXPERIMENT 1A: EFFECTS OF PRE- AND POSTPURCHASE PRICES AND CONTROL ON SATISFACTION

	One postpurchase price given				Three postpurchase prices given			
	Regret not included		Regret included		Regret not included		Regret included	
	β	t	β	t	β	t	β	t
Price paid (O)	-.11	-3.6***	-.05	-2.1*	-.01	-.2	.18	3.2**
Prepurchase comparison (PD)	-.17	-4.5***	.00	.1	-.11	-1.8 ⁺	-.06	-.8
Postpurchase comparison (FD)	-.54	-15.2***	-.13	-3.8***	-.46	-8.1***	-.08	-1.1
Control (C)	-.23	-5.0***	-.05	-1.5	-.37	-5.6***	-.26	-2.7**
C × PD	-.13	-2.9**	-.04	-1.1	-.16	-2.3*	-.11	-1.2
C × FC	-.02	-.7	-.01	-.3	-.03	-.6	-.01	-.1
Regret (R)	NA	NA	-.82	-23.2***	NA	NA	-.57	-7.5***
R ²	.63		.90		.56		.62	
N	436		199		312		155	

NOTE.—NA = not applicable.
⁺ $p < .1$.
^{*} $p < .05$.
^{**} $p < .01$.
^{***} $p < .001$.

($r(912) = .30, p < .0001$), and satisfaction was negatively related to price paid ($r(1,872) = -.31, p < .0001$). Regret and satisfaction also depended on the other prices presented. Regret was negatively related to the mean of the future prices ($r(552) = -.12, p < .004$) but was not significantly related to the mean of the past prices ($r(600) = -.05, NS$), not controlling for the other variables in the study. Satisfaction was positively related to the mean of the postpurchase prices ($r(1,152) = .12, p < .0001$) and to the mean of the prepurchase prices ($r(1,240) = .07, p < .05$).¹

The Role of Counterfactual Valence. The results of previous research show that upward comparisons, in which an obtained outcome is compared to a better possible outcome, have a greater effect on satisfaction than downward comparisons. For completeness, we test this assumption in our current purchase-timing task. First, consider the role of prepurchase prices. We can compare satisfaction when the immediately prior price was greater than, equal to, or less than the purchase price, controlling for the magnitude of the upward and downward comparison. For example, mean satisfaction was 69.3, 56.4, and 30.6 when the immediately prior price was 50¢ greater than, equal to, or 50¢ less than the purchase price, respectively. The effect of the upward comparison ($56.4 - 30.6 = 25.8$) was significantly greater than the effect of the downward comparison ($69.3 - 56.4 = 12.9; t(1) = 26.5, p < .01$). There are four such tests in each product category, and the mean difference for the upward comparison was significantly greater than the mean difference for the downward comparison in six of these eight tests. Similarly, when considering the postpurchase com-

parisons, all eight tests show the predicted pattern. Thus, upward comparisons had a greater effect on satisfaction than downward comparisons of the same magnitude for both pre- and postpurchase prices, consistent with Hypothesis 1.

Pre- versus Postpurchase Prices. What effects do pre- and postpurchase prices have on regret and satisfaction? First, we consider the case of satisfaction in situations in which subjects were given only one postpurchase price. We performed a linear regression in which we predicted satisfaction ratings from the price obtained (O), the difference between the obtained price and the immediately prior price (PD), the difference between the obtained price and the immediately future price (FD), control (C), and the C × PD and C × FD interactions. Control was effects coded in the regression: -1 denoted a voluntary purchase, and +1 denoted an involuntary purchase. Including price in the regression equations allowed us to unconfound its effects from those of control.

The resulting standardized regression coefficients and t -statistics are shown in the first two columns of Table 1. Pre- and postpurchase comparisons both had a negative and statistically significant effect on satisfaction; satisfaction decreased as the difference between the obtained price and the other prices that one could have obtained increased. Furthermore, the effect of the postpurchase comparison ($\beta = -.54$) was significantly greater than that of the prepurchase comparison ($\beta = -.17; F(1, 429) = 31.9, p < .0001$). This result indicates that postpurchase comparisons had a greater effect on satisfaction than did prepurchase comparisons.

Hypothesis 2 predicts that the effects of prepurchase comparisons depend on control. In separate regressions, we found that the effect of prepurchase comparisons was greater

¹It should be noted that other measures of pre- and postpurchase prices could be used. Similar results were seen using the maximum pre- and postpurchase prices and the immediately prior and subsequent prices.

for involuntary purchases ($t(196) = -4.69, p < .001$) than for voluntary purchases ($t(232) = -1.4, NS$). The interaction between prepurchase comparison and control was statistically significant ($t(429) = -2.9, p < .01$), consistent with Hypothesis 2.

Hypothesis 3 predicts the converse result for postpurchase comparisons, namely, that postpurchase comparisons will have a greater effect for voluntary purchases than for involuntary purchases. Separate regressions showed that the effects of postpurchase comparisons were negative and significant for both voluntary ($t(232) = -11.4, p < .001$) and for involuntary purchases ($t(196) = -10.2, p < .001$). Furthermore, the interaction between postpurchase comparison and control was not statistically significant ($t(429) = -0.7, NS$), contrary to Hypothesis 3.

Next, we performed the same regression, including regret ratings as a predictor. These results are shown in the second two columns of Table 1. Not surprisingly, regret was a significant predictor of satisfaction ($t(191) = -23.2, p < .001$). More interestingly, we found that the effects of prepurchase comparisons were no longer significant ($t(191) = 0.1$). This result, coupled with the significant effect of prepurchase comparisons on regret ($\beta = 0.16, t(191) = 2.56, p < .05$) indicates that the effects of prepurchase price comparisons on satisfaction are mediated by regret (cf. Baron and Kenny 1986). Postpurchase comparisons also significantly affected regret ($\beta = 0.48, t(191) = 8.16, p < .0001$), and including regret greatly reduced their effect on satisfaction ($\beta = -0.13$ vs. $\beta = -0.54$). However, this effect was still significant ($t(191) = -3.8, p < .001$), indicating that regret provides only partial mediation of the relationship between postpurchase comparisons and satisfaction.

We then performed similar analyses using subjects who had seen three postpurchase prices for each purchase. In this case, we predicted satisfaction from the same variables, except that we used the mean of the past and future prices instead of using the immediately past and future prices. When regret was not included as a predictor (cols. 5 and 6 of Table 1), the effects of postpurchase comparisons were again negative ($t(305) = -8.1, p < .001$), and the effect of prepurchase comparisons was marginally significant ($t(305) = -1.8, p < .07$). The effect of postpurchase comparisons was also greater than that of prepurchase comparisons ($F(1, 305) = -13.3, p < .001$). Furthermore, the effects of prepurchase comparisons again depended on control. The effect of prepurchase comparison was negative and significant when purchases were involuntary ($t(129) = -2.5, p < .05$), but not when purchases were voluntary ($t(175) = -0.5, NS$), and the interaction between prepurchase comparison and control was significant ($t(305) = -2.4, p < .05$). The effects of postpurchase comparisons were significant both for voluntary and involuntary purchases ($t(175) = -6.5$, and $t(129) = -5.6$, respectively), and postpurchase comparison did not interact significantly with control ($t(305) = -0.6, NS$). When regret was included as a predictor (cols. 7 and 8 of Table 1), the effects

of pre- and postpurchase comparisons disappeared ($t(147) = -0.8$, and $t(147) = -1.1$, respectively, both NS). Combined with the significant effect of regret on satisfaction ($t(238) = -19.4, p < .0001$) and the effects of postpurchase comparisons on regret ($\beta = 0.47, t(175) = 6.2, p < .0001$), our results establish that regret mediates the significant effects of price comparisons on satisfaction.

Discussion

The results of experiment 1A show that counterfactual price comparisons have a significant effect on satisfaction in a purchase-timing scenario. Both pre- and postpurchase price comparisons tend to have negative effects on satisfaction, and upward comparisons tend to have greater effects than downward comparisons, both as expected. Furthermore, postpurchase comparisons have a greater effect than prepurchase comparisons. However, the control that the consumer has over the timing of the purchase appears to influence the magnitude of counterfactual effects: The effect of prepurchase comparisons was greater for involuntary than for voluntary purchases, consistent with Hypothesis 2. The effect of postpurchase comparisons, however, did not vary with control, contrary to Hypothesis 3. Finally, the evidence shows that regret mediates the effects of price comparisons on satisfaction.

EXPERIMENT 1B

In study 1A, subjects often saw fewer postpurchase prices than prepurchase prices. It could therefore be claimed that the postpurchase prices have a greater effect simply because there are fewer of them, and thus they each command a greater proportion of attentional resources. Furthermore, it could be argued that the probability of being forced to buy in the current period was relatively low, allowing subjects to focus more attention on the postpurchase prices than on the prepurchase prices. Finally, because only half the subjects in experiment 1A rated both regret and satisfaction, the above tests that include regret have fewer observations than those that do not include regret. Thus, the nonsignificant results in the presence of regret might be accounted for by a lack of power.

To explore these issues, we ran a second study. Experiment 1B was similar to experiment 1A in all respects, except for the following. First, for every trial, we showed the same number of pre- and postpurchase prices. For each trial, we took the number of prepurchase prices that the subject had seen and showed him or her an equal number of postpurchase prices, implying that the number of postpurchase prices depended on subjects' stopping decisions. This procedure removes the confound between number of prices and pre- versus postpurchase prices. Second, we increased the likelihood of being forced to purchase on a trip from 20 percent to 33 percent. Last, all subjects rated their regret and satisfaction with every purchase. A total of 72 subjects participated in this study.

The results of experiment 1B are shown in Table 2. Re-

TABLE 2

EXPERIMENT 1B: EFFECTS OF PRE- AND POSTPURCHASE PRICES AND CONTROL ON SATISFACTION

	Regret not included		Regret included	
	β	t	β	t
Price paid (O)	-.10	-3.0**	-.02	-.8
Prepurchase comparison (PD)	-.18	-3.7***	-.05	-1.6
Postpurchase comparison (FD)	-.41	-8.5***	-.06	-1.7+
Control (C)	-.23	-4.3***	-.04	-1.3
C \times PD	-.12	-2.3*	-.01	-.3
C \times FC	-.01	-.2	-.02	-.5
Regret (R)	NA	NA	-.79	-25.3***
R^2		.52		.80
N		472		472

NOTE.—NA = not applicable.

+ $p < .1$.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

sults closely paralleled those in experiment 1A. When regret was not included as a predictor, we observed significant negative effects of the pre- and postpurchase comparisons on satisfaction ($t(465) = -3.7$, and $t(465) = -8.5$, p 's $< .001$, respectively). Prepurchase comparisons had a significant effect for involuntary purchases ($t(280) = -5.3$, $p < .001$), but not for voluntary purchases ($t(184) = -1.5$, NS), consistent with Hypothesis 2. The interaction of prepurchase comparisons with control was significant ($t(465) = -2.3$, $p < .05$). Postpurchase comparisons, however, had significant effects for both voluntary ($t(184) = -5.0$, $p < .001$) and involuntary ($t(280) = -7.7$, $p < .001$) purchases. The interaction between postpurchase comparisons and control did not reach significance ($t(465) = -0.2$, NS), contrary to Hypothesis 3.

When regret was included as a predictor, it was found to be significant ($t(464) = -25.3$, $p < .001$), and all other factors had nonsignificant effects. Coupled with the fact that pre- and postpurchase comparisons were significant predictors of regret ($\beta_{PRE} = 0.17$, $t(464) = 3.48$, $p < .001$; $\beta_{POST} = 0.45$, $t(464) = 9.57$, $p < .0001$ for pre- and postpurchase prices, respectively), this result establishes the mediating role of regret in the effects of pre- and postpurchase comparisons on satisfaction.

Discussion

Experiments 1A and 1B demonstrate that counterfactual price comparisons affect satisfaction, especially when the comparison price is lower than the price paid. Consumers are less satisfied when the price paid is more than other prices that they could have paid, either by purchasing earlier or by purchasing later. However, the effects of pre- and postpurchase prices are not symmetric. The effects of post-

purchase comparisons are consistently greater than those of prepurchase comparisons. In addition, the effects of prepurchase comparisons interact with control whereas the effects of postpurchase comparisons do not. Prepurchase comparisons have a greater effect for involuntary purchases than for voluntary purchases, consistent with Hypothesis 2. The effects of postpurchase comparisons, however, were always present, contrary to Hypothesis 3. This last result is particularly intriguing. It implies that consumers consider the prices offered after they made a purchase, despite the fact that they ran out of the product and could not have deferred their purchase to obtain the better price. In other words, subjects' satisfaction ratings were affected by comparisons with prices that they could never have obtained. Note that this result does not necessarily imply that consumers will actively search for postpurchase information after an involuntary purchase. We are unable to address this question with our current studies. However, consumers are frequently exposed to postpurchase information without actively searching for it. Our results suggest that these comparisons affect satisfaction with the purchase, even when consumers had no control over the timing of that purchase.

Finally, we demonstrate that the effects of these counterfactual prices on satisfaction are mediated by regret. This result suggests that regret may prove very powerful for understanding and controlling consumer satisfaction. But first, we must establish that consumers do not simply experience this regret, but also learn to anticipate and avoid it in their purchase-timing decisions. The second set of experiments will examine if consumers also learn to anticipate these effects and adjust their behavior accordingly.

Do Consumers Avoid Future Regret?

Experiments 1A and 1B studied how price comparisons affect consumers' satisfaction with their purchase: upward comparisons have a greater effect than downward comparisons, and postpurchase comparisons have a greater effect than prepurchase comparisons. In other words, the most important effect of additional price information is the "future regret" created by comparisons with lower prices offered after the purchase. Although we know that future regret is the most important consequence of comparisons with additional prices, we do not know if consumers naturally try to avoid future regret. Experiments 2A and 2B will explore this issue by studying how the experience of postdecision comparisons influences subjects' behavior.

EXPERIMENT 2A

The following experiments examined the effect of postdecision information on subjects' behavior in the context of an economic search task (Hey 1981, 1982; Simon 1955; Stigler 1961). The economic search task is similar to the purchase-timing task used in experiments 1A and 1B. It presents subjects with values over time and requires them to either accept the value or continue with the next value, knowing that there is a chance of being forced to accept the

next value. However, unlike experiments 1A and 1B, subjects in this task attempt to maximize value rather than minimize cost—that is, they are looking for the best value, rather than for the lowest price. We changed the task from one of minimizing costs to one of maximizing gains as we wanted to motivate subjects by paying them based on their performance. Because of ethical and logistic concerns, it was not desirable to have subjects incur real losses. Nor could we provide them with a prior budget to offset these losses, as this approach has been shown to alter subjects' decision strategies (Thaler and Johnson 1990). In sum, if we wanted to tie financial consequences to their performance, we had to work with gains instead of losses.

In each trial, subjects were presented with a series of values, which were uncovered sequentially. The values were independently drawn from a uniform 1 to 100 distribution. For each value the subject had to decide whether to accept the value or continue in the hope of uncovering a better value. However, there was a 10 percent chance that she would be forced to accept the next value if she continued, a situation analogous to running out of a product and being forced to buy. After having accepted a value, subjects in the experimental condition were shown what the next values would have been, while subjects in the baseline condition did not receive any postdecision information. By manipulating the presence of postdecision information, we could test whether the experience of postdecision regret leads people to anticipate and avoid it. Postdecision values were drawn from the same distribution as predecision values, implying that all values were equally informative. After all the values had been presented, subjects rated their satisfaction with the obtained outcome and continued with the next trial.

All subjects were told that the optimal strategy for maximizing their earnings was to select a threshold value and accept all values greater than that threshold (analogous to a reservation price in a purchase-timing task). Subjects were also informed that there was an optimal threshold value, which they had to discover through experience. To this end, subjects were provided with a large number of learning trials. In each trial, subjects continued until they reached a value that was acceptable or until they were forced to stop and accept the current value. The learning trials were followed by a single test trial in the beginning of which subjects had to explicitly state a threshold value. The computer then played out one trial using the stated threshold: Values were successively displayed until a value greater than or equal to the threshold was encountered or until the subject was forced to stop and accept a value below her threshold. Thus, the subject's task was analogous to that of providing an agent with a reservation price in a purchase-timing task. Subjects were paid according to the value obtained in the test trial. By comparing the average thresholds selected in the experimental and baseline conditions, we can assess whether providing postdecision information affected subjects' decision strategies.

Will there be any difference between the thresholds selected by the experimental and baseline groups? Let us con-

sider two possible perspectives on consumer behavior. The first perspective holds that consumers will try to maximize the expected value of their outcomes, implying that their decision strategies will not be affected by the experience of regret. The second perspective, however, holds that consumers are primarily motivated by the hedonic consequences of their outcomes and will therefore try to minimize the experience of regret. In experiments 2A and 2B, the maximization of value and the minimization of regret are placed in direct conflict with each other. Subjects may maximize the expected value of their decisions, but only by increasing the regret that they are likely to experience. Likewise, subjects may minimize the regret that they experience, but only at the expense of their expected earnings. Our point is not to say that one strategy or the other is "correct." Instead, we want to examine which strategy subjects adopt when the two goals conflict with each other. Let us consider how subjects should approach the experimental task according to each perspective.

The first perspective holds that consumers are primarily motivated by the utility of the alternatives. For this perspective to hold any predictive power, the utility that an alternative offers must be independent of the other alternatives available. According to this view, utility is unaffected by presenting postdecision information. Although the regret associated with stopping too early may be psychologically uncomfortable, experienced regret has no bearing on the consumer's choice. Together with the fact that pre- and postdecision values are drawn from the same distribution, this implies that the presentation of the postdecision information in the learning trials should not affect the threshold specified in the test trial. In other words, there should be no difference between the average thresholds specified in the experimental and baseline conditions.

H4: The availability of postdecision comparisons does not affect consumers' subsequent decisions.

The second perspective holds that consumers are primarily motivated by the hedonic consequences of their outcomes. The results of experiments 1A and 1B indicate that the presence of postdecision information will mainly affect consumers' satisfaction through "future regret" (i.e., comparisons with more favorable outcomes that the consumer could have obtained if she had continued). If consumers are indeed motivated by the hedonic consequences of their outcomes, they should learn to anticipate this future regret and try to avoid it as much as possible. The only way to avoid this regret is by requiring better prices before making a purchase. In other words, subjects in the experimental condition should learn to avoid future regret during the learning trials and set a higher threshold in the test trial than do subjects in the baseline condition.

H5: The availability of postdecision comparisons changes consumers' decision strategies: consumers who have received postdecision information will avoid situations that tend to produce postdecision regret.

Method

Procedure and Instructions. Experiment 2A consisted of two parts: a learning phase and a test phase. In the learning phase, the task was explained to the subjects, and they were told that they could maximize their earnings by selecting a particular threshold value, although they were not told this value. Given the task parameters, subjects should set a threshold of 76 in order to maximize their earnings (see the Appendix for a derivation of this threshold). They were then provided with 80 learning trials in which they could attempt to learn this threshold value. We provided this many trials so that subjects would have ample opportunity to experiment with different thresholds and discover one that they liked. Subjects in the baseline condition saw only the value that they received and the values that they had previously foregone. Subjects in the experimental condition saw these values as well as the next three values in the series. All values were independently and randomly drawn from a uniform distribution ranging from 1 to 100. After each trial, subjects in both groups evaluated their satisfaction with their outcome.

In the test phase, subjects were asked to explicitly state a threshold value, after which one trial of the task was played out according to their stated value. Values were successively displayed until one was found that had a value greater than or equal to their specified criterion or until the subject was stopped involuntarily. Subjects were paid 5¢ for every point on the paid trial. (Payments could range from 5¢ to \$5.00.)

Participants. Fifty-two undergraduate students participated in this experiment for extra course credit. Twenty-three subjects participated in the baseline condition, and twenty-nine subjects participated in the experimental condition. Seven additional subjects were excluded from the analyses due to computer malfunction or failure to follow instructions.

Results

As in the previous studies, we found that pre- and postdecision information influenced satisfaction. We performed two regressions on the learning trials: The first predicted satisfaction among baseline group subjects based on the obtained value (O), the difference between this value and the mean of the prior values (PD), whether the decision to stop was voluntary or involuntary (C), and the PD × C interaction. The second predicted satisfaction for experimental group subjects based on the same factors as used in experiments 1A and 1B. The results are shown in Table 3. As before, pre- and postdecision information affects satisfaction. (Estimated coefficients for these effects are positive because subjects are attempting to maximize value rather than minimize cost.) Furthermore, the effect of postdecision comparisons ($\beta = .20$) is greater than the effect of predecision information ($\beta = .07$, $F(1, 1,933) = 10.6$, $p < .001$). Finally, the interaction between predecision comparisons and control is marginally significant ($t(1,933) = 1.9$, $p <$

TABLE 3

EXPERIMENT 2A: EFFECTS OF PRE- AND POSTDECISION INFORMATION ON SATISFACTION

	No postpurchase prices		Three postpurchase prices	
	β	<i>t</i>	β	<i>t</i>
Price paid (O)	.53	15.6***	.33	7.7***
Prepurchase comparison (PD)	.05	2.1*	.07	2.5**
Postpurchase comparison (FD)	NA	NA	.20	7.2***
Control (C)	-.31	-9.6***	-.27	-8.2***
C × PD	-.03	-1.3	.05	1.9*
C × FD	NA	NA	-.00	-.2
<i>R</i> ²	.74		.62	
<i>N</i>	1,560		1,940	

NOTE.—NA = not applicable.

* $p < .1$.
 ** $p < .05$.
 *** $p < .01$.

.06), whereas the interaction between postdecision comparisons and control is not significant ($t(1,933) = -0.2$, NS). In short, our results show that despite the difference in surface characteristics of the tasks in experiments 1A and 1B and experiment 2A, pre- and postdecision information affects satisfaction in a similar fashion.

Decision Thresholds. The mean threshold for the baseline group was 75.8 (with a standard deviation of 10.9), and the mean threshold for the experimental group was 81.4 (8.6). The difference between these two means was statistically significant ($t(50) = 1.84$, $p < .05$, one-tailed). Furthermore, the mean threshold for the baseline group does not differ statistically from 76, the threshold suggested by the expected value maximizing rule ($t(22) = .06$, NS), suggesting that in the absence of postchoice information, these subjects learned, on average, to maximize their earnings. The mean threshold for the experimental group, however, did differ significantly from the expected value maximizing rule ($t(28) = 3.39$, $p < .01$, one-tailed). These results are inconsistent with the view of consumers as expected value maximizers expressed in Hypothesis 4. However, the results are consistent with the view of consumers as hedonic maximizers expressed in Hypothesis 5.

It is important to point out that in selecting higher decision rules to minimize regret, subjects also reduced their average earnings. Subjects not receiving future information earned an average of \$3.89, whereas subjects in the future information condition earned an average of \$3.36. Despite the stochastic nature of subjects' earnings, the difference between conditions is marginally significant ($t(50) = 1.35$, $p < .1$, one-tailed). Furthermore, the observed differences in criteria cannot be attributed to differences in subjects' perceptions of the distributions. Subjects who received no future information estimated that the mean of all the values they had seen was 60.3, whereas subjects who received three

future values estimated that the mean was 62.2. These means are not significantly different ($t(34) = .56$, NS), suggesting that both groups perceived the distribution similarly.

Finally, we performed an analysis to test whether the higher criteria in the experimental condition were indeed the result of the anticipation of regret. The satisfaction ratings given by each subject in the experimental condition were regressed on obtained values and maximum future counterfactual values. This process yielded a separate regression coefficient for each subject, and each coefficient indicated the extent to which that subject's satisfaction was influenced by future counterfactuals. Standardized regression coefficients were then used to predict the criteria chosen by these subjects. Subjects whose reported satisfaction ratings were most influenced by the future values also tended to chose higher criteria ($t(28) = -3.31$; $p < .01$), consistent with our explanation.

Discussion

The results of experiment 2A indicate that people who receive postdecision information do adjust their decisions. The direction of the change is consistent with the hypothesis that people avoid regret by setting higher decision thresholds. Furthermore, the above analysis shows that subjects whose reported satisfaction ratings were most sensitive to postdecision information also tended to select higher thresholds, indicating that changes in satisfaction were linked to the observed changes in thresholds.

However, providing postdecision values may influence subjects in a number of ways that are unrelated to regret, and these changes may in turn cause subjects to change their decision strategies. For example, subjects who received postdecision values may also have found the task more enjoyable or engaging, and this may have caused them to change their strategies.² Furthermore, experiment 2A did not directly ask subjects to rate the regret they felt after each decision. We did this to insure that our results could not be attributed to demand artifacts, but it also meant that we were unable to directly assess the role of regret in the study.

EXPERIMENT 2B

In order to gain further evidence of regret-avoidance and to rule out alternative explanations, we ran a second study similar to experiment 2A. Experiment 2B was the same as experiment 2A except for the following: First, subjects were asked to rate the regret that they felt after each decision. Second, subjects were not given a financial incentive to maximize points. Third, after the test trial had been played out, subjects were asked to rate the degree to which they felt involved in the game. Fourth, all data was collected over the Internet, using the same subject population (but not the same subjects) as in the previous studies.

A total of 80 subjects were randomly assigned to the two conditions. We excluded 13 subjects whose total task du-

ration was less than 10 minutes from the analysis, resulting in 39 subjects in the baseline condition and 28 subjects in the experimental condition.

Results

Results in experiment 2B paralleled those of experiment 2A. We fitted linear regression models like those for study 2A. The parameter estimates for these models are shown in Table 4. Parameter estimates of the obtained value (O), the prepurchase comparison (PD), and the postpurchase comparison were negative and statistically significant in all cases. (These relationships are negative, unlike those in Table 3, because here we are predicting subjects' regret ratings, as opposed to their satisfaction ratings.) Furthermore, the effect of postpurchase comparisons is greater than that of prepurchase comparisons ($F(1, 1,314) = 3.44$, $p < .06$), consistent with previous results.

We also found that criteria were higher for subjects who received postdecision information than for subjects who did not. The mean threshold was 76.5 in the baseline condition and 81.5 in the experimental condition. The difference between the two sets of thresholds was marginally significant ($t(65) = -1.5$, $p < .06$, one-tailed). Once again, subjects who received postdecision information tended to set higher decision thresholds and, as a result, received fewer points on average (73.7 vs. 80.0 for the baseline condition).

Recall that the decision thresholds were 75.8 (baseline condition) and 81.4 (experimental condition) in study 2A. It is remarkable how similar the mean thresholds are across studies 2A and 2B, given the changes in experimental procedure. Study 2B did not provide subjects with financial incentives for performance or ask subjects to rate their satisfaction on each trial, and it was run with very little control over the testing environment (i.e., over the Internet). Despite the increased noise these changes engendered, mean deci-

TABLE 4
EXPERIMENT 2B: EFFECTS OF PRE- AND POSTDECISION INFORMATION ON REGRET

	No postpurchase prices		Three postpurchase prices	
	β	t	β	t
Price paid (O)	-.24	-6.2***	-.16	-2.7**
Prepurchase comparison (PD)	-.27	-7.7***	-.18	-4.5***
Postpurchase comparison (FD)	NA	NA	-.29	-7.2***
Control (C)	.28	9.2***	.10	2.1*
C \times PD	-.08	-3.6***	-.12	-3.3***
C \times FD	NA	NA	.16	5.2***
R^2	.47		.45	
N	1,865		1,320	

NOTE.—NA = not applicable.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

**** $p < .001$.

²We thank a reviewer for pointing out this possibility.

sion thresholds were virtually identical. The increased noise in study 2B may, however, account for the marginal significance of the manipulation. If the data are combined across the two studies, the effect of comparison condition is shown to be quite robust ($t(117) = -2.4, p < .01$).

Furthermore, the differences between conditions in study 2B could not be attributed to differential involvement. Subjects in the experimental condition rated their involvement as slightly greater than subjects in the baseline condition (means of 6.1 vs. 5.8), but the difference was not statistically significant ($t(65) = -0.6, NS$).

GENERAL DISCUSSION

Counterfactual Determinants of Satisfaction

Purchase-timing decisions often involve the possibility of counterfactual comparisons. We can compare the price that we paid for a good to the price that we would have paid had we bought earlier or later. This research is an attempt to examine how counterfactual comparisons affect satisfaction and decision strategies in purchase-timing decisions. We find that evaluations are not made in a consequentialist fashion. Our satisfaction depends not only on what we receive but also on what we could have received but did not. Counterfactual price comparisons affect satisfaction in a number of interesting ways.

First, while both pre- and postpurchase comparisons can affect satisfaction, their effects are not identical. Postpurchase comparisons have a greater effect on feelings of regret and purchase satisfaction than do prepurchase comparisons. This result may reflect the way in which consumers learn about distributions of prices and qualities. At any given point in a purchase sequence, all the information to be gleaned from prior prices has already been incorporated into the current decision. However, future prices may have greater significance because they provide additional information about the distribution that has not yet been incorporated into the decision.

Second, the degree to which consumers control the timing of their purchases influences their reactions to price comparisons. Prepurchase comparisons have less effect on satisfaction when subjects controlled the timing of their purchase than when they did not. However, the effects of postpurchase comparisons did not depend on consumer control. Postpurchase comparisons had a significant effect in both voluntary and involuntary purchases. The fact that consumers consider postpurchase prices even in situations in which the distribution of prices is known and the future prices are not obtainable points to the possible automaticity of these comparison processes. We find this result counterintuitive: it conflicts with functional theories of regret, which posit that consumers should only feel regret when it serves to improve their subsequent decisions (Markman et al. 1993; Roesse and Olson 1995a). This result also conflicts with attributional theories of emotion, which posit that causal attributions are necessary to produce particular emotions (Weiner 1986).

Third, the effects of postpurchase prices depend on the valence of the comparison, as has been suggested by other researchers (Kahneman and Miller 1986; Markman et al. 1993; Roesse and Olson 1993). Experiments 1A and 1B demonstrate that upward comparisons have greater effects than downward comparisons, consistent with the phenomenon of loss aversion.

Making Decisions by Anticipating Counterfactuals

A considerable body of literature has demonstrated that counterfactual comparisons can have a substantial impact on satisfaction. More recently, work has demonstrated that people have insight into these counterfactual effects; people can reliably anticipate their feelings in the face of various counterfactual comparisons (Mellers et al. 1997; Schwartz 1998). Other work has shown that explicitly anticipating regret associated with an outcome can lead consumers to alter their decisions (Simonson 1992).

A growing body of literature suggests that given a choice between equally valued alternatives, people often choose the option offering less feedback and, therefore, less opportunity for regret (Zeelenberg and Beattie 1997; Zeelenberg et al. 1996). Our second experiment is consistent with this result. Furthermore, our results suggest that people may avoid regret in situations in which there is substantial disincentive. In experiments 2A and 2B, subjects who received postdecision information did select significantly higher criteria, thereby avoiding the greatest regrets. However, this strategy led to lower earnings (of actual money in 2A and of points in 2B).

These results have important implications for consumer tasks that involve monitoring information over time (e.g., buying a computer, selling a used car, or trading stocks). It is natural to assume that consumers will make better decisions if they are given more information or at least that providing additional information should not impair their decisions. However, the preceding studies suggest that people who continue to monitor outcomes after making a decision may actually perform worse on subsequent decisions.

Experiments 1A and 1B showed that people tend to focus on better outcomes that occur after their decisions. Experiments 2A and 2B demonstrated that people try to avoid these negative comparisons by deferring their decisions. However, adopting this strategy resulted in subjects being forced to make unattractive purchases. While these purchases did not produce strong feelings of regret, they did lower objective performance in the task. Like the phenomenon of meaningless differentiation (Carpenter, Glazer, and Nakamoto 1994), these results suggest that providing consumers with nondiagnostic product information may lead consumers to make worse decisions.

Why Consider Regret?

Counterfactual phenomena pose something of a conundrum for behavioral researchers. Why do people compare their outcomes to outcomes that did not occur? Some authors

have postulated that upward comparisons serve to direct our attention toward events from which we should learn (e.g., Markman et al. 1993; Roesch and Olson 1995b). According to this view of regret, the reason that we compare our outcomes to better outcomes that we did not obtain is to teach us that our decision was suboptimal.

In experiment 2A, however, the normative goal is to maximize the money that one earns, which is accomplished by learning the optimal decision criterion. Subjects who do not receive any future counterfactual information behave, on average, in this fashion. However, subjects who receive future counterfactual information select higher decision criteria, on average, and correspondingly fare less well in their earnings.

Why do subjects make upward comparisons in experiments 2A and 2B, if those comparisons only induce them to adopt a suboptimal decision strategy? One possibility is that upward comparisons teach us better decision strategies in most situations but can cause us to adopt suboptimal strategies in a given situation. Because we are largely unable to consciously control whether or not we make counterfactual comparisons, we are unable to determine in which situations these comparisons are beneficial and in which situations they are detrimental.

A second possibility is that we may never have learned that counterfactual comparisons lead to suboptimal outcomes. The feedback that we receive in most situations comes from information about outcomes obtained and not obtained in a given situation. If counterfactual comparisons affect our satisfaction, and we maximize satisfaction, there is little to teach us that we should not consider those comparisons. Given that we make counterfactual comparisons, the only way to maximize value is at the cost of satisfaction. It may be difficult to follow a course that, while right in the mind, feels wrong in the soul.

APPENDIX

The following solution depends on the nature of the distribution of values. Assume that values are independent and uniformly distributed from 1 to 100 and that the probability of being stopped by the computer is 0.1. Denote the decision criterion x . On any given value, the probability that the next value will be greater than or equal to x is $(100 - x)/100$, and the probability that it will be less than x is $x/100$. If the current value is greater than or equal to x , you will accept the value. Otherwise, you will attempt to continue to the next value. However, there is a 0.1 probability that the computer will stop you on the current value. If you are not stopped, the decision repeats itself with the same parameters.

The probability of receiving a value greater than or equal to x on the first window is $(100 - x)/100$. The probability of receiving a value greater than or equal to x on the second window is $(x/100) \cdot 0.9 \cdot (100 - x)/100$, because there is an $x/100$ probability that an acceptable value was not received on the first window, a 0.9 probability that you were allowed to continue to the second window, and a $(100 - x)/100$ probability that the second window's value was ac-

ceptable. Proceeding in this manner, the overall probability of receiving a value greater than or equal to x is given by

$$p(\text{win}) = \sum_{i=1}^{\infty} \frac{(100 - x)}{100} \cdot \left(.9 \cdot \frac{x}{100} \right)^{i-1} \quad (\text{A1})$$

This is the sum of a geometric series. Because $x \leq 100$, $|.9 \cdot x/100| < 1$, and the series is convergent. Its sum is equal to (Leithold 1986):

$$p(\text{win}) = \left(\frac{100 - x}{100} \right) \cdot \left(\frac{100}{100 - .9x} \right) = \frac{100 - x}{100 - .9x} \quad (\text{A2})$$

(This value is the theoretical probability of obtaining a value greater than or equal to x . Alternatively, one can compute the first sum from one to 16 to calculate the empirical probability.)

Because values are independently and uniformly distributed $[1, 100]$, the average value when one receives a value greater than x is $(x + 100)/2$, and the average value when one receives a value less than x is $x/2$. Thus, the expected value of continuing when the decision criterion is x is

$$EV = p(\text{win}) \cdot \frac{x + 100}{2} + (1 - p(\text{win})) \cdot \frac{x}{2} = \frac{100^2 - .9x^2}{200 - 1.8x} \quad (\text{A3})$$

Differentiating EV with respect to x and setting the derivative equal to zero results in

$$\frac{d(EV)}{dx} = \frac{(200 - 1.8x) \cdot (-1.8x) - (100^2 - .9x^2) \cdot (-1.8)}{(200 - 1.8x)^2} = 0 \quad (\text{A4})$$

For $d(EV)/dx$ to be defined, $200 - 1.8x$ cannot equal zero, implying that x cannot equal 111. Because we are only interested in x between one and 100, we need only consider the numerator. We set it equal to zero, resulting in the following quadratic equation:

$$1.62x^2 - 360x + 18,000 = 0 \quad (\text{A5})$$

This equation has one root in the range $[1, 100]$, at $x = 76$. Thus, under the assumptions specified, the optimal criterion for maximizing value is 76 points.

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