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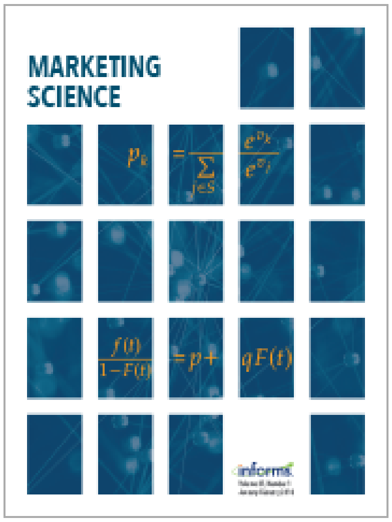

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Biases in Valuation vs. Usage of Innovative Product Features

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We investigate biases in product valuation and usage decisions that arise when consumers consider a new generation of a product that offers an expanded set of capabilities of uncertain value. Two experiments using a novel computer game show evidence of a *valuation-usage disparity*: Participants display a high willingness to pay for a new version of the game that offers a new set of controls, but fail to fully exercise the option to use these controls after purchase. This discrepancy is attributed to a fundamental difference in how new capabilities are valued at the time of purchase versus use. Consumer usage decisions appear to be driven by such myopic concerns as a desire to avoid short-term learning costs, whereas purchase decisions often fail to take into account the factors that drive usage, and are further inflated by global optimism in the future usefulness of new capabilities. We show that this lack of foresight can be explained by an intertemporal judgment model in which consumers attempt to value the option to use new capabilities as would be prescribed by economic theory, but are prone to *hyperbolic discounting* in their temporal valuation of present versus future costs and benefits.

Key words: new product adoption; judgment under uncertainty; hyperbolic discounting; optimism bias; projection bias; option theory
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Introduction

Rarely can consumers predict with certainty the amount of pleasure they will derive from new product features prior to ownership. While a car shopper might anticipate that a navigation system will increase the enjoyment of driving a car, and a cell phone shopper might be attracted to a model offering a built-in MP3 player, the incremental utility of these features often can only be known by experiencing them over a period of use (e.g., Guo 2006). In this way consumer decisions to adopt new generation products to gain access to novel capabilities might be viewed as analogous to risky investments in options: By purchasing the product, the consumer is not acquiring a known stream of benefits, but rather a flexible stream whose value depends on what is learned about the utility of new capabilities during the early stages of use, and how this knowledge is used to guide subsequent usage decisions.

In this paper, we investigate biases that arise when consumers are asked to make such prepurchase assessments of the value of experiential product attributes. We build on previous research on consumer judgments of unfamiliar new products (e.g., Mukherjee and Hoyer 2001, Thompson et al. 2005, Wathieu and Bertini 2007) by empirically examining the degree to which *prepurchase valuations* accurately anticipate *postadoption* patterns of attribute learning and use. Our core finding is that the relationship between valuation and use is not as strong as rational theories of consumer choice might predict. Given the opportunity to purchase a product that offers an expanded set of capabilities participants reveal a high willingness to pay (WTP), suggestive of highly optimistic beliefs about the long-term benefits of their use. After paying to acquire the product, however, adopters make inefficient use of the capabilities they paid for, e.g., by procrastinating investments in learning and making inconsistent use of features that offer the highest performance. We attribute this discrepancy to a fundamental difference in how consumers evaluate products when they are considered for future versus immediate consumption, with the former failing to fully take into account the short-term factors (such as learning costs) that form the focus of the latter.

We organize the presentation of our research in three phases. First, we provide a theoretical framework for our analysis by describing the empirical problem that is the focus of our study, and reviewing how it would be solved by a rational consumer seeking to maximize the long-term utility of ownership. Second, we develop a series of hypotheses about how biases in intertemporal choice found in other contexts might cause actual adoption and usage behavior to depart from this normative benchmark. Finally, we present evidence from two experiments that test the empirical validity of our hypotheses.

Background

In this work, we examine how consumers solve a class of new product adoption problems that share the following structure. A consumer has a durable good in working condition that is equipped with an array of features of known utility (e.g., a conventional cell phone with calling and text messaging). The consumer is given the opportunity to purchase a next generation model that augments these basic features with an expanded set of novel capabilities (e.g., voice-to-text conversion for messages) whose utility is uncertain but can be discovered through use. These new capabilities operate independently of the more traditional ones, such that if they go unused, the utility provided by the next generation model reverts to that provided by the incumbent (in this case, a conventional cell phone).

Note that this structure excludes innovations where new features are an integral part of the design or functioning of a product, such as capabilities that come as part of a new operating system or cosmetic add-ons to a car. In the current case, the sole downside risk in adopting the next-generation product is the foregone opportunity cost of having paid too much for new capabilities that are never used. In our conclusion, we extend the ideas developed here to a more general class of buying problems.

How good are consumers at anticipating future utility and use in such contexts? Although a great deal of literature exists that has explored how consumers come to value new product features (e.g., Hauser et al. 2006, Nowlis and Simonson 1996, Wathieu and Bertini 2007), we are aware of no work that has examined the degree to which valuations of attributes made at the time of purchase rationally foreshadow actual patterns of subsequent use. One notable exception is a recent study by Thompson et al. (2005) who offer evidence that consumers may be prone to overestimating the value they will later extract from product attribute enhancement. In a laboratory study they found that participants were most attracted to those innovations that offered the widest assortment of bells and whistles prior to use, but that more complex models were associated with a higher level of dissatisfaction after use. Whether this dissatisfaction is evidence that consumers are systematically prone to overbuying technology is far from clear. As suggested earlier, one explanation may be that participants were simply making rational speculative investments in attribute options that did not pay off; that is, decisions to adopt new technology whose features did not prove as worthwhile as could have been the case ex ante.

The Rational Principles of Product Valuation and Postadoption Usage

If a consumer's goal is to pay an amount that reflects the long-term utility he or she will receive through use, how should the value of uncertain new product capabilities be assessed? If the consumer is a student of finance or economics, she might recognize the problem as akin to that of valuing a risky financial option (see, e.g., Amram and Kulatilaka 1999). Specifically, she should first assign a subjective likelihood to the different possible discoveries she might make about the quality of the new capabilities, and then formulate a policy for how subsequent usage decisions (e.g., permanently abandon use if the experienced utility is below a certain threshold) will be made in light of possible discoveries about quality. The rational value of the capabilities would then be the expected value of the usage policy applied to the prior distribution of possible experienced qualities.

This analysis can be illustrated through a simple example. Consider the case of a consumer who owns a product that conveys utility through an incumbent feature α, and who is considering purchasing a new generation that augments α with a new capability δ. Use of the new capability is discretionary, such that at any time, the consumer can either draw utility from the new product by using the traditional feature α alone (e.g., use a new cell phone in the traditional manner) or in conjunction with the new feature δ (such as by using a new voice-to-text feature to retrieve calls). The incremental utility provided by δ is uncertain at the time of purchase, but can be discovered after a single period of learning—a problem similar to that recently considered by Guo (2006). Upon using δ for the first time, the consumer incurs a learning cost c (reflecting, e.g., down time needed to read a manual), and after the first use the consumer discovers that the new capabilities either enhance the overall utility of the good by the amount η or diminish it by an amount γ. The probabilities of these two outcomes are p and $(1-p)$, respectively.

Note that in this case the consumer's optimal usage policy (π^*) is to learn the true quality of δ as soon as possible after assuming ownership, then use the new feature on all future occasions if it proves worthwhile (hence realizing η) or permanently abandon its use if not (avoiding the loss $-\gamma$; see; e.g., DeGoot 1970). To compute the ex ante value of owning the new capability given π^*, assume that ownership is considered over an infinite horizon, and evolves in three stages: the purchase decision is made at time $t=0$, learning costs (c) are incurred at $t=1$, and the first period of realized benefits (either η or γ) occurs at $t=2$. The lifetime expected value of the option of owning $\delta(V(\delta \mid \pi^*))$ is thus

$$V(\delta \mid \pi^*) = -\beta c + p\sum_{t=2}^{\infty}\beta^t\eta - (1-p)\beta^2\gamma, \qquad (1)$$

where β is the consumer's rate of discounting.

Verbally, Equation (1) articulates the central trade-off that the consumer faces when deciding whether it is worthwhile to acquire the new capabilities: one trades off up front learning costs (c) and a $(1-p)$ chance that the new capabilities will not prove worthwhile for a chance p of enjoying the superior long-run discounted stream of utility $\sum_{t=2}^{\infty}\beta^t\eta$. Adoption of the innovation will thus be worthwhile if the balance of these trade-offs (Equation (1)) is greater than the utility-scaled purchase price.

Why Ex Ante Valuations Might Overstate the Expost Utility of Usage

It is important to emphasize that the general approach to computing the option value of a new capability described does not necessarily require that the consumer's conditional usage policy be optimal. If a consumer is aware that she is prone, for example, to procrastinate when faced with the task of learning about new capabilities after ownership, she could, in principle, factor that into the prior valuation of ownership by defining an alternative conditional usage policy π. But here is the catch: While such flexibility theoretically exists, it presumes that consumers can perfectly foresee such biases. That is, the decisions about use the consumer *foresees* herself making will be the same as those that she will actually make at the time of use.

How likely is such an assumption to hold in practice? Central to this research is a hypothesis that decisions about product purchase and use will be driven by separate judgment systems that display limited intertemporal consistency. Specifically, judgments about value made at the time of purchase are hypothesized to frequently display signs of excessive optimism both about the potential benefits of new capabilities and their long-term frequency of use. Judgments about use, however, will display little obligation to optimistic prior assessments, displaying procrastination and variety-seeking biases that act to suppress realized levels of use. The joint effect of this discrepancy will be a tendency for expressions of WTP for new capabilities to exceed the utility that is actually realized through use.

Why Purchase and Usage Utility May Differ: The Effect of Hyperbolic Discounting

The idea that judgments made for the future can differ from those made in the future has a long history of documentation in both social psychology and behavioral economics.

Examples of such asymmetries are numerous: Consumers buy health club memberships and subscriptions to theater series that later go unused (Gourville and Soman 1998), underestimate the rate at which they will adapt to life-changing events (Wilson and Gilbert 2003), and are more likely to prefer risky gambles when they are to be played out in the distant future rather than immediately (Trope and Lieberman 2003). While a number of mechanisms have been proposed to explain these discrepancies, one of the simplest is hyperbolic discounting—the tendency of individuals to give disproportionately large weight to immediate outcomes when making trade-offs in time (Laibson 1997, Loewenstein and Prelec 1992, O'Donoghue and Rabin 1999, Soman 1998).

It is straightforward to show that a consumer who engages in hyperbolic discounting will be prone to two biases in *using* products that will not be fully foreseen when *buying* them:

(1) A tendency to procrastinate investments in learning; and

(2) A tendency to underpredict future temporal variability in the use of features, such as that accrued to variety seeking.

The joint effect of this underestimation is a predicted tendency for prior valuations to overstate the utility realized from actual use in the course of ownership.

The mechanism that leads to unforeseen procrastination can be seen through the following example. Suppose we modify the above product evaluation problem by assuming that consumers valued the future using the quasi-hyperbolic (rather than constant) function suggested by Laibson (1997) and O'Donoghue and Rabin (2001):

$$f(t) = \begin{cases} 1 & \text{for } t = 0, \\ k\beta^t & \text{for } t > 0. \end{cases} \qquad (2)$$

In Equation (2), k is a (0, 1) bounded constant that reflects the degree to which future costs and benefits are disproportionately discounted relative to immediate ones. Note that as long as consumption lies in the future, the equation for the prior valuation of ownership (Equation (1)) under quasi-hyperbolic discounting would be structurally identical to that under constant discounting, differing only by a scaling factor k. Hence, if the consumer is inclined to prefer the new-generation product over the old under constant discounting, she will be no less likely to prefer it under hyperbolic discounting.

But here is the paradox: While a hyperbolic discounter may be happy to acquire the new capabilities, she would, under some circumstances, perpetually procrastinate incurring the learning costs required to use them. To see this, assume that a consumer has adopted the new capabilities and is debating whether to invest in learning about them now or postpone learning until the next usage occasion. These two decisions have the following payoffs:

(1) If learning is undertaken now, the consumer incurs an immediate, undiscounted, learning cost c with the expectation of receiving the discounted usage utilities $k\beta(E(\tilde{u}(\delta)))$ and $k\beta^2\max(0; \tilde{u}(\delta))$ over the next two usage occasions;

(2) If learning is deferred, the consumer receives a neutral usage utility on the current occasion, the *discounted* learning cost $k\beta c$ on the next usage occasion, and the discounted usage utility $k\beta(E(\tilde{u}(\delta)))$ on the subsequent occasion.

If $E = E(\tilde{u}(\delta)) = p\eta - (1-p)\gamma$ is the expected single-period value of the new capabilities, the consumer will find it worthwhile to delay if the following inequality is satisfied:

$$c - k\beta c > k\beta E \;\Rightarrow\; \frac{c}{k} - \beta c > \beta E. \tag{3}$$

That is, if the psychic benefit of deferring learning costs ($c/k - \beta c$) exceeds a one-period loss of the expected conditional benefit of the capabilities. Hence the stronger the hyperbolic discounting effect as measured by the fraction k (i.e., the smaller the k), the greater the perceived attractiveness of delay.

The more important feature of this analysis, however, is that a hyperbolic discounter who finds it worthwhile to delay may *not* form this same conclusion when imagining the merits of *future* delays. Formally, a consumer viewing the merits of delay from a future time t to $t+1$ will imagine the delay to be worthwhile if

$$k\beta^t c - k\beta^{t+1} c > k\beta^{t+1} E \;\Rightarrow\; c - \beta c > \beta E. \tag{4}$$

Because $c/k > c$, the prospect of delaying in the present (Equation (3)) will always appear more attractive than it is imagined to be in the future (Equation (4)). This result, in turn, gives rise to the possibility of perpetual procrastination in product learning: The consumer may have a sincere intention to learn about the new capabilities of an innovation in the future, but perpetually find the merits of doing so tomorrow to be higher than today. Because this usage bias will not be anticipated at the time of purchase ex ante valuations will, by definition, exceed ex post realizations of utility.

Hyperbolic discounting also predicts that consumers will underforesee a second likely feature of usage that has been observed in other contexts of repeated product choice: a tendency for choices to display variation over time, either out of an inherent desire to seek variety in use (e.g., Ratner et al. 1999) or a short-term tendency to switch away from capabilities that display momentary lapses in performance (Meyer and Shi 1995, Siegel 1959). The mechanism by which hyperbolic discounters would underforesee such variation is the same as that which leads to a failure to foresee procrastination: When viewed from a temporal distance, the urge to switch away from a preferred option after making a choice will seem less acute than when the choice is immediate.

Formally, assume in our example that the consumer discovers that the new capability is worthwhile (hence use usually delivers the enhanced utility η), but its use on a given occasion induces satiation that decreases its anticipated utility of use on the next occasion. Formally, if the consumer uses the new capability now ($t=0$), its anticipated utility for the next occasion ($t=1$) under constant discounting will be

$$u(\delta)_1 = \beta\eta - \lambda. \tag{5}$$

Note that Equation (5) will always have a positive sign—prescribing repeated use of the new capability—whenever $\lambda < \beta\eta$, regardless of whether the moment of consumption is immediate ($t=0$) or at some future time t. But this temporal invariance will *not* hold if consumers use the quasi-hyperbolic schedule given in Equation (2). In that case, repeated use of the better capability would again be supported as long as $\lambda < \beta\eta$ for any imagined *future* time of consumption t, but would require the more stringent standard $\lambda < k\beta\eta$ when consumption is immediate.[1] Hence, a hyperbolic discounter looking to the future would envision herself making more consistent (hence frequent) use of the better capability over time than would actually turn out to be the case.

Other Sources of Excessive Optimism and Its Moderators

In addition to underestimating barriers to future use, excessively high valuations could arise by at least two other mechanisms: overly optimistic beliefs about the probability that the new feature will prove worthwhile (in Equation (1), the probability p), and optimistic projections of value based on the performance of incumbent goods. The first of these is the most

[1] Under hyperbolic discounting usage of δ at a future time $t > 0$ yields the anticipated utility $u(\delta)_{t+1} = k\beta^{t+1}\eta - k\beta^t\lambda$. Thus, repeated use is supported whenever $\lambda < \beta\eta$, the same as constant discounting. When consumption occurs at $t=0$, however, anticipated utility is $u(\delta)_1 = k\beta\eta - \lambda$, requiring $\lambda < k\beta\eta$.

straightforward. It has often been observed in other judgment domains that individuals tend to be excessively optimistic when judging the probability of personally relevant events, such as the odds of winning a lottery or avoiding a deadly disease (Budescu and Bruderman 1995, Weinstein 1980). While a diversity of mechanisms have been proposed to explain optimism biases, the most often cited is *motivated reasoning*, which is the tendency of individuals to be more likely to imagine scenarios that are *consistent* rather than *inconsistent* with the desired outcome of a task when forming subjective probabilities (e.g., Krizan and Windschitl 2007, Kunda 1990). Hence we tend to be overly optimistic when gambling because we are motivated to imagine scenarios by which we win (the object of gambling) more than those by which we lose (Budescu and Bruderman 1995). By extension, subjective priors about the odds that a set of new capabilities will prove worthwhile may be prone to a similar inference bias. Because the primary reason for acquiring an innovation is to use and enjoy the new capabilities, consumers may be differentially prone to imagine future use scenarios under which they prove worthwhile rather than useless.

A second mechanism that could produce biased beliefs about future value is if consumers form valuations using simple projection heuristics in which inferences about new features are based on the observed value of current features. Such a possibility is raised by the work of Loewenstein et al. (2003) and Wilson and Gilbert (2003), who find that forecasts of future affective states (such as happiness) are often anchored by the decision maker's current state. As examples, we buy too much food when shopping on an empty stomach because it is hard to imagine the feeling of being satiated (Read and van Leeuwen 1998), and overbuy winter clothing in response to a cold snap because it is hard to imagine the weather being warm again (Conlin et al. 2007). If consumers are similarly prone to projection when forecasting the costs and benefits of new product features, those who found previous product capabilities beneficial and easy to use might be excessively prone to assume that the same will be true for any new features—and hence be excessively optimistic—while the reverse would be true for consumers who have had bad experiences with incumbent features.

Note that if consumers were to use projection as a widespread basis for forecasts, it raises the possibility that new product adoption markets would display a curious dysfunctionality: Those who would benefit the most from new features would be the least likely to adopt. Specifically, if there is a stochastic element to the benefits consumers realize from new product features, those who are happiest with the current generation will suffer something akin to a winner's curse: They will be the most likely to adopt the new generation, but when they do, they will be prone to find that the new generation fails to live up to expectations. On the other hand, those who had, by chance, a bad experience with the first generation will be reluctant to adopt the new one, thus potentially missing out on the chance to experience a real increase in usage utility.

Empirical Analysis

Whether the valuation and usage biases hypothesized here will be observed in real decision contexts is far from certain. One might argue, for example, that the psychological forces that lead to underusage of new capabilities may be offset by psychological forces that encourage *excessive* use, such as sunk-cost effects. A sunk-cost bias would predict that after investing in an innovation consumers may feel obliged to learn and use new capabilities, not as the result of a rational forward-looking calculation, but simply because *not* using them would seem wasteful (e.g., Arkes and Blumer 1985, Thaler 1985). Likewise, Mukherjee and Hoyer (2001) offer survey evidence that consumers are often dissuaded from buying products that appear complex, which suggests that the motivated reasoning biases underlying excess optimism in lotteries, as noted earlier, may not have a simple translation to complex products where there could be competing motivations for adoption.

In this section, we describe the results of a program of experimental research designed to address this question. We explore both the degree to which product use and prior valuation errors occur in tasks involving real decision makers, and the processes that underlie these errors. The program draws data from a realistic product ownership simulation in which laboratory participants are given the opportunity to purchase and then use the features of successive generations of a novel arcade game. In the simulation, we observe the degree to which participants' WTP for new generations anticipates the actual rewards they realize from ownership, and how actual use departs from optimal usage patterns.

Study 1

Overview and Participants

In the experiment we used a computer simulation that mimicked the process of learning and buying successive generations of a new computer game that is played for a monetary incentive. The game was called "Catch'em" and bore similarities to the arcade game *Pac-Man* that was popular in the late 1970s and early 1980s (see Figure 1). The goal of the game was to move a large icon around a grid in an effort to capture as many smaller icons ("cookies") as possible while avoiding capture by a robotic opponent, called the

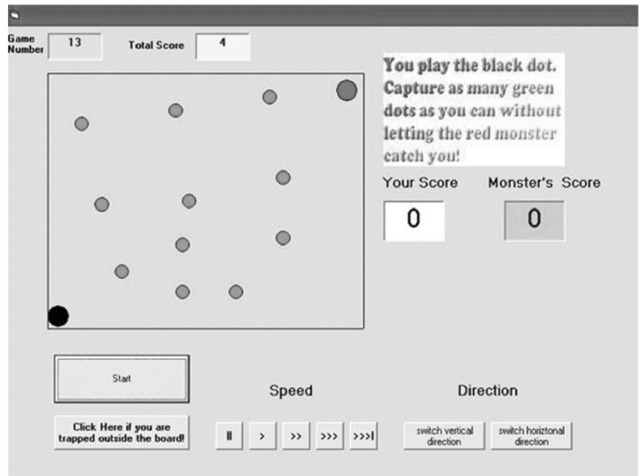
"Monster." At the start of the simulation, participants were given a game platform that was equipped with one of two types of controls for adjusting speed and direction: a *button control* (Figure 1(a)) or a *scrollbar control* (Figure 1(b)). After training on a base platform, treatment participants were then given the opportunity to purchase and play with a *combo platform* that offered *both* sets of controls (Figure 1(c)).

A natural concern in the design of the experiment was that decisions to adopt the combo platform might be driven by aesthetic considerations or desires for prestige—factors that lie outside the current theoretical analysis. To minimize these effects, the appearance of the combo platform was designed to be nearly identical to that of the two base platforms (see Figure 1(c)), and all the games were administered and resolved in private. Moreover, instructions emphasized the pecuniary goal of the task: to make choices that would maximize monetary payoffs.

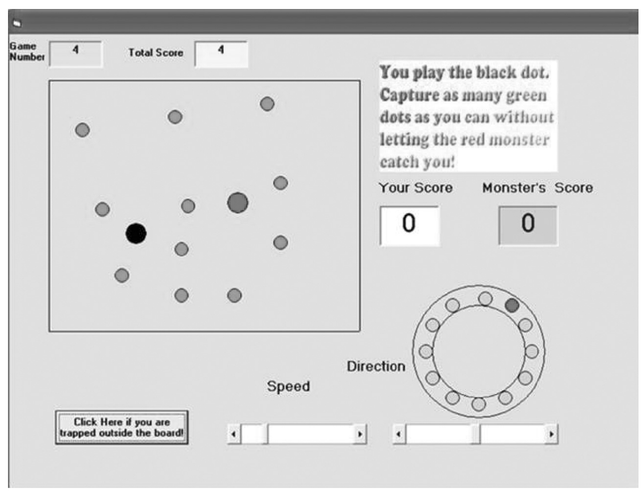
Participants were 200 business school graduates and undergraduates who volunteered to complete the task for a monetary incentive. They performed the experiment seated in computer cubicles in the school's behavioral research laboratory. At the beginning of the experiment, participants were told that its purpose was to study how consumers such as themselves learned to play gaming devices, and that they would be paid depending on their performance in the game. There was a show-up fee of $10, and a $100 prize was awarded to the player with the highest overall score. Participants were told that they would be playing the "Catch'em" game for a total of 30 times, with the first 15 games being practice and the last 15 for compensation.

Figure 1 Game Platforms Used in Studies 1 and 2

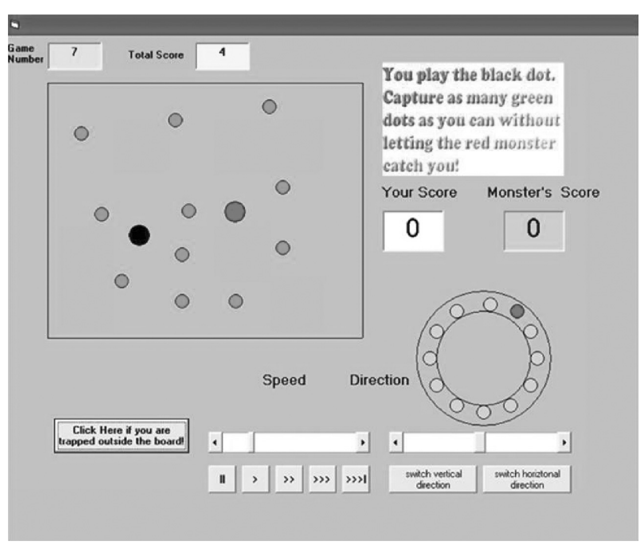
(a) Buttons



(b) Scrollbars



(c) Combo version



Design

The experiment was a 3×3 between-subjects factorial design that manipulated

(1) The nature of the controls provided by the base platform (scrollbars, buttons of high reliability, or buttons of low reliability); and

(2) The buying task (no opportunity to upgrade, an opportunity to upgrade after forecasting likely usage, and an opportunity to upgrade with no forecast).

The three platforms provided a manipulation of the quality of experience participants had with the base platform. This manipulation tested the hypothesis that participants' valuations of the new platform would be moderated by the success they enjoyed with the incumbent platform—with those having the highest success being the most prone to being overly optimistic about the gains they would realize from a new one. Scrollbars and high-reliability buttons allowed experienced players to achieve similar high scoring levels, but had different inherent learning costs. Whereas players with buttons could reach asymptotic levels after 3 to 5 training rounds, players with scrollbars required 10 to 12 rounds to achieve the same level. The low-reliability button condition was introduced to provide a case of a poorly performing incumbent. Although the control was easy to learn, the icon did not always move in response to a click, causing a 50% decrease in asymptotic levels.[2]

The three buying tasks formed the central manipulation of the study. After the training rounds, those in the no-upgrade condition went on to play the money rounds with the original platform. Participants in the upgrade and upgrade-with-forecast groups, however, were told that a new game was available for purchase, and that the new game included their familiar controls plus a new set of supplemental controls on which they had not previously trained (i.e., the combo version). Participants were shown a screen capture of the new platform (as in Figure 1(c)) but were not permitted to actually use it (the effects of allowing trial use are reported in Study 2).

After reading this description and before being allowed to purchase the new platform, participants in the forecast condition were also asked to predict the performance they would likely realize in the money rounds, playing with the base and combo platforms were they to adopt it. These forecasts were expressed on a 7-point scale indicating different percentage changes relative to the training rounds. Participants were also asked to predict the likely percentage of time they would use the new control were they to adopt the combo platform.

The Pricing and Buying Mechanism. After seeing the screen capture of the combo platform, participants in the upgrade and upgrade-with-forecast groups were told that they could purchase the combo platform by paying a point handicap that would be applied to their realized score in the money rounds. Payment was expressed in game points to eliminate the need for subjects to translate expected performance gains (in points) to monetary equivalents. Hence, in principle, the points that subjects were willing to pay would be a direct expression of the minimum increased point performance they expected to realize by having access to the new controls.

WTP for the combo platform was elicited using a procedure akin to that suggested by Becker et al. (1964; the Becker-DeGroot-Marschak (BDM) procedure). Participants were asked to indicate the maximum number of points they would be willing to forego for the ability to play the money rounds with the new platform. They were told that a fixed price (in points) for the platform would be revealed after they provided their estimate, and if their WTP were greater than this price, they would receive the new platform and its price would be deducted from their point total in the money rounds. To ensure that subjects fully understood how the process would work, they first participated in a practice round where they set a WTP price, and an illustrative actual price was drawn by lottery. Participants were given the opportunity to repeat this exercise until they felt comfortable with the procedure.

The actual price of the combo game was held constant for all participants at 120 points, a value at which subjects would break even if the new game allowed them to realize a modest (8 points per game) increase in performance over the incumbent platform. After participants submitted WTP, those who submitted valuations greater than 120 were informed that they would be playing with the combo platform, and the purchase price was immediately deducted from their total score shown on their game screen.

Results

Table 1 presents the descriptive statistics on participants' performance for training and money rounds as well as their *WTP* estimates by initial game platform. Because the upgrade-with-forecast group behaved similarly to the upgrade group in terms of their performance in training rounds ($p > 0.2$) and *WTP* ($p > 0.7$), we pool their data in the following analyses.

Manipulation Checks. An analysis of the average performance achieved by subjects using each of these control formats during the training rounds reaffirmed the findings from pilot work about their respective speeds of learning and asymptotic scores. Subjects using scrollbars, for example, realized an average score of 31 points over the first three training rounds, but this increased over time to a maximum of 78 points over the last three rounds. The high-reliability buttons, in contrast, yielded high performance throughout, with a mean score of 51 on the first three rounds, increasing to a maximum of 80 points toward the end, comparable to the best achieved by the scrollbars. Finally, the low-reliability buttons yielded comparatively poor performance throughout; respondents realized an average score of 30 points in the first three rounds, which increased only to 33 points in the last three rounds.

Evidence of Optimism Biases. When given the opportunity to purchase the new platform, participants were, on the whole, quite enthusiastic about the prospect: The mean *WTP* across training conditions was 347 (median = 300), which translated to an 80% adoption rate. This means that *WTP* was

[2] We did not manipulate reliability of the scrollbar control because of its inherently slow rate of learning. With low-reliability scrollbars, few subjects would have been able to discern whether low scores stemmed from the difficulty of learning how to use the controls or from an inherent unreliability. In contrast, the distinction was easier to make for the button controls.

Table 1 Performance for Training and Money Rounds and *WTP* by Initial Game Platform in Study 1

		No-upgrade group		Upgrade group	
Measures	Initial game platform	Mean	S.D.	Mean	S.D.
Total score for training rounds	Low-reliability buttons	1,672.3	401.9	1,553.6	269.2
	High-reliability buttons	1,800.4	401.7	2,028.5	658.1
	Scrollbars	1,847.6	588.7	1,700.8	496.8
	Average	1,772.0	470.6	1,761.5	535.8
Total score for money rounds	Low-reliability buttons	1,987.3	584.2	1,556.1	452.8
	High-reliability buttons	2,200.7	715.5	2,132.7	919.6
	Scrollbars	2,333.0	1,167.2	2,048.9	897.3
	Average	2,171.0	855.7	1,910.5	821.7
Score improvement	Low-reliability buttons	315.0	614.9	2.5	514.4
	High-reliability buttons	400.2	653.8	104.2	609.5
	Scrollbars	485.4	759.5	335.6	711.0
	Average	399.0	671.7	144.5	626.1
WTP	Low-reliability buttons			377.0	264.6
	High-reliability buttons			346.0	255.7
	Scrollbars			316.0	243.9
	Average			347.0	254.3

Note. $N = 70$ for control group, and $N = 130$ for upgrade group (including the upgrade-with-forecast group).

equivalent to an expectation that having access to a second control would allow participants to realize a 23-point score improvement per game over retaining the basic platform. As a point of reference, during the training rounds, participants realized, on average, a 26-point increase in their performance. Hence, participants were revealing an expectation that not only would the new control allow them to realize a gain in scoring similar to that which they achieved during the initial training rounds, but—because they were paying for the new platform—that this gain would also be *beyond* that which would be naturally realized by continuing to gain expertise with the incumbent platform (which was free).

A more precise description of the accuracy of *WTP* estimates is provided by comparing these values to the net improvement in performance realized in money rounds over training rounds (i.e., score improvement in Table 1). We first compared participants' *WTP* with their score improvement after acquiring the new capabilities via a 3 (initial platform) × 2 (measures: *WTP* versus score improvement) mixed analysis of variance with the latter as the repeated factor. The results revealed that, on average, participants' mean *WTP* was significantly higher than the net improvement in performance after acquiring the new capabilities ($M = 425$ versus 139 for *WTP* and net improvement in performance, respectively; $F(1, 100) = 16.88$, $p < 0.0001$). The initial game platform did not have a main effect ($F(2, 127) = 2.18$, $p > 0.1$), nor was the initial game platform × measures interaction significant, $F(2, 100) = 2.08$, $p > 0.1$. We also compared *WTP* and score improvement for each initial game platform. This analysis indicates that the large discrepancy between *WTP* and score improvement was significant for those who trained on either low-reliability buttons (paired $t(36) = 4.19$, $p < 0.001$) or high-reliability buttons (paired $t(34) = 2.49$, $p < 0.05$), and not significant for the scrollbars group (paired $t(30) = 0.80$, $p > 0.4$). We should add that the net improvement in performance realized by the adopters was significantly *lower* than that realized by those participants in the no-upgrade group ($M = 139$ versus 399; $F(1, 194) = 6.63$, $p < 0.05$). The initial game platform had no effect (ps > 0.2). Thus, even with the most lenient criterion, our participants' *WTP* estimates proved overly optimistic.

A second line of evidence for an optimism bias was obtained by regressing adopters' actual achievement playing with the new combo platform against a battery of three predictors of playing success: cumulative performance during the money rounds, gender (male players were observed to score somewhat higher than female), and the log of the player's *WTP* for the platform. The results (shown in Table 2) support a highly significant partial *negative* effect of *WTP* on performance ($b = -192.21$, $p < 0.05$). In other words, an adopter with a *WTP* of 400 would score more than 260 points fewer than an adopter with a *WTP* of 100. The more optimistic participants were about the gains to be realized by playing with the new platform (as revealed through their *WTP*), the lower the gains that were actually realized.

Evidence of Projection Biases. We hypothesized that optimism biases would be moderated by participants' experiences with the training platform. To explore this effect, we regressed log-transformed individual *WTP* estimates against indicator variables for training platform (reliable and unreliable

Table 2 Effect of *WTP* on Subsequent Performance in Money Rounds by Adopters in Study 1

Variable	Parameter estimate	SE	t-value	p-value
Intercept	1,654.87	505.19	3.28	0.0015
Total score in training rounds	0.93	0.12	7.91	< 0.0001
Gender (0 = Male, 1 = Female)	−288.47	80.31	−3.59	0.0005
Log of *WTP*	−192.21	80.00	−2.41	0.0180
Omnibus $F(3, 99) = 30.75$, $p < 0.0001$, adj. $R^2 = 0.47$				

buttons as contrasts with the scrollbar) and the maximum game score participants achieved during the last six training games—an individual measure of past achievement. The results are presented in Table 3. Consistent with a projection bias, the analysis revealed a positive effect of past peak performance on *WTP*; after controlling for base platform, there was a significant positive residual effect of past peak achievement ($b = 0.005$, $p < 0.05$). Perhaps paradoxically, the more participants mastered the incumbent controls, the more they were willing to pay for the option to play the money rounds with a new set of controls over which they had no mastery. As noted earlier, this exaggerated belief in skill transference then led these participants to perform comparatively poorly in the money rounds.

Biases in Feature Use. We hypothesized that use would be suppressed by two biases in ownership: a tendency to procrastinate investments in learning and to display variation in the use of controls over time, and not always using that with the highest learned level of performance. To test these hypotheses, in Figure 2 we plot histograms of rates of usage of unfamiliar controls over the first 3 games in the money rounds (dark columns) and over all 15 games (white columns). Every time a participant used a control to move the icon on the computer screen, it was counted as one use. The usage rate of the unfamiliar controls was calculated as the total use of the unfamiliar controls as a proportion of the total use of all controls over a specified number of game rounds. Thus the usage rate of unfamiliar controls may range from 0% to 100%. The usage rate during the first three money rounds (dark columns in Figure 2) supports a hypothesis of limited early investments in learning. During the initial three games of the money rounds, when the optimal policy calls for near exclusive experimental use of the new controls, participants who had trained on the high-reliability buttons and the scrollbar (dark columns in Figures 2(b) and 2(c)) used the new capabilities only 20% of the time, on average, with a mode at 0%. Even participants who had trained on the low-reliability buttons—and who thus had a strong rational motivation to see if the new controls might work better—failed to make full experimental use of them. For these participants, the mean usage rate over the first three games was 53% (dark column in Figure 2(a)), with a mode between 60% and 70%, and four participants never used the scrollbar during the first three games.

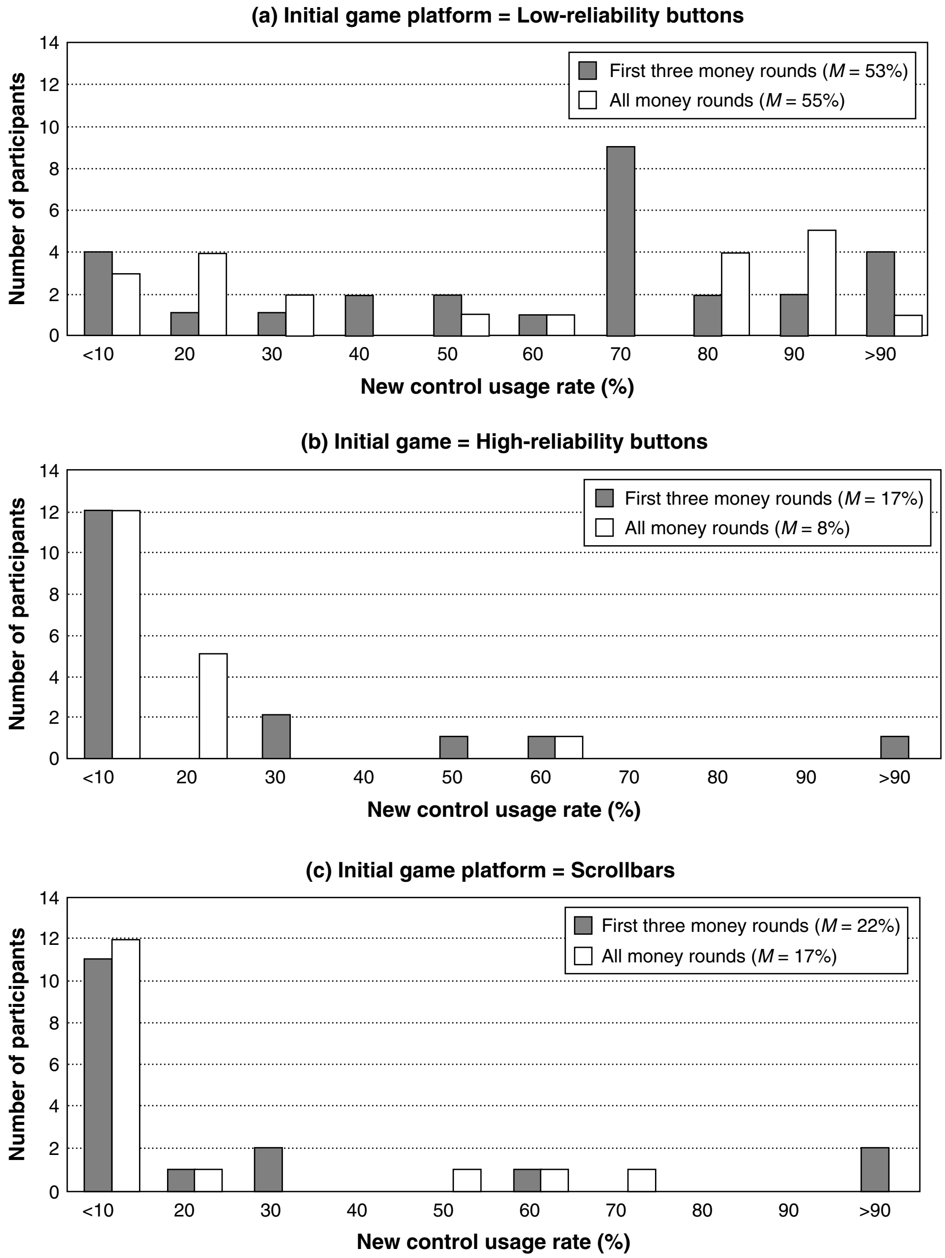
Table 3 Influence of Prior Performance on *WTP* in Study 1

Variable	Parameter estimate	SE	t-value	p-value
Intercept	4.700	0.370	12.87	< 0.0001
Initial game platform				
Low-reliability buttons	0.340	0.340	0.99	0.3200
High-reliability buttons	0.100	0.330	0.29	0.7700
Maximum score during last six rounds	0.005	0.002	2.42	0.0300
$F(3, 123) = 1.92$, $p < 0.10$, adj. $R^2 = 0.02$				

The data on new control usage across all money rounds (white columns in Figure 2) also support the predicted failure to abandon occasional use of an inferior control. For example, while participants in the scrollbar and reliable buttons conditions gradually withdrew use of their respective novel control as the money rounds wore on, withdrawal was not complete (see white columns in Figures 2(b) and 2(c)). Eight subjects in these conditions never used the new controls over the entire 15 games. Of greater interest, however, was the usage pattern displayed by participants in the low-reliability buttons condition, who could have achieved higher scores if they had fully switched to the scrollbar during the money rounds. Few participants learned to make exclusive use of this better control, and long-run use patterns were bimodal: While some appeared to discover its advantages (leading to a modal 90% usage rate), an equally large number never discovered the latent advantage, displaying a 20% modal usage rate (white column in Figure 2(a)).

Forecasts. As a final analysis, we investigated whether the excessive optimism revealed by adopters in their valuations would also be manifested in explicit forecasts made about likely postadoption usage and scoring. Consistent with an optimism bias, participants anticipated using the new control much more frequently than was the case; while the mean actual use across training platforms was 28%, the mean forecast usage was 51% ($\chi^2 = 7.592$, $p < 0.01$). Note, however, that this forecast of higher anticipated use did not translate to equally optimistic beliefs about the scoring gains they would likely realize playing with the new platform. On average, participants predicted only a modest (11%) increase in money rounds performance from training rounds if they continued to play the training game, a prediction that was conservative relative to both actual achievement

Figure 2 Frequency Distribution of New Feature Utilization in Money Rounds by Training Game in Study 1



and their own *WTP*. One explanation that would be consistent with our prior theories is that the task of estimating likely future scores required subjects to consciously adopt a postpurchase mindset that heightened the attention they gave to the possible ways that the new controls might *not* prove worthwhile—something that led them to foresee score gains that were much lower than the much larger amounts they had implicitly revealed when previously expressing a WTP.

Discussion

The data from Study 1 support three hypothesized features of participants' adoption and usage decisions: a tendency to form initial assessments of value that overstate the returns actually realized from the innovation (an optimism bias), a hesitancy to engage in learning about new features after adoption, and failure to fully exploit what has been learned (as reflected by persistent use of both controls). We hypothesized that these behaviors could be explained by at least three complementary mechanisms: a hyperbolic discounting effect that caused participants to underweight the future impact of learning costs and desires for temporal variation in control usage, a global optimistic bias in beliefs about the odds that the new controls would be successful (triggered by motivated reasoning), and a tendency to project future performance based on experienced performance with the incumbent controls.

There are, however, two obvious sources of concern that might be raised about this interpretation of the data. First, a case could be made that the latter two mechanisms alone would be sufficient to explain both the overvaluation and usage biases. Given that participants had no objective information on which to base forecasts, reports of WTP may have been a visceral expression of optimism that participants immediately recognized as an error the moment they had a chance to try the new control. Second, an even simpler explanation for the findings was that valuations were inflated by a desire among participants to satisfy their curiosity about how the new platform would function—a desire they were willing to pay to satisfy.

A straightforward means of testing these alternative explanations would be to see if we observe the same overvaluation biases in a variation of the task where participants could try out the new platform before making a purchase decision. Trial use would, presumably, satisfy any unfulfilled sense of curiosity among participants about the functioning of the new platform, and greatly reduce participants' prior uncertainty about the likely ability of the new controls to enhance scoring, hence the need to engage in speculative projection. Hyperbolic discounting, however, predicts that even with accurate priors excessive valuations would persist to some degree. This is because what drives excessive prior valuations under this mechanism is not optimism per se, but rather the temporal separation between valuations formed at the time of purchase and at the time of use—a separation that causes the former to underweight factors that suppress usage in the latter.

To test this hypothesis, we conducted a second study that provided participants with a chance to gain some real experience with the new game platform before making WTP decisions.

Study 2: Effect of Product Experience on Optimism Bias

Method

Thirty-three students participated in the study. They were recruited from the same subject pool as in Study 1 using the same recruitment procedure and incentive. The study followed the same procedure as the upgrade group in Study 1 with one exception: After participants completed the training rounds and before they indicated their WTP for the new combo platform, they were allowed to play the new combo platform for two rounds. They were also told that their score for the two rounds "will not count towards [their] final score in the money rounds." They were, however, free to accept or reject the free trial offer. After the free trial rounds, they were asked to indicate their WTP for the new combo platform and then proceeded with the money rounds with their game platform of choice as in Study 1.

In the experiment, all participants trained on the high-reliability-buttons platform for the training rounds. This decision was based on two considerations. First, data from Study 1 generally did not indicate a strong qualitative difference between the different training platforms, which suggests that the training platform did not significantly influence their decision process. Second, we observed the most pronounced bias for the group that trained on the high-reliability buttons. This suggests that, to the extent that the observed bias in Study 1 was driven by the lack of diagnostic information about the performance of the new platform, experience with the new game should be the most informative for the high-reliability-buttons group, so that they should easily conclude that the new scrollbar controls were difficult to learn, and thus unlikely to provide any short-term gains in scoring ability. This knowledge should, in turn, drastically decrease their WTP for the new combo platform.

Results

Descriptive statistics of the free trials group are given in Table 4.

Behavior During Free Trials. All participants took advantage of the free trial opportunity and experimented with the new controls at least once during the two free trials. On average, the new control was used 51% of the time on the first free trial, but dropped to 33% for the second free trial (paired $t(32) = -1.95$, $p = 0.06$). The significant drop implies that our participants appeared to conclude after one trial that the new control had only limited, if any, value.

Influence of Free Trials Experience on *WTP*. Did this knowledge eliminate the overvaluation bias observed in Study 1? The answer is no. As in Study 1, participants' expressions of WTP suggested highly optimistic beliefs about the performance gains to be

Table 4 Performance for Training and Money Rounds and *WTP* for the Free Trials Group in Study 2

Condition	Total score for training rounds	Total score for money rounds	Score improvement	*WTP*
Free trials ($N = 33$)	2,211.2 (523.5)	2,163.0 (690.4)	−14.0 (690.1)	320.0 (245.2)
No free trials ($N = 44$)	2,028.5 (658.1)	2,132.7 (919.6)	104.2 (609.5)	346.0 (255.7)

Note. The no free trials group was the Upgrade Group in Study 1. Standard deviations are in parentheses.

realized from acquiring the new platform. The mean *WTP* was 320 points (median = 300), sufficient to allow 79% of participants to adopt the innovation. Among adopters, the mean *WTP* was 404 points (median = 400). In contrast to these optimistic priors, adopters actually experienced a 20-point *decrease* in mean scoring (median = −15) during the money rounds compared to their performance in the training rounds ($F(1, 25) = 8.41$, $p < 0.01$). Participants with access to free trials also displayed the same postadoption bias of failing to make use of the new controls they paid for; on average, participants used the new controls only 12% of the time during the first three money rounds, and 10% overall.

Discussion

The data from Study 2 indicate that the overvaluation bias reported in Study 1 could not be entirely attributed to simple mistakes arising from the lack of information that participants had about the value of the new control, or a need to satisfy curiosity about the functioning of the control. The bias persisted after trial use, we suggest, for a simple reason: While Free trials provided participants with a sounder normative basis for making forecasts of usage, it did *not* remove what we see as the core source of the bias—the inherent temporal difference that exists between decisions to buy versus use. Whereas the former is a prospective assessment of the cumulative value of a series of imagined future usage decisions, the latter is a series of more myopic decisions about which control will yield the highest utility on a single immediate trial.

General Discussion

Consumer decisions to purchase products with expanded sets of capabilities are often motivated by factors other than rational projections of future use (e.g., Mick and Fournier 1998). Some consumers buy a top-of-the-line cell phone not because they have anticipated need for its features but because they can afford to do so—an act that may convey feelings of wealth and status to the consumer. But in a large number of cases, we suggest, consumers and managers *do* aspire to achieve efficiency in the product decisions they make. Decisions to pay extra for novel capabilities that are never used ultimately seen as regrettable mistakes, despite whatever social boost the acquisition first conveyed.

In this paper, we examined the biases that arise when consumers attempt to make such thoughtful product adoption decisions. We focused on a special class of problems where new features of uncertain value are introduced as independent add-ons to an existing functional platform—such as enhanced steering or visualization aids added to a car model, or enhanced graphing capabilities in a new release of an established statistical software package. Across two experimental studies, we find consistent evidence that such adoption decisions are marked by a *valuation-usage disparity*: On average participants exhibited a high WTP for a new generation game platform offering a supplemental set of controls, but after assuming ownership, they made limited use of them, such as by underinvesting in learning and inconsistently using the capabilities that offered the higher long-run benefits.

We argued that this observed disparity could be explained by a set of theoretical mechanisms that have been used to characterize the difference between present and future consumption decisions in other domains (Loewenstein and Prelec 1992). The most well known of these, hyperbolic discounting, was shown to offer a parsimonious account for why otherwise rational participants would have failed to fully foresee two of the major usage biases uncovered in the task: the tendency to procrastinate investments in learning and to oscillate in the use of game controls. When making usage decisions, participants who engaged in hyperbolic discounting would have placed excessive weight on the most immediate consequences of choices, such as the prospect of incurring learning costs and the urge to switch from a control that just performed poorly. Judgments made from a temporal distance, however, would fail to fully foresee this attention shift, and hence would have underestimated the future likelihood of both procrastination and switching.

We suggested that prior valuations may have been further inflated by two other, less analytic, biasing mechanism in inference: a motivated reasoning bias that fostered excessively optimistic priors about the likely performance value of the new controls, and projections from the mostly positive experiences they had playing with the incumbent set of controls in the training rounds. The first of these biases may have been encouraged by the payoff structure of the task, which rewarded the top overall scorers. Desire to win what was construed to be a competition may have exaggerated participants' natural tendencies to engage in wishful thinking about the scoring gains that could be realized by adopting the new game platform (de Mello et al. 2007).[3]

We should emphasize, however, that the data could also be interpreted in light of other related theoretical frameworks. For example, the finding that ex ante *WTP* exceeded ex post benefits from use of new controls may have reflected the difference in construal level associated with purchase and usage decisions (e.g., Thompson et al. 2005, Trope and Liberman

[3] We thank an anonymous reviewer for this suggestion.

2003). According to the construal-level theory, when considering options that will be used in the future, decision makers have a tendency to base assessments on more abstract features (such as the long-run pleasure of ownership), whereas short-term assessments tend to focus on more concrete features (such as acquisition and usage costs; see Trope and Liberman 2003). Hence, participants may have focused on the imagined long-run benefits of new platforms (higher scores) while making purchase decisions, but focused on short-term learning costs instead while making short-term usage decisions.

Finally, note that while these findings are new in the context of new product evaluation, data showing similar tendencies to overvalue options has been uncovered in other domains. For example, the current experiments recall Simonson's (1990) finding that when consumers are faced with the task of preordering an assortment of snacks to be consumed in the future, they tend to purchase more variety than is actually preferred in the course of daily use. Also, Shin and Ariely's (2004) found a reluctance of decision makers to close off options when engaged in an experimental search game—even options that are never used. While the blend of mechanisms that drove these particular findings may have differed from those that operated in the current experiments, they nevertheless underscore the universality of valuation differences that can occur before and in the course of consumption.

Boundaries, a Followup Experiment, and Extensions

It is important to emphasize that the data reported here come from a particular task structure. Thus, care must be taken before attempting to generalize it to others. For example, while the dominant bias observed here was that of overpaying for attribute enhancements, one presumably could construct tasks where the opposite bias tends to be the rule—cases where consumers prefer to avoid upgrading to next generation technologies (e.g., Johnson et al. 2003, Mukherjee and Hoyer 2001, Zauberman 2003). Because our interest was in observing usage biases that arise among consumers who make positive decisions to adopt an innovation, we were careful to avoid imbedding features in the new game platform that would have inflated prior beliefs about switching costs, such as changing the physical appearance of the controls with which the participant was already familiar.

To test whether such a design change would have altered our findings, we ran a followup study that varied the visual similarity of the new combo platform to that of the training platform. In this task, participants ($N = 220$) completed training and money rounds as in the upgrade group in Study 1, but were placed in two platform-similarity conditions: a low similarity condition where the incumbent (familiar) control was rotated 90 degrees from the orientation it had in the training platform, and a high similarity condition where the incumbent control had the same orientation. We analyzed the data by regressing each participant's decision about whether to upgrade to the new platform and their log-transformed *WTP* on two predictors: (1) individual ratings of the perceived similarity of the incumbent (training) platform to the new one and (2) their performance in the training rounds. The analysis supported a significant positive effect of judged similarity on both upgrade probability (Wald $\chi^2 = 8.824$, $p < 0.01$) and on *WTP* ($b = 0.21$, $t(218) = 2.39$, $p < 0.05$). The finding thus supports a tactic for encouraging users of existing technologies to upgrade to new ones that is presumably part of the intuition of most product designers: While consumers may be attracted to the prospect of gaining access to new capabilities, they will be reluctant to do so if such access comes at the cost of having to relearn the use of existing capabilities.

A related feature of the current task that facilitated adoption is that participants did not face switchback costs if they found that the new control performed poorly. In actual settings, of course, such costs can be formidable; a consumer who changes her computer's operating system to gain access to new capabilities cannot easily switch back to the old system should the new one prove difficult to use. A natural hypothesis is that in such cases, we might observe the opposite bias of that observed here, i.e., an excessive reluctance to upgrade to new product generations. In such cases, a hyperbolic discounter would have difficulty foreseeing the long-term benefits of adoption that lie beyond the large immediate switching costs, and thus elect to remain with an older technology despite its being outmoded (i.e., fall prey to the lock-in effect discussed by Johnson et al. 2003).

A natural area for future research would be to explore how repeated exposure to successive generations of new technologies might attenuate over- or under-buying biases. For example, a key tactical implication that comes from this work is that if firms want to encourage consumers to adopt new generations of a product or service, they might find success by endowing it with an array of novel add-on features that convey the *perception* of usability. But it should be clear that any such success could be short lived if the add-ons turn out, in fact, not to be useable. While the firm might be able to deceive consumers into upgrading once, they would presumably have a hard time doing so again, even if the next phase of the add-ons have genuine value.

Finally, in future work, it would be useful to further explore the theoretical mechanisms that were hypothesized to underlie the current findings. For example, the hyperbolic discounting model we advanced was not intended as a literal description of how participants made valuation judgments, but to provide an existence proof; namely, that rational consumers with unbiased expectations about the costs and benefits associated with product use would nevertheless be prone to overvaluing ownership. In actual settings, of course, consumers are unlikely to have such unbiased beliefs, and these distortions could work to either exacerbated or mollify valuation errors. As an illustration, the model's prediction that consumers will underforecast future variation in the use of capabilities would be mollified or even reversed if actual use turned out to be more inertial than was anticipated ex ante; that is, in terms of Equation (5) if the hedonic penalty λ was much lower in practice than was anticipated at the time of purchase. If such were the case, consumers might well anticipate making more varied use of capabilities than would actually be the case—as noted earlier, a bias Simonson (1990) found in his empirical work on preorders of frequently purchased snack items. An important avenue for future research would be to explore in greater detail the heuristic beliefs that consumers hold about product ownership when making purchase decisions, and how variations in these beliefs will affect the size and direction of valuation errors.

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