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(When) Are We Dynamically Optimal? A Psychological Field Guide for Marketing Modelers

A common assumption made in structural approaches to empirical strategy research in marketing is that firms and consumers satisfy the assumptions of dynamic optimality when making decisions. When faced with problems of how best to allocate resources, firms are assumed consider the future consequences of different strategic options and, in each point in time, choose the option that maximizes long-term utility. The validity of such assumptions, however, is often called into question by behavioral researchers who point to work in psychology that finds that assumptions of optimality are frequently violated in experimental settings. If this is indeed the case, it would lend support to approaches that argue that markets have inefficiencies that can be discovered and exploited by simpler, largely correlational, methods. In this article, the authors attempt to reconcile these contrasting views by proposing a framework for assessing when assumptions of dynamic optimality are likely to be good ones and when they are likely to be untenable in empirical analysis.

Keywords: empirical strategy, structural modeling, dynamic optimization, decision making, behavioral economics

Prelude: The Empirical Strategy Wars

If there is a single research question that unites the field of marketing, it is likely this: How should a firm best allocate its resources to maximize profits? It is the question that lies at the heart of decades of research in advertising, sales force management, pricing, and competitive strategy. Moreover, much behavioral research in marketing is designed to better understand the choice processes of both consumers and managers. Yet as universal as this question may be, it is also one for which there is no deeper philosophical divide regarding how best to answer it, with each side doubting the validity of the answers provided by the other.

On the one hand, we have the correlational approach to explanation. In essence, it pursues an approach to optimizing resource allocation that many would find the most intuitive: if a firm wants to be successful, it should identify a successful firm and imitate its actions. Although this is often done through the mechanism of case studies, academic analyses tend to proceed more systematically, typically by looking for the statistical correlates of success in cross-sectional and longitudinal databases. For example, much of what we know about the benefits of market orientation come from cross-sectional studies of the statistical association between firm performance and marketing investments (e.g., Kirca, Jayachandran, and Bearden 2005), and the literature on drivers of new product success has similarly drawn on evidence provided by correlational studies of innovation performance within and across firms (e.g., Montoya-Weiss and Calantone 1994). Implicit to such work is the belief that statistical associations, if properly identified, carry strategic insights; if firms that invest more heavily in marketing instrument X perform better than those that spend less, then spending more on X represents best practice.

Yet as straightforward as this approach might seem, its success hinges on the validity of an assumption over which some have expressed doubts: that markets have inefficiencies that can be exploited by any firm willing and able to look for them. Markets are implicitly viewed as natural experiments in which firms make naive choices and the winners are those that happen to stumble upon the right ones. For example, consider Luo's (2008) finding that higher pre–initial public offering (IPO) marketing spending tends to be associated with higher post-IPO stock valuations. This result is useful because it would seem to carry a simple strategic recipe for enhancing the success of an IPO: spend more on marketing beforehand. However, for this interpretation to be valid, one has to believe that the firms that spent less on pre-IPO marketing could have done better had they spent more; that is, there was an inefficiency. Yet this mistaken lack of spending was fortuitous because, had the firm indeed spent more, there would not have been the statistical variation needed to detect the association in the first place. Thus, the approach depends on a certain amount of inertia in markets that allows suboptimal strategies to survive, at least for a while.

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On the other hand, there are researchers who have a deeply opposing view of what such analyses can teach us about normative firm strategy; these are the structural modelers. Rather than viewing variations in firm actions and profits as the machinations of naive firms trying out different allocations to see what works, these researchers view these same data as the "outer envelope" of the behavior of smart firms that know what works and have made their decisions accordingly (see, e.g., Chintagunta et. al 2006). In this view, if a firm reduces its marketing spending prior to a planned IPO, it is taken as evidence of an optimal act; it is the Nash equilibrium of a game in which the firm has thought through the likely competitive market reactions to different expenditure strategies and concluded that a reduction will maximize long-term firm welfare. The empirical challenge of the analyst is thus to identify models that rationalize observed behavior rather than assume that firms are making errors.

It is with this claim that the structural modelers argue that they have the decided advantage in the explanatory wars. The argument is simple. Even the most ardent follower of the descriptive camp would concede that there are limits to the prescriptive value of statistical associations. Much like a gambler who tries to take advantage of a statistical anomaly in a sports betting market, trying to exploit an apparent inefficiency in a market runs the risk of being self-defeating. Once word gets out, for example, that the better-performing firms are currently the ones that invest more in marketing, competitors will tend to match the strategy, causing whatever advantage there originally was to vanish—or even reverse if the investments become excessive. Economists term this the "Lucas critique" of statistical approaches to policy research (Lucas 1976); because the models simply characterize the associations that happen to exist within a specific data ecology, the minute a firm tries to act on an association, it alters the ecology that originally gave rise to it—thus invalidating the earlier finding. However, if a firm understands the full workings of that ecology—the recursive structure of the game being played by firms and consumers—it has a potential way of eluding this problem.

Emboldened by this prospect, structuralists approach data by positing the long-term game that they assume is being played and optimized and then identify the parameters of the game (e.g., the shape of utility functions and discount rates for firms and consumers) that best enable a normative model of the game to be brought in line with observed data. Strategic insights follow *not* by pointing to mistakes that firms seem to be making (after all, such an approach assumes that there are no mistakes) but rather by running counterfactual simulations that show the long-term consequences of different strategic allocations—predictions that have the endogenous feedback loops of the competitive ecology already built in (e.g., Bronnenberg, Rossi, and Vilcassim 2005).

As an example, Sun, Neslin, and Srinivasan (2003) use this idea to argue that standard "reduced-form" regression models of sales response will be prone to overestimating promotion elasticities, making them a questionable basis for brand planning. If a seller sees a large boost in sales when it puts a brand on promotion, the boost likely comes from two sources that can be difficult to tease apart empirically: one due to customers' inherent utility for the brand, and another due to customers' strategic response to the seller's promotional strategy. Because these two drivers are confounded in traditional regression elasticities, a large contemporaneous response may lead to the mistaken belief that this same response can be counted on every time a promotion is offered. Structural models, however, avoid this problem by explicitly modeling the forward-planning process, at least under the assumption that the planning is optimal. Examples of such dynamic analyses in marketing are numerous and include such wide-ranging topics as choice of sales-force compensation schemes (Misra and Nair 2011), purchase quantity decisions by consumers (Erdem, Imai, and Keane 2003), temporal demand for cigarettes (Gordon and Sun 2015), and even cyclical demand for fashions (Soysal and Krishnamurthy 2012).

The Cold Reality of Psychological Evidence

As appealing as the structural approach might seem, it immediately hits an intellectual speed bump, one that behavioral researchers are all too quick to point out: the empirical evidence to support assumptions of dynamic optimality is thin at best. Since the 1960s, a large stream of literature has evolved in experimental psychology and behavioral economics that has examined the degree to which people act as intuitive statisticians when making decisions under uncertainty over time, and the advice it offers would seem bleak. For example, one of the earliest findings in work on behavioral decision theory is that Bayes's rule—one of the basic hammers of the structural modeler—provides a poor descriptive account of how people actually update probabilistic inferences in light of data (e.g., Achtziger et al. 2014; Grether 1980; Kahneman and Tversky 1973). Although people certainly hold and update prior beliefs, the process often strays from the one that would prescribed by optimal Bayesian updating. Sometimes people pay too much attention to the data (display base-rate neglect) or too much attention to the priors (the representativeness heuristic). Likewise, studies of strategic thinking have shown that people rarely consider consequences beyond the shortest of future horizons and almost never engage backward induction, the solution used to compute optimal behavior in many dynamic planning problems (e.g., Johnson et al. 2002; Loewenstein and Prelec 1992; O'Donohue and Rabin 1999; Rust 1992). Perhaps most destructively, there is also little support for the presumption that people have stable utility functions that are invariant to future outcomes, another key lynchpin of normative analysis. If years of research in judgment and decision making have reached any definitive conclusions at all, it is that preference functions are highly malleable; the moment a firm changes the contextual ecology, it immediately changes the preferences that are operant in that ecology (e.g., Payne, Bettman, and Johnson 1992; Tversky and Simonson 1993). Although the evidentiary value of some of this work is not without its share of critics (e.g., Myagkov and Plott 1997), even if half of the research is true, it would seem to severely impede the ability of structural models to provide useful guidance for strategy.

When confronted with such critiques, structuralists tend to offer up two lines of defense. One is that most structural models are not nearly as behaviorally naive as behavioral critics sometimes like to claim. Myopia in forward planning, for example, is naturally accommodated by allowing for flexibility in the intertemporal discount rate, and choices are typically assumed to be probabilistic, a concession to the fact that not all of the factors that influence choice can be known (e.g., Erdem and Keane 1996). The counterargument, however, is that parametric flexibility typically ends there; consumers and firms are still assumed to be dynamic optimizers, only now with respect to a discounted utility function. To a psychologist, the greater threat to the descriptive validity of structural models is this optimization assumption rather than the accuracy of the assumption about the curvature of the embedded discount function.

The second, and more powerful, line of defense is that the models are simply meant to provide "as-if," not literal, models of markets and decision makers (e.g., Hutchinson and Meyer 1994). As-if optimality is often defended by invoking the "playing pool" analogy made famous by Friedman and Savage (1948): much in the same way that expert pool players do not do mentally compute torque and angular momentum before executing a shot, structural modelers do not assume that firms literally solve Bellman equations when deciding on their marketing actions. Yet, just like pool players, firms are led to "as-if" optimal behavior by the invisible hand of trial-and-error learning, something naturally imposed by the market. Firms that make mistakes are quickly ejected from the market, and consumers who make the wrong choices learn to make the right ones the next time—leaving, in the long run, just the "outer envelope" of decisions that are not mistakes.

However, the contrarian view is just as forceful. Although behavioral decisions researchers would certainly concede that learning occurs, they would also argue that the pool analogy is mythical. Real markets are not like games of pool in which optimally angled shots are always rewarded and suboptimal ones always punished. Markets afford few opportunities to learn from mistakes, and when they occur, there is too much ambient noise to learn from them. Markets *are* something of a natural experiment, one kept alive by a constant influx of naive firms and consumers who routinely leave resources and opportunities on the table.

Bridging the Gap: A Theory of Descriptive Strategic Optimality

Which of these discordant perspectives is closer to the truth? In this article, we try to resolve this question by offering some ideas on when "as-if" assumptions of dynamic optimality are likely to be good ones and when they might plausibly be brought into question. We draw on evidence from examples of experimental work completed in our lab over the past 20 years that illustrate the full spectrum of evidence on as-if optimality. In the remainder of this article, we describe three types of research that examine the extent to which people can learn to use dynamically optimal strategies. Type 1 research finds that people can easily learn strategies that are close to optimal, despite starting from intuitions that are far from optimal. Type 2 research finds that people can adapt their strategies to become more like optimal strategies, but doing so is difficult and many people never learn. Type 3 research finds that, despite the opportunity to learn, people persist in using far-from-optimal strategies. Overall, we argue that Type 2 behavior is the most common. People violate dynamic optimality but do so in ways that can be repaired within the structural framework by allowing for empirical flexibility in the assumptions that are made.

How We Think Strategically

Figure 1 provides a general model that is helpful in explaining why we might see empirical variation in as-if optimality. The essential features of the model draw heavily on sampling-based theories of concept learning first developed in psychology in the early 1960s (e.g., Levine 1966; Restle 1962), schema-based models of memory and problem solving developed in the 1970s and 1980s (e.g., Alba and Hasher 1983; Hummel and Holyoak 2003; Johnson-Laird 2001; Shank and Abelson 1977), and more recent quasi-Bayesian models of learning (e.g., Griffiths et al. 2010; Sanborn, Griffiths, and Navarro 2010). The intuition is that when faced with a decision problem, the decision maker comes to a solution through five recursive steps:

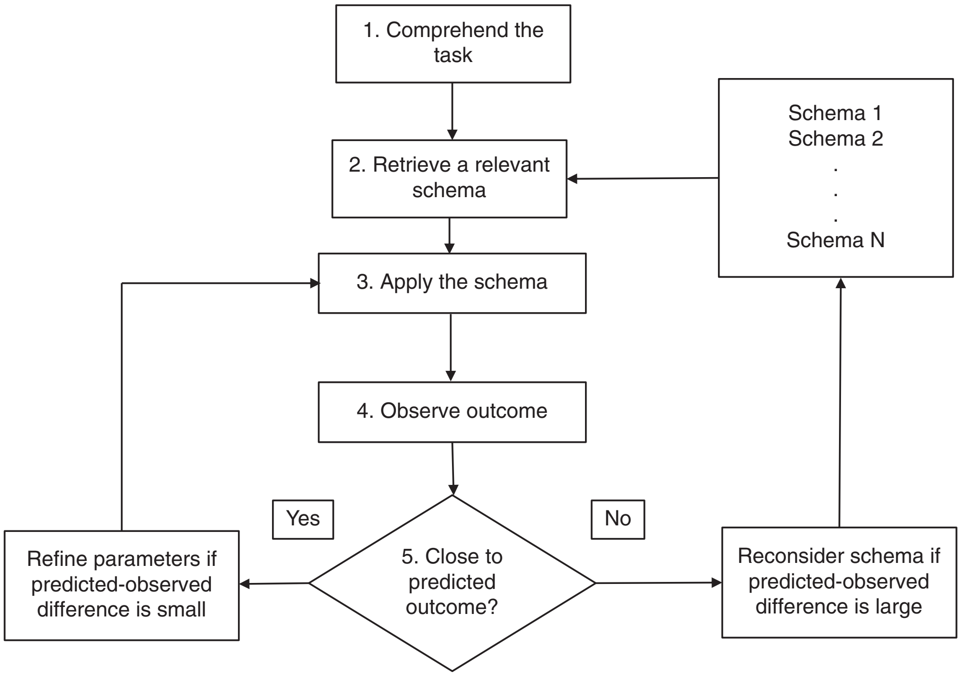
1. Initial problem recognition;
2. Retrieval from memory of a tentative solution hypothesis or schema;
3. Application of the schema;
4. Validation; and
5. Refinement, either by the selection of a new schema or by refinement of the incumbent schema.

Within this model, the degree to which decisions reflect as-if optimality is critically driven by the efficiency of steps 2 and 5: respectively, the degree to which the chosen solution schema mirrors the central features of the normative solution and how its parameters are refined in light of feedback in sequential decision tasks. We use the term "schema" in its most general sense and mean it to include traditional schemas for knowledge maintained in semantic memory (Hummel and Holyoak 2003), mental models (Huang and Hutchinson 2013; Johnson-Laird, 2001), rule-based production systems (Anderson 1983, 2007), game-playing strategies (e.g., Camerer and Ho 1999), memory structures (Alba and Hasher 1983; Stangor and McMillan 1992), scripts (Shank and Abelson 1977), and hierarchical goal structures (e.g., Cooper and Shallice 2006).

Type 1: When Learning from Experience Succeeds and Dynamic Optimality Is a Good Approximation of Actual Behavior

To illustrate how such a process can produce as-if optimal behavior, we recently undertook a program of experimental work that examined people's ability to form accurate intuitions about the expected maximum of multiple draws from a distribution (Hutchinson, Meyer, and Brenner 2016). We focused on this problem because it is an intuitive skill that people are assumed to possess when performing a wide range

FIGURE 1
A Simple Schema-Based Model of Dynamic Decision Making



of strategic decision problems, such as sequential search and bidding at auctions. To illustrate, when searching for the lowest price in a market, what rationally matters most when deciding when to stop is not how the current price compares with the average price in the market but, rather, how it compares with the lowest price one might expect to realize given continued search—a calculation that gives rise to a reservation stopping price (e.g., Cox and Oaxaca 1996).

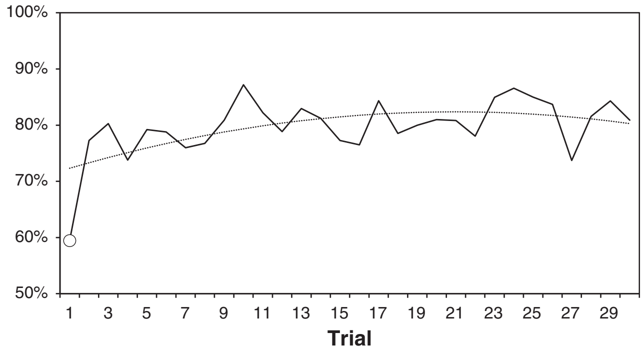
To study how accurate people are in intuitively performing such calculations, we asked people to directly estimate both the expected value of the sample maxima of multiple draws from the uniform distribution and the probabilities with which such sample maxima would exceed certain values. Virtually all participants dramatically underestimated the true values, and approximately one-third of the participants believed that the sample maximum is not affected by sample size. Thus, their estimates were extremely biased.

However, a second group of participants did far better when asked to play a gambling game that required the same mental calculation but for which only choices, not numerical estimates, were required. In the game, participants first observed k draws from the uniform distribution. Then, they decided between keeping the observed sample maximum or taking the sample maximum of a second k draws. Finally, the second k draws was realized, and the outcome (i.e., win or lose) was observed. People played three blocks of 30 games for monetary compensation, with each block corresponding to a different number of draws (two, four, or six).

The results of this game could not have differed more from those observed in the previous direct estimation task. In Figure 2, we plot the average percentage of normatively correct choices in the task over the first 30 games played. We find that not only did participants do quite well on average (more than 80% of choices were correct), but they reached that level of achievement almost instantly, after a single trial. Specifically, whereas people struggled in making their first decision (choices were just slightly above chance, with a 59% success rate), in the second and subsequent decisions, choices were close to the normative model. Moreover, when quantitative learning models were fit to the data, the hit rate for the best-fitting learning model was not much better than a model in which decisions were always optimal (83% vs. 80%; see Hutchinson, Meyer, and Brenner 2016), though the psychological model did fit the data better.

Our proposed schema-based model of dynamic decision making (Figure 1) provides an explanation for this dramatic improvement in achievement. When first exposed to the task, participants retrieved a schema from memory that matched their initial appraisal of the problem (steps 1 and 2). Although we did not observe this initial appraisal process, subjective estimates were clearly influenced by a schema based on sample averages rather than sample maxima. Approximately one-third of participants gave the sample average as a response and often verbalized their belief that sample size should not influence their estimate. Even participants who provided estimates that did increase with sample size underestimated the true values substantially. They appeared to use some sort of anchoring-and-adjustment strategy that insufficiently adjusted their response (as is often found in behavioral research; e.g., Frederick and Mochon 2012; Tversky and Kahneman 1974). Thus, some participants erred because they tried to solve the

FIGURE 2
Percentage of Correct Choices in the Maximum-Value Game by Trial



problem using the wrong schema (an appraisal error), whereas others erred because they misapplied the right schema (a calibration error).

So, what enabled such mistakes to vanish so quickly when participants played a repeated game? We suggest that the improvement came from two different sources: improved appraisal (phases 1 and 2) and corrective action (phases 4 and 5). First, for many people, merely observing the sampling process and outcomes in the first trial was enough to rule out the "maxima-are-like-averages" schema. Seeing the draws makes it clear that the maximum will increase with more draws, unlike the average. Second, we argue that the sudden jump from 59% to more than 80% occurred as a result of the successful application of the feedback-and-resampling mechanism depicted in Figure 1 (steps 4 and 5). Those whose initial choices were guided by the overly conservative intuitions about maxima observed in the first task would have had a good chance of losing in the first game and would have observed two sample maxima that were likely to have been close to the true expected value. Thus, it would have been obvious that they needed to set a higher threshold, bringing most players into the ballpark of the optimal decision threshold. Although learning after the first trial was slower (see Figure 2), Hutchinson, Meyer, and Brenner (2016) show that outcomes exhibit a very flat maximum when the threshold is close to optimal (e.g., Von Winterfeldt and Edwards 1982), which inhibits parameter adjustments (i.e., steps 4 and 5), and as noted previously, modeling of the choice behavior shows that hit rates are very similar for optimal and modeled decision processes.

Type 2: When Learning from Experience Partly Succeeds

The case of on-time airline departures. The notion that people can, given the right task and feedback, learn to exhibit behaviors that approximate statistical optimality might be viewed as being of limited help in addressing the core question of this article—namely, whether optimality assumptions are tenable in the kind of complex real-world tasks that consumers and firms typically face. There is good reason to be skeptical; after all, even in the ideal situation illustrated in the previous subsection, participants were never able to achieve perfect as-if optimality, and real-world tasks would be far more challenging. In the real world, the set of possible schemas would potentially be much larger, feedback less frequent and less diagnostic, and ambient noise levels much higher.

To provide a working example, consider the following problem in sequential airline choice, akin to that studied by Meyer and Shi (1995):

> Mr. Smith is a frequent business traveler and needs to make 20 trips from Philadelphia to Dallas over the next year. In the past, he has taken American, which has a 70% on-time departure rate. Buzz Airlines has just started servicing the same route. Based on service elsewhere, experts expect that its on-time departure rate may end up ranging anywhere from 50% to 90%. He assumes that each airline's departure rate will be constant over time, and he does not get loyalty program credit for flying either airline. On which airline should he book his first flight? After observing whether it was on time or delayed, how should he use that information to decide which airline to take for his second flight? His third?

Readers familiar with dynamic decision theory will recognize this as a variant of the "armed-bandit" problem of sequential statistics (e.g., DeGroot 1970), which forms the foundation of several dynamic structural models of consumer choice (e.g., Chintagunta et al. 2006 Erdem, Imai, and Keane 2003; Erdem and Keane 1996). Note that at first glance, this does not seem like it should be a particularly difficult problem to solve. While few of us may have ever faced this exact problem in airline choice, all of us routinely face the equivalent dilemma of deciding whether to choose a more or less familiar option—be it when choosing a restaurant to dine at, a television show to watch, or flavor of ice cream to try. It is a decision we face and resolve repeatedly and, usually, effortlessly.

Yet, from a mathematical perspective, the optimal solution to this problem is anything but simple. Knowing which is the right option to choose on any trial requires one to find the right balance between two considerations: the probability that the current flight will be on time, as inferred from the available past history of departures, and the long-term expected departure rate that one might expect to realize if (s)he invests in learning about the new airline by taking trial flights on it. Thus, at a minimum, some sort of "explore/exploit" schema is needed to approximate optimal decision making. Although the basic notion that there is value in experimentation is hopefully an intuitive one, computing an exact stay-or-switch policy requires the application of a kit bag of mathematical tools that poses numerous opportunities for decision errors. In this case, the optimal policy has the following form:

> On the first trial, book the unfamiliar airline. Observe the outcome and keep flying on the new airline as long as the number of experienced on-time departures, S, does not fall below a critical success frequency that varies by trial, S^*_t. If S falls below S^*_t, switch to the familiar airline for all remaining flights.

S^*_t is solved by backward induction and assumes that the decision maker both uses Bayes's rule to update beliefs about the true on-time departure rate of the new airline after each flight and makes all decisions using the optimal policy.[1]

Of course, a person actually faced with the task in our example is unlikely to employ this solution. Even if the backward-induction logic were to occur to the traveler, it is implausible that (s)he would have the cognitive hardware to compute the optimal critical success threshold series. If there were ever a task in which the "as-if" assumptions of the structural modeler might reasonably be brought into question, this would seem to be it.

So, if people would not solve this task optimally, how *would* they solve it? The prospects for as-if optimality might not actually be as bleak as they first appear. As we have noted, unlike the sample maxima problem, life is replete with decisions that resemble this one, and over the years most of us have accumulated a large kit bag of schemas that would be helpful in solving them—some bearing features of the normative policy. For example, the notion that there is information value in trying things that are novel would seem quite intuitive and even evolutionarily hardwired. It is the instinct that enables small children to acquire skills and tastes and underlies the familiar adage one hears when exposed to something new: "Try it, you might like it." As such, although the traveler may have never heard of either backward induction or Bayes's rule, simple application of a "try it, you might like it" heuristic might produce a sequence of choices that appears to be generated by the normative policy: the traveler would start by booking the new airline to see what luck (s)he has with it and then stick with it as long as it continues to perform well (for a discussion of win-stay-lose-switch models used in psychology, see Worthy and Maddox 2014).

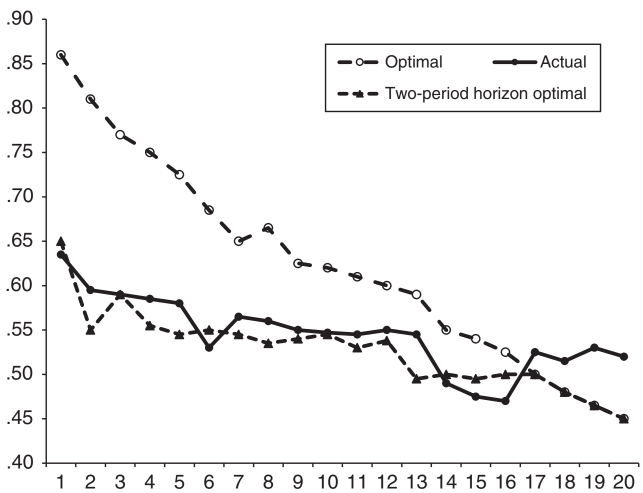
However, there is a fly in the ointment. Although the airline problem might well invoke a "try it, you might like it" schema, it could just as easily evoke competing schemas that are even more instinctive. For example, the problem may evoke schemas that discourage choices that involve risk and/or repeating choices that have had an unfavorable outcome (the core property of reinforcement learning; see Worthy and Maddox 2014). If these schemas are the most cognitively salient, we would see a series of choices that no longer resemble those prescribed by the optimal policy: the traveler might start with the more familiar airline and then aimlessly oscillate between the two on subsequent trials as a function of whether the last flight happened to be on time. There would be no evidence of strategic foresight, and no evidence of meaningful learning. Moreover, outcome feedback would be of little help in steering a decision maker to the optimal policy. Observing whether the plane departed on time or was delayed speaks only to the ex post wisdom of that one choice, not to the long-term performance of the policy that gave rise to it—something that can only be assessed by comparing success rates of different policies over a large number of choices.

[1]For a discussion of solution methods for armed-bandit problems, see DeGroot (1970).

To determine whether simple heuristics could lead to as-if optimality, Meyer and Shi (1995) had 60 participants play six different versions of the airline task, varying the number of trips they would make (6 or 20) as well as the on-time departure rate of the incumbent airline (30%, 50%, or 70%). In each case, the true on-time departure rate for the new airlines was a random draw from a uniform distribution bounded by 20% more or less than that of the incumbent—something participants could only learn by taking trial flights on the new airline.

Were participants as-if optimal? In Figure 3 (adapted from Figure 6 in Meyer and Shi [1995]), we plot the mean percentage of times participants chose the unfamiliar airline, pooling over base rates for the case of a 20-period planning horizon—the scenario in which investing in trial flights of the new airline is most strongly normatively supported. Figure 3 plots the actual choice rates for each trial with those that would be predicted if participants used the optimal forward-looking policy to make their choice, with beliefs based on the outcome of previous choices (which may or may not have been dynamically optimal). In contrast, if participants were completely myopic and simply chose the option that had the higher expected value on each trial choice shares would hover around .5 for all trials. The contrast between these two sets of normative predictions provides a direct test of the degree to which participants held normatively consistent beliefs about the long-term value of experimentation. If participants were as-if optimal, we should observe almost all of them choosing the new airline on early trials, with the choice proportion converging to .5 over time as they discovered its true value—half of participants learning that the new airline is worth sticking with, and the other half learning that the incumbent is better. In contrast, under a myopic policy, we should see no such evidence of an interest in experimentation.

FIGURE 3
Percentage Optimal Versus Actual Choices of Unfamiliar Airline by Trip Number



The key finding is that actual choices fell between the two normative benchmarks; on almost all trials, participants were indeed more likely to try the unfamiliar airline than would be predicted if they were completely myopic, but to a much lower degree than would have been predicted had they been dynamically optimal. What drove their choices? Although we did not observe the actual process, one hypothesis is that the task was marked by an ongoing cognitive tug-of-war between two instinctive solution heuristics: (1) a "try it, you might like it" rule that urged participants to patiently experiment with the new airline and (2) a "don't repeat a mistake" schema that urged them to end the experiment as soon as they encountered a delayed flight. Reflecting this notion, a logistic regression analysis of choices revealed that as the task evolved, one of the key empirical drivers of choice was the lag outcome, which is what we would expect to find from a simple reinforcement-learning process. Yet at the same time, there was a much higher incidence of choices of the unfamiliar airline early in the task, which suggests an intuitive appreciation of the fact that this is when the normative value experimentation would have been the greatest.

There is, however, yet another explanation for the results of the study, one that might restore the normative model as an as-if account of participants' choices. Suppose that participants *were* as-if optimal but were simply bending the rules of optimality a bit to accommodate two cognitive limitations: (1) a tendency to focus more on outcomes over the short-term horizon than those on the distant horizon and (2) a tendency to make occasional—and random—mistakes in choice. To determine whether this could explain the observed choices, on the plot of actual choices in Figure 3 we superimposed the predictions of a dynamic-optimal model that assumed that participants were optimizing over only the next two trials rather than all remaining trials and used Bayes' rule to update beliefs in light of previous choices (some of which may have been mistakes). As Figure 3 illustrates, this produces a set of predictions that closely align with actual behavior. Although it may well be that the actual process was a tug-of-war between competing schemas, it is a war that produces an outcome that is largely indistinguishable from a process that assumes participants were optimal—but myopic—dynamic decision makers.

The case of repeated price search. Huang and Hutchinson's (2013) results from a series of repeated price search experiments provide a second example of learning from experience that produces behavior similar to a modified version of dynamic optimality. These experiments directly manipulated factors that should encourage adoption of normative schema and thereby provide a test of our schema-based model (see Figure 1).

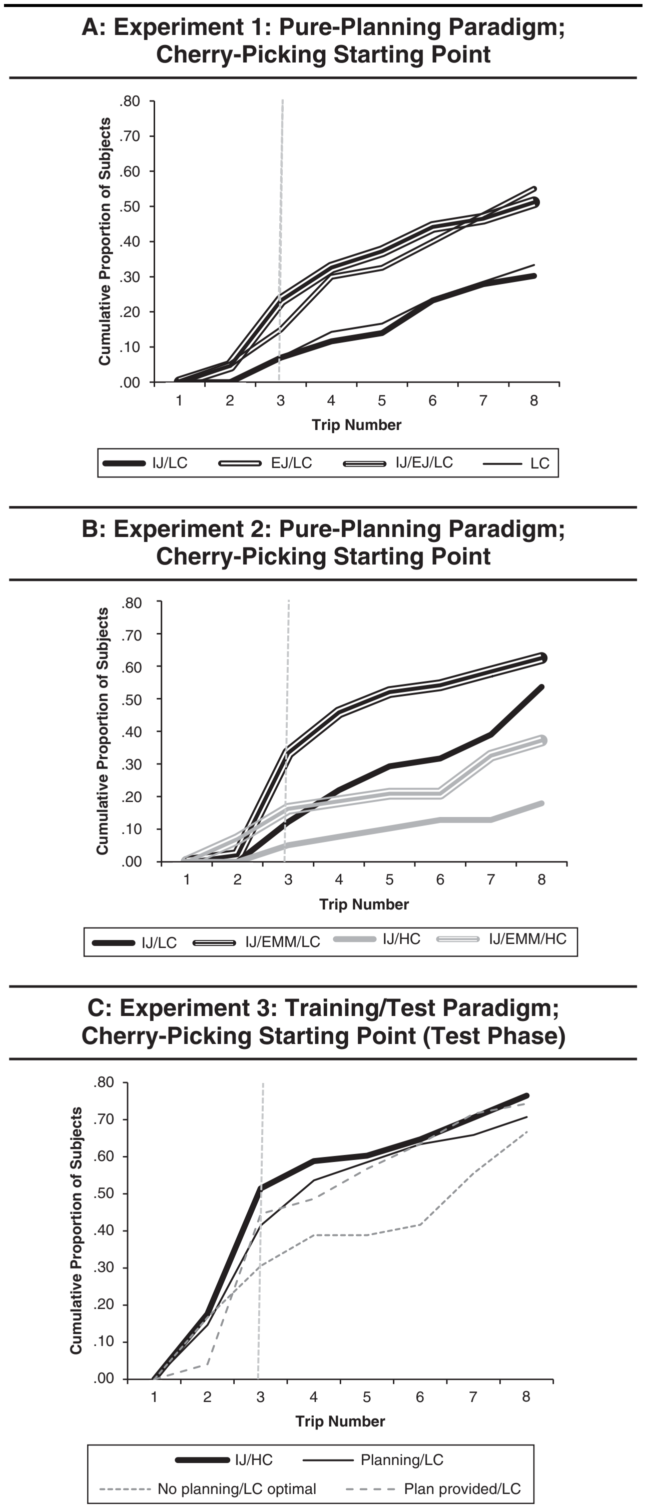
In the experiment, participants were asked to make eight purchases on eight consecutive stylized shopping trips. There were three stores: an everyday low price (EDLP) store that had constant prices, a rip-off store whose prices were constant and higher than the EDLP store, and a promotional (hi–lo) store whose prices were sometimes higher and sometimes lower than the EDLP store. Before shopping, however, participants did not know the identities of these stores. They were thus faced with an explore/exploit-type problem.

The exploit part was straightforward. When the store identities are known, the optimal strategy is to first visit the hi–lo store and make a purchase only if the low price is offered. If the price at the hi–lo store is high, the best option is to travel to the EDLP store and make the purchase there; that is, engage in "cherry-picking." However, learning the store's identities—the explore part of the problem—requires more patience. When search costs are sufficiently low (as was the case in these experiments), the optimal learning strategy is to visit all three stores on every trip (purchasing the lowest-price product) until a price change at one of the stores is observed. At that point, the store pricing policies are known and cherry-picking can be used on all remaining trips.

The experiments encouraged the adoption of appropriate schemas in several ways. In "pure-planning" paradigms, we manipulated schema retrieval before the first shopping trip through (1) enhanced mental models—participants received a set of questions and visual aids designed to guide them toward the optimal explore/exploit strategy, (2) extrinsic justification—participants were required to provide and justify a written plan for their shopping trips and were given a hint that early search costs can be outweighed by later price savings, (3) intrinsic justification—participants were required to provide and justify a written plan for their shopping trips (but they received no hint about search costs), and (4) no justification—participants were required to provide a written plan for their shopping trips, but no justification was required. In "training/test" paradigms, we manipulated schema retrieval for a training phase of eight shopping trips as follows: (1) a group in which participants did not plan their shopping trips; (2) a group in which participants planned, but did not justify, their shopping trips; (3) a group in which participants planned and intrinsically justified their shopping trips; and (4) a group in which participants read and were tested on a description of the optimal strategy. Then, all participants faced the same test phase, which was another repeated price search problem that had the same abstract structure, but the store names were different and prices were given in a different (fictitious) currency. Thus, across three experiments, we used a wide range of subtle and heavy-handed manipulations to increase the likelihood that participants would use an appropriate schema. Intrinsic justification was of particular interest because it is a simple intervention that can be applied to any dynamic decision problem; it simply asks people to develop and justify a plan before they take any actions.

In Figure 4, Panels A–C, we show the results of the schema-retrieval manipulations. We measured use of the optimal exploit strategy by identifying the trial on which cherry-picking was used (and then repeatedly used on all subsequent trials). Note that cherry-picking is a clear behavioral pattern regarding which stores are visited and in which order. For the experimental shopping task, the optimal explore/exploit strategy would reveal store identities in the second trial, so cherry-picking would be expected from the third trial onward. Of particular interest are the results for the intrinsic justification condition (thick solid lines in Figure 4). Although few people exhibited cherry-picking by the third trial (even for heavy-handed manipulations), there is a clear effect of the schema-retrieval enhancements resulting in more cherry-picking sooner.

FIGURE 4
Strategy Growth Curves in a Repeated Price Search Task for Which Cherry-Picking Is Optimal When All Prices Have Been Observed



Notes: Adapted from Huang and Hutchinson (2013). The dashed vertical line indicates when cherry-picking begins (i.e., when planning is optimal). IJ = intrinsic justification; EJ = extrinsic justification; EMM = enhanced mental models; LC = low search cost; HC = high search cost.

The training test paradigm yielded particularly strong effects, and intrinsic justification was just as good as explicitly providing the optimal strategy (suggesting that the optimal strategy was somewhat difficult to comprehend and remember). Thus, we did not observe the step function on the third trial, but more than 60% of participants eventually adopted the optimal exploit strategy.

This result provides important boundary conditions on when people can be dynamically optimal. For relatively simple dynamic strategies, such as cherry-picking, there is reason for optimism. For more complex strategies, such as the complete explore/exploit strategy in this task, there is reason to doubt that people can behave optimally—at least within the range of manipulations explored in these experiments.

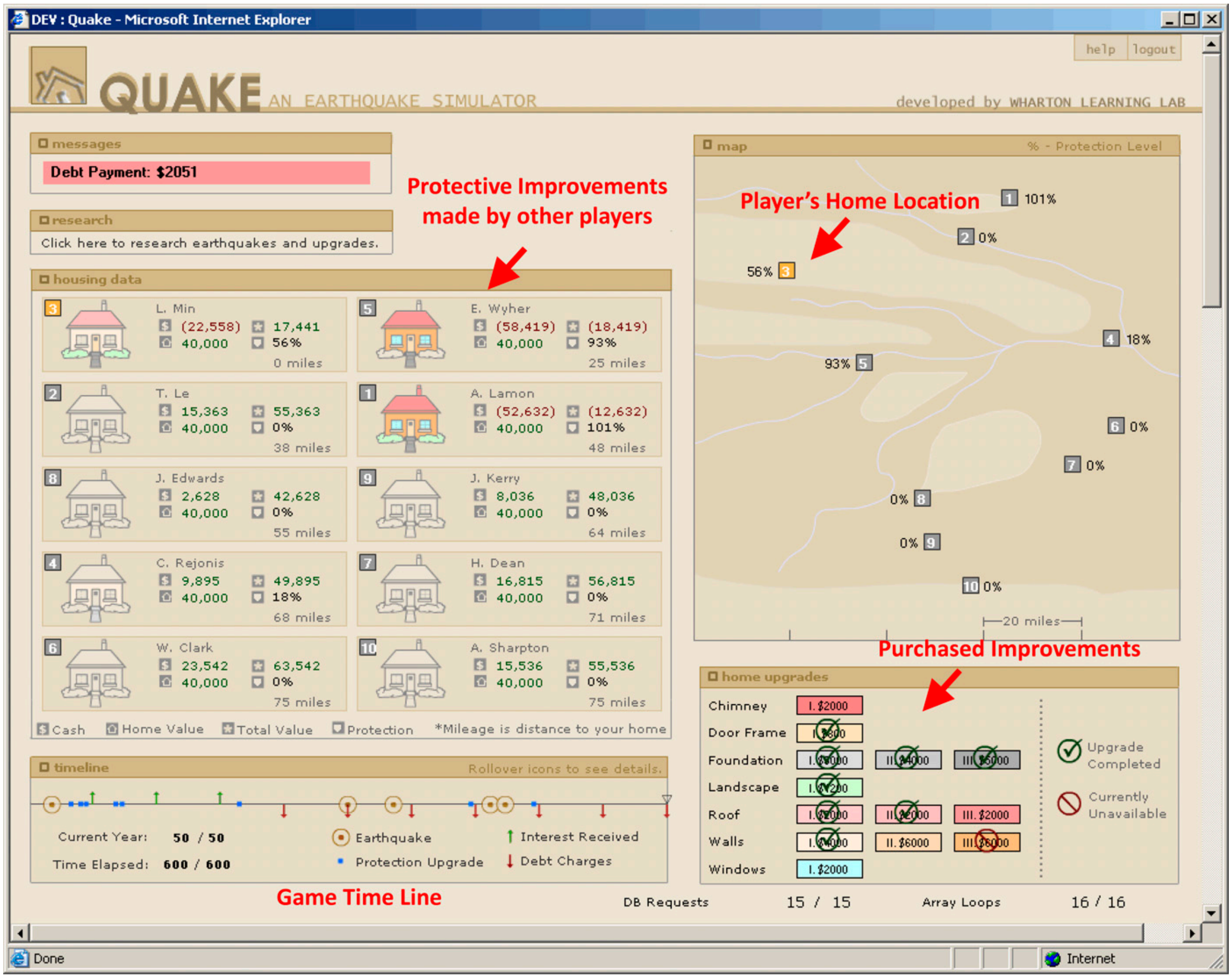
Finally, and most optimistically, the intrinsic justification manipulation worked quite well and is easily implemented for any dynamic decision problem. Huang and Hutchinson (2013) conducted a field experiment on actual mall shoppers that demonstrated the effectiveness of this simple intervention. Specifically, when shoppers were asked to verbalize and justify their shopping plans before they began shopping, they engaged in more search and spent less money, without reducing either their likelihood of making planned purchases or their satisfaction with those purchases.

Type 3: When Dynamic Optimality Cannot Be Salvaged

At this stage, we would be seem to be advocating an optimistic view of the ability of dynamic-optimal models to serve as good as-if accounts of actual decision making. There seem to be two paths to as-if optimality, the union of which would cover a wide range of consumer and managerial decisions. In cases in which a decision maker faces a well-structured novel problem for which (s)he does not have a ready-stored solution schema—as illustrated by our sample maxima game—all that seems to be needed is a feedback structure that rules out implausible answers and points the decision maker in the direction of the right answer. In playing the game, when participants' initial expected maxima were far too conservative, the observation of outcomes immediately instructed them to set a higher threshold that was close to optimal. Likewise, when a person faces a complex problem that has familiar elements—such as the airline problem or repeated price search—all that seems to be needed is (1) the right basic schema (i.e., appreciating that there are long-term benefits to initial exploration) and (2) an ability to modify the structure of the normative model to align with the maximization problem that the decision maker is actually trying solve (e.g., maximizing over a discounted rather than undiscounted time horizon or paying the lowest prices while minimizing overall search costs).

However, what about decisions that are complex and *un*familiar? Here, unfortunately, optimality assumptions run the risk of becoming seriously derailed. In particular, if the decision maker holds the wrong schema for the task, and the feedback structure is not amenable for rapid correction, we may observe behavior that not only departs severely from normative benchmarks but also persists. Consider, for

FIGURE 5
Screenshot from the Earthquake Simulation



example, our previous example of the effect of pre-IPO marketing spending on stock valuations. If a manager mistakenly believes that cutting back on spending is a good thing for IPOs, this will be a difficult belief to dispel, even if the subsequent IPO falters. If the IPO fails, the set of possible reasons is too large to uniquely attribute it to a lack of marketing. Moreover, even if the manager suspects that the firm should have spent more, (s)he would be unable to observe the counterfactual of what would have happened had the firm indeed done so.

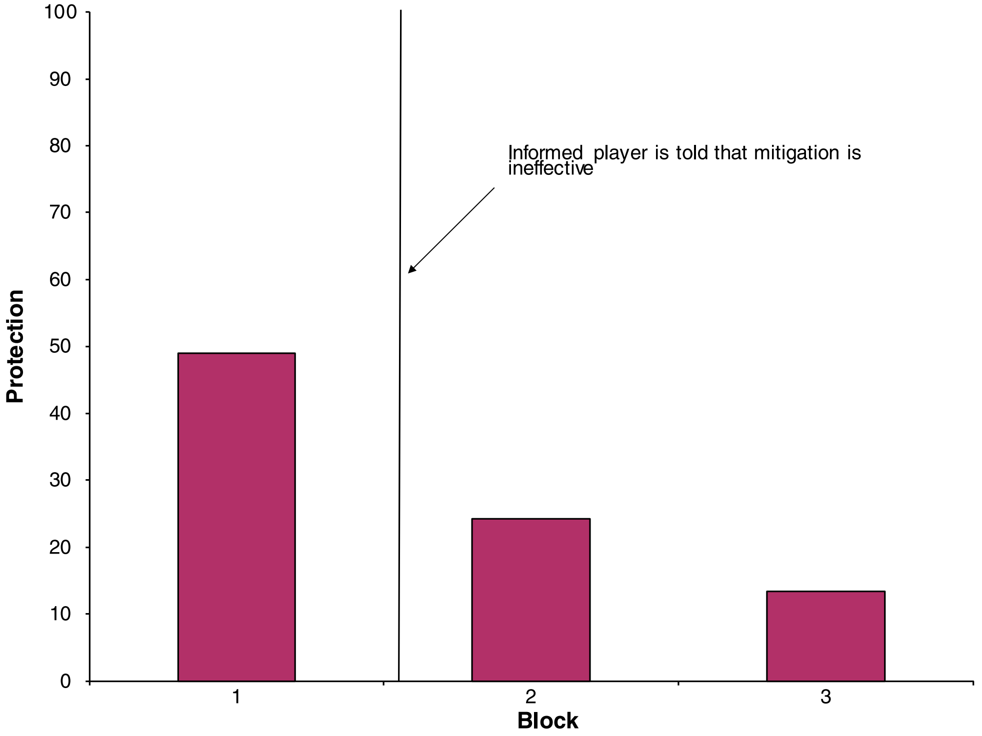
To illustrate such failures, we designed an experiment that examined people's ability to learn optimal strategies in a context well-known for suboptimality: investing in protective action against low-probability, high-consequence events (e.g., Kunreuther et al. 2002). What makes such contexts ripe for error is that, when confronted with the prospect of a rare hazard, such as a terrorist attack or earthquake, people are unlikely to have well-honed schemas for dealing with it. Opportunities for learning are rare, and when they do arise, the feedback that is received is often ambiguous. For example, it is impossible to know with certainty whether better airport security on the morning of September 11 would have fully eliminated or simply redirected the attacks.

To illustrate this issue, we designed a multiplayer simulation that aimed to examine people's ability to learn to make optimal investments to protect homes in a social setting against a well-known natural hazard: earthquakes. Participants were put into groups of six and given the following basic instructions:

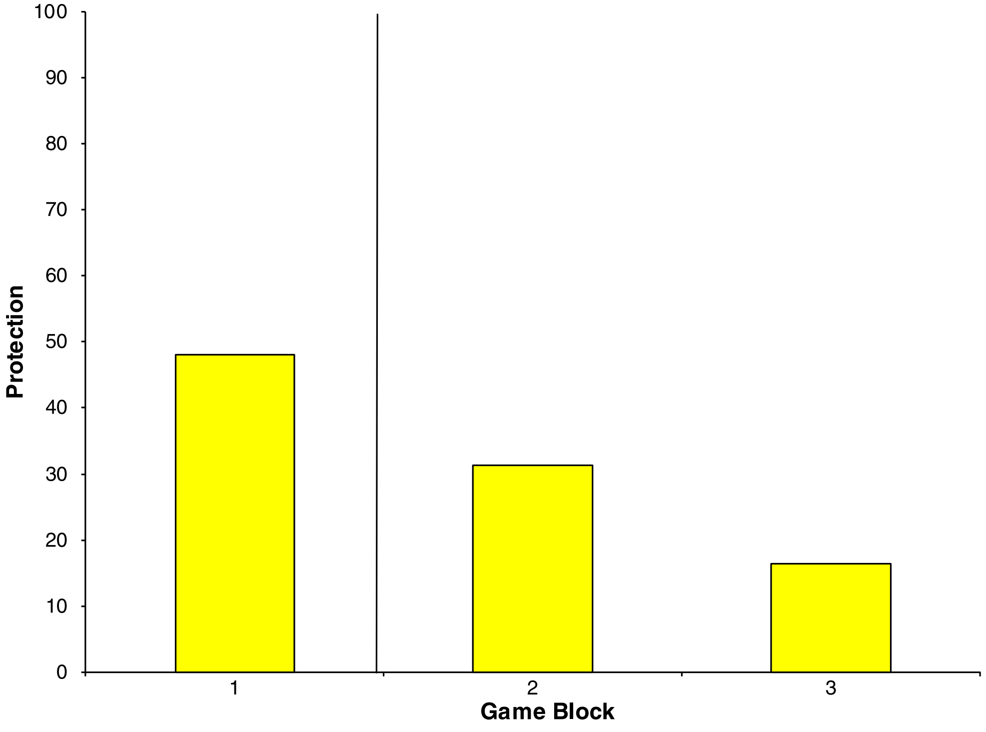
> You and several other players have just bought a house in a hypothetical country that is prone to periodic earthquakes. You each will live there for ten years, which is collapsed to ten minutes in the simulation. Your houses all have an initial value of $40,000, and you have a $20,000 budget that you can use either to purchase protection against earthquakes or to invest at 10% APR. The extent of your investment in protection is measured on a 0%–100% scale, and at any point during the simulation you can choose to make additional purchases as well as observe the level of investments

FIGURE 6
Investments in Protection over Time in the Earthquake Simulation

A: Actual Protection by Game When Optimal = 0



B: Actual Protection When Optimal = 100 by Game Block



Notes: Panel A illustrates when the optimal investment was 0%; Panel B illustrates when the optimal investment was 100%. One player was informed of his or her group's true condition after the first game.

being made by others. There is, however, uncertainty in the degree to which such investments are effective. Some experts believe that they are dollar-for-dollar effective, meaning that if you have a 100% investment, your home will suffer little or any damage should an earthquake occur, while others believe that they are ineffective, and investments will do little to mitigate damage. Each scenario is equally likely. You and the other players will play the simulation three times, which will give you an opportunity to learn whether the protective investments are worthwhile. Your final score will be the sum of your final asset values in each game.

Figure 5 depicts the computer map interface viewed by participants. Note that participants purchased protection by making hypothetical structural improvements (e.g., a stronger chimney) and could continually observe the improvements being made by neighbors. In addition, prior to the start of the simulation, the interface allowed participants to learn about the earthquake risk they would be facing; for example, that over the course of each ten-minute game they could experience anywhere from two to five earthquakes whose epicenter could be anywhere on the map, and their severity would mirror the real-world distribution of intensities (e.g., extreme earthquakes that have the potential of inflicting severe damage would be rare).

In the task, the optimal sequential investment strategy was straightforward: because the optimal level of investment was either 100% or 0%, a player could learn with certainty which condition (s)he was in by making a 100% investment in the first game and then observing whether damage occurred in the event of an earthquake. If there was no damage, the participant should make either a 100% investment or a 0% investment in the next two games.

When we first ran the simulation on ten pilot groups of participants (five groups in each condition), we found no evidence of ability to learn the optimal investment level. The reason, simply, was that no one displayed an intuitive appreciation for the value of experimentation. Regardless of condition and game, the mean investment hovered around 50%—an investment level that would have prohibited learning about whether protective investments were effective or ineffective. In particular, when damage was experienced with a 50% investment, it would have been impossible to assess whether mitigation was effective (whereby the damage would have been less had the participant invested more) or ineffective (whereby the observed damage was reflective of the strength of the earthquake).

In light of this result, we then aimed to examine whether participants could be taught optimality by endowing one participant in each community with perfect knowledge of the optimal investments strategy. To examine this possibility, we recruited 19 groups of participants to play the earthquake simulation, with one important difference: after the first game, each player was given a slip of paper, with one player in each group being told his or her group's experimental condition (i.e., whether mitigation was dollar-for-dollar ineffective or effective). The informed player was not allowed to directly communicate this knowledge to other players but had to reveal it indirectly by his or her investment behavior. For example, if a player knew that mitigation was effective, (s)he would ideally make a 100% investment at the start of the game, which would tip off to other players that they must be in the highly effective condition.

In Figure 6, we plot observed investment levels over the course of the three games for the two conditions. As we observed in the pilot studies, in both conditions the mean investment level was approximately 50%—a level that would have been uninformative of the optimal investment. The surprising result, however, was the trend in investments in games two and three, after one player had been informed of his or her community's true condition. In both cases, we observe that investment levels decline over time—a trend that would be optimal in the ineffective condition but the *opposite* of what would be optimal in the effective condition.

What explains this dysfunctional result when protective investments were effective? An analysis of the nature and timing of investments by the informed player provided an apparent answer. Although the optimal use of this information would have been to invest 100% in protection as soon as possible in each game (the marginal benefit decreases with time), none of the informed participants actually did this. In debriefings, the participants indicated that although they were well aware that protective investments were cost effective, because they had the opportunity to make money from interest payments, they opted to wait a bit before making the investment. This delay, in turn, sent a misleading signal to other players; seeing no one make a major investment in protection, they concluded they must be in the ineffective condition. The lack of investments by other players then further emboldened the knowledgeable player to further delay investments until it was too late and (s)he suffered major damage from an earthquake.

Although perhaps extreme, the earthquake-protection experiments are useful in illustrating the Achilles' heel of assumptions of as-if optimality: what happens when poor task schemas are applied in a setting with ambiguous feedback. Here, participants' schemas displayed two dysfunctional properties that, when working together, severely degraded their ability to learn the value of protection when it had real value: (1) a belief that there was benefit in delaying investments in protection (perhaps out of fear of spending resources to protect against an event that never materializes) and (2) the absence of appreciation for the information value of exploration. Over time, experience led participants away from, rather than to, optimality—the joint consequence of their being unable to observe the benefits of counterfactual investment strategies and their tendency to misconstrue the behavior of others.

Discussion: Settling the Argument over As-If Optimality

Perhaps no other paradigm shift in academic marketing has attracted more attention—and has been more controversial—in recent years than the emergence of structural approaches to empirical strategy research (e.g., Bronnenberg, Rossi, and Vilcassim 2005; Chintagunta et al. 2006). To the converted, such approaches have been championed as the Holy Grail of empirical analysis tools: a means by which researchers and policy analysts can, at long last, overcome the endogeneity critique that has always burdened correlational or reduced-form approaches to analysis: the idea that statistical associations are not invariant to changes in policy (Lucas 1976). Where the structural approach hits a speed bump, however, is in its founding requirement that the researcher must identify an underlying behavioral mechanism that gives rise to these associations—in essence, the underlying psychology of the market. In the absence of any better ideas, structural modelers presume that this is given by normative theory, the idea that consumers and firms make decisions as if they are statistical optimizers, choosing the course of action that maximizes long-term expected utility by being forward-looking Bayesian learners. The drawback of the structural approach is that the value of the policy recommendations that emerge from the analysis are only as good as the quality of this as-if assumption. The more consumers and firms are, in fact, merely groping in the dark, the less useful the guidance will be from a model that assumes they are nearly optimal decision makers.

The Optimality Checklist

How good is this assumption? The growing consensus from the work of both our lab and others is that assumptions of as-if optimality can be surprisingly good—at least as long as certain conditions hold. Our schema-based model of dynamic decision making (Figure 1) suggests that the conditions are fivefold:

1. *The decision maker's initial schema for the problem aligns with that assumed by the normative model.* In most applied contexts, this will be a modest requirement; for example, as in the model proposed by Erdem and Keane (1996), consumers might reasonably be assumed to make purchases strategically in a way that maximizes long-term utility. Our first experiment, however, was a case in which this condition failed; some participants initially misconstrued the task as asking about the effect of sample size on estimates of the mean, not the maximum, of a distribution. As a result, we initially observed large departures from optimality.
2. *The normative policy is among the set of solution heuristics considered by the decision maker.* While more formidable than condition 1, we suggest that this condition will also usually be met. For example, our second set of experiments suggests that consumers intuitively grasp the idea central to Erdem and Keane (1996) that, when making repeated choices among uncertain options, there are long-term rewards to exploration.
3. *The decision problem provides recurrent corrective feedback.* This is a condition that is likely to be met by many—but certainly not all—problems considered in marketing. Consumers make repeated choices among brands; firms make repeated decisions about advertising budgets and observe year-end performance. The challenge is that this recurrence is often sparse (e.g., learning about optimal budgeting strategies may be limited if there is frequent turnover in the management team).
4. *Corrective feedback is not overly noisy.* Unlike conditions 1–3, this condition will be harder to satisfy in most real-world settings. The link between advertising budgets and year-end performance, for example, is typically a distant one, and consumers may have few insights into the utility that would have been realized had other choices been made. Our first and last experiments illustrate extreme consequences of this condition. In the extreme-value game, in which feedback was frequent and clear, we observed rapid convergence to optimality. However, in the earthquake protection study, in which the link between investments and performance was highly noisy, we saw no convergence at all, and in one condition performance actually degraded with experience.
5. *The optimal policy has a sharp maximum.* This condition will also often be difficult to satisfy. As long as there are multiple solution heuristics that give answers in the vicinity of the optimum, a decision maker may retain a suboptimal policy and never quite converge to the true optimum—a result we saw in our experiments on airline choice and price search. In such cases, an analyst may be able to find parameters of a normative model that rationalize observed behavior in a given context, but because the generating process is misidentified, the parameters may not generalize to new settings.

Remedies: The Outlook for Quasi-Optimal Models

We suggest that if the first three conditions are widely met in applied settings, but last two are not, consumers and firms will exhibit behavior that is *directionally* but not *parametrically* optimal. That is, they will respond to cues in the same direction as normative theory would predict, but perhaps not with the optimally prescribed magnitude. This was the pattern of results we observed in our Type 2 examples. Participants could understand the information value of early exploration, and their behavior became more optimal over time; however, in most cases, substantial deviations from optimality persisted.

What should the structural modeler do in this case? Here is where the potential controversy kicks in. On the positive side, because most structural models are parametrically flexible, as long as directional optimality holds, it should be possible to identify a set of utility functions and discount rates that bring observed choices in line with a hypothesized model. Yet, as noted previously, this apparent congruence may be an illusion. If the actual generating process departs from the one assumed by the structural model, the validity of the policy simulations that follow from estimated parameters become an unknown. In such cases, paradoxically, structural models become subject to the same predictive limitations as the reduced-form regression models that they were intended to supplant.

One natural solution in such cases is to consider structural models that embrace more realistic assumptions about decision making. For example, in recent years researchers have made several proposals for utility functions that capture reference dependence, hyperbolic discounting, non-Bayesian learning, and limited forward-thinking abilities (for reviews of much of this work, see, e.g., Erdem et al. 2005; Gabaix et al. 2006; Narasimhan et al. 2005). To date, however, such forms have had not made their way into routine dynamic-structural work for a combination of practical and philosophic reasons. A dynamic-structural model that assumes, for example, that utility is reference dependent would be quite complex; it would require the analyst to assume that consumers and firms not only strategically look to the future when making decisions but also are capable of maximizing a long-term utility function whereby each new decision changes the reference point for assessing the expected utility for all subsequent decisions. Aside from the computational challenges that such a form would pose, it would be psychologically challenged; if decision makers struggle to make decisions that align with those prescribed by very simple dynamic models, it would be difficult to trust the insights from models that assume that people are capable of far more formidable mental feats.

So how might an empiricist best navigate these waters? At a very basic level, we suggest that structural modelers need to think more deeply and rigorously about the plausibility of as-if optimality assumptions than is currently the operant norm—a factor that has often been missing from structural applications. Note that this is not an insistence that the mathematical structure of a model literally describes the psychological process that actually underlies decisions; after all, all models are paramorphic. Rather, we suggest that policy recommendations need to be considered in light of the five conditions of as-if optimality listed previously—for example, whether the model accurately captures the optimization problem decision makers are actually trying to solve and whether environmental feedback is sufficiently clear as to enable learning. Such an analysis would suggest that as-if optimality assumptions are probably quite good for expert judgments in repeated tasks (setting prices in betting markets and some auctions), are marginal for most consumer and managerial decisions (e.g., setting advertising budgets, deciding when to adopt an innovation), and probably quite poor for rarely encountered tasks in which the normative policy contradicts common intuition (e.g., paying to insure a low-probability, high consequence event) or tasks that are of such complexity that optimal solutions are unknown or known only for narrowly defined special cases.

In light of this, a reasonable take on the current state of affairs is that the extant technology for structural modeling offers a step in the right direction but is far from the policy panacea that its biggest proponents often trumpet it to be. The more a modeler needs to make unrealistic assumptions about a behavioral process to accommodate the limitations of a given data set, the less believable the insights from resulting policy simulations become. At some point, simple reduced-form statistical models will offer a better source of guidance. But therein, we argue, lies an opportunity: if the Holy Grail of empirical strategy work is to be found, it will be through a fusing of economic *and* psychological modeling, one that aims to capture the ideal decisions consumers aspire to make as well as the mechanisms through which contextual and cognitive constraints leave them short of that goal. Note that this will take more than simply inserting more complex functional forms into Bellman equations; rather, it will require a rethinking of how consumers and firms think strategically—a process that may stray far from that commonly captured by statistical optimization models (e.g., Lin, Zhang, and Hauser 2014).

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