Concealing Prices: Can Delayed Price Disclosure Influence Consumer Purchase Decisions?

Amin Shiri

Felipe M. Affonso

Diego Aparicio

Minzhe Xu

Xiang Wang

Chris Janiszewski

Marco Bertini

Amin Shiri (amin.shiri@tamu.edu) is a doctoral student in Marketing, Mays Business School, Texas A&M University, USA. Felipe M. Affonso (felipe.affonso@okstate.edu) is Assistant Professor of Marketing, Spears School of Business, Oklahoma State University, USA. Diego Aparicio (daparicio@iese.edu) is Assistant Professor of Marketing, IESE Business School, University of Navarra, Spain. Minzhe Xu (minzhexu@iastate.edu) is Assistant Professor of Marketing, Ivy College of Business, Iowa State University, USA. Xiang Wang (xiang.wang@warrington.ufl.edu) is Assistant Professor of Marketing, Lingnan University, China. Chris Janiszewski (chris.janiszewski@warrington.ufl.edu) is the Russell Berrie Eminent Scholar Chair and Professor of Marketing, University of Florida, USA. Marco Bertini (marco.bertini@esade.edu) is Professor of Marketing, Esade, Universitat Ramon Llull, Spain. The first three authors contributed equally to this research. Supplementary materials are included in the Web Appendix accompanying the online version of this article.

ABSTRACT

This article reports the first systematic test of a pricing question long debated by businesspeople: Is it better to show prices upfront or later in a purchase process? Across four online experiments and three large-scale field experiments, involving close to 800,000 prospective customers, the authors show that delayed price disclosure can increase or decrease sales relative to immediate price disclosure. Importantly, the authors build on information integration theory to further show that these effects occur because delayed price disclosure influences the perception and importance of price in purchase decisions. Collectively, these findings extend research on price obfuscation—a literature that to date has focused almost exclusively on the shrouding of ancillary charges (add-ons, mandatory fees, taxes, etc.). The findings provide retailers of physical and virtual stores with guidance on when to use the tactic. The findings also contribute to the broader discussion, in management, about the role of "friction" in shopping experiences.

Keywords: Price disclosure, price obfuscation, information integration theory, pricing, retailing, friction.

A popular tactic in retailing is to delay the presentation of a product's price. This tactic has stood the test of time: window displays invited passersby to "ask inside for prices," printed catalogs asked readers to "contact us for prices," and more recently online stores urge shoppers to "click to reveal prices" or "add to cart to see price."

Businesspeople are of two minds as to whether delayed price disclosure improves sales (Nielsen Norman Group 2013; Singer 2019; Witherill 2017). Proponents claim "it is downright stupid to show your prices" next to products (Silva 2018). The argument is that shoppers need to first pay attention to the product and internalize its value. A price is secondary to product benefits and showing it too early can cause sticker shock and exacerbate price sensitivity. This position applies to settings where the value of a product is unclear upfront, as may be the case with experience or credence goods including art, jewelry, and fashion. Opponents claim instead that it is important to be forthcoming and "own" the price conversation (Hubspot 2017). Immediate price disclosure is critical to building trust with customers. They reason that delaying the disclosure of a price can cause confusion and frustration, which only draws more attention to the price. This position applies to settings where the value of a product is relatively clear and tangible, as with fast-moving consumer goods, durables, and technology products.

Given the enduring debate, it is remarkable that research on the relationship between the timing of price disclosure and product sales is limited. There is theoretical and empirical work suggesting that companies benefit from "obfuscating" prices—understood as actions that "frustrate consumer search or make it less damaging to firms" (Ellison and Ellison 2009, p. 427)—when at least some consumers in a market are unwilling or unable to ascertain what a product costs them. Yet, this literature focuses on companies that advertise a base good at a low price and shroud ancillary charges including add-ons, shipping and handling fees, and taxes. In

this context, the claim that some consumers choose to save on search costs or make a computational error is reasonable. In the more general case of a company that shrouds the entire, single price of a product, however, the argument loses credibility. First, consumers are unlikely to make a purchase without any understanding of what they must pay. Second, with just one price there is no sum to calculate and, therefore, no room for errors.

Against this backdrop, we report a series of online and field experiments showing that, relative to immediate disclosure, delayed disclosure of a price can increase or decrease sales based on two factors derived from information integration theory: (1) *price perception*, as delayed disclosure prompts consumers to use information about the product and purchase context to update their beliefs about whether a price is likely to be high or low, resulting in different perceptions once the actual price is revealed, and (2) *price importance*, as delayed disclosure encourages consumers to place more or less weight on this feature depending on the shopping goal adopted by them.

The next section reviews the literature on price obfuscation. We then present our conceptual approach, describe our research plan, and report the experiments. The final section discusses limitations and highlights contributions to research and practice. In terms of contributions, three stand out. First, we add to the extant literature by focusing on a tactic with a longer track record and broader use in business than the one typically studied. To our knowledge, we are also the first to study price obfuscation from the perspective of information integration theory. Second, we contribute empirical evidence to a debate (about the wisdom of delaying price disclosure) that, to date, has relied exclusively on anecdotes and hindsight. Third, our research speaks to the broader discussion in management about the role of friction in shopping experiences. The popular advice to companies is to eliminate all friction from the purchase

journey. Facebook, for example, is adamant that the future of retail is "zero-friction" (Facebook 2023). Our experiments clearly show that not all friction is bad all of the time.

PRICE OBFUSCATION

From a normative standpoint, price obfuscation should not exist: rational consumers should infer that hidden prices must be high prices and stay away from companies that use the tactic (Jovanovic 1982). Yet, price obfuscation thrives in business, giving way to a rich literature, in economics and marketing, looking into plausible explanations.

The extant work studies almost exclusively the case of a company that advertises a low base price and shrouds ancillary charges for discretionary add-ons, mandatory shipping and handling fees, taxes, gratuity, and the like to muddle price discovery (for an excellent review, see Chiles 2021). On the theory side, this tactic is interpreted as a means to exploit consumers who, intentionally or otherwise, underestimate what they end up paying. For example, Ellison (2005) develops a model in which consumers are heterogenous in their sensitivity to price differences, whereas Carlin (2009) and Gabaix and Laibson (2006) model instead consumers that are myopic or sophisticated with respect to future charges. Further, Ellison and Wolitzky (2012) assume differences in search costs, and Chioveanu and Zhou (2013) assume that some consumers are confused by obfuscation. Independent of the mechanism that enables discrimination, these studies conclude that companies will hide prices in equilibrium under fairly mild assumptions.

Similarly, empirical research reports ample evidence that "dripping" ancillary charges later in a purchase process, rather than posting them upfront, increases demand (Blake et al. 2021; Brown, Hossain, and Morgan 2010; Chetty, Looney, and Kroft 2009; Feldman and Ruffle

2015; Goldin and Homonoff 2013; Hossain and Morgan 2006; Morwitz, Greenleaf, and Johnson 1998).

In the context of online shopping, Hossain and Morgan (2006) show that starting eBay auctions at low opening prices and charging high shipping fees produces more bidders and higher revenue than in the opposite case. The authors suggest that obfuscation takes advantage of the fact that some people naturally neglect ancillary charges. Brown et al. (2010) refines this result, showing that eBay sellers capture more revenue by raising shipping fees when they are shrouded, but not when they are disclosed. Then, Blake et al. (2021) report a field experiment on StubHub, a secondary marketplace for event tickets, in which consumers are more likely to select and purchase more expensive tickets when mandatory fees are initially hidden. An analysis of click-stream data further shows that price shrouding seems to frustrate price comparisons.

In a physical retail setting, Chetty et al. (2009) report that disclosing a product's sales tax on the shelf rather than at the cashier reduces sales by approximately 8%. The authors attribute this result to changes in attention by consumers rather than ignorance about taxes. In a later study, Goldin and Homonoff (2013) replicates the finding and investigate heterogeneity in consumer attentiveness, showing that low-income consumers pay more attention to taxes levied at the register than high-income consumers.

Finally, in the laboratory Morwitz et al. (1998) find that separating a buyer's premium from the product price increases demand. The author team also shows that people who are exposed to base prices before they are exposed to surcharges anchor on the former and insufficiently adjust for the latter in their calculations of the total price.[1]

[1] There is far less research on the long-run effect of price obfuscation. In one article, Santana, Dallas, and Morwitz (2020) find that self-justification and mistaken perceptions about additional search costs or potential gains from switching sellers makes initial decisions excessively sticky. Yet, Chiles (2021) argues that reputational concerns

In sum, one conclusion that we draw from this literature is that the type of price obfuscation under scrutiny is different from the one that motivates our work. We find this surprising because the practice of hiding a single price from consumers has a longer history and is inherently more general. To the best of our knowledge, there are just two articles to reference. First, Lynch and Ariely (2000) manipulate price "usability" much as we do: under low usability, participants had to click on a wine name to bring up a screen with its price. They find that increasing price usability had no reliable effect, although they do this in just one laboratory study and a small sample of 72 participants for an eight-cell between-subjects design. Second, Karmarkar, Shiv, and Knutson (2015) use functional magnetic resonance imaging to study the relationship between product primacy (i.e., product information precedes price information) or price primacy (i.e., the opposite) and brain activity. While the authors show that presenting a price tag after presenting the product shifts the attention of participants from considerations of monetary worth to considerations of product liking, they struggle to tie this effect to purchase intentions in their hypothetical scenarios. Moreover, in reality prices are never communicated before products, as they have no meaning on their own.

The second conclusion that we draw from the literature on price obfuscation is that companies benefit from shrouding because consumers ultimately misjudge the total out-of-pocket cost: they ignore or pay limited attention to ancillary charges, hold incorrect beliefs about them, are confused by them, or make computational errors. But can we presume that this logic holds in the case of a company that hides the entire, single price of a product?

---

may counteract the incentive of companies to obfuscate prices if consumers find a way to punish deceptive behavior. Specifically, the article studies the impact of shrouded resort fees on guest ratings in the U.S. hotel industry. The author exploits differences in surcharge disclosure across booking channels to identify the causal effect of obfuscation, finding that hidden fees decrease ratings by roughly 0.15 points on a 1 to 5 rating scale.

One reason for doubt is that we study the sale of single products rather than of assortments comprising base goods and add-ons. For example, the models developed by Ellison (2005) and Gabaix and Laibson (2006) assume that companies offer different qualities of a product simultaneously by allowing consumers to purchase performance-enhancing add-ons. We are not interested in the market for add-ons. A second reason for doubt is that partitioning a price and dripping some components over the purchase process requires that the split makes sense. All of the articles we reviewed break the price at a natural place, separating the product of interest (i.e., the reason why the consumer is shopping in the first place) from one or more ancillary charges. In this paradigm, the extant explanations are reasonable because the smaller, secondary expenses are less consequential. Yet, it is hard to argue that the whole price of a product is less salient just because it is hidden at first. Most consumers will not (and probably cannot) complete a purchase without any understanding of what it costs them. It is also hard to argue that there is room for confusion or any type of mental error—this only applies when consumers have something to be confused about or calculate.

Our research, then, seems to overlap with the existing literature only in that all obfuscation tactics delay price discovery. Time is the common denominator. However, we also see fundamental differences in "what" is obfuscated and "why" it may impact purchase behavior.

PRICE DISCLOSURE AND PURCHASE DECISIONS

Our conceptual approach builds on information integration theory (IIT), a classical framework in social psychology long used by consumer researchers to understand how people integrate multiple pieces of information to form single judgments about products (Bettman, Capon, and Lutz 1975; Kahn and Meyer 1991; Meyer 1981). Specifically, IIT recognizes three

stages in the way information is used by consumers to generate a response: valuation, integration, and response (Anderson 1981; Lynch 1985). In the valuation stage, consumers form perceptions of product benefits, price, and other pertinent (contextual) stimuli. In the integration stage, consumers combine these perceptions into a single judgment based on the importance they assign to each. The integration of attribute values supports overt responses.

We argue that delaying the disclosure of a price affects purchase decisions by acting on its perception and importance. That is, the timing of price disclosure affects responses through the valuation and integration stages in IIT.

Delayed Price Disclosure and Price Expectations

With respect to how price is perceived (i.e., the valuation stage in IIT), we propose that delayed disclosure gives consumers an opportunity to update their beliefs about the amount demanded by sellers. Specifically, we argue that withholding a price prolongs uncertainty about this feature, which prompts consumers to use the information available to them about the product and purchase context (the availability and attractiveness of competing products, commercial activities such as a sale or promotional campaign, characteristics of the shopping environment, etc.) to refine their expectations.

Naturally, this information can cause consumers to expect that the price is higher or lower than initially anticipated. For instance, consumers may conclude that the price is likely higher when they learn that product quality is strong, there are no or few attractive competing products, the seller communicates it is a premium product or experience, and so on. Likewise, consumers may conclude that the price is likely lower when they learn that product quality is poor, there are many attractive competing products, the seller communicates a promotional

event, and so on. Hereafter, we refer to these revised expectations as *price pessimism* and *price optimism*, respectively.

Accordingly, relative to immediate disclosure, we predict that delayed disclosure increases (decreases) sales when consumers are pessimistic (optimistic) about price because the actual price, once revealed, is likely lower (higher) than their expectations. In other words, price pessimism makes consumers perceive the actual price as "cheap," while price optimism makes consumers perceive the actual price as "expensive." We capture this logic in hypothesis 1[2]:

H1a: When information about the product and/or purchase context promotes price pessimism, delayed price disclosure shifts the expected price upward and makes a product offer more appealing.

H1b: When information about the product and/or purchase context promotes price optimism, delayed price disclosure shifts the expected price downward and makes a product offer less appealing.

Delayed Price Disclosure and Price Importance

With respect to how price is integrated with other sources of information (i.e., the integration stage in IIT), we propose that delayed disclosure shifts the importance of price relative to product benefits. Specifically, we argue that withholding a price encourages consumers to place more or less weight on the price depending on the shopping goal adopted by consumers. Building on prior work on the duality of consumer information search routines (Janiszewski 1998), we focus on two fundamental goals: a *purchase goal* or a *browsing goal*.

[2] The hypotheses follow a logic that has been dominant in the pricing literature for over 50 years. There is a negative correlation between changes in the expected price and the appeal of an offer price (Compeau and Grewal 1998; Mazumdar, Raj, and Sinha 2005; Winer 1986). As an expected price increases (decreases), an offer price is perceived as more (less) attractive, and the product offer is rated more (less) appealing.

These goals impact the information consumers attend to, and subsequently use, in their purchase decisions.

When a purchase goal is active, consumers hold a specific need or desire to find and purchase a given product. Purchase goals make consumers goal directed and attribute focused (Baumgartner and Pieters 2008), encouraging them to attend to the salient pros and cons of the target products (benefits, price, etc.) using a search routine stored in memory (Solway and Botvinick 2012). As a result, the attention of consumers tends to be focused and limited to the attributes immediately salient (Corbetta and Shulman 2002; Petersen and Posner 2012; Posner and Petersen 1990).

Delayed disclosure implies that price is not available (i.e., not immediately salient) to consumers when they first encounter the purchase option. Accordingly, consumers who hold a purchase goal should perceive information about product benefits to be more salient relative to price under delayed disclosure than immediate disclosure. As the salience of product benefits increases relative to price, so does their relative importance, which makes the purchase more appealing to consumers (Kim and Kachersky 2006).

When a browsing goal is active, consumers are looking for opportunities to buy, but the search is not driven by a need or desire to purchase a specific product. Instead, consumers with a browsing goal are searching for options worthy of further consideration (Xia 2010). Browsing goals are characterized by exploratory search behavior, which is a moment-by-moment activity less focused on attributes immediately salient. During exploratory search, missing information increases the motivation to “fill in the blanks” so that consumers can assess whether a particular product should be investigated further (Irmak, Kramer, and Sen 2017).

Again, delayed disclosure implies that price is not available at first. Accordingly, consumers who hold a browsing goal have the opportunity to attend to and assess the relevance of this missing piece of information. This implies that, compared to immediate disclosure, delayed disclosure should increase the salience of price relative to product benefits. As the salience of price increases relative to product benefits, so does its relative importance, which makes the purchase less appealing to consumers (Johnson and Levin 1985). Formally:

H2a: When a purchase goal is active, delayed price disclosure decreases the relative importance of the price and makes a product offer more appealing.

H2b: When a browsing goal is active, delayed price disclosure increases the relative importance of the price and makes the product offer less appealing.

RESEARCH PLAN

To summarize, IIT posits that products are judged based on how product benefits, price, and other contextual stimuli are valued and integrated. Similarly, we posit that delayed price disclosure affects purchase decisions by acting on the perception and importance of price, with the first path moderated by expected price (pessimism versus optimism), and the second moderated by shopping goal (purchase versus browsing).

The four "states" that emerge from combining the two moderators populated the matrix displayed in figure 1 and guided our empirical strategy for testing H1 and H2. Independent tests of H1 and H2 are not straightforward because information integration theory assumes weights are used to integrate attribute perceptions and this result leads to overt responses (i.e., both perceptions and weights contribute to a response). Given this reality, our empirical strategy was twofold. First, we investigated situations where the effects of delayed disclosure on price

expectation and price importance should influence sales in the same direction (cells 1 and 4), and avoided instances where the effects should influence sales in the opposite direction (cells 2 and 3). Second, given that the results of tests in cells 1 and 4 would be consistent with both hypotheses, we used mediation to provide support for individual hypotheses.

FIGURE 1

THE EFFECT OF DELAYED PRICE DISCLOSURE DEPENDS ON PRICE EXPECTATION AND PRICE IMPORTANCE

	Purchase Goal (H2a)	**Browsing Goal (H2b)**
	- Delayed price decreases price importance	- Delayed price increases price importance:
	- Likelihood of purchase increases	- Llikelihood of purchase decreases
Price Pessimism (H1a)		
- Delayed price allows attribute/contextual information to shift expected prices up	***Cell 1:*** *Price expectations and importance both increase purchase likelihood*	***Cell 2:*** *The influence of price expectations and price importance are in conflict*
- Likelihood of purchase increases		
Price Optimism (H1b)		
- Delayed price allows attribute/contextual information to shift expected prices down	***Cell 3:*** *The influence of price expectations and price importance are in conflict*	***Cell 4:*** *Price expectations and importance both decrease purchase likelihood*
- Likelihood of purchase decreases		

With this in mind, our research plan included three steps: (1) test the positive effect of delayed price disclosure predicted by H1a and H2a (cell 1), (2) test the negative effect of delayed price disclosure predicted by H1b and H2b (cell 4), and (3) isolate the effect of price expectation predicted by H1, focusing on the contrast between price pessimism and price optimism (the vertical axis in figure 1); and isolate the effect of price importance predicted by H2, focusing on the contrast between purchase goal and browsing goal (the horizontal axis).

We first completed these three steps in a series of controlled online experiments. Specifically, study 1 finds support for H1a and H2a. It also includes a parallel mediation analysis that provides additional support for the hypotheses. Study 2 does the same for H1b and H2b. Study 3 adopts a setting where shopping goal has minimal influence and confirms that switching

between price pessimism and price optimism moderates the influence of delayed price disclosure on purchase intentions (in this case, a renewal decision). Finally, study 4 adopts a setting where expected price has minimal influence and confirms that switching between a purchase goal and a browsing goal moderates the influence of delayed price disclosure on product evaluations. The raw data, Qualtrics .qsf files, and preregistration documents for these studies are available on OSF at https://shorturl.at/nopU5.

Next, we replicated step 1, step 2, and the first part of step 3 in the field in collaboration with a single retailer of household appliances and consumer electronics. Study 5 involved consumers who visited the virtual store. We find support for H1a and H2a in a setting where the available information naturally promotes price pessimism and consumers pursue a purchase goal. Study 6 involved consumers who received an email newsletter. We find support for H1b and H2b in a setting where the available information naturally promotes price optimism and consumers pursue a browsing goal. Finally, study 7 involved consumers who visited the physical stores. Here, the retailer sells to shoppers with a purchase or browsing goal. Delaying the disclosure of high-end products (a proxy for price pessimism) increases sales, whereas delaying the disclosure of low-end products (a proxy for price optimism) decreases sales (H1).

STUDY 1: WHEN DELAYED PRICE DISCLOSURE INCREASES SALES

Our first experiment (preregistration at https://aspredicted.org/2CR_YMB) was designed to test H1a and H2a using conditions consistent with cell 1 of figure 1. We created a setting that promoted price pessimism and the pursuit of a purchase goal, such that delayed price disclosure was expected to act on price expectation (H1a) and price importance (H2a) to increase purchase intentions. Mediation analyses were used to provide more focused support for each hypothesis.

Method

Participants and Design. Two-hundred and one Prolific workers (40.8% female; M_{age} = 35.13, *SD* = 11.50) completed the study for a fixed nominal payment. No participants were excluded. The study had two between-subjects price disclosure conditions: immediate price disclosure (IPD) vs. delayed price disclosure (DPD).

Procedure. Participants were asked to imagine that their current sofa, which they bought four years earlier, had just broken. They needed to buy a new one. To highlight the purchase goal, we asked participants to further imagine that they needed to buy a new sofa as soon as possible because of its daily use. To elicit pessimism about sofa prices, we told participants that sofa prices, especially the prices of designer sofas, were rising due to factors such as higher costs of materials and labor (see Web Appendix A for full materials).

Below the scenario, participants were presented with a sofa option named "Vavilon Sectional Designer Sofa." In the IPD condition, participants saw a picture of the sofa and its price of $989.00. In the DPD condition, participants only saw the picture of the sofa. All the participants were forced to spend at least six seconds on this page, after which they were asked to add the sofa to their shopping cart. Next, all the participants proceeded to the shopping cart, which displayed the picture of the product and its price.

On the shopping cart page, all participants indicated their likelihood of purchasing the sofa ("How likely would you be to buy this sofa?" 0 = extremely unlikely, 50 = somewhat likely, 100 = extremely likely). They also reported the price they expected to see for the sofa prior to learning the actual price ("Before seeing the actual price of the sofa, how much (in $) did you expect it to cost?") by dragging a slider on a scale between $0 and $3,000. Then, participants

reported how important they considered the price and design of the sofa by allocating 100 points between the two attributes. Lastly, participants reported their gender and age.

Results

Purchase likelihood. We predicted that DPD would increase purchase likelihood. Consistent with our prediction, a one-way ANOVA revealed higher likelihood of buying the sofa in the DPD condition (M = 59.91, SD = 29.91) than in the IPD condition (M = 48.95, SD = 29.23; $F(1, 199) = 6.90$, $p = .009$, $\eta_p^2 = .034$).

Expected price and price importance. According to H1a, when the shopping context makes consumers feel pessimistic about the price of a product, participants should "fuel" that pessimism and increase the expected price. Supporting H1a, a one-way ANOVA yielded a positive effect of DPD on the expected price of the sofa (M_{DPD} = 1,676.38, SD = 663.66; M_{IPDe} = 1,427.02, SD = 663.54; $F(1, 199) = 7.10$, $p = .008$, $\eta_p^2 = .034$). H2a was also supported: a one-way ANOVA found that participants in the DPD condition (M = 46.25, SD = 24.38) considered price to be less important than those in the IPD condition (M = 56.98, SD = 22.22; $F(1, 199) = 10.64$, $p = .001$, $\eta_p^2 = .051$).

Mediation. The mediation analysis tested if the positive effect of DPD on purchase likelihood was driven by an increase in the expected price (H1a) *and* a decrease in price importance (H2a). The mediation analysis (PROCESS model 4; Hayes 2022) used price disclosure (0 = IPD, 1 = DPD) as the independent variable, the likelihood of purchasing the sofa as the dependent variable, and the expected price of the sofa and price importance as parallel mediators (5,000 bootstrapped samples). Consistent with H1a and H2a, the indirect effects via

expected price (β = 3.64, *SE* = 1.68, 95% CI = [.78, 7.27]) and price importance (β = 2.83, *SE* = 1.43, 95% CI = [.56, 5.97]) were significant (see figure 2).

FIGURE 2

MEDIATION EFFECTS OF EXPECTED PRICE AND PRICE IMPORTANCE IN STUDY 1.



Note: **$p < .01$, ***$p < .001$

Discussion and Ancillary Experiment

Study 1 provides evidence for H1a and H2a. In a setting where information about the product and purchase context promoted price pessimism, and participants pursued a purchase goal, delayed price disclosure increased the likelihood of buying the option under consideration. This effect was mediated by a higher expected price and lower price importance.

One concern, however, is that measuring purchase likelihood prior to the mediators prompted demand effects: that is, participants in the delayed price disclosure condition reported a higher expected price and a lower price importance than participants in the immediate price disclosure condition because they wanted to justify their higher purchase likelihood. To address

this concern, we re-ran study 1 and elicited the two mediation measures prior to the purchase likelihood measure. The results replicated the results of study 1 (see Web Appendix B).

STUDY 2: WHEN DELAYED PRICE DISCLOSURE DECREASES SALES

Our second experiment (preregistration at https://aspredicted.org/J8C_RYJ) was designed to test H1b and H2b using conditions consistent with cell 4 in figure 1. We created a setting that promoted price optimism and the pursuit of a browsing goal, such that delayed price disclosure was expected to act on price expectation and price importance to decrease purchase intentions. Mediation analyses were used to provide more focused support for each hypothesis.

Method

Participants and Design. Two hundred Prolific workers (44.6% female; M_{age} = 40.29, *SD* = 13.55) completed the study for a fixed nominal payment. As preregistered, we excluded six participants who failed the attention check, leaving 194 effective responses. The attention check failure rate did not differ between conditions (p_{IPD} = 1.0%, 1/100, p_{DPD} = 5.0%, 5/100; exact Fisher's test: p = .212). Including these participants does not change the results of the analyses. The study used two between-subjects conditions: immediate price disclosure (IPD) vs. delayed price disclosure (DPD).

Procedure. Participants imagined that they would move to a new place several months from now. They were thinking about buying a hot water kettle and found two options on an online shopping website. To activate the browsing goal, we asked all participants to explore these options and think about which one they would consider buying in a few months.

Next, participants saw two kettle options, Kettle A (the target option) and Kettle B (the outside option), along with their pictures and attribute information including capacity, material, function, and price (see figure 3). To make participants optimistic about the price of Kettle A (i.e., expecting a low price), added a label stating "CLEARANCE SALE (BIG SAVINGS)" to this option.

Importantly, we manipulated whether the disclosure of Kettle A's price was immediate or delayed. In the IPD condition, participants saw the price of Kettle A ($52) directly. In the DPD condition, the price of Kettle A was replaced by a "Click to see price" button. Participants needed to click the button, waited four seconds for the price to load, and then saw the price of Kettle A, which was also $52. In both conditions, the price of Kettle B ($47) was shown immediately.

FIGURE 3

THE STIMULI IN THE DELAYED PRICE DISCLOSURE CONDITION (STUDY 2)



Kettle A

CLEARANCE SALE
(BIG SAVINGS)

Capacity	1.8 liters
Material	Premium stainless steel
Function	Fast boil
Price	Click to see price



Kettle B

Capacity	1.7 liters
Material	Stainless steel
Function	Fast boil, temperature control
Price	$47

Afterward, participants reported the likelihood of purchasing Kettle A ("How likely would you be to buy Kettle A (on the left)?" 1 = Not likely at all, 7 = Very likely). Then, they allocated 100 points across the five attributes (price, capacity, material, functions, and appearance) to indicate their importance. Next, they reported the expected price of Kettle A before seeing its actual price ("Before seeing the price of Kettle A (the option labeled "Clearance Sale (Big Savings)"): How much (in $) did you expect it to cost?") by dragging a slider on a scale between $0 and $100. Finally, participants answered an attention check question ("Previously, you saw 2 kettle options. What were their prices?") and reported their gender and age. See Web Appendix C for full materials.

Results

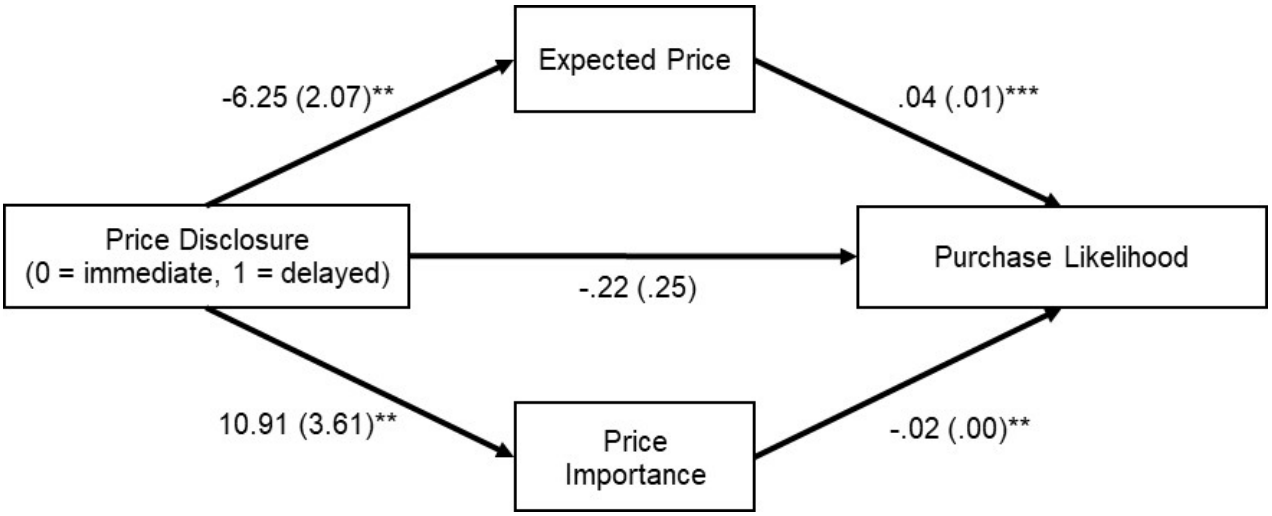
Purchase Likelihood. We anticipated a lower likelihood of purchasing Kettle A in the DPD condition than in the IPD condition. Consistent with this, a one-way ANOVA found that the delayed disclosure of Kettle A's price decreased the likelihood of buying Kettle A (M_{DPD} = 2.89, SD = 1.69; M_{IPD} = 3.54, SD = 1.90; $F(1, 192) = 6.15$, $p = .014$, $\eta_p^2 = .031$).

Expected Price and Price Importance. H1b claims that, in contexts where consumers are optimistic about price, DPD decreases the expected price of a product. Supporting H1b, a one-way ANOVA found a lower expected price of Kettle A in the DPD condition (M = 37.82, SD = 10.25) than in the IPD condition (M = 44.07, SD = 17.52; $F(1, 192) = 9.12$, $p = .003$, $\eta_p^2 = .045$). H2b argues that, in contexts where consumers have a browsing goal, delayed disclosure of a price renders the price information more important in the decision-making process. Supporting H2b, a one-way ANOVA found a significant positive effect of DPD on price importance (M_{DPD} = 50.67, SD = 26.68; M_{IPD} = 39.77, SD = 23.59; $F(1, 192) = 9.10$, $p = .003$, $\eta_p^2 = .045$).

Mediation. H1b and H2b further argue that the lower purchase likelihood is driven by a lower expected price and an increase in price importance, respectively. To examine whether both mediators drove the negative effect of DPD on purchase likelihood, we conducted a mediation analysis using PROCESS model 4 (Hayes 2022). The independent variable was price (0 = IPD, 1 = DPD), the dependent variable was the likelihood of purchasing Kettle A, and the parallel mediators were the expected price of Kettle A and price importance. With 5,000 bootstrapped samples, the analysis revealed both a significant indirect effect via expected price (β = -.25, *SE* = .10, 95% CI = [-.50, -.09]) and a significant indirect effect via price importance (β = -.17, *SE* = .08, 95% CI = [-.37, -.05]; see figure 4), supporting both H1b and H2b.

FIGURE 4

PARALLEL MEDIATION EFFECTS OF EXPECTED PRICE AND PRICE IMPORTANCE (STUDY 2)



NOTE. — $^{**}p < .01$, $^{***}p < .001$

Discussion and Ancillary Experiment

Study 2 provides evidence for H1b and H2b. In a setting where information about the product and purchase context promoted price optimism, and participants pursued a browsing

goal, delayed price disclosure decreased the likelihood of buying the option under consideration. This effect was mediated by a lower expected price and higher price importance. Yet, as discussed in study 1, it is possible that measuring the purchase likelihood prior to the two mediators prompted demand effects. To address this concern, we re-ran the experiment and elicited the two mediation measures prior to the purchase likelihood measure. The results replicated the results of study 2 (see Web Appendix D).

STUDY 3: ISOLATING THE EFFECTS PRICE EXPECTATIONS

The first two experiments focused on settings where delayed price disclosure affected both price expectation and price importance to cause similar effects on purchase decisions. That is, they focused on cells 1 and 4 of figure 1, respectively. The next two experiments, however, focus exclusively on price expectation (by varying price pessimism vs. optimism) or exclusively on price importance (by varying the shopping goal). Specifically, study 3 (preregistration at https://aspredicted.org/VTV_9HN) was designed to isolate the role of price expectations in the effect of delayed price disclosure on purchase intention. We expected a positive effect of delayed price disclosure (relative to immediate price disclosure) on purchase intention in the context of price pessimism (H1a) and a negative effect in the context of price optimism (H1b).

Method

Participants and Design. We recruited 404 U.S. participants from Prolific in exchange for a fixed nominal payment. As preregistered, we removed four participants who failed the attention check, resulting in an effective sample of 400 (43.5% female; 18 to 80 years, M = 35.71, SD = 12.42). The failure rate did not differ between conditions. Including the excluded responses did not change the results of the analyses. The study used a 2 (price disclosure:

immediate price disclosure (IPD) vs. delayed price disclosure (DPD)) × 2 (price expectation: pessimistic vs. optimistic) between-subjects design.

Procedure. The experiment started by informing participants that we were interested in how people made judgments and decisions. Then, we elicited an acceptable monthly rent (in $, utility fees not included). Next, we told participants to imagine they were living alone in an apartment, paying an amount equal to their acceptable monthly rent, and that they were considering whether to renew the lease or not. Specifically, participants read that they received a notice from the leasing office, titled "It's time to renew!" The notice explained: "Dear Resident, you are an important part of our community, and we want to thank you for being a wonderful resident. We hope that you have found our community to be a happy one! We invite you to continue to be our resident and renew your lease. Note that your monthly rent will change if you decide to renew." We anticipated that this setting, a renewal decision, would not activate a shopping goal because of the absence of alternative options—that is, participants had no reason to engage in exploratory search behavior.

All participants received a rent offer that was 8% above their current rent. This was calculated dynamically according to the amount each participant indicated as acceptable. However, the offer was framed differently depending on whether we evoked price pessimism or price optimism, and whether the offer was presented upfront or once participants accessed their resident account.

To give an example, in the price pessimism condition a participant who indicated that $1,000 is an acceptable sum read "Your current rent is $1,000. Your new rent is $1,080" if price disclosure was immediate (IPD condition) but read "Your current rent is $1,000. To check your new rent, please sign into your resident account" if price disclosure was delayed (DPD

condition). In the price optimism condition, a participant who indicated that $1,000 is an acceptable sum read “Your current rent is $1,000. Since you have been living here with an excellent payment history, we would like to offer you a discount! Your new rent should be $1,120, but we are offering you a $40 discount for prompt payments, making the rent $1,080” in the IPD condition, but read “Your current rent is $1,000. Since you have been living here with an excellent payment history, we would like to offer you a discount! To check your new rent, please sign into your resident account” in the DPD condition.

All participants then advanced to log into their resident accounts and received additional details. Participants in the price pessimism condition read a summary of charges that included their current rent (the amount the indicated as acceptable), the rent increase (8% of the current rent), and the new rent (the current rent plus the increase). Participants in the price optimism condition read a summary of charges that included their current rent (the amount they indicated as acceptable), the rent increase (12% of the current rent), the discount (4% of the current rent), and the new rent (the current rent plus the net increase).

We measured likelihood of renewing their lease by asking “How likely is it that you would renew the lease?” (1 = “not likely at all,” and 7 = “very likely”). We then elicited the expected price with the question “What was your expectation of your new monthly rent before you saw the actual amount?” (1 = “much lower than the current rent,” 4 = “same as the current rent,” and 7 = “much higher than the current rent”). Next, we asked participants to allocate 100 points to different attributes people consider when renewing their lease—location, relationship with landlord, relationship with management, property condition, terms of the lease, and rent amount—based on importance. We adopted this paradigm from Rahinel and Ahluwalia (2015). The number of points allocated to rent is a proxy of price importance in the context of lease

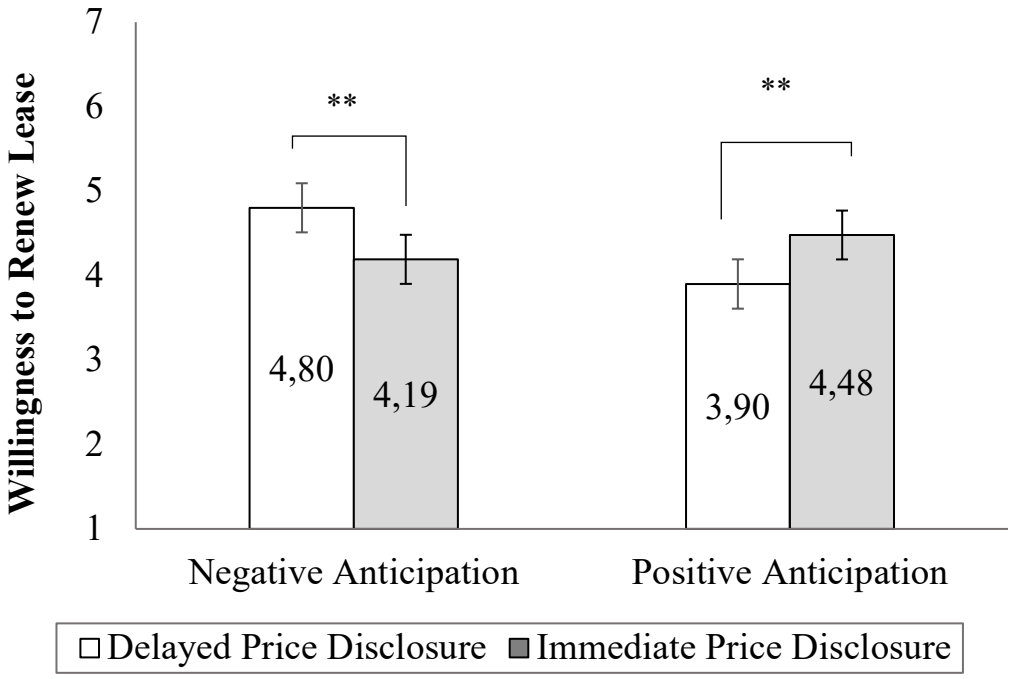
renewal. Finally, we administered an attention check and collected gender and age. As preregistered, the baseline acceptable rent was used as covariate in all analyses. Web Appendix E provides the full materials.

Results

Likelihood of Renewing the Lease. A 2 (price expectation) × 2 (price disclosure) ANOVA with baseline rent as a covariate revealed a main effect of price expectation ($F(1, 395) = 4.10$, $p = .043$) and no main effect of the timing of price disclosure ($F(1, 395) = .01$, $p = .922$). More important to our predictions, there was a significant interaction ($F(1, 395) = 16.75$, $p < .001$, $\eta_p^2 = .041$; see figure 5). Under pessimistic price expectations, participants were more likely to renew the lease in the DPD condition ($M = 4.80$, $SD = 1.40$) than in the IPD condition ($M = 4.19$, $SD = 1.46$; $F(1, 395) = 8.79$, $p = .003$, $\eta_p^2 = .022$). Under optimistic price expectations, however, participants were less likely to renew the lease in the DPD condition ($M = 3.90$, $SD = 1.61$) than in the IPD condition ($M = 4.48$, $SD = 1.44$; $F(1, 395) = 7.98$, $p = .005$, $\eta_p^2 = .020$). In this and subsequent analyses, the results are similar, and significant, if we remove the baseline rent covariate.

FIGURE 5

STUDY 3 – LIKELIHOOD OF RENEWING AN APARTMENT LEASE



NOTE.— Error bars = +/− 1 SE. †p < .10. *p < .05. **p < .01

Mediation by Price Expectations. A 2 (price expectation) × 2 (price disclosure) ANOVA revealed a main effect of price expectation ($F(1, 395) = 48.80$, $p < .001$) and a main effect of price disclosure ($F(1, 395 = 7.07$, $p = .008$) on the expected price. More important to our predictions, there was a significant interaction ($F(1, 395) = 34.82$, $p < .001$, $\eta_p^2 = .081$). In the pessimistic price expectation condition, participants reported a higher expected price in the DPD condition ($M = 5.23$, $SD = 1.13$) than in the IPD condition ($M = 4.80$, $SD = 1.02$; $F(1, 395) = 5.27$, $p = .022$, $\eta_p^2 = .013$). In the optimistic price expectation condition, however, participants reported a lower expected price in the DPD condition ($M = 3.53$, $SD = 1.70$) than in the IPD condition ($M = 4.66$, $SD = 1.31$; $F(1, 395) = 36.64$, $p < .001$, $\eta_p^2 = .085$).

We conducted a moderated mediation analysis using PROCESS Model 8 (Hayes 2022) with price disclosure as the independent variable (0 = IPD, 1 = DPD), price expectation as the moderator, expected price as the mediator, and the likelihood of renewing the lease as the

dependent variable (5000 bootstrapped samples). The index of moderated mediation was significant (*Index* = .46; 95% CI: [.21 to .75]). In the pessimistic price expectation condition, the pathway to the likelihood to renew the lease through expected price was positive and significant (β = .13, *SE* = .06, 95% CI: [.03 to .25]). In the optimistic price expectation condition, the pathway to the likelihood to renew the lease through expected price was negative and significant (β = -.33, *SE* = .10, 95% CI: [-.55 to -.15]).

Price Importance. There were no main effects or interactions on the price importance measures (*F*'s < 1).

Discussion

Supporting H1a and H1b, study 3 demonstrated that, relative to immediate price disclosure, delayed price disclosure increased (decreased) the likelihood of "purchase" (in this case, the renewal of a rental agreement at a higher amount) when the context promoted price pessimism (optimism). This response was driven by a change in the expected price.

STUDY 4: ISOLATING THE EFFECTS OF PRICE IMPORTANCE

Study 4 (preregistration at https://aspredicted.org/CG5_R7X) was designed to test H2, namely, that the impact of delayed price disclosure on sales is a function of the shopping goal pursued by consumers. This is the case because delayed price disclosure and shopping goal interact to shift the importance of price relative to product benefits in a purchase decision. We expected a positive effect of delayed price disclosure on product evaluations when participants had a purchase goal (H2a) and a negative effect of delayed price disclosure on product evaluations when participants had a browsing goal (H2b).

Method

Participants and Design. We recruited 401 U.S. participants from Prolific in exchange for a nominal payment (49.6% female; 19 to 81 years, $M = 38.71$, $SD = 13.57$). This experiment adopted a 2 (price disclosure: immediate price disclosure (IPD) vs. delayed price disclosure (DPD)) × 2 (shopping goal: purchase vs. browsing) between-subjects design.

Procedure. We explained to participants we were interested in consumers' personal preferences and opinions. Participants in the purchase goal condition read: "Imagine your keyboard has just broken, and that you need to purchase a new keyboard. Because you use it for everyday work, you need it as soon as possible. On the next page, you will see different keyboards. We want you to look at these options and decide which one you would buy." Participants in the browsing goal condition read: "Imagine you will move to a new place several months from now, and you are considering whether to buy a new keyboard to use in the future. On the next page, you will see different keyboards. We want you to explore these options and indicate which one you would consider buying a few months from now."

Then, we showed participants six ergonomic computer keyboards, in a randomized display, and asked them to place their choice in the shopping cart. In addition to a product picture, the keyboards were described based on the following attributes: brand, wireless (yes or no), palm-rest (yes or no), Amazon rating (1 to 5), numeric keypad (yes or no), and price. The attributes were negatively correlated, such that choosing between one option over the others involved making tradeoffs between attributes. Appendix A shows the product display without prices plus a view after a product was selected.

Participants in the IPD condition could see the price for all options upfront. Participants in the DPD condition read "Add to cart to see price" instead of seeing the price. Once participants selected their preferred option, they clicked "next" to proceed to the shopping cart, which displayed their chosen option. At this point, participants in the IPD condition saw their selected option again and a subtotal highlighting the price. However, those in the DPD condition saw the price and respective subtotal for the first time.

Next, as the dependent variable, we asked: "Please provide your overall evaluation for the keyboard you selected" (1 = "very bad" and 9 = "very good"; 1 = "very negative" and 9 = "very positive"; 1 = "very unfavorable" and 9 = "very favorable") (α = .97). We measured the proposed mediator—price importance—using a paradigm adapted from Rahinel and Ahluwalia (2015). Specifically, we asked participants to allocate 100 points across the ergonomic keyboard attributes (design, brand, wireless connection, palm-rest, customer-reviews, numeric keypad, price) based on the importance weight they gave to each in their evaluation. Next, to assess participants' expected prices, we asked participants: "What is the maximum (and minimum) amount you think an ergonomic keyboard equal or similar to the one you chose should cost?" We used this measure instead of a single expected price measure (as in our previous studies) to gather participants' range of expected prices rather than a single price. Finally, we measured demographics. See Web Appendix F for full materials.

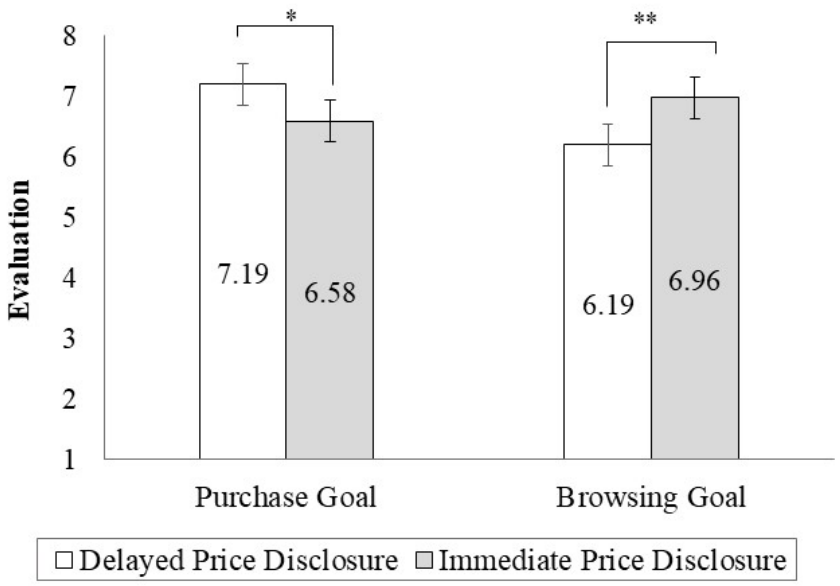
Results

Evaluation. A 2 (shopping goal) × 2 (price disclosure) ANOVA on product evaluation revealed a marginally significant main effect of shopping goal ($F(1, 397) = 3.09$, $p = .080$) and no main effect of price disclosure ($F(1, 397) = .224$, $p = .636$). More important to our

predictions, there was a significant interaction ($F(1, 397) = 15.57$, $p < .001$, $\eta_p^2 = .038$; see figure 6). In the purchase goal condition, participants evaluated the keyboard more positively in the DPD condition ($M = 7.19$, $SD = 1.53$) than in the IPD condition ($M = 6.58$, $SD = 1.71$; $F(1, 397) = 5.99$, $p = .015$, $\eta_p^2 = .015$). In the browsing goal condition, however, participants evaluated the keyboard more negatively in the DPD condition ($M = 6.19$, $SD = 2.05$) than in the IPD condition ($M = 6.96$, $SD = 1.71$; $F(1, 397) = 9.84$, $p = .002$, $\eta_p^2 = .024$).

FIGURE 6

STUDY 4 – EVALUATION OF A KEYBOARD



NOTE.— Error bars = +/− 1 SE. †p < .10. *p < .05. **p < .01

Mediation by Price Importance. A 2 (shopping goal) × 2 (price disclosure) ANOVA on price importance revealed no main effects of shopping goal ($F < 1$) or price disclosure ($F < 1$). More important to our predictions, there was a significant interaction ($F(1, 397) = 13.48$, $p < .001$, $\eta_p^2 = .033$). In the purchase goal condition, participants rated the price as less important in the DPD condition ($M = 16.49$, $SD = 18.90$) than in the IPD condition ($M = 23.77$, $SD = 22.85$;

$F(1, 397) = 6.22$, $p = .013$, $\eta_p^2 = .015$). In the browsing goal condition, however, participants rated the price as more important in the DPD condition ($M = 22.56$, $SD = 25.18$) than in the IPD condition ($M = 14.75$, $SD = 13.64$; $F(1, 397) = 7.28$, $p = .007$, $\eta_p^2 = .018$).

We conducted a moderated mediation analysis using PROCESS Model 8 (Hayes 2022) with price disclosure as the independent variable (0 = IPD, 1 = DPD), goal as the moderator, price importance as the mediator, and product evaluation the dependent variable (5000 bootstrapped samples). The index of moderated mediation was significant (*Index* = -.76; 95% CI: [-1.23 to -.34]). In the purchase goal condition, the pathway to evaluation through price importance was positive and significant (β = .37, *SE* = .15, 95% CI: [.07 to .67]). In the browsing goal condition, the pathway to evaluation through price importance was negative and significant (β = -.39, *SE* = .15, 95% CI: [-.71 to -.12]).

Price Expectations. There were no main effects or interactions on the expected price measures (minimum, maximum, and midpoint) (*F'*s < 1).

Discussion

Study 4 demonstrated that, relative to immediate price disclosure, delayed price disclosure increased (decreased) product evaluations when a purchase (browsing) goal was active. This effect was driven by a decrease (increase) in the relative importance of price (H2a, H2b).

EVIDENCE FROM THREE FIELD EXPERIMENTS

It was important to us to contribute real-world empirical evidence to support the findings of studies 1 through 4. To this end, we collaborated with a leading retailer of household

appliances and consumer electronics to conduct large-scale field experiments in settings that, in our view, match the cells displayed in figure 1.

We agreed with the retailer to conduct three experiments. In each experiment, we manipulated the moment when shoppers first encountered prices for some or all of the products in a selection of categories, where the average price in U.S. Dollars ($) ranged approximately from $25 to $400. In the immediate price disclosure conditions, we followed the retailer's convention of posting prices upfront, right alongside information about the corresponding products. In the delayed price disclosure conditions, we posted prices at the next logical step in the purchase process: when consumers clicked on a product displayed on the web page, clicked on a product displayed in the email, or approached a salesperson.

Running these field experiments involved nontrivial operational challenges. For example, for study 5 the retailer had to modify the backend code of the website such that, in the delayed disclosure condition, products in the category page did not appear with prices. Similarly, for study 7 the retailer had to redesign and place new price tags for many products. Moreover, we followed a cross-over design that implied rotating price tags depending on the store and week. While we measured performance primarily in terms of sales, and this metric is important to the retailer, we are mindful that the collaboration was costly to the company in terms of time and resources. In fact, the experiments exposed the retailer not only to complex logistics, but also to the opportunity cost associated with manipulating the purchase setting of dozens of products for close to 800,000 prospective customers.

Study 5: A Field Setting with Price Pessimism and a Purchase Goal

A field study was conducted in an online retail store. The website was a department store and did not display any sale or deal signage. In the absence of any information suggesting a deal, price pessimism is the default expectation (i.e., it is consistent with prior experience; Grewal and Baker 1994; Koschmann and Isaac 2018). In addition, shoppers visited the site of their own volition, suggesting that most shoppers had a purchase goal. Thus, the field setting was one where consumers could form pessimistic price expectations and had a purchase goal, both of which we expected to lead to a positive effect of delayed price disclosure on sales (H1a and H2a, respectively).

Method. We conducted this experiment at the retailer's virtual store. The product categories were electric kettles and espresso machines. The average number of products per category was 23, and the average price ranged approximately from $25 to $150. Each web page presented four to eight products of similar quality. The two conditions were immediate price disclosure (IPD), with prices displayed alongside the products, and delayed price disclosure (DPD), with prices displayed only after a product was clicked on.

A JavaScript HTML code, integrated with Google Analytics, randomly assigned shoppers to the IPD or DPD condition based on web-traffic splitting. The key dependent measures were a binary daily purchase indicator, daily units sold (log transformed), and daily revenue (log transformed). The binary daily purchase indicator was set at 1 if a purchase took place in either category on date *t*, or at 0 otherwise. The daily units sold measure is the total number of units purchased across the two product categories on a day, and daily revenue represents sales (in $) across categories on a day. We also collected the bounce rate, defined as the percentage of sessions ending after a single page view with no clicks. A high bounce rate is undesirable in e-commerce, as few purchase decisions are made after a single page view.

The experiment lasted 63 days for a total of 53,938 site visits. The average number of distinct daily shoppers who looked at either product category was 286 in the IPD condition and 298 in the DPD condition. Thus, approximately 17,146 customers visited the site more than once in a day (53,938 - ((286+298) × 63) = 17,146). Repeated visits may occur, for example, when consumers are using multiple retailers to build a consideration set, are checking prices at multiple retailers, or are undecided and visit the online store multiple times. In anticipation of these repeated visits, shoppers who closed the web browser but returned at a later point in time were assigned to the same condition. For example, a consumer assigned to the DPD condition faced the same condition on any subsequent visit across the duration of the experiment.

Results. We estimate the model $DM_{i,t} = \alpha + \beta Price + \delta_t + \varepsilon_{i,t}$ for each dependent measure, where $DM_{i,t}$ denotes the dependent measure in experimental condition i on day t, *Price* is an indicator variable equal to 1 in the DPD condition and 0 in the IPD condition, and δ_t denotes time fixed effects.

As anticipated, the data show that DPD increased the daily probability of purchase (β = .127, SE = .048, $t(62) = 2.65$, $p = .010$) by 13%, increased the daily units sold (β = .160, SE = .075, $t(62) = 2.13$, $p = .037$) by 17.4% (exp(0.16) – 1 = .174), and increased daily revenue (β = .426, SE = .245, $t(62) = 1.76$, $p = .084$) by 53.7% (exp(0.426) – 1 = .537). Table 1 summarizes these results.[3]

[3] There were 126 observations in the data (2 conditions × 63 days). There were 62 control variables for days, one degree of freedom (df) for the constant, and one df for the treatment, which resulted in 62 df for tests (126 - 62 - 1 – 1 = 62). Applying OLS to a binary outcome is a Linear Probability Model (LPM). A LPM has implementation and interpretation advantages that make it more appealing than logistic regression. In LPM, parameters represent mean marginal effects that are easier to interpret than log odds ratios. LPM and logistic regression usually yield similar effects (i.e., the impact of violating assumptions about the distribution of the dependent measure is

We worried that DPD might be perceived as novel and more interesting than IPD, and that this altered how people shopped. However, we also observe that the daily bounce rate is higher in the DPD condition than in the IPD condition ($\beta = 2.942$, $SE = .516$, $t(62) = 5.70$, $p < .001$). Notice that this reduces, rather than increases, the likelihood a sale would be made in the DPD condition (because the bounce rate is higher), so this bias makes a test of the hypothesis more conservative.

TABLE 1: STUDY 5, SUMMARY OF FINDINGS

	Probability Daily Purchase		Log Daily Units		Log Daily Revenue	
Delay Prices	.127	**	.160	**	.426	*
	(.048)		(.075)		(.245)	
Constant	.825	***	1.205	***	3.506	***
	(.039)		(.053)		(.173)	
Observations	126		126		126	

Note: Standard errors in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

Study 6. A Field Setting with Price Optimism and a Browsing Goal

A field study was conducted using an e-mailed daily sales flyer. The first word of the e-mail subject line read "DEALS." Thus, the setting provided information that promoted price optimism and, because the retailer proactively reached out to potential customers, invited people who might not normally be shopping for a product to investigate purchase opportunities, both of which we expected to lead to a negative effect of delayed price disclosure on sales (H1b and H2b, respectively).

Method. In this experiment, the retailer e-mailed a sales flyer on seven consecutive days

minimal), so econometric researchers prefer LPM for estimating treatment effects (Deke 2014). In our case, we find similar results in a logistic regression model.

to over 700,000 current and prospective customers—a random subset of their total customer base. The company considered the sales flyer a powerful tool for promoting products, encouraging engagement in social media, and generating future sales. The communication itself featured eight to 12 unique consumer electronics and household appliances each day (electric kettles, video consoles, headphones, microwaves, blenders, speakers, etc.), all offered at a discount.

The retailer's database included information about the number of items consumers clicked on in the seven e-mails. (Note that, after an initial click on an item, consumers could return to the e-mail and click on additional items or click items from multiple e-mails.) Given that the e-mail was an invitation to browse, the number of items clicked can be used as an indicator of the strength of the browsing goal. In particular, evidence for H2b should be stronger for consumers who clicked on more items in the e-mail.

A total of 771,583 people participated in the experiment. Email recipients were randomly assigned to the immediate price disclosure (IPD) or delayed price disclosure (DPD) condition. We manipulated the content of the sales flyer such that consumers in the IPD condition received the regular daily sales flyer, which featured approximately 10 products from different categories and their corresponding (discounted) prices. In contrast, consumers in the DPD condition received the same daily sales flyer but without prices. Prices were displayed when consumers clicked on the corresponding content, which led to the product page in the online store.

Similar to study 5, we then measured the following key dependent variables: a binary daily purchase indicator, daily units sold (log-transformed), and daily revenue (log-transformed). We also measured the bounce rate. Each of these variables was aggregated at the individual level for the entire seven-day period. The binary weekly purchase indicator was set at 1 if the user

made a purchase during the seven-day period. In addition, the data allowed us to record whether an e-mail recipient made a credit transaction at one of the retailer's physical stores (rather than the online store). Unfortunately, we could not match cash transactions. Finally, the moderator measured the number of products each consumer clicked on as a proxy of the strength of the browsing goal.

Results. We estimate the model $DM_i = \alpha + \beta Prices + \varepsilon_i$, where DM_i denotes the dependent measure of consumer *i*, and *Prices* is an indicator variable that takes value 1 in the DPD condition and 0 in the IPD condition. The analysis is summarized in table 2. Consistent with H1b and H2b, the data show that delayed price disclosure decreased purchases by 0.3% (β = -.297, *SE* = .035, *t*(771,580) = -8.43, *p* = .001), decreased the number of units sold by 0.2% (β = -.002, *SE* = .001, *t*(771,580) = -8.54, *p* = .001; (exp(-.002) - 1 = -.002), and decreased revenue by 1.7% (β = -.017, *SE* = .002, *t*(771,580) = -9.11, *p* = .001; (exp(-.017) - 1 = -.017). Additionally, delayed price disclosure increased the individual-level bounce rate by 2% (β = 2.022, *SE* = .114, *t*(771,580) =17.81, *p* = .001).

TABLE 2: STUDY 6, SUMMARY OF FINDINGS

	Probability Weekly Purchase		Per-User Log Weekly Units		Per-User Log Weekly Revenue	
Delay Prices	-.297	***	-.002	**	-.017	***
	(.035)		(.000)		(.002)	
Constant	2.608	***	.019	***	.136	***
	(.026)		(.000)		(.001)	
Observations	771,583		771,583		771,583	

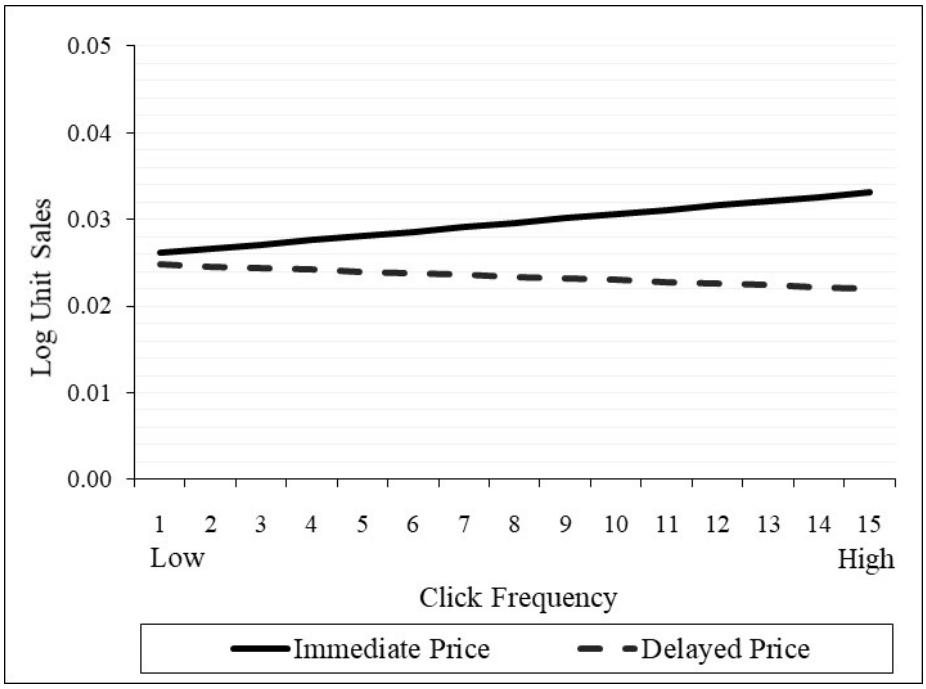
NOTE: Standard errors in parentheses. *** $p < .01$, ** $p < .05$, * $p < .10$.

H2b predicts that, when a browsing goal is active, DPD increases the relative importance of price relative to product benefits and, therefore, makes a purchase less appealing. In addition, if we assume that the number of products consumers clicked on is a proxy for browsing

motivation, then the effect should be stronger for consumers who clicked on more products during the seven days of the experiment. To test this idea, we conducted a *Prices* (DPD vs. IPD) × click frequency (continuous) regression analysis with log unit sales as the dependent measure. Consumers clicked on 2.87 products on average (*SD* = 4.46). In support of H2b, we find a significant interaction between *Prices* (DPD vs. IPD) and click frequency (ß = -.0007, *SE* = .0001, $t(771{,}579) = 8.23$, $p < .001$). To decompose this interaction, we conducted a floodlight analysis using PROCESS Model 1 (Hayes 2022; Spiller et al. 2013). This analysis, depicted in figure 7, reveals a Johnson-Neyman difference point at 1 (effect = -.002, *SE* = .0004, $z = -4.66$, $p < .001$).

FIGURE 7

STUDY 6: THE EFFECT OF DELAYED PRICE DISCLOSURE ON SALES IS MODERATED BY CLICK FREQUENCY



Study 7: A Field Setting to Contrast Price Pessimism and Price Optimism

A field study was conducted in the physical retail stores of the partner company. The company's stores cater to shoppers who have purchase goals and browsing goals (i.e., a mix of the two types of shoppers). Immediate versus delayed price disclosure varied for high-priced products, low-priced products, or all products. We expected delayed price disclosure for high-priced (low-priced) products would allow for price pessimism (optimism) to update expected prices and increase (decrease) sales. No prediction was made in the "all products" condition.

Method. For the last field experiment, we worked with the retailer's physical stores. The experiment focused on four product categories with significant existing sales: stoves, refrigerators, stereo speakers, and electric kettles. The average number of SKUs per category was 63, and prices ranged from $30 to $400.

The intent of the experiment was to investigate the influence of price expectations while collapsing across shopping goals. Presumably, the retailer's physical stores cater to shoppers who pursue a specific purchase objective (e.g., those who visit the store with products in mind) or are simply browsing (e.g., those who are "looking around"), making the sample a mix of shopping goals. In turn, we manipulated price disclosure to include one immediate price disclosure (IPD) condition and three delayed price disclosure (DPD) conditions.

In physical stores, a relatively simple way to impose a delay is to turn price tags upside down or prepare special tags that require shoppers to peel off or fold away a cover. However, this solution relies on the sales staff to constantly check and ready the tags, which can lead to errors. Instead, we agreed with the company to print and place the normal tags, but without prices on them. In turn, prospective customers could only access prices if they approached one of

the store employees. Management ensured that sufficient staff was on hand to control the tags and respond to inquiries.

Specifically, in the IPD condition, the retailer displayed prices next to the product. In the DPD-HIGH condition, the retailer placed tags without prices on products sold above the median price of the respective category. In the DPD-LOW condition, the retailer placed tags without prices on products sold below the median price of the respective category. Finally, in the DPD-ALL condition, the retailer placed tags without prices on all products. We added the DPD-HIGH and DPD-LOW conditions to our experimental design to evoke price pessimism and price optimism, respectively.

The experiment was conducted in 30 stores that offered similar assortments and recorded similar unit sales and revenue in the previous two months. These stores also had equivalent median daily store-category revenue prior to the study—the difference was only \$9.70 ($t(30) = .507$, $p = .616$). No pair of stores was within a 45-minute walking distance of each other.

Table 3 displays the allocation of the stores to conditions: 21 to the IPD condition and nine to one of the DPD conditions. The experiment spanned three time periods (*t1, t2, t3*), each lasting one week. To account for possible time-fixed and store-fixed effects, we adopted a cross-over design (Hainmueller, Hiscox, and Sequeira 2015; Jones and Kenward 2015) in which stores S1, S2, S4, S5, S7, and S8 rotated through a DPD condition after each period, stores S3, S6, and S9 remained in one DPD condition throughout the experiment, and stores S10-S30 remained in the IPD condition throughout the experiment. As in the other field experiments, our dependent measures were a purchase indicator (0 = no, 1 = yes), unit sales, and revenue on each day at a given store in the target product categories. We also collected these measures (with the same

frequency and at the same stores) for five additional product categories. This resulted in 18,630 observations (30 stores × 69 days × 9 categories).

TABLE 3: STUDY 7, ALLOCATION OF STORES TO CONDITIONS

	t_1	t_2	t_3
Delayed High Price Disclosure	S_1, S_2, S_3	S_4, S_5, S_3	S_7, S_8, S_3
Delayed Low Price Disclosure	S_4, S_5, S_6	S_7, S_8, S_6	S_1, S_2, S_6
Delayed All Price Disclosure	S_7, S_8, S_9	S_1, S_2, S_9	S_4, S_5, S_9
Immediate Price Disclosure	S_{10}, … S_{30}	S_{10}, …, S_{30}	S_{10}, …, S_{30}

We have two specific predictions. The first prediction is that delaying the disclosure of high prices evoked price pessimism, and this in turn prompted more sales and higher revenue for the corresponding products than in the IPD condition (H1a). This prediction is consistent with the notion that high-quality products tend to have unexpectedly high prices (Chang, Siddarth, and Weinberg 1999; Winer 1986). This intuition was shared by management, who felt that getting shoppers to request prices would be most beneficial for expensive items because it mitigates sticker shock.

Our second prediction is that delaying the disclosure of low prices evoked price optimism, and this in turn prompted less sales and lower revenue for the corresponding products than in the IPD condition (H1b). This prediction is consistent with the belief that cheap products offer the best deals (e.g., loss leaders), and that companies sometimes hide these prices because they are too favorable. We do not formulate a prediction for the DPD-ALL condition because it is difficult to anticipate whether price pessimism or price optimism would dominate.

Results. We estimate separate fixed-effects models for high-priced products, low-priced products, and all products. These three models are estimated with controls for shocks associated with time and store × product category (analogous to Fisher et al. 2018; Hainmueller, Hiscox,

and Sequeira 2015). Specifically: $DMs,c,t = \alpha 0 + \beta_1 Highs,c,t + \beta_2 Lows,c,t + \beta_3 Alls,c,t + \delta t + \delta s,c + Es,c,t$, where $DM_{s,c,t}$ denotes the dependent measure of store s in product category c on day t, and δ_t and $\delta_{s,c}$ are fixed effects for time and store × category, respectively. We estimate fixed-effects models for the following dependent measures at the store-category-date level: a binary purchase indicator (see Web Appendix G), log-transformed total units sold, and log-transformed total revenue.

We are interested primarily in β_1 and β_2—the effect of delaying the disclosure of high or low prices (table 4). In the case of high prices, delayed disclosure increased unit sales and revenue of the corresponding products by 3.9% ($\beta_1 = .038$, SE = .020, $t(18,288) = 1.88$, $p = .071$; exp(.038) -1 = .039) and 31.1% ($\beta_1 = .271$, SE = .126, $t(18,288) = 2.15$, $p = .040$; exp(.271) -1 = .311), respectively, relative to the (baseline) IPD condition.[4] These results support H1a. On the other hand, delaying the disclosure of low prices reduced unit sales and revenue of the corresponding products by 5.2% ($\beta_2 = -.053$, SE = .026, $t(18,288) = -2.04$, $p = .043$; exp(-.053) – 1 = -.052) and 28.7% ($\beta_2 = -.338$, SE = .158, $t(18,288) = -2.14$, $p = .033$; exp(-.338) – 1 = -.287), respectively, relative to the IPD condition. These results support H1b. Finally, an exploratory analysis shows that delaying the disclosure of all prices increased unit sales and revenue by 3.9% ($\beta_3 = .038$, SE = .021, $t(18288) = 1.82$, $p = .069$; exp(.038) – 1 = .039) and by 36.2% ($\beta_3 = .309$, SE = .114, $t(18288) = 2.72$, $p = .001$; exp(.309) – 1 = .362), respectively.

TABLE 4: STUDY 7, SUMMARY OF FINDINGS

[4] There are 18,630 observations in the data set. There are 68 control variables for day, 270 control variables for store × category, 3 independent measures, and one df for the constant (18,630 - 68 - 270 - 3 - 1 = 18,288). The data obtained covered two months prior to the experiment plus three experiment weeks.

	Category of Products											
	High-priced Products				Low-priced Products				All Products			
	Log Unit-sales		Log Revenue		Log Unit-sales		Log Revenue		Log Unit-sales		Log Revenue	
Delayed High Price Disclosure	0.038	*	0.271	**	0.025		0.149		0.053	*	0.256	**
	(.020)		(.126)		(.030)		(.170)		(.028)		(.095)	
Delayed Low Price Disclosure	0.016		0.048		-0.053	**	-0.338	**	-0.019		-0.028	
	(.043)		(.285)		(.026)		(.158)		(.045)		(.220)	
Delayed All Price Disclosure	0.072	***	0.469	***	-0.028		-0.073		0.038	*	0.309	**
	(.024)		(.161)		(.030)		(.146)		(.021)		(.114)	
Constant	0.359	***	2.256	***	0.239	***	1.858	***	0.559	***	3.171	**
	(.001)		(.004)		(.001)		(.003)		(.001)		(.003)	
Observations	18,630		18,630		18,630		18,630		18,630		18,630	
R-squared	0.218		0.205		0.226		0.238		0.324		0.318	

NOTE: Standard errors in parentheses. $*** = p < 0.01$, $** = p < 0.05$, $* = p < 0.10$.

Discussion. Study 7 provides evidence consistent with the hypothesis that delayed price disclosure increases or decreases unit sales and revenue as a function of price expectation. When we withheld the prices of expensive products, unit sales and revenue increased. We believe that this effect occurred because of price pessimism. Conversely, when we withheld the prices of inexpensive products, unit sales and revenue decreased. We believe that this effect occurred because of price optimism.

It is important to note that the design of this experiment helped to control for potential threats to internal validity. First, a missing price in a retail context is somewhat unusual. This may have increased customer engagement. However, if this is true, then sales and revenue should have increased regardless of what prices were initially hidden. Second, asking a salesperson to provide price information could increase commitment toward a purchase. Again, if this is true, then sales and revenue should have increased across the board. Third, seeking price information from the sales staff creates an opportunity for interpersonal interaction. Interpersonal interactions allow salespeople to provide additional information about products and to use social pressure to close the sale. Yet again, if this is true, then we should have observed a uniform

increase in sales and revenue. Overall, then, despite having limited control over internal validity, the experimental design helped to rule out several alternative hypotheses.

GENERAL DISCUSSION

Many retailers hold back prices from shoppers thinking that this simple tactic can boost sales. While this form of price obfuscation has a long history, businesspeople still debate its effectiveness and academic research provides surprisingly little guidance in sorting through the different intuitions, both in favor and against.

Using online and field experiments, we find that delayed price disclosure can increase or decrease sales. Thus, it appears that both sides of the debate have merit. Importantly, we identified two reasons for these effects. First, delayed price disclosure prompts consumers to use information about the product and purchase context to update their beliefs about whether a price is likely to be high or low, resulting in different price expectations (and, therefore, different purchase intentions) once the actual price is revealed. Second, delayed price disclosure encourages consumers to place more or less weight on this feature depending on whether they hold a browsing or purchase goal, respectively. Again, the change in relative importance of price carries through to purchase decisions.

Contributions to Theory and Practice

Our research makes at least two contributions to theory. First, we extend research on price obfuscation in economics and marketing. This literature focuses almost exclusively on companies that attempt to muddle price discovery by posting a low price for a base good and shrouding separate charges for add-ons, additional services, taxes, and so on. In this setting, the

idea that a portion of consumers chooses to save on search costs or makes a computational error is reasonable. In comparison, we study the simpler, more general case of a company that decides to shroud the entire, single price of a single product—a practice common across physical, catalog, and now online retail. When consumers are temporarily blocked from seeing the whole price of a product, the search costs and computation error arguments make less sense.

Second, information integration theory is a well-established paradigm for understanding how consumers judge products and services (Anderson 1981; Lynch 1985). Much of the attention here has gone into understanding when and how contextual factors affect perceptions of value and information integration. For example, anchors, reference values, and attribute ranges have been shown to influence perceptions of attribute values (Janiszewski and Lichtenstein 1999; Kalyanaram and Winer 1995; Simonson and Drolet 2004), while attribute valence, accessibility, and alignability have been shown to influence information integration weights (Baumeister et al. 2001; Markman and Medin 1995; Sanbonmatsu et al. 2003). Our contribution differs from prior research in that we identify a phenomenon, the delayed disclosure of a price, that affects the valuation and integration stages in the classic information integration model. Conceptualizing this dual effect allows us to anticipate when the delayed price disclosure increases, decreases, or has no influence on sales (see figure 1).

With respect to practice, we see several additional contributions. Our most direct contribution is to the enduring debate about whether delayed price disclosure is a benefit or a hindrance. A recent example is the spirited discussion among software vendors on the merits of posting prices on product webpages, with many encouraging business owners to show prices upfront and "own" the price conversation, and an equal number advising to hold back prices so that prospective customers can first internalize the value of the product. Ultimately, there are lots

of opinions and intuitions in the public domain, but little solid evidence. We add empirical data to the discussion. We show that delayed price disclosure can have both effects. Importantly, we have a logic for why this is the case.

Next, the results of our experiments provide an opportunity for companies using delayed price disclosure to check whether this decision is sound. A key factor is the type of information that influences price expectations. Generally speaking, price optimism is likely to prevail when consumers perceive excess product, a negotiating advantage, or competing products that are attractively priced. Conversely, price pessimism is likely to prevail when consumers perceive scarcity, a lack of bargaining power, or competing products that are priced at a premium. Moreover, there are cases of companies holding back prices because they are "too good to be true" or "below manufacturer-approved prices." This tactic is unlikely to be detrimental when consumers have extensive price knowledge. However, it could easily backfire when consumers lower their expected price based on these contextual cues. Similarly, companies that shroud prices in promotional flyers may come to regret it if consumers become optimistic and are then disappointed once the actual price is revealed. Overall, situations that create price optimism make the delayed price disclosure a poor pricing tactic.

Finally, our research speaks to the broader question in management about the impact of friction in consumer experiences. The common advice is to make purchase journeys frictionless. This is particularly true when the source of friction is prices and payments. But our experiments clearly show that not all friction is bad all of the time. At first, it may seem peculiar to think that a company can benefit from making things more complicated for customers, but it is exactly what delayed price disclosure does. In fact, one opportunity for companies is to explore a more "adaptive" form of price disclosure, one where the timing of price presentation may be

personalized (perhaps by machine-learning algorithms) according to the estimated price sensitivity of individual shoppers. Such an approach could strike a balance between enhancing the shopping experience and achieving sales outcomes—a friction "sweet spot," if you will (Bertini, Aparicio, and Aydinli, forthcoming).

Limitations and Future Research

Our research focuses on consumer reactions to delayed price disclosure. We believe this is an important starting point, especially considering the lack of empirical evidence on the phenomenon. However, we do this at a general level. That is, we do not consider heterogeneity among consumers, which may allow a company to use the timing of price disclosure as a tool for discrimination. To the extent that companies can tailor prices to individual shoppers, delayed price disclosure may help companies learn something about consumer valuations and serve the "right" price.

Second, we do not consider the cost implications of different disclosure decisions. One reasonable argument is that, relative to delayed disclosure, immediate disclosure "weeds out" bargain shoppers and reduces a company's cost of acquisition. If the key metric were profit, then this additional argument matters and should be incorporated.

Third, our experiments adopt a delay that we think is meaningful to consumers, but conservative given that it extends only to the next logical step in the purchase process. Harsher delays are certainly possible, all the way to the moment of payment. Varying the length of the delay is an interesting extension of our work, one that would allow researchers to contrast the possibility of an escalation of commitment with the risk of making the shopping experience somewhat artificial.

Finally, it would also be interesting to extend our research to consider competition. If prices are disclosed not only to consumers, but also to competitors, then is it better for a retailer to hide them from the more "public" places of the purchase process (a store shelf, a landing page, etc.)? The answer to this question probably depends on several factors, including the underlying pricing policy of the retailer and its competitor. For instance, a company with a policy of matching the lowest price might benefit from posting prices early on because consumers who price-compare will better realize which is the cheapest alternative.

Appendix A: Study 4, Illustration of the Delayed Price Disclosure Condition

Price delay condition

Ergonomic Keyboards



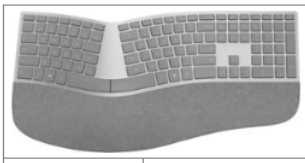
Brand	Logitech
Wireless	Yes
Palm-Rest	Yes
Amazon Rating	4.0/5
Numeric Keypad	Yes
Price	Add to cart to see price



Brand	Microsoft
Wireless	No
Palm-Rest	Yes
Amazon Rating	3.2/5
Numeric Keypad	Yes
Price	Add to cart to see price



Brand	Microsoft
Wireless	Yes
Palm-Rest	Yes
Amazon Rating	4.5/5
Numeric Keypad	No
Price	Add to cart to see price



Brand	Microsoft
Wireless	Yes
Palm-Rest	Yes
Amazon Rating	4.5/5
Numeric Keypad	Yes
Price	Add to cart to see price



Brand	Cloud Nine
Wireless	No
Palm-Rest	Yes
Amazon Rating	4.3/5
Numeric Keypad	Yes
Price	Add to cart to see price



Brand	Microsoft
Wireless	Yes
Palm-Rest	Yes
Amazon Rating	4.0/5
Numeric Keypad	Yes
Price	Add to cart to see price

View after clicking on a product in the delayed price disclosure condition (in this example, the clicked keyboard was the first one):



Brand	Logitech
Wireless	Yes
Palm-Rest	Yes
Amazon Rating	4.0/5
Numeric Keypad	Yes
Price	$89.99

SUBTOTAL (1 ITEM): $89.99

REFERENCES

Anderson, Norman H. (1981), *Foundations of Information Integration Theory*, New York, NY: Academic Press.

Baumgartner, Hans, and Rik Pieters (2008), "Goal-directed Consumer Behavior: Motivation, Volition, and Affect," in *Handbook of Consumer Psychology*, eds. Curt P. Haugtvedt, Paul M. Herr, and Frank R. Kardes, New York, NY: Routledge, 367–92.

Baumeister, Roy F., Ellen Bratslavsky, Catrin Finkenauer, and Kathleen D. Vohs (2001), "Bad is Stronger than Good," *Review of General Psychology*, 5 (4), 323–70.

Bertini, Marco, Diego Aparicio, and Aylin Aydinli (forthcoming), "Can Your Customer Experience Benefit from Some Friction?" *MIT Sloan Management Review*.

Bettman, James R., Capon, Noel, and Lutz, Richard J. (1975). Cognitive Algebra in Multi-Attribute Attitude Models, *Journal of Marketing Research*, 12 (2), 151–64.

Blake, Tom, Sarah Moshary, Kane Sweeney, and Steve Tadelis (2021), "Price Salience and Product Choice," *Marketing Science*, 40 (4), 619–36.

Brown, Jennifer, Tanjim Hossain, and John Morgan (2010), "Shrouded Attributes and Information Suppression: Evidence from the Field," *The Quarterly Journal of Economics*, 125 (2), 859–76.

Carlin, Bruce I. (2009), "Strategic Price Complexity in Retail Financial Markets," *Journal of Financial Economics*, 91 (3), 278–87.

Chang, Kwangpil, S. Siddarth, and Charles B. Weinberg (1999), "The Impact of Heterogeneity in Purchase Timing and Price Responsiveness on Estimates of Sticker Shock Effects," *Marketing Science*, 18 (2), 178–92.

Chetty, Raj, Adam Looney, and Kory Kroft (2009), "Salience and Taxation: Theory and Evidence," *American Economic Review*, 99 (4), 1145-77.

Chiles, Bennet (2021), "Shrouded Prices and Firm Reputation: Evidence from the U.S. Hotel Industry," *Management Science*, 67 (2), 964–83.

Chioveanu, Ioana, and Jidong Zhou (2013), "Price Competition with Consumer Confusion," *Management Science*, 59 (11), 2450–69.

Compeau, Larry D. and Dhruv Grewal (1998), "Comparative Price Advertising: An Integrative Review," *Journal of Public Policy & Marketing*, 17 (2), 257–273.

Corbetta, Maurizio, and Gordon L. Shulman (2002), "Control of Goal-Directed and Stimulus-Driven Attention in the Brain," *Nature Reviews Neuroscience*, 3 (3), 201–15.

Deke, John (2014), "Using the Linear Probability Model to Estimate Impacts on Binary Outcomes in Randomized Controlled Trials" *Mathematica Policy Research*, 6, 1–5.

Ellison, Glenn (2005), "A Model of Add-On Pricing," *The Quarterly Journal of Economics*, 120 (2), 585–637.

Ellison, Glenn, and Alexander Wolitzky (2012), "A Search Cost Model of Obfuscation," *The RAND Journal of Economics*, 43 (3), 416–21.

Ellison, Glenn, and Sara Fisher Ellison (2009), "Search, Obfuscation, and Price Elasticities on the Internet," *Econometrica*, 77 (2), 427–52.

Facebook (2023), "Zero Friction Future Retail," https://www.facebook.com/business/m/facebook-iq/zero-friction-future, accessed August 12, 2023.

Feldman, Naomi E., and Bradley J. Ruffle (2015), "The Impact of Including, Adding, and Subtracting a Tax on Demand," *American Economic Journal: Economic Policy*, 7 (1), 95–118.

Fisher, Gregory (2023), "Measuring the Factors Influencing Purchasing Decisions: Evidence from Cursor Tracking and Cognitive Modeling," *Management Science*, 69 (8), 4558–78.

Fisher, Marshall, Santiago Gallino, and Jun Li (2018), "Competition-Based Dynamic Pricing in Online Retailing: A Methodology Validated with Field Experiments," *Management Science*, 64 (6), 2473–514.

Gabaix, Xavier, and David Laibson (2006), "Shrouded Attributes, Consumer Myopia, and Information Suppression in Competitive Markets," *The Quarterly Journal of Economics*, 121 (2), 505–40.

Goldin, Jacob, and Tatiana Homonoff (2013), "Smoke Gets in Your Eyes: Cigarette Tax Salience and Regressivity," *American Economic Journal: Economic Policy*, 5 (1), 302–36.

Grewal, Dhruv, and Julie Baker (1994), "Do Retail Store Environmental Factors Affect Consumers' Price Acceptability? An Empirical Examination," *International Journal of Research in Marketing*, 11 (2), 107–15.

Hainmueller, Jens, Michael J. Hiscox, and Sandra Sequeira (2015), "Consumer Demand for Fair Trade: Evidence from a Multistore Field Experiment," *The Review of Economics and Statistics*, 97 (2): 242–56.

Hayes, Andrew F. (2022), *Introduction to Mediation, Moderation, and Conditional Process Analysis: A Regression-Based Approach*, 3rd edition, New York, NY: The Guilford Press.

Hossain, Tanjim, and John Morgan (2006), "...Plus Shipping and Handling: Revenue (Non) Equivalence in Field Experiments on eBay," *The B.E. Journal of Economic Analysis & Policy*, 5 (2), 1–30.

Hubspot (2017), "Embrace the Controversial: Why You Should Publish Pricing on Your Website," https://blog.hubspot.com/marketing/why-publish-pricing-on-website-var, accessed August 12, 2023.

Irmak, Caglar, Thomas Kramer, and Sankar Sen (2017), "Choice Under Incomplete Information on Incumbents: Why Consumers with Stronger Preferences are More Likely to Abandon their Prior Choices," *Journal of Consumer Psychology*, 27 (2), 264–9.

Janiszewski, Chris (1998), "The Influence of Display Characteristics on Visual Exploratory Search Behavior," *Journal of Consumer Research*, 25 (3), 290–301.

Janiszewski, Chris, and Donald R. Lichtenstein (1999), "A Range Theory Account of Price Perception," *Journal of Consumer Research*, 25 (4), 353–68.

Johnson, Richard D., and Irwin P. Levin (1985), "More than Meets the Eye: The Effect of Missing Information on Purchase Evaluations," *Journal of Consumer Research*, 12 (2), 169–77.

Jones, Byron, and Michael G. Kenward (2015), *Design and Analysis of Cross-Over Trials*, 3rd edition, London: CRC Press.

Jovanovic, Boyan (1982), "Truthful Disclosure of Information," *Bell Journal of Economics*, 13 (1), 36–44.

Kalyanaram, Gurumurthy, and Russell S. Winer (1995), "Empirical Generalizations from Reference Price Research," *Marketing Science*, 14 (3), G161–G169.

Karmarkar, Uma R., Baba Shiv, and Brian Knutson (2015), "Cost Conscious? The Neural and Behavioral Impact of Price Primacy on Decision Making," *Journal of Marketing Research*, 52(4), 467–81.

Kahn, Barbara E., and Meyer, Robert J. (1991). Consumer Multiattribute Judgments Under Attribute-Weight Uncertainty, *Journal of Consumer Research*, 17(4), 508–22.

Kim, Hyeong Min, and Luke Kachersky (2006), "Dimensions of Price Salience: A Conceptual Framework for Perceptions of Multi-dimensional Prices," *Journal of Product & Brand Management*, 15, 139–47.

Koschmann, Anthony, and Mathew S. Isaac (2018), "Retailer Categorization: How Store-Format Price Image Influences Expected Prices and Consumer Choices," *Journal of Retailing*, 94 (4), 364–79.

Lynch, John G. (1985), "Uniqueness Issues in the Decompositional Modeling of Multiattribute Overall Evaluations: An Information Integration Perspective," *Journal of Marketing Researc*h, 22 (1), 1–-19.

Lynch, John G., and Dan Ariely (2000), "Wine Online: Search Costs Affect Competition on Price, Quality, and Distribution," *Marketing Science*, 19 (1), 83–103.

Markman, Arthur B., and Douglas L. Medin (1995), "Similarity and Alignment in Choice," *Organizational Behavior and Human Decision Processes*, 63 (August), 117–30.

Mazumdar, Tridib, S.P. Raj, and Indrajit Sinha (2005), "Reference Price Research: Review and Propositions," *Journal of Marketing*, 69 (4), 84-102.

Meyer, Robert J. (1981). A Model of Multiattribute Judgments Under Attribute Uncertainty and Informational Constraint, *Journal of Marketing Research*, 18(4), 428–41.

Morwitz, Vicki G., Eric A. Greenleaf, and Eric J. Johnson (1998), "Divide and Prosper: Consumers' Reactions to Partitioned Prices," *Journal of Marketing Research*, 35 (4), 453–63.

Nielsen Norman Group (2013), "State the Price to Give B2B Sites a Competitive Advantage," https://www.nngroup.com/articles/show-price/, accessed August 12, 2023.

Petersen, Steven E., and Michael I. Posner (2012), "The Attention System of the Human Brain: 20 Years After," *Annual Review of Neuroscience*, 35 (1), 73–89.

Posner, Michael I., and Steven E. Petersen (1990), "The Attention System of the Human Brain," *Annual Review of Neuroscience*, 13 (1), 25–42.

Rahinel, Ryan, and Rohini Ahluwalia (2015), "Attention Modes and Price Importance: How Experiencing and Mind-wandering Influence the Prioritization of Changeable Stimuli," *Journal of Consumer Research*, 42 (2), 214–34.

Sanbonmatsu, David M., Frank R. Kardes, David C. Houghton, Edward A. Ho, and Steven S. Posavac (2003), "Overestimating the Importance of the Given Information in Multiattribute Consumer Judgment," *Journal of Consumer Psychology*, 13 (3), 289–300.

Santana, Shelle, Steven K. Dallas, and Vicki G. Morwitz (2020), "Consumer Reactions to Drip Pricing," *Marketing Science*, 39 (1), 188–210.

Simonson, Itamar, and Amy Drolet (2004), "Anchoring Effects on Consumers' Willingness-to-pay and Willingness-to-accept," *Journal of Consumer Research*, 31, 681–90.

Silva, Pia (2018), "Should I put My Prices on My Website," *Forbes*, September 26.

Singer, Lisa (2019), "Should You Make Your Prices Public on Your Website?" *Forrester*, December 4.

Solway, Alec, and Matthew M. Botvinick (2012), "Goal-directed Decision Making as Probabilistic Inference: A Computational Framework and Potential Neural Correlates," *Psychological Review*, 119 (1), 120–54.

Spiller, Stephen A., Gavan J. Fitzsimons, John G. Lynch, and Gary H. Mcclelland (2013), "Spotlights, Floodlights, and the Magic Number Zero: Simple Effects Tests in Moderated Regression," *Journal of Marketing Research*, 50 (2), 277–88.

Winer, Russell S. (1986), "A Reference Price Model of Brand Choice for Frequently Purchased Products," *Journal of Consumer Research*, 13 (2), 250–6.

Witherill, Liston (2017), "When You Should (and Shouldn't) Publish Prices on Your Website," *Medium*, April 30.

Xia, Lan (2010), "An Examination of Consumer Browsing Behaviors," *Qualitative Market Research: An International Journal*, 13 (2), 154–73.