
| RESEARCH ARTICLE

Communicating Price Increases: A Systematic Review of Price Fairness and Consumer Responses

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| ABSTRACT

When firms raise prices, customers assess both the financial loss and how the firm treats them. This systematic review synthesizes 48 journal articles assessed from full text, identified across four databases without a publication-year restriction. Single-reviewer screening and extraction yielded 26 core direct, 18 supporting, and four conceptual records across seven themes. Tests of cost and other causal explanations form the largest direct-evidence cluster. Their effects depend on perceived control, credibility, fit to the affected offering, and the magnitude of the increase. Disclosure, reference-price framing, and package downsizing change what customers see. Some improve immediate judgments in the tested settings; others raise concerns about comprehension or deception. Reputation and vulnerability chiefly inform fairness appraisals in this corpus; budgets and alternatives may constrain action, but their interactions with specific messages on observed behavior are largely untested. The proposed framework organizes relationships examined across separate studies. Experimental evidence chiefly supports effects on immediate fairness judgments. Evidence connecting those judgments to later behavior is less direct, and the proposed feedback path is conceptual. Fewer, more varied studies observe choice, demand, or attrition; none tests the full sequence from communication through judgment to behavior. Conceptual sources provide ideas rather than independent effect estimates.

| KEYWORDS

Price Increases; Communication; Price Fairness; Causal Attribution; Reference Price; Shrinkflation; Consumer Behavior

| ARTICLE INFORMATION

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1. Introduction

Firms can face a genuine increase in costs and still lose customers when they raise prices. Customers consider the amount, the reason, and how much control the firm had. They also notice how the firm announces and implements the change. Even a credible explanation may leave the new transaction unaffordable or less attractive than alternatives. Research on price-increase communication consequently measures fairness and trust, while a smaller set of designs follows transactions, purchase, or retention.

The evidence comes from several settings. Some studies use pricing experiments or laboratory markets; others examine services, reference prices, or package downsizing. These studies do not all measure the same response. A fairness rating is not the same as an intention to switch or a laboratory transaction. Observed retail demand and field attrition are different again. Nor can a study of an increase with no message identify the effect of an explanation. The synthesis therefore distinguishes studies that test a message or pricing tactic from those that illuminate a mechanism or response. Conceptual papers supply possible strategies.

The review groups 48 publications into seven themes to examine how communication and economic burden shape consumers' reactions. The findings point to a conditional pattern: a credible cause can change inferences about responsibility and motive, but an explanation must fit the affected offering and the amount charged. Reference prices and customer vulnerability shape fairness judgments; relationships and available alternatives may separately influence what customers do. The proposed framework

organizes findings from separate studies about the firm's responsibility and the customer's loss. It treats observed behavior as a distinct outcome whose connection to those judgments remains uncertain.

2. Review Methodology

The review covers English-language, peer-reviewed journal articles identified in four databases, with no publication-year restriction. This scope reflects the records available through those searches, not every publication on price increases. Work outside those indexes, languages, and formats may be absent. The included studies also answer different questions. A study that compares explanations, disclosures, or frames can assess their effects on the outcomes measured in that setting. A study that observes a price change without testing a message can describe responses to the change, but it cannot isolate the effect of communication. Both types of study inform the review, and the synthesis keeps their evidence separate.

2.1 Scope and Eligibility

This review examines how consumers respond when firms raise prices or make economically equivalent changes, such as reducing package size. It asks how firms explain and present these changes, how customers judge their fairness, and what customers do next. Each publication represents a unit of synthesis. Studies that directly compare explanations, disclosures, price frames, or implementation tactics form the core evidence; other studies inform the mechanisms or responses but did not test communication. Conceptual papers guide the framework and research agenda but offer no new effect estimates. Eligible records were peer-reviewed journal articles in English that addressed the scope of this review; public-policy price interventions and financial-market price movements were excluded. Operations models without consumer response and articles without an applicable price-increase mechanism were excluded as well.

Five questions guide the synthesis:

RQ1. *Which reasons for an increase reduce negative consumer reactions, and when?*

RQ2. *How do disclosure, price presentation, and implementation affect fairness and behavior?*

RQ3. *Under what conditions does the same message affect customers differently?*

RQ4. *Which judgments and feelings might connect communication with consumer and market outcomes?*

RQ5. *Where are the gaps in evidence and methods, and what should future studies test?*

2.2 Sources and Searches

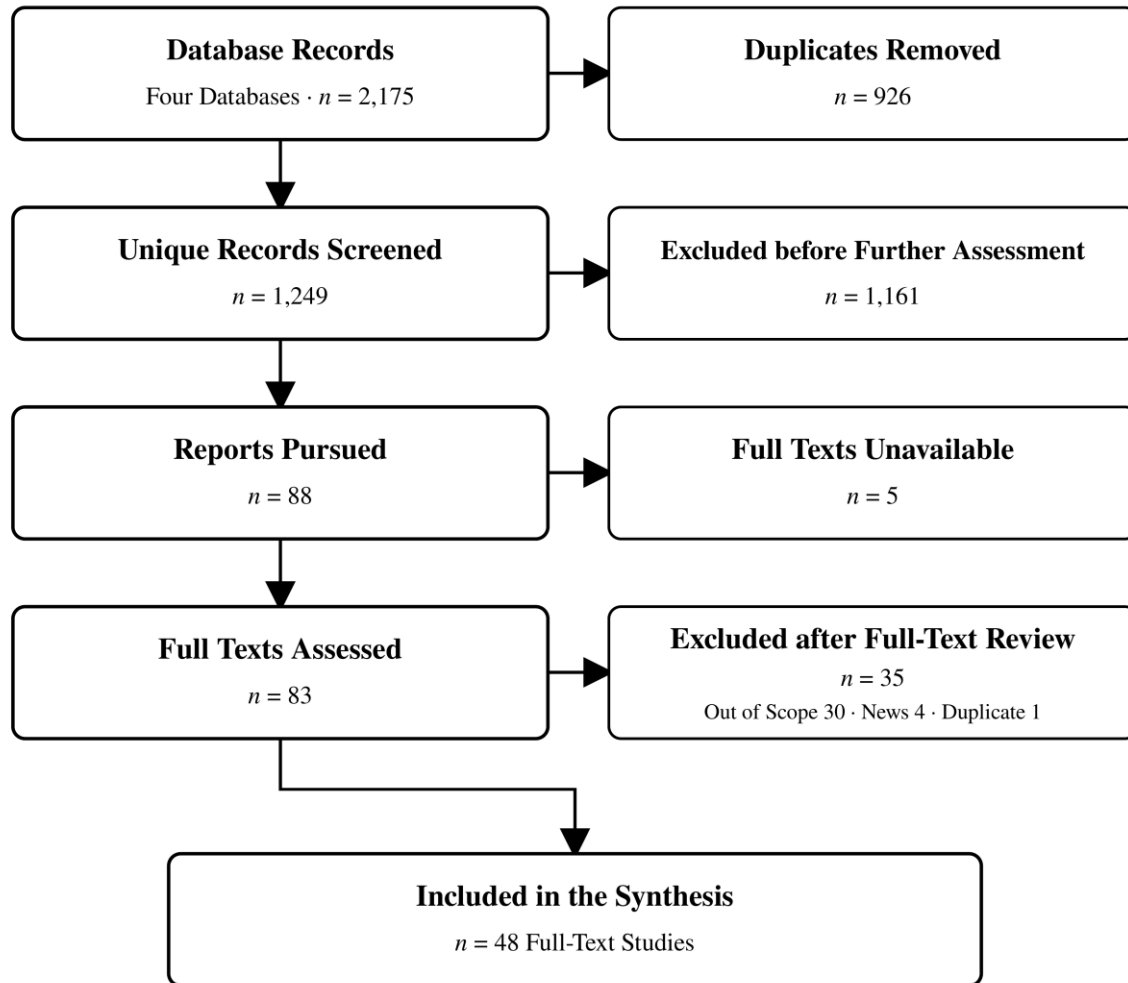
Five title-and-abstract search sets were run in ABI/INFORM Complete, Academic Search Complete, Business Source Premier, and ProQuest Research Library. Applying each set in every database produced 20 database-by-set searches. No publication-year restriction was applied. Searches were limited to peer-reviewed English-language academic journal articles, without a full-text availability limiter. The complete query texts are reproduced in Appendix A. Table A1 reports the number of records retrieved for each database and search set. These counts represent exported hits before duplicate removal.

2.3 Selection and Extraction

Records were combined in a structured review dataset, and duplicates were removed using an exact-match key. A normalized DOI was used when available. When no DOI was available, the key was a normalized title. Normalization converted text to lowercase and removed leading articles, punctuation, special characters, and excess whitespace. Records with the same key were combined while retaining every database and search-set source. A publication found through multiple routes therefore entered screening as one record.

The reviewer screened unique titles and abstracts against the scope specified above. Further, the reviewer recorded each decision and its reason and checked text availability for records pursued and assessed retrievable reports for eligibility. The record-level decisions retain initial and later labels when both were recorded. For the study-selection flow, a record with a documented later exclusion is counted at that stage once. This places each exclusion at the most specific documented decision point.

The four-database search log records 2,175 exported hits and 926 duplicates removed, leaving 1,249 unique records. The screening records mark 1,196 records excluded at title/abstract screening and 53 retained. Thirty-five of those 1,196 records also carry a full-text exclusion. Each of those 35 is counted only at the later stage. These 35 records account for the difference between the initial 1,196 screening exclusions and the 1,161 shown before further assessment. This leaves 1,161 excluded before further assessment and 88 pursued. Five of the 88 records could not be assessed from full text. Abstracts or indexed excerpts were available for three of the five records. Those partial sources were not substituted for full-text assessment, so none of the five publications contributes to the synthesis. The remaining 83 were assessed from full text: 35 were excluded and 48 included. The evidence map therefore contains 48 full-text-reviewed studies. The 48 studies in that map should therefore be distinguished from the 53 records initially marked as retained at title/abstract screening. Figure 1 shows the reconciled selection flow.

Figure 1. Study Selection and Evidence Availability

Note: The 35 records excluded after full-text review also carry an earlier title/abstract Exclude label in the review dataset. They are counted at the later documented stage only. Five records could not be assessed from full text; three had abstracts or indexed excerpts. None entered the synthesis.

For each publication, the reviewer recorded its source, setting, sample, design, price change, message or tactic, comparison, outcomes, findings, proposed mechanism, and notes. Recording the outcomes separately helps distinguish fairness ratings and stated intentions from observed choices or other behavior. Earlier abstract- and excerpt-based notes for the three unassessed records remain identifiable in the review records; those records do not contribute findings to the synthesis. Each included publication received one dominant theme, one synthesis role, and one primary design family. These are publication-level labels, so a paper containing several studies still appears once in the descriptive counts. The theme reflects the main price change or communication comparison in the extraction record and determines where the publication appears in those counts. It does not mean the paper studies only one mechanism. For example, a cost-justified increase studied through percentage framing is counted under framing (Rikala, 2022), while a transparency-related fairness finding in a size-versus-price comparison is counted under shrinkflation (Evangelidis, 2024). The other mechanisms can still inform the synthesis without counting either publication twice.

Core studies test how a message, frame, disclosure, or pricing tactic affects consumers. They provide the most direct evidence for the review's questions about communication, although the outcomes they measure differ. Supporting studies examine related mechanisms, moderators, or responses without that direct comparison. For instance, evidence about responses to a price change may help explain the setting in which a message is received without establishing an effect of the message itself. Conceptual papers propose strategies or discuss earlier work; they add no new effect estimate. The design-family code summarizes a publication that may contain more than one study or design. The narrative therefore identifies what each source contributes and keeps fairness

judgments, stated intentions, and observed behavior distinct. It does not pool unlike effect estimates or treat a change in fairness ratings as evidence of a general effect on sales or retention.

2.4 Exclusion Categories

Table 1 groups the 1,161 records without a full-text review designation by their recorded screening reasons. The 35 records excluded during full-text review comprise 30 out-of-scope studies, four news items, and one duplicate executive summary.

Table 1. Records Excluded before Further Assessment by Screening Category ($n = 1,161$)

Preliminary-screen category	Records
No direct price-increase communication or consumer-response focus	721
Financial-market price movements	196
Public-policy price interventions	136
Operations or supply-chain models without consumer response	68
Other documented reasons, including ineligible formats and one duplicate	40
Total excluded without full-text review designation	1161

3. Descriptive Profile of the Evidence Base

The 48 included publications span 1991–2026 and were assessed from full text. Thirty-four are classified as experimental, 10 as observational or survey, and four as conceptual or review. As shown in Table 2, twenty-six publications are core direct evidence, 18 supply supporting or mechanism evidence, and four supply conceptual background. Each paper counts under one dominant theme, resulting in 48 total publications. Table 3 reports the theme totals and the number of core direct studies in each. These totals reflect the papers' dominant-theme assignments, not how often each mechanism appears across all papers. Individual papers may inform several mechanisms in the thematic discussion.

Table 2. Synthesis Roles and Design Families of Included Publications ($n = 48$)

Measure	Category	Studies
Synthesis role	Supporting/mechanism evidence	18
Synthesis role	Core direct evidence	26
Synthesis role	Conceptual/background	4
Design family	Experimental	34
Design family	Conceptual/review	4
Design family	Observational/survey	10

Table 3. Dominant Themes and Core Direct Evidence ($n = 48$)

Theme	Total	Core Direct
Cost/causal justification and attribution	15	15
Transparency, disclosure, and source credibility	3	3
Reference-price, numeric, and promotional framing	8	3
Shrinkflation, implementation tactic, and unit-price salience	6	5
Relationship, reputation, firm, and contextual moderators	6	0
Consumer response without a communication treatment	6	0
Conceptual strategy and preimplementation assessment	4	0

Experiments compare explanations, disclosure, framing, and pricing tactics. Most measure immediate fairness or intentions; a few observe choice or field behavior. Observational and survey publications add market, service, and customer settings, but their associations do not isolate an explanation unless communication was assigned. Conceptual and review publications organize strategies and hypotheses without adding independent effect estimates. The thematic synthesis identifies these evidence types when interpreting each result.

4. Theoretical Foundations

A price increase can be defensible for the seller and still be rejected by a customer. Customers compare the new price with a reference transaction and ask who caused the change. They weigh what they lose and whether the firm treated affected people fairly. These distinct judgments help explain why an accepted reason need not yield an accepted price. Whether either judgment changes repeat purchasing requires a separate test.

4.1 Reference Transactions and Dual Entitlement

A customer evaluates an increase against an expected transaction, while recognizing that a seller may need to preserve a viable return. This view helps explain why experiments treat cost pass-through differently from a discretionary margin increase. A cost shock may justify some adjustment to protect the seller's previous return. Greater willingness to pay alone does not necessarily justify charging more for the same offer. The corpus supports this mechanism through price-fairness comparisons of external cost shocks and seller-controlled changes (Gielissen et al., 2008; Huangfu & Zhu, 2012; Vaidyanathan & Aggarwal, 2003). It does not yield a universal formula for the amount of pass-through consumers regard as legitimate.

The benchmark itself is neither fixed nor singular. Earlier paid prices, list prices, competing offers, customary fees, and the expected value of a service can all supply reference points. Park et al. (2023) document the relevance of a displayed list price in an online marketplace; Prakash and Spann (2022) estimate an asymmetric response around an internal reference price. These findings imply that the same monetary increment can be perceived differently depending on the comparison price, without establishing that every supplied benchmark is credible or fair.

4.2 Attribution and Motive Inference

An explanation asks consumers to assign responsibility: where did the pressure originate, how much control did the firm have, and who benefits from the response? A cost shock can support an external attribution, but an explanation loses force when its scope or magnitude fails to fit the observed action. Bolton and Alba (2006) show the value of matching a cost to the affected offering, while Ferguson et al. (2011) demonstrate the suspicion generated by a broader increase than the shock seems to require. Lu et al. (2020) show that market power changes how readily customers believe a firm lacked choice.

Motive is inferred from prior conduct as well as a current message. Reputation, satisfaction, and corporate social responsibility set different expectations. They need not soften reactions to the same increase in the same way (Helm, 2013; Homburg et al., 2005; Sipilä et al., 2022). The appropriate question is whether the total evidence supports a defensible action, not whether a message contains a preferred cost term.

4.3 Reference Price, Perceived Loss, and Value

Even a convincing reason does not erase the consumer's loss against a remembered or displayed price. Magnitude, frequency, budgets, and the availability of substitutes determine the felt burden. Homburg et al. (2010) examine budgets and framing in relation to stated future purchase likelihood; Huber (2011) shows that frequent increases can raise perceived aggregate inflation even when individual price changes are judged accurately. The customer also compares what is received for the new payment. A service improvement, a changed package, or an altered promotion can shift that value comparison without changing the underlying reason for the firm's price decision.

The routes can diverge. Rikala (2022) finds that percentage presentations can raise fairness ratings while encouraging neglect of the base values. Scanner data and demand models suggest a weaker immediate demand response to downsizing in the studied markets (Çakir & Balagtas, 2014; Kim, 2024). In fairness experiments, noticed downsizing produced a penalty linked to perceived deception (Evangelidis, 2024). A review of communication must therefore ask whether apparent acceptance reflects informed agreement, attention, or an inability to adjust.

4.4 Justice, Transparency, and Harm

Customers can judge both the higher payment and the process behind it. They consider what the firm discloses, how it applies the decision, and whom it harms. Proactive announcement can improve evaluations of an increase relative to learning it elsewhere, but more disclosure can also expose a contested margin or an implausible allocation of costs (Ferguson & Ellen, 2013; Franciosi et al., 1995; Kachelmeier et al., 1991). The effect of transparency depends on what is disclosed, to whom, and whether the information enables a meaningful evaluation.

A second fairness question concerns harm. Campbell et al. (2026) link price judgments to perceived customer vulnerability and material consequences, so an economically explicable cost rise may remain objectionable when it harms those with little room to adjust. Deception inferences from unannounced downsizing and the anger associated with unfair increases broaden the account beyond whether a cost story is logically coherent (Evangelidis, 2024; Mayer & Avila, 2014). Taken together, these lenses imply two

simultaneous consumer appraisals: who is responsible for a price decision, and what the new transaction costs the customer relative to an acceptable alternative.

5. Thematic Findings

A justification does not act in isolation. Its credibility depends on who controls the cause, whether it fits the affected offering, and whether the resulting price is bearable for the customer. Disclosure and presentation can improve immediate ratings, but a favorable rating need not demonstrate comprehension or predict action. Observed choice, demand, and attrition come from fewer and more varied settings than the fairness experiments. The following sections report what each study measured. Tables B1–B7 in Appendix B give the corresponding study-level findings and limits. Section 6 considers how those outcomes might connect.

5.1 Cost/Causal Justification and Attribution

The largest direct cluster concerns the reason given for an increase (15 studies, all core direct; Table B1), with repeated experimental evidence on immediate judgments. Consumers ask who caused the change and whether the firm could have avoided or absorbed it. They also infer a motive from the explanation. Vaidyanathan and Aggarwal (2003) separated causal locus from controllability across three experiments. An externally caused increase that raised the seller's own costs was judged fairer than a seller-caused increase or a rise justified by market prices when the seller's costs had not changed. In a related experiment, Huangfu and Zhu (2012) found cost-based increases more acceptable than demand-based increases, especially where monopoly or state ownership heightened expectations of restraint. Gielissen et al. (2008) likewise reported more favorable judgments for wholesale-cost pass-through than a demand-driven increase. These studies support an attributional account, rather than the claim that any mention of cost will suffice.

The perceived fit between cause and price action matters. Bolton and Alba (2006) showed that an increase tied to the offering generating a new cost was more acceptable than an economically equivalent increase placed on a less clearly aligned item. Ferguson et al. (2011) found greater suspicion when a retailer raised prices on products unaffected by a cost shock. Suspicion accompanied negative feelings, unfairness judgments, and stated intentions to complain, spread negative word of mouth, or exit. The study did not observe those later actions. Koschate-Fischer et al. (2016) found that a charitable donation offered as a reason for an increase worked best when donation size matched or exceeded the increase and the firm's CSR history supported its claimed motive. An apparently prosocial reason can therefore invite more scrutiny when the financial contribution or company history contradicts it.

Magnitude and firm context further qualify these patterns. Martin et al. (2009) found that a cost explanation outperformed a profit explanation for a smaller increase. The difference was not significant for a much larger increase, where prior loyalty also ceased to improve fairness judgments. Homburg et al. (2005) found that satisfaction buffered repurchase intentions after increases and led customers to infer fairer motives when none was stated. Lu et al. (2020) showed that a cost explanation benefited a lower-power retailer more readily than a higher-power one: explicitly stating that the shock was beyond the firm's control removed the higher-power firm's disadvantage in an experiment. Campbell et al. (2026) extend the account beyond firm entitlement by showing that perceived consumer harm and vulnerability shape fairness even where the firm faces rising costs.

The effects of particular justifications also depend on the outcome measured. In a coffee-shop experiment, Campbell et al. (2015) found a fair-trade explanation more favorable than silence or a tax explanation. Only 52 self-selected participants returned for the choice measure, so that result says little about sustained purchasing. Woisetschläger et al. (2008) found that communicating procurement costs alone did little to improve utility evaluations relative to adding customer value, and none of their communication manipulations significantly improved loyalty intentions.

Direct field evidence of subsequent behavior comes mainly from a specific subscription setting. Damavandi et al. (2026) ran online mechanism experiments and a randomized field test with storage customers. A truthful market-based reason lowered exit hazard by 29.5% compared with no reason. Average effects of cost and quality reasons were not statistically significant in the field. Their scenarios linked market reasons to scarcity, quality reasons to perceived value, and concrete wording to less skepticism. The field result establishes an effect for the market justification in that setting; the scenario evidence on scarcity, value, and skepticism does not by itself establish which appraisal carried the observed attrition effect. Nor does it show that a cost explanation generally retains customers.

Kyle et al. (1999) found that cost-of-service information raised the price participants expected for a prospective public event fee. Adding gain/loss or beneficiary frames did not independently improve that result. The study shows that information about costs can shift price expectations in this setting, but expected price was the measured outcome. It does not show what participants would have paid for admission.

Chang and Lin (2012) found that low- and moderate-control explanations produced more favorable responses than a highly controllable reason or silence. This pattern is consistent with consumers reacting differently when a cause appears less subject to the firm's discretion. The moderate-control condition, however, combined two reasons, so its result cannot be attributed to perceived control alone. Relationship-type interactions were not significant overall, limiting claims that the explanation worked differently across customer relationships. Both studies inform how consumers interpret a proposed increase, without establishing its effect on observed purchasing.

5.2 Transparency, Disclosure, and Source Credibility

Buyers use cost and profit disclosure to judge whether a higher price reflects added expense or a larger margin. Table B2 gives the disclosure studies and their evidentiary limits. In laboratory markets, Kachelmeier et al. (1991) found that revealing sellers' marginal costs hastened movement toward a higher predicted price after a tax shock, whereas profit information produced resistance, lower transaction prices, less volume, and reduced efficiency. Franciosi et al. (1995) likewise found that profit disclosure initially delayed price adjustment relative to cost disclosure, although prices eventually converged. These studies observed laboratory transactions. Cost information helped prices adjust, while profit information exposed a margin that buyers could challenge. Neither effect establishes a general benefit of disclosure for customer retention. Strategic behavior in repeated laboratory markets may also contribute to demand withholding.

Source and explanatory fit matter outside the laboratory. Ferguson and Ellen (2013) found that a firm announcement produced higher immediate fairness ratings than learning of an otherwise comparable increase from an external source. In their second experiment, a detailed explanation tied to the product was most effective for a 20% increase. For a 2% increase, a limited reason scored higher than detailed aligned information, while detailed nonaligned information also scored highly; a simple rule to maximize detail is therefore unsupported. The positive finding in the announcement experiments is an immediate fairness judgment. Their higher ratings do not establish clearer understanding of the economic change or later retention. The laboratory market evidence addresses another outcome: exposing a contested profit margin can change short-run transactions, without establishing a long-run customer effect.

5.3 Reference-Price, Numeric, and Promotional Framing

The studies show that customers evaluate an increase against a constructed comparison price, not solely against its absolute amount. Table B3 lists the reference-price and framing studies. In an Amazon marketplace panel, Park et al. (2023) observed that introducing a list price alongside a higher selling price improved sales rank relative to a pure increase; favorable reviews strengthened the pattern. The panel cannot isolate the list price as the cause, and sales rank is not unit sales. It also cannot show whether shoppers understood or trusted the comparison price.

In controlled framing experiments, Rikala (2022) found higher fairness ratings when price and cost changes were both presented as percentages. This presentation invites consumers to compare two figures that appear directly comparable. Yet the percentages apply to different starting amounts: matching percentage changes do not establish that the firm's monetary profit stayed the same. The experiments also found that consumers neglected these underlying base values. That finding helps explain how a percentage frame can seem balanced while leaving consumers with an incomplete picture of the actual profit change. The fairness rating captures an evaluation of the presented increase, not necessarily an accurate understanding of its economics. It should therefore be read alongside the evidence of base-value neglect rather than treated as informed approval. The experiments also do not show whether consumers subsequently bought the product. However, Rikala's findings illustrate why a frame that improves an immediate judgment may still raise questions about comprehension.

Promotional history creates another reference point. Liu and Chou (2015) held the economic offer constant while varying free-gift and bundle language. The frame shifted the prices consumers assigned to individual items, as well as perceived increases, repurchase intentions, and willingness to pay when the promotion ended. Zhang et al. (2021) found that a discounted-quantity limit could become a purchase target; consumers sometimes bought fewer units despite a more generous discount. Both studies examine price steps created by promotions rather than standard announced list-price increases. They inform the mechanism, but stated intentions do not establish later purchases.

Boundary conditions temper the framing conclusion. Malc et al. (2016) found no statistically significant differences among the external reference prices tested, although fairness correlated with intended shopping and switching. Prakash and Spann (2022) estimated asymmetric demand around an internal reference price; their field experiment randomized products rather than customers, and the online replication gave weaker support for the moderators. Homburg et al. (2010) found lower stated later purchase likelihood under percentage rather than absolute framing in a low-income, same-category budgeting setting, without support for the predicted income-by-frame interaction. Ma et al. (2026) found that a linear temporal prime could raise immediate purchase intention after a disclosed increase through expected future prices, but the pattern disappeared with a one-month

comparison interval or increases of 25% or 40%. These studies show how reference points, budgets, and timing can shape responses. They do not establish that a favorable frame improves understanding or makes an increase affordable or justified.

5.4 Shrinkflation, Implementation Tactic, and Unit-Price Salience

Package downsizing raises the effective unit price while keeping the prominent shelf price less changed. Table B4 summarizes the studies of downsizing and unit-price salience. In scanner-data studies, Çakir and Balagtas (2014) found substantially greater price than package-size sensitivity for ice cream and substitution toward larger packs after downsizing. Kim (2024) compared observed downsizing of a beverage with a modeled economically equivalent package-price increase and estimated substantially stronger demand sensitivity to the explicit price rise. These studies use market behavior, but the modeled counterfactual depends on its assumptions and product setting. Neither study shows that customers would approve of downsizing if they noticed the higher unit price.

Fairness experiments expose the tradeoff. Kachersky (2011) found greater inferred persuasive intent for downsizing than for a direct price increase; knowledge of pricing tactics strengthened profit-motive inferences and reduced product-brand attitudes. Participants saw both packages and noticed the change, so this experiment cannot estimate real-world detection. Across five vignette studies, Evangelidis (2024) found equivalent downsizing less fair than a direct increase. Participants viewed the tactic as deceptive because it might go unnoticed; that perception helped explain the fairness penalty. Explicit communication eliminated the fairness gap in one study.

Observational policy comparisons address unit-price salience. Wan et al. (2026) estimated greater sensitivity to canned-tuna prices per ounce where unit prices were disclosed, especially at smaller chains. This pattern is consistent with shoppers paying closer attention to the effective price when a comparable measure is visible. The matched policy comparison, however, cannot eliminate unobserved differences between markets or verify compliance at every store. It also does not test whether an explanation for a price increase changes fairness judgments or later purchases.

Conceptual synthesis supplies a different kind of evidence. Franc-Dąbrowska et al. (2026) synthesize heterogeneous secondary evidence from Japan concerning shrinkflation. Their account brings together contexts and possible responses that individual studies may not cover. Because it does not provide an independent primary effect estimate, it cannot confirm the size of a disclosure or downsizing effect. Its value here is in identifying questions that could be tested directly with comparable price and package-size information.

Scanner data and a modeled counterfactual suggest weaker immediate demand sensitivity to a less salient unit-price change. Separate fairness experiments show a different consequence: noticed downsizing can invite deception inferences and a fairness penalty, while explicit communication removed that penalty in one vignette study. The market data do not measure deception judgments. The experiments do not establish how often shoppers notice downsizing during ordinary purchases. The vignette result does not show whether explicit disclosure changes real-world demand. Neither design establishes how long responses last or a general long-run revenue advantage for concealment.

5.5 Relationship, Reputation, Firm, and Contextual Moderators

Experimental evidence identifies one source of variation. The studies of relationship and context moderators are listed in Table B5. Prior relationships and firm identity can shift motive inferences even when no explanation is offered. Helm (2013) manipulated favorable versus unfavorable airline reputation and found less negative-motive attribution, greater price fairness, and higher purchase intention under favorable reputation; the scenario provided no reason for the increase.

A favorable reputation does not always soften the response. Kwak et al. (2015) found that humanized brands showed stronger absolute price elasticities in household panel data, and their experimental increases tended to look less fair when anthropomorphism made the brand's action seem intentional. Relationship norms and agency orientation qualified these effects. A warmer firm image can raise expectations as well as goodwill.

Sipilä et al. (2022) found that stronger corporate social responsibility heightened the negative fairness response to an increase, mediated by violated fairness expectations especially among consumers with low CSR skepticism. The relevant boundary is the expectation a firm has created, rather than whether its prior conduct is favorable in isolation. Chavanne et al. (2022) held a lost-revenue explanation constant and varied the COVID context, business type, and increase size. Smaller changes and hardware-store scenarios were generally rated fairer, while message tone had no overall main effect. Magnitude varied in importance across the bus and store settings and contextual frames. Xia and Roggeveen (2022) link collective stress with nostalgia, empathy, benevolent motives, and fairness. Supplemental experiments test nostalgia, but the three focal studies are correlational and do not test an explanation written by the seller.

Finally, Mayer and Avila (2014) presented an increase attributed to negative motives and varied service dependence and relevance. Unfairness and anger were greatest when both were high, with complaint and retaliation intentions elevated in that cell. The scenario bundles several aspects of relevance and dependence, and mediation is based on measured rather than randomized unfairness and anger. These studies give more direct evidence of variation in fairness and motive judgments than of variation in observed customer action. The household panel identifies a brand-related difference in price elasticity; the dependence scenario measures complaint and retaliation intentions. These findings do not establish that a particular increase message changes observed purchases differently by reputation or dependence. Vulnerability, budgets, and alternatives may limit what customers can do. These studies do not show that those factors change how a specific message affects observed behavior. Direct interaction tests are needed. Evidence on firm market power and prior satisfaction appears in Section 5.1 because those studies were assigned to the dominant justification theme.

5.6 Consumer Response Without a Communication Treatment

Evidence on response without a communication treatment is thinner and less comparable than the fairness experiments. Table B6 lists these studies and their limits. In a gasoline survey, Paswan et al. (2014) associated awareness of rising prices, price paid, and consumption with anxiety; anxiety was associated with reported lifestyle, vehicle, and transport adjustments.

In an experimental shopping task, Huber (2011) found that more frequent increases made perceived aggregate inflation larger despite accurate judgments about individual product changes. This result concerns perceived inflation in the task; it does not measure later buying or the effect of a seller explanation.

Other studies ask what customers would do or compare different groups before and after a real increase. A sport-facility before-and-after comparison found lower satisfaction, value, and future intentions after a real fee increase, with perceived value most strongly linked to future intentions (Calabuig et al., 2014). The groups were different respondents, so concurrent changes cannot be excluded. In hypothetical Australian alcohol-purchase scenarios, Hobday et al. (2016) found more stated switching toward cheaper brands or drinks as the increase moved from 10% to 50%, especially among some higher-risk drinkers. These are stated reactions to price magnitude, not observed price effects or message effects. Martinčić et al. (2022) found near-equal shares of respondents reporting substitution and loyalty when asked about an increase, illustrating that behavioral direction is not uniform in a broad survey.

Adomi (2005) provides a small real-world case in which seven of nine Nigerian cybercafés reported reduced patronage after a coordinated rise and all nine later cut prices following complaints and threatened protest. Operators supplied the outcomes; no customers were surveyed, and the increase was short-lived and lacked a control group. These surveys, scenarios, before-and-after comparisons, and one small case report different responses in different settings. They cannot isolate the contributions of affordability, habit, alternatives, or collective voice to observed action, or estimate a common behavioral effect. None permits an inference that a well-crafted notification caused, prevented, or reversed that behavior.

5.7 Conceptual Strategy and Preimplementation Assessment

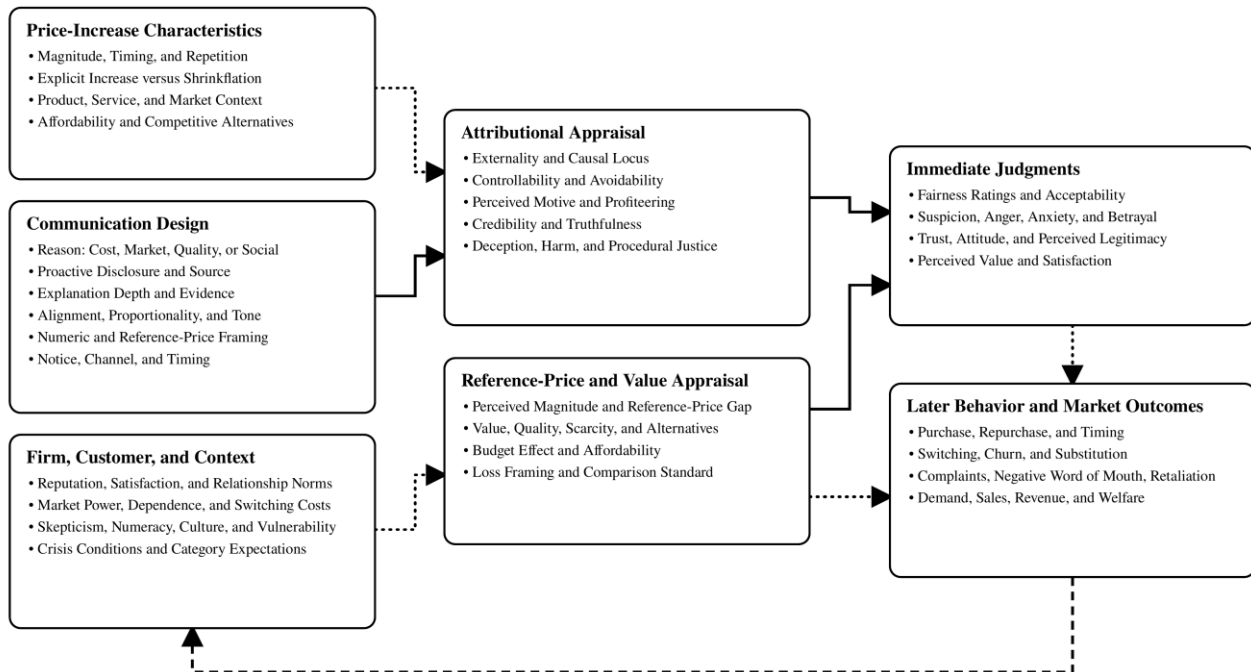
Conceptual accounts propose ways to introduce an increase, while an observational case illustrates how managers assessed likely resistance before acting. Table B7 distinguishes the conceptual accounts from the preimplementation case. Ferguson (2014) advances transparency, familiar practices, downsizing, rebates, and matching as approaches to reduce unfairness. Crompton (2011, 2015) discusses external comparison prices, advance notice, transitional terms, and fee changes within a zone of acceptance. These accounts draw attention to choices beyond the amount of the increase, including when customers learn about it and how the change appears in the transaction. Some proposals could clarify a real burden; others could make that burden less visible. The distinction matters because a favorable reaction to an altered presentation need not mean that customers understand the effective increase. These papers offer no new effect estimates, and some cite studies included elsewhere in this review. Their recommendations should therefore be read as proposals, with the cited empirical studies supplying the evidence for any tested effects.

Crompton and Love (1994) illustrate a preimplementation diagnostic using historical attendance, substitute prices, reference-price strength, income, admission as a share of trip cost, and value perceptions at a festival. These measures gave managers several ways to consider the proposed increase before choosing a price. Some spoke to what visitors might regard as a familiar or acceptable admission price; others concerned affordability and available alternatives. Managers then raised the gate price while retaining a lower advance price, but the article does not report outcomes after that decision. The case shows how managers brought several clues about likely resistance into a pricing decision. It cannot establish whether the change affected attendance or revenue, or whether the advance price altered visitors' reactions. It also does not test advance communication. Neither the conceptual strategies nor this preimplementation case estimates the effect of a communication sequence on later behavior.

6. Proposed Integrative Framework and Cross-Theme Conclusions

A customer encounters an increase both as a change in payment and as a signal about the firm's conduct. The proposed framework in Figure 2 organizes relationships examined across separate studies. Solid arrows identify components with relatively direct experimental evidence, especially effects on immediate fairness judgments. They do not represent a single experimentally tested sequence. Dotted arrows indicate less direct evidence for links to purchase, switching, or attrition. The dashed feedback path from later market response to future relationships is conceptual and untested in this corpus. Related affective outcomes appear in the immediate-judgment box but have been studied less consistently than fairness.

Figure 2. Proposed Framework for Price-Increase Communication and Consumer Response



Note: Solid arrows mark components supported by experiments, chiefly immediate fairness judgments; they are not an end-to-end test. Dotted arrows indicate limited or indirect evidence, especially toward later behavior. The dashed arrow proposes untested feedback through trust, relationship quality, and market response. Credibility, fit, customer burden, and alternatives are proposed contingencies. Behavioral studies are fewer and heterogeneous. The framework synthesizes separate studies: none tests the full path from communication through judgment to later action, and the figure does not estimate purchasing, retention, or long-run response.

Explanations that establish a credible and proportionate cause can improve immediate fairness or motive judgments. A firm announcement can score better than an externally discovered increase, and percentage framing can raise fairness ratings under the tested presentation. These results identify immediate appraisals, chiefly in experiments; they do not show that customers understood the economic change or stayed with the seller (Ferguson & Ellen, 2013; Rikala, 2022).

The studies of comprehension and deception ask different questions. Percentage framing also led participants to neglect base values in one experiment; it did not test deceptive intent. In laboratory markets, profit disclosure exposed margins that buyers resisted. In vignette experiments, shown downsizing elicited deception inferences and poorer fairness judgments, while explicit communication removed the fairness gap in one study. Scanner and modeled-demand evidence from specific products suggests a weaker immediate demand response to downsizing than to an explicit rise, but does not show whether customers detected or endorsed the change. Neither the judgment gains nor the demand estimates establish a sustained behavioral benefit from these tactics (Evangelidis, 2024; Kachelmeier et al., 1991; Kim, 2024; Rikala, 2022). The following propositions describe testable implications rather than a validated pathway to retention.

Studies have examined how reputation, prior satisfaction, market power, dependence, skepticism, and crisis conditions affect fairness or motive judgments. Corporate social responsibility can also shape what customers expect of a firm. Budgets and available

alternatives may constrain what customers do after an increase, but the corpus offers little direct evidence that these characteristics alter the effect of a particular message on observed action. Moderation of appraisals is better supported here than moderation of purchasing, exit, or complaint behavior.

The smaller behavioral evidence base measures different outcomes in different settings. Laboratory markets record trades after cost or profit disclosure. Scanner and demand studies track responses to prices or package size, but do not test explanations. One randomized subscription test measures attrition after customers receive different reasons. The subscription finding applies to one market justification and setting, and the scanner results cannot be attributed to a communication message. None of these designs observes the entire proposed sequence from message to appraisal, fairness, and subsequent action. Fairness ratings and stated intentions cannot therefore be converted into a general retention or sales effect (Damavandi et al., 2026; Kachelmeier et al., 1991; Kim, 2024).

P1. Causal legitimacy: Immediate-judgment experiments suggest that a credible, difficult-to-control cause can reduce inferences of discretionary profiteering. Separately, one randomized subscription field test found lower exit after a truthful market-based reason; accompanying scenarios linked that reason to perceived scarcity. The studies do not establish that either appraisal mediated the field effect or that an external cost claim generally prevents exit (Damavandi et al., 2026; Lu et al., 2020; Vaidyanathan & Aggarwal, 2003).

P2. Alignment and proportionality: A stated cause appears more likely to improve immediate evaluations when it applies to the affected offering and plausibly accounts for the amount or scope charged. Increases extended beyond the affected products invite suspicion even when an upstream cost did rise (Bolton & Alba, 2006; Ferguson et al., 2011).

P3. Disclosure and credibility: Proactive, accurate disclosure may improve immediate assessments when its source, detail, and economic content fit the change; disclosing profit margins or adding excessive detail can instead invite scrutiny. This conditional account follows both fairness experiments and laboratory market transactions (Ferguson & Ellen, 2013; Franciosi et al., 1995; Kachelmeier et al., 1991).

P4. Dual appraisal: The proposed framework distinguishes judgments about firm responsibility from the customer's loss relative to a reference price and budget. A reason that shifts motive inference may not prevent demand loss when the new payment is burdensome, and percentage framing may raise fairness while impairing profit-change comprehension (Campbell et al., 2026; Homburg et al., 2010; Rikala, 2022).

P5. Contextual contingency: Prior satisfaction, reputation, relationship norms, market power, CSR expectations, dependence, and vulnerability may condition immediate appraisals, sometimes in opposing directions. Whether these factors also change the effect of particular messages on observed customer action is largely untested (Helm, 2013; Homburg et al., 2005; Kwak et al., 2015; Sipilä et al., 2022).

P6. Implementation and salience: In the studied settings, economically comparable direct increases and package reductions appear to elicit different immediate fairness and deception judgments. Separate market studies estimate different demand responses. Explicit package-size disclosure eliminated the fairness gap in one vignette experiment; its effect on observed demand remains untested (Çakir & Balagtas, 2014; Evangelidis, 2024; Kim, 2024; Wan et al., 2026).

P7. Judgment and behavior: The transition from fairness or intentions to repeat purchase, exit, substitution, or complaint remains an inferential link in this corpus. Budgets, alternatives, and switching costs may constrain action, but whether they change the effect of a specific message on observed behavior is largely untested. Field attrition, observed demand, real fee changes, and small naturalistic episodes should therefore be assessed alongside attitude experiments rather than inferred from them (Adomi, 2005; Çakir & Balagtas, 2014; Calabuig et al., 2014; Damavandi et al., 2026).

7. Managerial Implications

Before announcing an increase, document the underlying cost, capacity, quality, or strategic decision and determine which offerings and customers it actually affects. A general cost claim for an increase extending far beyond the affected products invites suspicion (Bolton & Alba, 2006; Ferguson et al., 2011). If the cause is controllable or market-driven, say so accurately and explain the commercial decision instead of presenting it as an unavoidable cost shock. A matched cause and a defensible magnitude matter as much as the presence of a reason.

Plan disclosure as part of the transaction. Where feasible, announce a material change before the customer encounters a new charge. Use a credible firm channel, state the old and new prices or quantities, and give the effective date. Ferguson and Ellen

(2013) support the importance of disclosure source and explanatory fit, while the laboratory-market and unit-price studies show that detailed information can also make margin or burden more salient. Check whether percentage, list-price, promotion, and package-size presentations allow customers to understand the effective price. Estimates of weaker immediate demand sensitivity to downsizing do not establish a general sales advantage; fairness experiments also show a cost when the change is noticed and judged deceptive.

Assess the burden of an increase for customers who are budget constrained, dependent on a service, or unable to substitute. These conditions may affect available choices, but the reviewed studies do not establish how they moderate the effect of a specific message on observed behavior. Consider actual burden and alternatives alongside average fairness ratings. Where a transition, lower-cost option, or service improvement is offered, specify who can use it and whether it offsets the loss. After launch, track complaints, churn, repeat purchases, and responses to the unit price. Existing experiments provide substantially more evidence about immediate judgments than repeated market outcomes. These are evidence-informed principles, not an estimated formula for optimal price or wording.

8. Directions for Future Research

The most direct next test is a factorial field design that crosses truthful cause, controllability, product-level fit, and amount while holding the actual price path constant. The existing literature establishes that these features matter in experiments but rarely separates them under real customer stakes. Pre-register measures of fairness, inferred motive, understanding, repurchase, and attrition. Test whether effects vary with market power, prior relationships, or customer burden. A comparison of cost, market, quality, and candid profit reasons should permit negative as well as beneficial effects and should independently verify the stated reason.

Disclosure work should vary timing, sender, evidence, level of detail, and exact economic presentation. Researchers could ask why early notice helps: does it change inferred motives, give customers time to adjust, or simply reduce surprise? They could also test whether an honest unit-price comparison reduces the deception penalty of downsizing and how it affects short-term sales. Comparisons should hold quantity, total expenditure, and product value constant where feasible and report comprehension as well as fairness. Repeated increases and post-discovery responses require longitudinal designs, because the existing scanner, experimental, and attrition studies observe different windows of the same process.

Behavioral and distributional evidence is especially thin. Link communication exposure to verified purchases, renewal, churn, complaints, and welfare; distinguish an inability to switch from genuine acceptance. Test essentials against discretionary products, public fees against private sellers, and settings with different norms of firm entitlement and customer vulnerability. Use transparent secondary coding and independent screening or extraction in future updates.

9. Limitations

The four-database title-and-abstract searches had no publication-year restriction and were limited to peer-reviewed English-language journal articles. Relevant work outside those indexes and search fields, or in other languages and formats, may be absent.

As a single reviewer made eligibility decisions, extracted findings, and assigned synthesis roles, design families, and themes, reviewer agreement and error rates cannot be estimated. Any eligible studies missed during screening would affect coverage; a mistaken extraction or classification could change the emphasis of the narrative synthesis. These materials do not independently verify that the review captured every eligible study or extracted every finding correctly. Further, the single-reviewer process limits confidence in the completeness of coverage and consistency of classification; it does not erase the distinct strengths and weaknesses of the included studies. Five records could not be assessed from full text. Abstracts or indexed excerpts were available for three; none of the five was included in the synthesis. Their findings therefore do not support the review's conclusions. The dataset also marks 35 full-text exclusions as excluded at title-and-abstract screening.

The single-theme and primary-design labels simplify publications that examine several mechanisms, studies, or outcomes; no second reviewer resolved possible alternative labels. The theme counts reflect dominant-theme assignments; they do not measure how often each mechanism appears across the literature. Four conceptual records contribute ideas rather than independent effect estimates. Studies use different samples, price changes, messages, and outcomes. The extracted results cannot support a meaningful pooled effect. Randomized experiments give the clearest evidence on immediate fairness judgments, although many also rely on hypothetical scenarios or stated intentions. Observed choice, demand, and attrition come from fewer and more varied designs, each with its own identification and setting limits. The included studies do not validate a complete causal pathway from communication through appraisal and fairness to later customer behavior; proposed mediation and long-run feedback require direct tests.

10. Conclusion

A higher price is an economic decision that consumers also judge as a social action. They consider the size of the loss, the firm's reason for imposing it, and whether that reason fits the affected offering. In the experiments reviewed here, plausible reasons and explanatory fit repeatedly shape immediate fairness and motive judgments. A credible account of a real cost may reduce suspicion, but a favorable judgment does not remove the customer's financial burden. Disclosure and framing are more ambivalent. They can change what customers notice and how they evaluate the increase, yet a favorable rating can coexist with misunderstanding of a profit change. Noticed downsizing can also carry a fairness penalty when consumers perceive deception.

Behavioral results answer narrower questions in fewer and more varied settings. One randomized subscription test found lower attrition after a truthful market-based reason, but that result comes from a particular service and message. Scanner and modeled-demand studies examine responses to specific products, package sizes, and pricing tactics; they do not test how explanations work across markets. Fairness ratings and stated intentions therefore cannot be converted into a general retention or sales effect.

The proposed framework organizes findings from these separate studies. Experimental support is strongest for immediate appraisals. The links from those appraisals to later behavior are less direct, and feedback from market response remains conceptual. None of the included studies tests the full sequence from communication through judgment to behavior. Future studies could follow customers through repeated real price changes, measuring what they understand, how fair they judge each change, and what they subsequently do. Such designs would clarify when an improved immediate evaluation carries over into a verified customer decision.

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Appendix A. Final Search Strings and Search Log

The five title-and-abstract searches are reproduced below. Table A1 reports the corresponding record counts by database and search set. Search A focuses on firm communication, B on consumer responses, C on reference-price and presentation terms, D on cost and profit information, and E on explicit price-increase explanations. Database-by-search counts and the field implementation follow the strings.

Search A

AB, TI(("price increase" OR "price increases" OR "price hike" OR "price hikes" OR "price rise" OR "price rises" OR "higher prices" OR "raising prices" OR shrinkflation) AND (communicat* OR announc* OR notif* OR disclos* OR transparen*) AND (firm* OR compan* OR retailer* OR seller* OR brand* OR manufacturer* OR merchant* OR marketer* OR "service provider*"))

Search B

AB, TI(("price increase" OR "price increases" OR "price hike" OR "price hikes" OR "price rise" OR "price rises" OR "higher prices" OR "raising prices" OR shrinkflation) AND ("price fairness" OR "price unfair*" OR trust OR loyalty OR satisfaction OR "purchase intention*" OR "consumer response*" OR "customer response*" OR "price perception*" OR "consumer perception*" OR "customer perception*" OR react*))

Search C

AB, TI(("price increase" OR "price increases" OR "price hike" OR "price hikes" OR "increase in price*") AND ("reference price*" OR "list price*" OR "price display" OR "price presentation" OR discount* OR framing))

Search D

AB, TI(("consumer price response*" OR "customer price response*" OR "price increase*" OR "price hike*" OR "price perception*") AND ("cost information" OR "profit information" OR "cost disclosure" OR "profit disclosure" OR "cost justification" OR "price justification" OR "costs and profits" OR "cost and profit*"))

Search E

AB, TI(("price increase*" OR "price hike*" OR "increase in price*") AND ("price increase justification*" OR "price increase explanation*" OR "justification for price increase*" OR "explanation for price increase*" OR "reason* for price increase*" OR "justify price increase*" OR "explain price increase*"))

Table A1. Records Retrieved before Deduplication by Database and Search Set

Database	A	B	C	D	E
ABI/INFORM Complete	140	386	143	6	4
Academic Search Complete	67	226	83	1	4
Business Source Premier	192	451	168	11	11
ProQuest Research Library	55	165	58	4	0

Note: All searches had no publication-date restriction; peer reviewed, English, academic journal/article; no full-text limiter. A–E counts sum to 2,175 before deduplication. AB, TI denotes title and abstract. EBSCO implementation of each concept block: (TI(terms) OR AB(terms)).

Appendix B. Study-Level Evidence Tables

Tables B1–B7 summarize the 48 publications by dominant theme, including each paper’s design, setting, sample size where available, main finding, and key limitation. Each publication is counted once, even when it informs more than one theme. The tables distinguish direct tests of communication from related and conceptual evidence.

Table B1. Cost and Causal Justification and Attribution

Study and Evidence Role	Design, Setting, and Sample Size	Price Action or Communication	Finding and Evidentiary Limit
Campbell, C. L. (2015) Core direct evidence	Coffee-shop experiment; $N=512$. Observed choice among 52 returning customers.	No reason, tax, or fair-trade explanation for a coffee-price increase.	Fair-trade reasoning improved fairness and intentions relative to tax or silence. Observed choice came from 52 returning participants.
Damavandi, H. (2026) Core direct evidence	Canadian self-storage field test, $N=1,626$ customers (694 exits); two scenario studies, $N=966$ and 1,640.	Truthful cost, market, or quality reasons in storage-price notifications.	A market reason reduced field exit hazard by 29.5% versus silence; average cost and quality effects were nonsignificant. Scenario mechanisms differed.
Homburg, C. (2005) Core direct evidence	German university scenarios; $N=80$, 80, and 174 students.	Fair cost-pressure versus unfair profit motive, with satisfaction varied.	Satisfaction buffered stated repurchase and motive inference; fair motives helped especially satisfied customers. Restaurant choices were hypothetical.
Huangfu, G. (2012) Core direct evidence	Chinese student, employee, and rural samples; $N=600$.	Cost, demand, ownership, and public-interest reasons in scenarios.	Cost-based increases were fairer than demand-based ones; monopoly and state ownership intensified objections to demand-based pricing.
Campbell, M. C. (2026) Core direct evidence	Eight U.S. online and university experiments; combined $N=4,074$.	Cost changes, customer vulnerability, welfare impact, and inferred harm.	Inferred customer harm predicted unfairness even with rising seller costs; vulnerability increased harm judgments. Most outcomes were hypothetical.
Gielissen, R. (2008) Core direct evidence	Dutch online experiment; $N=307$ respondents.	Wholesale costs, demand, conservation, profits, and distributive cues.	Wholesale-cost pass-through was fairer than demand pricing; conservation motives also improved judgments over personal profit in vignettes.
Martin, W. C. (2009) Core direct evidence	U.S. retail scenarios; main $N=545$ (pretest $N=171$).	Cost, profit, or no explanation crossed with two increase sizes.	Cost outperformed profit at 7.1%; at 42.9% both reasons outperformed silence but did not differ. Loyalty no longer buffered fairness at the larger rise.
Bolton, L. E. (2006) Core direct evidence	Four experiments, $N=70$, 64, 50, and 198; replication $N=19$. Setting unreported.	Aligned versus nonaligned cost-to-price placement for goods and services.	An increase applied to the cost-generating offering was fairer; services were preferred for overhead costs that could not be directly aligned.
Woisetschläger, D. M. (2008) Core direct evidence	German utility mail experiment; $N=254$ (pretest $N=30$).	Procurement-cost reason versus personalized tariff or energy-saving service.	Cost information alone did little for utility evaluations; value-adding options fared better. No manipulation significantly raised loyalty intention.
Ferguson, J. L. (2011) Core direct evidence	U.S. grocery scenarios after hurricanes; $N=173$ and 214.	Cost rationale and breadth of affected products after a cost shock.	A broad increase beyond affected products elevated suspicion, negative affect, and unfairness, with greater complaint and exit intentions.
Chang, S.-S. (2012) Core direct evidence	Restaurant scenario; $N=153$ (three pretests).	No reason versus high-, moderate-, or low-control explanations.	Low- and moderate-control reasons improved attitudes over a high-control reason or silence. The moderate condition combined two reasons.
Kyle, G. T. (1999) Core direct evidence	Pittsburgh event-fee survey; $N=468$, with 434 reference-price responses.	Cost-of-service message plus optional gain/loss and beneficiary frames.	Cost information raised expected event fees; added frames made no independent difference. No actual purchase or fairness outcome was measured.

Study and Evidence Role	Design, Setting, and Sample Size	Price Action or Communication	Finding and Evidentiary Limit
Lu, Z. (2020) Core direct evidence	U.S. orange-juice scanner data (eight UPCs); four experiments, combined $N=892$.	Cost, demand, or no reason crossed with retailer market power.	Low-power firms benefited more readily from a cost reason; stating lack of control removed the high-power fairness disadvantage in scenarios.
Koschate-Fischer, N. (2016) Core direct evidence	Four supermarket-product experiments; $N=128, 575, 141$, and 176 .	Charitable donation amount, price increase, and CSR reputation.	A donation at least matching the increase helped when the firm's CSR history supported it; small donations could backfire under poor fit or reputation.
Vaidyanathan, R. (2003) Core direct evidence	Grocery and appliance scenarios; $N=136, 51$, and 80 students.	Externality, controllability, seller cost, and market price.	External, unavoidable seller cost increases were judged fairest; matching a market rise without an actual cost increase did not justify the action.

Table B2. Transparency, Disclosure, and Source Credibility

Study and Evidence Role	Design, Setting, and Sample Size	Price Action or Communication	Finding and Evidentiary Limit
Kachelmeier, S. J. (1991) Core direct evidence	U.S. posted-offer laboratory markets; $N=90$ students in nine markets.	Marginal-cost, profit, or no disclosure after a tax shock.	Cost disclosure sped price convergence; profit disclosure reduced prices, volume, and efficiency on average. One profit market was an exception.
Franciosi, R. (1995) Core direct evidence	U.S. posted-offer laboratory markets; $N=144$ in 12 markets.	Cost, profit, or no disclosure in repeated posted-offer markets.	Profit information initially delayed adjustment, but transaction prices eventually converged. Strategic demand withholding remains possible.
Ferguson, J. L. (2013) Core direct evidence	Two U.S. experiments; $N=57$ and 195 adults.	Firm versus external announcement; explanation detail and cost alignment.	Firm disclosure raised immediate fairness. Detailed aligned reasons helped a 20% rise, but a limited reason beat aligned detail at 2%.

Table B3. Reference-Price, Numeric, and Promotional Framing

Study and Evidence Role	Design, Setting, and Sample Size	Price Action or Communication	Finding and Evidentiary Limit
Zhang, S. (2021) Supporting/mechanism evidence	Four Chinese fruit-shop experiments; $N=212, 336, 267$, and 148 buyers.	Discounted-quantity limit as a visible price-increase point.	The increase point could become a purchase target and reduce quantity purchased, even with larger discounts.
Prakash, D. (2022) Supporting/mechanism evidence	European electronics field test: 181 products, 2,878 product-days; online replication $N=809$.	Demand above or below an estimated internal reference price.	Prices above the reference point reduced field demand asymmetrically; effect weaker online.
Malc, D. (2016) Supporting/mechanism evidence	Convenience-sample experiment; $N=288$ respondents.	Alternative comparison prices shown with an increase.	Reference-price conditions did not differ significantly in fairness; fairness correlated with stated shopping, switching, and word of mouth.
Park, S. (2023) Core direct evidence	Amazon observational data: 1,739 vacuum cleaners; 482,964 product-days.	A displayed list price reframing a higher selling price as a discount.	Sales rank improved after the combined list-price and selling-price rise relative to pure increases.
Homburg, C. (2010) Supporting/mechanism evidence	Clothing and entertainment scenarios; analytic $N=193$ and event-visitor $N=163$.	Percentage versus absolute presentation and same-category budgets.	Percentage framing lowered stated future purchase likelihood. Lower income amplified the effect of earlier price increases, but income-by-framing interaction unsupported.
Rikala, V.-M. (2022) Core direct evidence	Five U.S. online retail experiments; $N=300, 300, 150, 150$, and 300 .	Price and cost changes shown as percentages or dollar amounts.	Dual-percentage framing raised fairness partly through base-value neglect; no advantage when price percentage exceeded cost percentage.

Study and Evidence Role	Design, Setting, and Sample Size	Price Action or Communication	Finding and Evidentiary Limit
Liu, H.-H. (2015) Core direct evidence	Six Taiwanese experiments, including a pilot; combined $N=755$.	Economically identical free-gift versus bundle promotion wording.	Framing shifted assigned item prices and subsequent perceived increases after promotion expiration.
Ma, J. (2026) Supporting/mechanism evidence	Five hotel and travel scenarios; $N=240, 260, 379, 359$, and 240 .	Linear versus cyclical temporal prime around a disclosed change.	A linear prime raised immediate intention after a small, recent increase through future-price expectations; no effect with distant or large changes.

Table B4. Shrinkflation, Implementation Tactic, and Unit-Price Salience

Study and Evidence Role	Design, Setting, and Sample Size	Price Action or Communication	Finding and Evidentiary Limit
Çakir, M. (2014) Core direct evidence	Chicago ice-cream scanner data; 609 households, 14,522 purchases.	Observed price, package size, and promotion variation for ice cream.	Price elasticity exceeded size elasticity by roughly fourfold; downsizing prompted substitution toward larger packs in one scanner-data market.
Kim, I. K. (2024) Core direct evidence	South Korean milk data; 41,488 region-retailer-weeks across 624 markets and 75 products.	Observed beverage downsizing versus modeled equivalent price rise.	Modeled demand was about 4.6 times more sensitive to the explicit rise; the counterfactual rests on single-product market assumptions.
Evangelidis, I. (2024) Core direct evidence	Five packaged-goods experiments; $N=501, 301, 1,000, 4,010$, and $1,202$.	Economically equal downsizing versus direct price rise; size notice.	Downsizing looked more unfair because it might go unnoticed; explicit package-size notice eliminated the gap in one vignette study.
Kachersky, L. (2011) Core direct evidence	U.S. potato-chip experiment; $N=155$ consumers.	Noticed potato-chip downsizing versus a direct price increase.	Downsizing raised persuasive-intent inference, especially profit-motive inference among knowledgeable consumers; all participants noticed the change.
Franc-Dąbrowska, J. (2026) Conceptual/background	Conceptual synthesis of Japanese secondary evidence; no empirical sample.	Secondary Japanese surveys, point-of-sale examples, and cases.	Favors transparent increases over concealed downsizing; heterogeneous secondary evidence provides no independent causal effect.
Wan, X. (2026) Core direct evidence	U.S. canned-tuna scanner data; roughly 30,000–50,000 stores, with 363 regulated and 465 matched control counties.	Unit-price disclosure and price-per-ounce sensitivity for canned tuna.	Estimated elasticity was -0.26 with disclosure versus -0.07 without; matched policy data cannot eliminate unobserved market differences.

Table B5. Relationship, Reputation, Firm, and Contextual Moderators

Study and Evidence Role	Design, Setting, and Sample Size	Price Action or Communication	Finding and Evidentiary Limit
Kwak, H. (2015) Supporting/mechanism evidence	U.S. retailer panel; 63,066–123,807 purchase incidences per category. Validation $N=599$; three experiments $N=157, 327$, and 288 .	Humanized brand cues and relationship norms around price changes.	Humanized brands showed greater absolute panel elasticities and less favorable experimental fairness for increases, conditional on brand traits and norms.
Chavanne, D. (2022) Supporting/mechanism evidence	U.S. price scenarios; analytic $N=1,025$.	Business type, price magnitude, and COVID framing; same lost-revenue reason.	Smaller and hardware-store increases were fairer overall; message tone had no main effect, and magnitude varied by setting and frame.

Study and Evidence Role	Design, Setting, and Sample Size	Price Action or Communication	Finding and Evidentiary Limit
Sipilä, J. (2022) Supporting/mechanism evidence	German retail customers and two further studies; analytic $N=3,286, 384$, and 405.	CSR engagement and expectations before a price increase.	Higher CSR intensified the fairness decline through violated expectations, particularly among consumers low in CSR skepticism.
Xia, L. (2022) Supporting/mechanism evidence	U.S. stress and nostalgia studies; core $N=257, 248$, and 283; supplemental $N=285$ and 224.	Collective stress, nostalgia, empathy, and inferred seller motives.	Core surveys linked stress to nostalgia and benevolent-motive fairness; supplemental experiments supported downstream nostalgia effects, not a firm message effect.
Helm, S. V. (2013) Supporting/mechanism evidence	Hypothetical airline scenario; analytic $N=250$.	Favorable versus unfavorable prior airline reputation without a reason.	Reputation reduced negative-motive inference and raised fairness and stated purchase intention in a hypothetical airline scenario.
Mayer, V. F. (2014) Supporting/mechanism evidence	Brazilian service scenario; $N=248$ (pretest $N=126$).	Service relevance and dependence with a negatively motivated rise.	High relevance and dependence raised unfairness, anger, and retaliation intentions; mediator variables were measured, not randomized.

Table B6. Consumer Response Without a Communication Treatment

Study and Evidence Role	Design, Setting, and Sample Size	Price Action or Communication	Finding and Evidentiary Limit
Paswan, A. K. (2014) Supporting/mechanism evidence	Survey of 351 drivers in a southwestern U.S. city.	Awareness and paid price of gasoline increases; no seller explanation.	Noticing and paying more for gasoline correlated with anxiety and reported lifestyle, vehicle, and transport adjustments.
Martinčić, M. (2022) Supporting/mechanism evidence	Consumer-product convenience survey; $N=214$.	Self-reported product response to an increase; no message test.	Substitution and loyalty were almost evenly split (105 versus 109); results came from a broad convenience survey.
Calabuig, F. (2014) Supporting/mechanism evidence	Five Spanish sport facilities; $N=673$ before and 679 after a fee increase.	Municipal sport-fee increase with one-month notice but no message comparison.	Perceived value and future intentions fell after a real increase; different before-and-after samples limit causal interpretation.
Huber, O. W. (2011) Supporting/mechanism evidence	Three Swiss simulated-shopping experiments; $N=30, 20$, and 36.	Frequency of increases in simulated shopping; no seller explanation.	More frequent increases raised perceived aggregate inflation despite accurate item judgments; small artificial shopping tasks limit transfer.
Hobday, M. (2016) Supporting/mechanism evidence	Hypothetical alcohol-purchase scenarios; $N=831$ Australian adults.	Hypothetical 10%, 25%, and 50% alcohol-price increases.	Stated cheaper-product substitution increased with magnitude; these within-person scenarios did not observe purchases or communication.
Adomi, E. E. (2005) Supporting/mechanism evidence	Nine Nigerian cybercafé operator responses; no customer survey.	Coordinated cybercafé increase and subsequent customer complaints.	Seven of nine operators reported lower patronage and all nine later cut prices; no customer survey or control group was available.

Table B7. Conceptual Strategy and Preimplementation Assessment

Study and Evidence Role	Design, Setting, and Sample Size	Price Action or Communication	Finding and Evidentiary Limit
Ferguson, J. L. (2014) Conceptual/background	Conceptual paper; no empirical sample.	Conceptual transparency, familiar tactics, rebates, matching, and downsizing.	Five managerial approaches proposed from earlier research; concealment claims warrant caution.
Crompton, J. L. (2011) Conceptual/background	Conceptual paper; no empirical sample.	Conceptual comparison prices, premium anchors, and discount framing.	Prior leisure examples suggest credible external anchors may shift acceptable fees.
Crompton, J. L. (2015) Conceptual/background	Conceptual paper; no empirical sample.	Conceptual advance notice, transitions, cost explanation, and fee anchors.	The synthesis recommends gradual increases and clear transitions for public leisure fees.

Study and Evidence Role	Design, Setting, and Sample Size	Price Action or Communication	Finding and Evidentiary Limit
Crompton, J. L. (1994) Supporting/mechanism evidence	Galveston festival survey; N=471 visitors.	Festival-fee diagnostic using attendance, substitutes, budget, and value.	The case triangulated likely resistance before a \$6-to-\$8 increase; post-increase demand and message effects were not reported.