



# Lifting the Veil: Essays on Firm Transparency and Consumer Behavior

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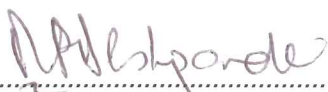
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**Lifting the Veil: Essays on Firm Transparency and  
Consumer Behavior**

presented by **Bhavya Mohan**

candidate for the degree of Doctor of Business Administration  
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**Lifting the Veil: Essays on Firm Transparency and Consumer Behavior**

A dissertation presented

by

Bhavya Mohan

to

The Marketing Unit at Harvard Business School

in partial fulfillment of the requirements

for the degree of

Doctor of Business Administration

in the subject of

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## Lifting the Veil: Essays on Firm Transparency and Consumer Behavior

### ABSTRACT

This research examines the effects of firm transparency on consumer behavior. Three essays investigate how consumer behavior changes when firms are transparent about costs, wages, and promotional strategies. Essay one investigates when and why firms benefit from revealing confidential unit cost information to consumers. A natural field experiment conducted with an online retailer suggests that cost transparency can boost sales. Subsequent controlled lab experiments replicate this basic effect and provide evidence for why it occurs. Essay two examines whether consumer behavior is influenced by the disclosure of a firm's pay ratio - the ratio of the total compensation of the CEO to the average annual compensation of all other employees. Pilot field data and a series of experiments show that pay ratio disclosure affects the purchase intentions of a subset of consumers, via perceptions of wage fairness. Essay three examines how marketing offers that are framed as percentages can confuse consumers, due to highly non-linear impacts in terms of actual value. Three lab studies and one field experiment show that while even highly numerate consumers are prone to error, the transparent provision of rate information can help consumers evaluate offers more accurately.

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## INTRODUCTION

Consumers need protection from firms who deliberately exploit their weaknesses. At the same time, firms are obligated to maximize shareholder returns. Thus, tensions inevitably arise between firms' profit goals and consumers' well being. My research examines how transparency can begin to reconcile the tensions that are inherent between the marketing of goods for financial profit, and the well-being of consumers, employees, and society-at-large. My three streams of research specifically investigate how consumer behavior changes when firms are transparent about costs, wages, and promotional strategies. Over the course of my research career, I hope that my scholarly work will incentivize firms to act more openly and fairly, simply because it is in their own long-term best interest to do so.

In my first stream of research, I examine the impact of cost transparency on consumer behavior. A firm's costs are typically tightly-guarded secrets. Essay one of my dissertation investigates when and why firms benefit from revealing confidential unit cost information to consumers. A natural field experiment conducted with an online retailer suggests that cost transparency can boost sales. Subsequent controlled lab experiments replicate this basic effect and provide evidence for why it occurs: just as interpersonal disclosure of information increases trust, these studies suggest that when a firm discloses cost information, it makes consumers trust the firm more, in turn boosting their purchase interest.

In my second stream of research, I examine the impact of wage transparency on consumer behavior. Starting on January 1, 2017, public companies in the United States will be required to disclose their pay ratios: the ratio of the total compensation of the CEO to the average annual compensation of all other employees. Prior research examining consumer expectations of

equity has not addressed such forms of signaling wage fairness (or lack thereof). Essay two of my dissertation examines whether consumer behavior is influenced by such disclosure. Pilot field data and a series of experiments show that pay ratio disclosure affects the purchase intentions of a subset of consumers, via perceptions of wage fairness. The disclosure of high pay ratios can diminish firm performance and decrease willingness to pay.

In my third stream of research, I examine the impact of promotional transparency on consumer behavior. Essay three of my dissertation examines how marketing offers that are framed as a “percentage change” in consumer cost vs. benefit can confuse consumers, due to highly non-linear impacts in terms of actual value. Three lab studies and one field experiment show that while even highly numerate consumers are prone to error, the provision of salient rate information can reduce consumer error. In most retail contexts, however, rate information resulting from a percentage promotion is not calculated out for consumers; it is not mandatory when accompanying percentage promotions. If a firm is to be truly transparent in its marketing offers that promote a percentage change in cost or benefit, the firm should focus on clearly communicating the impact of an offer on the underlying rate.

## Lifting the Veil: The Benefits of Cost Transparency

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A firm's costs are typically tightly-guarded secrets. However, across a field study and five laboratory experiments we explore when and why firms benefit from revealing unit cost information to consumers. A natural field experiment conducted with an online retailer suggests that cost transparency boosts sales. Three subsequent controlled lab experiments replicate this basic effect, showing that revealing unit cost information increases purchase intention, relative to weaker forms of transparency (Studies 2-3). Study 4 provides evidence for why the effect occurs: just as interpersonal disclosure of information increases trust towards another person, cost transparency increases trust in a firm, in turn boosting consumer purchase interest.

*INTRODUCTION*

Cost transparency refers to the voluntary disclosure of the costs associated with producing a good or providing a service. It has been studied in operations and marketing within the context of supplier-firm relationships, whereby the two-way sharing of cost information between these parties facilitates collaboration on cost reduction measures (Lamming, Caldwell, Harrison, & Phillips, 2002; Zhu, 2004). In this paper, we explore cost transparency within a different context: customer-firm relationships. Although information on the costs associated with making a good is not typically shared with customers, we test whether disclosing it may, at least in some situations, confer benefits to the firm.

In the present research, we operationalize cost transparency as the practice of revealing the unit costs of production, disclosing variable costs and apportioning relevant fixed costs. Through four studies, we provide evidence that cost transparency can increase a consumer's interest in buying from the firm. First, in a natural experiment conducted in the field with an online retailer, sales increased when the variable costs to produce the good (a leather wallet) were revealed: cost transparency led to an increase in daily unit sales of the wallet. Subsequent controlled lab experiments replicate this basic effect (Study 2) and show that cost transparency is more effective at boosting purchase interest than weaker forms of transparency, such as operational transparency (Study 3). Study 4 provides evidence for why it occurs: just as interpersonal disclosure of information increases trust towards another person, cost transparency increases trust in a firm, in turn boosting consumer purchase interest.

*Cost, Operational, and Price Transparency*

Although there are multiple ways of operationalizing cost transparency, broadly, cost transparency refers to the disclosure of the costs associated with producing a given product or service. In its strong form, cost transparency entails divulging all costs associated with each component of producing a good, while also clearly indicating the total cost (i.e., the sum of all cost components). We refer to this strong form as “total cost transparency” (Figure 1, Panel c). A slightly weaker form of cost transparency entails simply divulging the costs attributable to each production factor, without explicitly highlighting the total cost. We refer to this form as “disaggregated cost transparency” (Figure 1.1, Panel d).

(Insert Figure 1.1 about here)

Operational transparency refers to a firm’s disclosure of its operating processes to customers (Buell, Tsay, & Kim, 2014). Research suggests that consumers prefer service web sites that are operationally transparent relative to those that are not (Buell & Norton, 2011). For example, the travel site Kayak.com is beloved in part because of its operational transparency; the site discloses which airline is being searched and updates the results throughout the search process. Such transparency increases consumers’ perception of the effort required to create the product, in turn heightening their sense of gratitude and willingness to pay (Buell & Norton, 2011; Gershoff, Kivetz, & Keinan, 2012; Morales, 2005). Consistent with this line of thinking, voluntary disclosure of social and environmental impacts, such as greenhouse gas emissions, can boost a firm’s market share (Kalkanici, Ang, & Plambeck, 2013). Similarly, information on production processes can also affect consumers’ perceptions of product quality. For example, fudge tastes better when consumers are told that it was produced using an expensive (as opposed to inexpensive) machine (Chinander & Schweitzer, 2003). Beyond the private sector, operational transparency has even been shown to increase trust in the government (Buell, Norton, & Porter,

2016).

Like operational transparency, cost transparency can entail revealing the steps (or processes) involved in the production of a good or service. This is because delineating the costs of each production step necessitates its revelation in some form. For example, the present experiments reveal costs through infographics that include a pictorial representation of each production factor. Price transparency refers to disclosing the beneficiaries of a product's revenues; for example, by dividing a price into gross retail proceeds, royalties, and taxes (Carter & Curry, 2010). Similarly, price partitioning refers to revealing the price of the component parts of a product; for example, by dividing a product's price into its base price and shipping and handling (Bertini & Wathieu, 2008; Morwitz, Greenleaf, & Johnson, 1998). Price transparency and price partitioning have both been found to increase purchase intentions, and to do so via a cognitive process (Morwitz et al., 1998). Specifically, by dividing a price into several sub-components, each of which is necessarily smaller than the total price, small prices are made salient. The result is that these tactics cause consumers to perceive prices to be relatively low, in turn increasing purchase intentions.

The logic behind the effectiveness of price transparency suggests that cost transparency might *decrease* purchase intent. Dividing prices into sub-components decreases perceived price. Therefore, this perspective would predict that breaking down the costs of production might decrease the perceived cost of making the good, thereby increasing the perceived profit margin. To the extent that consumers think that a firm is earning a large profit on each unit sold, they may perceive the price to be unfair (Kahneman, Knetsch, & Thaler, 1986). Bolton, Warlop and Alba (2003) find that cueing “total” costs by asking participants to estimate line items on a firm’s income statement leads to a lower perceived profit margin and higher ratings of a firm’s overall

price fairness. It is important to note, however, that merely cueing customers to estimate firm-level costs has not been shown to increase purchase intention (Bolton & Alba, 2006; Bolton, Warlop, & Alba, 2003). To our knowledge, no extant research in marketing indicates such a strategy would serve to actually increase a firm's sales. As detailed in the next section, we posit that cost transparency operates through an affective process by which the disclosure of variable costs increases trust in the firm, in turn boosting purchase interest.

### *Cost Transparency, Firm Disclosure, and Trust*

We suggest that cost transparency is a form of disclosure by a firm to the consumer, since cost breakdowns are usually confidential, proprietary knowledge. A substantial body of work on the social psychology of disclosure suggests that disclosure heightens relationship quality (Laurenceau, Barrett, & Pietromonaco, 1998). Self-disclosure has been studied not only in the context of interpersonal relationships, but also in the context of human-computer interactions. For one, the mere look and feel of a website has been found to affect consumers' willingness to disclose (John, Acquisti, & Loewenstein, 2011).

Importantly, increased disclosure is associated with a higher level of trust (Wheless & Grotz, 1977). The relationship works both ways – an individual's ability to self-disclose is also a product of trust in another individual (Steel, 1991). Moreover, those who choose to hide personal information are seen as less trustworthy than those who reveal personal information, even if the information revealed is particularly unsavory (John, Barasz, & Norton, 2015).

In the context of business relationships, trust has been defined as the willingness to rely on an exchange partner in whom one has confidence (Moorman, Deshpande, & Zaltman, 1993; Morgan & Hunt, 1994). A tactile experience with a product can build trust during an in-person

retail experience (Lee & Turban, 2001). Retailers can lose trust through their pricing practices; for instance, dynamic pricing and price segmentation tactics, when discovered by consumers, can diminish evaluations of trust and repurchase intentions (Grewal, Hardesty, & Iyer, 2004; Haws & Bearden, 2006). We predict therefore that disclosing detailed cost information can increase trust between a customer and a firm.

### *Overview of Studies*

Across four studies, we test the effect of cost transparency on consumer purchase behavior. We begin by presenting the results of a natural experiment conducted in the field with an online retailer. Cost transparency led to an increase in unit sales for the target products. Three subsequent controlled lab experiments replicate this basic effect, showing that revealing unit cost information increases purchase intention, relative to weaker forms of transparency (Studies 2-3). Study 4 provides evidence for why it occurs: just as interpersonal disclosure of information increases trust towards another person, cost transparency increases trust in a firm, in turn boosting consumer purchase interest. We conclude by discussing the implications of these findings for managers, as well as limitations and opportunities for future research.

### *STUDY 1: FIELD EVIDENCE*

On December 2, 2013, a privately-held online retailer launched a holiday gift shop with a single email to its mailing list, promoting a leather wallet offered in five colors (burgundy, black, grey, bone, and tan) and priced at \$115.00. At the end of January, in an effort to boost post-holiday sales, the retailer decided to add a cost transparency infographic to the online product



detail pages of each of the wallet's five color combinations. As the wallets differed only in color, the company intended to use the same infographic for every wallet in the line.

But what the company intended to do was not what actually happened. Serendipitously (at least for us!), the company inadvertently failed to introduce the infographic for two of the wallet colors (bone and tan). Thus the infographic was implemented for only three of the five wallet colors (burgundy, black, and grey), a mistake that was overlooked for five weeks, creating a natural experiment to test the impact of cost transparency on sales.

### *Procedure*

*Operationalization of cost transparency.* The company deployed total cost transparency. Specifically, along with the aggregated cost to produce the wallet, the infographic detailed the costs in a disaggregated manner; detailing the specific costs associated with each of the materials and processes involved in the production of the wallet, as follows: leather (\$14.68), construction (\$38.56), duties (\$4.26) and transportation (\$1.00). The infographic also included additional benchmark information (which we control for in the subsequent study), stating that the wallet had a 1.9x markup, compared to the 6x markup charged by a competitor.

*Empirical Approach.* The inadvertent provision of cost information for some, but not all, of the wallet colors served as an exogenous shock that created sets of comparable treatment (cost transparency) and control (no cost transparency) products (i.e., wallets). This treatment provides a conservative test of cost transparency, since customers browsing multiple wallet colors may have been exposed to the infographic and (correctly) inferred that the process and costs applied across color combinations. While the benefits of the infographic likely accrued to both groups, our identification comes from the fact that every customer who browsed wallets in the treatment

group was exposed to the infographic, while customers who browsed wallets in the control group may not have been.

We use a difference-in-differences approach to compare the daily sales between the treatment and control groups before versus after the infographic was introduced. By doing so, we isolate the effect of cost transparency on the daily count of wallets sold in each category. We analyze the sales performance of five color combinations over a 92-day period ( $N = 460$ ), starting with the launch of the site on December 2, 2013 and ending on March 6, 2014. The infographic was introduced on January 28, 2014.

We estimate the following linear fixed effect specification, using a Newey West estimator for standard errors that accounts for autocorrelation and heteroskedasticity within colors with a small number of products (Newey & West, 1987; Schaffer, 2010).<sup>1</sup> We do not use clustered standard errors, since clustering requires a large number of clusters – far more than the five product colors we have in our dataset – to approach the true variance of the error term. Using a Newey West estimator enables us to leverage the fairly long panel of sales data we have on each product color to generate a consistent estimator that accounts for the autocorrelation and heteroskedasticity within each product. This is particularly important when analyzing sales data of this type.

$$COUNT_{c,t} = f \left( \alpha_0 + \alpha_1 POST_t + \alpha_2 POST_t \times TREATMENT_c + \alpha_3 VISITS_{c,t} + \alpha_4 VISITS_{c,t}^2 + \alpha_5 NOVISITS_{c,t} + \alpha_6 NOVISITS_{c,t-1} + \alpha_7 NOSALE_{c,t-1} + \beta_c + \epsilon_{c,t} \right) \quad (1)$$

In the specification above,  $COUNT_{c,t}$  represents the count of items sold for color  $c$  on day  $t$ .  $POST_t$  is a dummy variable denoting observations after the introduction of the infographic.

While the cost transparency treatment is subsumed by the color fixed effect,  $\beta_c$ ,  $POST_t \times$

xxx\_\_\_\_\_

<sup>1</sup> We note that our results are substantively similar when coefficients are measured using a fixed effects Poisson specification.

$TREATMENT_c$  is a dummy variable that specifically highlights observations in the cost transparency treatment conditions after the introduction of the infographic and is the focal independent variable of our analysis.  $VISITS_{c,t}$  and  $VISITS^2_{c,t}$  control for daily differences in visits to the product detail pages for each wallet.

A limitation of our analysis is that we were not given data on inventory levels. While the assignment of the treatment was quasi-random as described above, given the low number of products in the treatment and control groups, a stock-out could materially influence sales. To address this concern, we introduce several additional control variables. When the product is out of stock on the company's website, a "SOLD OUT" message blacks out the product on search results pages, though the product page remains accessible.  $NOVISITS_{c,t}$  is a dummy variable indicating whether no visits to the product page occurred for a specific color on a given day.  $NOVISITS_{c,t-1}$  represents whether there were no visits on the preceding day. As a final proxy for stockouts, we also introduce  $NOSALE_{c,t-1}$ , which is a dummy variable indicating whether there were no sales of a particular product on the preceding day.

(Insert Figures 1.2 and 1.3 about here)

### *Results and Discussion*

Figure 1.2 depicts the average daily unit sales per color over the period of analysis; Figure 1.3 displays this pattern in aggregate. Units are withheld from both figures to protect confidential company data. Overall, sales declined over the period, reflecting diminished demand in the post-holiday season. More importantly however, there was an interaction between time and treatment: specifically, the post-holiday sales decline was smaller in the treatment condition relative to the control condition. Cost transparency therefore helped sales by serving as a buffer against post-holiday sales decline.

(Insert Table 1.1 about here)

Table 1.1, Column (1) presents our base specification, in which we model the daily number of units sold per color combination as a function of the time period and treatment classification of the product group. While the focal variable is insignificant (coefficient = 0.523;  $p = 0.12$  two-tailed), the difference becomes marginally significant in Column (2) after controlling for the number of visits (coefficient = 0.579;  $p < 0.10$  two-tailed). In Column (3), we introduce controls for whether customers visited the product on a given day, and the results intensify (coefficient = 0.660;  $p < 0.05$  two-tailed). In Column (4), we introduce an additional control, noting whether there were no sales the prior day, and results remain marginally significant (coefficient = 0.582;  $p < 0.10$  two-tailed). Finally, in Column (5), we increment the bandwidth selection in the fully specified model. While the standard errors inflate, the results remain unchanged (coefficient = 0.582;  $p < 0.10$  two-tailed).

Using the estimates from the fully-specified model, we calculate that the cost transparency infographic increased daily unit sales on a per-color basis by 44.0% relative to average unit sales across the period of observation. Given that sales in the control and treatment conditions are not completely independent, customers may substitute a purchase of one wallet color for another. Hence, the magnitude of the effects we estimate through this difference-in-differences procedure are overstated. However, even when we account for this, cost transparency increases sales in the treatment condition by at least 22.0%.<sup>2</sup> Regardless, our estimation procedure results in valid standard errors and highlights the significance of the causal effect of cost transparency on sales. As a robustness test of the significance of the effect, we perform an additional analysis, which is described below.

xiixixii—————

<sup>2</sup> If every customer who substituted a wallet in the treatment group for a wallet in the control group arrived with strong preferences for a control color, then the actual effect size of cost transparency on sales would be 22.0%.

### *Robustness Check*

To account for the substitution patterns among wallets in the treatment and control categories, we create an additional set of specifications that capture the daily percentage of all wallets sold that correspond with the treatment category,  $TSALES\_PCT_t$ . Importantly, we note that the ratio of wallets sold in the treatment and control conditions was time-invariant prior to the introduction of cost transparency. To facilitate our analysis, we also create an aggregated set of control variables consistent with those described above, which reflect total visits and total visits squared in each category,  $TVISITS_t$ ,  $TVISITS_t^2$ ,  $CVISITS_t$ , and  $CVISITS_t^2$ , the percentage of colors in each category with no visits,  $TNOVISIT_t$  and  $CNOVISIT_t$ , the one-day lagged percentage of colors in each category with no visits,  $TNOVISIT_{t-1}$ , and  $CNOVISIT_{t-1}$ , and the one-day lagged percentage of colors in each category with no sales,  $TNOSALE_{t-1}$ , and  $CNOSALE_{t-1}$ . We estimate the following linear specification with robust standard errors:

$$TSALES\_PCT_t = f \left( \begin{array}{l} \gamma_0 + \gamma_1 POST_t + \gamma_2 TVISITS_t + \gamma_3 TVISITS_t^2 + \gamma_4 TNOVISIT_t + \\ \gamma_5 TNOVISIT_{t-1} + \gamma_6 TNOSALE_{t-1} + \gamma_7 CVISITS_t + \gamma_8 CVISITS_t^2 + \\ \gamma_9 CNOVISIT_t + \gamma_{10} CNOVISIT_{t-1} + \gamma_{11} CNOSALE_{t-1} + \epsilon_t \end{array} \right) \quad (2)$$

(Insert Table 1.2 about here)

The results of this supplemental analysis are shown in Table (1.2). In all columns, the variable of interest is the coefficient on  $POST_t$ , which indicates the change in the percentage of sales coming from colors in the treatment category after the introduction of cost transparency. In Column (1) the base specification reveals that the percentage of wallets sold in the treatment category, relative to the sales of all wallets, rises 12.6% following the introduction of cost transparency (coefficient = 0.126;  $p < 0.01$  two-tailed). Columns (2-5) reveal that this significant

increase in sales percentage is robust to the inclusion of controls for the number of times products in the treatment and control conditions were visited during the day, the percentage of colors in each category that received no visits on the focal or preceding day, and the percentage of colors in each category that resulted in no sales on the previous day. In the fully specified model, presented in Column (5), the introduction of cost transparency corresponded with a 15.7% increase in the percentage of wallets sold in the treatment condition, relative to the sales of all wallets (coefficient = 0.157;  $p < 0.01$  two-sided). These results provide converging evidence of the beneficial effects of cost transparency on sales.

### *STUDY 2: LAB DEMONSTRATION OF FIELD STUDY*

In a natural experiment involving a real online retailer, Study 1 suggests that cost transparency boosts sales. In the subsequent experiment, we investigate this finding in a controlled setting, by showing participants a modified, conservative version of the product page used in the natural experiment. Specifically, in Study 2, we test whether disaggregated cost transparency alone – without favorable competitive benchmark information – increases purchase intent, relative to a control condition in which such information is not provided.

#### *Method*

*Design and Procedure.* Participants ( $N = 322$ ,  $M_{Age} = 36.8$ , 51% male) completed this online experiment in exchange for \$0.40 and were randomly assigned to one of two experimental conditions: no transparency or cost transparency. We modeled our stimuli after the wallet product page used in the field study. In both the test and control condition, the wallet was again

priced at \$115. In the control condition, participants saw a baseline interface depicting 5 images of the wallet from different angles, the name of the product, price, style details listed as bullet points, and a mechanism for selecting the desired quantity for purchase. In the cost transparency condition, the product page also included a similar cost component break down from the field study: materials (\$11.00), hardware (\$3.60), labor (\$38.56), duties (\$4.21) and transportation (\$1.00). Notably, unlike the field study, the infographic did not include any additional benchmark information (Figure 1.4).

(Insert Figure 1.4 about here)

*Dependent Measures.* Participants indicated their willingness to buy the t-shirt by responding to the following item: “Given the opportunity, how likely would you be to purchase this product?” (7-point response scale; 1 = Not at all likely to 7 = Very likely). As an attention check, participants entered the price of the wallet and the cost of the wallet in free response fields. Participants also provided their age, gender, highest level of education, and monthly household income.

## *Results*

*Attention Checks.* The estimated price of the wallet was no different in the cost transparency condition relative to the control condition ( $M_{\text{cost}} = 106.64$ ,  $SD = 30.58$ ;  $M_{\text{control}} = 109.30$ ,  $SD = 23.67$ ;  $t(299) = 0.84$ ,  $p = 0.40$ ). The estimated total cost was greater in the cost transparency condition relative to the control condition ( $M_{\text{cost}} = 48.91$ ,  $SD = 56.90$ ;  $M_{\text{control}} = 26.29$ ,  $SD = 20.23$ ;  $t(299) = 4.65$ ,  $p < 0.01$ ).

*Willingness to buy.* Willingness to buy was greater in the cost transparency condition relative to the control condition ( $M_{\text{cost}} = 2.69$ ,  $SD = 1.81$ ;  $M_{\text{control}} = 2.26$ ,  $SD = 1.72$ ;  $t(322) = 2.20$ ,

$p < 0.05$ ).

### *STUDY 3: COST VERSUS OPERATIONAL TRANSPARENCY*

Studies 1 and 2 suggest that cost transparency can boost sales. In the next study, we examine whether this effect is driven by merely showing the operational steps to produce a good. Participants were shown a product (a t-shirt) and indicated their interest in buying it. Between-subjects, we again varied transparency: in the control condition, participants were simply told the price of the shirt. In the operational transparency condition, participants were also informed of the steps that went into making the shirt. In aggregate cost transparency condition, participants were shown total cost alone. Finally, in the full cost transparency condition, participants were informed of the costs of each component plus the total cost.

#### *Method*

*Design and Procedure.* Participants ( $N = 402$ ,  $M_{Age} = 31.3$ , 68% male) completed this online experiment in exchange for \$0.40 and were randomly assigned to one of four experimental conditions varying in transparency: no transparency, operational transparency, aggregate cost transparency, and full (aggregate + component) cost transparency (Table 1.1). In Study 2, we modeled our stimuli after several burgeoning fashion e-retailers, such as the one from the field study, which have instated cost transparency (Neilson & Mistry, 2013). To maximize the salience of our manipulations, we used a streamlined version of a shopping interface, and setting the t-shirt price at \$10. In the control condition, participants saw a baseline interface depicting a model (same gender as participant) wearing the t-shirt, the name of the



product, price, available colors and sizes, and a mechanism for selecting the desired quantity for purchase (Figure 1.5).

[Insert Figure 1.5 About Here]

In the operational transparency condition, the infographic was entitled “What goes into the production of our Women’s [Men’s] V?” and depicted six operational steps: cotton, cutting, sewing, dyeing, finishing, and transport (Figure 1.1, Panel a). In the aggregate cost transparency condition, we only noted that the total cost of producing the t-shirt was \$6.70 (Figure 1.1, Panel b). In the full cost transparency condition, we noted the costs of each component plus the aggregate cost (i.e., the total sum of the component costs) (Figure 1.1, Panel c).

*Dependent Measures.* We measured willingness to buy using the same scale as in Experiment 2. We also measured brand likeability, as a secondary measure of purchase interest. Specifically, participants indicated the extent to which they agreed with each of the following four statements on a 7 point scale (1 = strongly disagree – 7 = strongly agree): this website represents a brand that is likeable; this website represents a kind brand; this website represents a helpful brand; this website represents a friendly brand. We summed participants’ responses to these items to create a composite measure of brand attraction (Cronbach’s  $\alpha = .93$ ). Finally, we administered attention checks, asking participants to state the cost (multiple choice: \$4.70, \$5.70, \$6.70, \$7.70) and price (multiple choice: \$5, \$10, \$15, \$20) of the item they just saw.

## *Results*

*Willingness to buy.* A one way ANOVA revealed significant differences in willingness to buy as a function of the transparency manipulation ( $F(3,398) = 5.02, p < 0.01$ ; Figure 1.6). Willingness to buy was greater in the full cost transparency condition relative to the control

( $M_{\text{fullcost}} = 4.91$ ,  $SD = 1.54$ ;  $M_{\text{control}} = 4.15$ ,  $SD = 1.88$ ;  $t(201) = 3.14$ ,  $p < 0.01$ ). However, relative to the control, willingness to buy was not significantly higher in either the operational transparency ( $M_{\text{operational}} = 4.55$ ,  $SD = 1.54$ ;  $t(199) = 1.61$ ,  $p = 0.11$ ) or aggregated cost condition ( $M_{\text{aggregate}} = 4.07$ ,  $SD = 1.87$ ;  $t(197) = 0.77$ ,  $p = 0.90$ ). A one way ANOVA also revealed significant differences in attraction to brand as a function of the transparency manipulation ( $F(3,398) = 13.34$ ,  $p < 0.01$ ). Relative to control, brand attraction was significantly higher in the cost transparency condition ( $M_{\text{fullcost}} = 5.40$ ,  $SD = 1.05$ ;  $M_{\text{control}} = 4.86$ ,  $SD = 1.34$ ;  $t(200) = -5.33$ ,  $p < 0.01$ ).

[Insert Figure 1.6 About Here]

#### *STUDY 4: PROCESS EVIDENCE*

In Study 4, we replicate the basic beneficial effect of cost transparency using a different product category – chocolate. We also provide evidence of the process underlying the effect of transparency on purchase intention. Specifically, we test whether the effect of cost transparency on willingness to buy is mediated by feelings of consumer trust resulting from such disclosure (John et al., 2015; Wheelless & Grotz, 1977).

#### *Method*

*Design and Procedure.* Participants ( $N = 612$ ,  $M_{\text{Age}} = 35.2$ , 55.1% male) completed this online experiment in exchange for \$0.50, as a part of a series of 3 unrelated studies. Participants were randomly assigned to one of two experimental conditions varying in cost transparency: no transparency vs. cost transparency. In the no transparency condition, participants were shown a graphic depicting the front and back of a chocolate bar. We worked with a chocolate

manufacturer to determine cost components for a bar of chocolate (beans, sugar, cocoa butter, packaging, labor, and utilities), and to develop a sample package for an imaginary brand called “Cocoa Passion.” In the control condition, a description of the bar (70% cacao), flavors, ingredients, and nutrition facts were all listed on the packaging. In the transparency condition, on the “back” of the package, participants were also shown the actual unit cost of each of the six cost components, provided by our partner retailer: \$0.29 (beans), \$0.03 (sugar), \$1.39 (cocoa butter), \$0.17 (packaging), \$0.90 (labor), and \$0.11 (utilities). The total cost of these components, \$2.89, was also listed (See Figure 1.7).

(Insert Figure 1.7 About here)

*Dependent Measures.* On the screen directly following the depiction of the chocolate package, we measured trust using a firm-specific variation of the sliding scale used by John, Barasz and Norton (2015). Participants were told: “On a previous page, you were shown a product from the brand, Cocoa Passion. Please move the slide to assess the firm. How trustworthy would you consider the firm?” (7-point response scale: 1 (Left endpoint) = Not at all trustworthy to 7 (Right endpoint) = Very trustworthy). Subsequently, the willingness to buy measure was administered as in Studies 2-4.

## *Results*

*Willingness to buy.* Willingness to buy was greater in the cost transparency condition relative to control ( $M_{\text{cost}} = 4.27$ ,  $SD = 2.00$ ;  $M_{\text{control}} = 3.74$ ,  $SD = 2.04$ ;  $t(610) = 3.26$ ,  $p < 0.01$ ).

*Trust in Firm.* Trust in the firm was greater in the cost transparency condition relative to control ( $M_{\text{cost}} = 5.27$ ,  $SD = 1.38$ ;  $M_{\text{control}} = 4.82$ ,  $SD = 1.38$ ;  $t(610) = 4.02$ ,  $p < 0.01$ ).

*Mediation Analysis.* Cost transparency predicted both trust in the firm ( $\beta = 0.45$ ,  $p < 0.01$ )

and willingness to buy ( $\beta = 0.53, p < 0.01$ ). When trust in the firm and cost transparency were included both in the regression model predicting willingness to buy, the effect of trust in the firm remained significant ( $\beta = 0.80, p < 0.01$ ), but the effect of cost transparency was reduced to insignificance ( $\beta = 0.17, p = 0.21$ ) providing support for full mediation. We used a bootstrap procedure to construct bias-corrected confidence intervals for the indirect effect based on 5000 resamples (Preacher & Hayes, 2008). The 95% bias-corrected confidence interval excluded zero (0.18, 0.55), suggesting a significant mediation effect.

### *GENERAL DISCUSSION*

We demonstrated that cost transparency – revealing a firm’s variable costs of production – can improve customer perceptions and firm performance. Our motivating study was a natural experiment conducted in the field with an online fashion retailer. The addition of a “cost transparency” graphic denoting the costs of producing a wallet significantly increased sales over a five-week period. Subsequent controlled lab experiments replicate this basic effect (Study 2) and show that cost transparency is more effective at boosting purchase interest than weaker forms of transparency, such as operational transparency or simply divulging the total cost to make the product (Study 3). Study 4 provides evidence for why it occurs: just as interpersonal disclosure of information increases trust towards another person, cost transparency increases trust in a firm, in turn boosting consumer purchase interest. This research implies that by revealing costs – typically tightly-guarded secrets – managers can potentially improve both trust in a firm and sales.

*Limitations*

Our experimental design presented operational transparency and cost transparency in a stylized way. The effectiveness of disclosing cost information is likely affected by the presentation of information, and the source of the information. The stimuli could be potentially strengthened if animation or other website design changes are used to make the manipulation more salient (Buell & Norton, 2011). Strengthening the manipulation in this way might engender the mutually reinforcing effects of cost transparency and operational transparency. Nevertheless, we note that even a simple infographic, when accompanied by the unit costs of production, is sufficient to bring about the effects we document.

All participants were American, with costs presented in US dollars. It is possible that the evaluability of different currencies could moderate consumer choices when faced with cost transparency (Hsee, 1996; Raghurir & Srivastava, 2002). Customers may also make negative inferences if costs are presented in other currencies that indicate low wages and poor working conditions (Paharia, Vohs, & Deshpandé, 2013).

From a practical standpoint, there are important caveats a retailer would need to consider before making the decision to reveal its costs. Firms may not want to disclose their costs if their cost structure is a competitive advantage (Porter, 2008). Moreover, a firm's suppliers may not allow the firm to make the costs associated with certain components public information. Thus, there could be strategic risks or contractual barriers to disclosure.

Even if firms want and are able to disclose their costs, they might not have the knowledge to do so. Disclosing the unit costs associated with the production of a single good could be particularly difficult for retailers that are not vertically integrated. While a retailer might be transparent about certain aspects of price, such as taxes (Carter & Curry, 2010), it might not be

feasible or even possible to comprehensively break out the costs of goods produced by a wide range of manufacturers. Moreover, for goods and services that are dependent on high fixed costs (R&D, overhead, constant labor costs), imputing unit costs may be complicated or even confusing to consumers. For example, imputing R&D costs on a per unit basis in a pharmaceutical context may require many assumptions, as well as a campaign to educate customers about how the sales of successful pharmaceuticals subsidize the production costs of less popular products as well as the costs of early-stage trials and failures. In our research, we focus on a context where unit costs can be more readily calculated and explained. We leave further investigation of the effect of cost transparency in high fixed cost environments as an opportunity for future research.

### *Open Questions*

Our research examines the effects of cost transparency for a single firm in a single industry. Future research could explore what would happen if consumers evaluated two or more products sold by the same firm, with varying levels of transparency. Cost transparency might also seem less intimate in competitive environments when other firms also disclose their costs. Thus, future research could further explore the competitive ramifications of cost transparency, to better understand what happens in markets when multiple players reveal their costs for similar products. There is still much to explore about the benefits and potential pitfalls of cost transparency.

	(1)	(2)	(3)	(4)	(5)
	Units Sold	Units Sold	Units Sold	Units Sold	Units Sold
Post	-0.945*** (0.273)	-0.921*** (0.258)	-0.959*** (0.257)	-0.850*** (0.238)	-0.850*** (0.262)
Post × Treatment	0.523 (0.331)	0.579* (0.330)	0.660** (0.328)	0.582* (0.311)	0.582* (0.340)
Visits		0.0591* (0.0312)	0.0488 (0.0316)	0.0451 (0.0302)	0.0451 (0.0312)
Visits <sup>2</sup>		-0.0002* (0.0001)	-0.0002 (0.0001)	-0.0002 (0.0001)	-0.0002 (0.0001)
No visit			-0.321** (0.153)	-0.300** (0.152)	-0.300** (0.153)
Lagged no visit			-0.158 (0.180)	-0.113 (0.183)	-0.113 (0.187)
Lagged no sale				-0.460*** (0.142)	-0.460*** (0.150)
Observations	460	460	460	460	460
Adjusted R-squared	0.044	0.050	0.052	0.067	0.067
Bandwidth selection	2	2	2	2	3

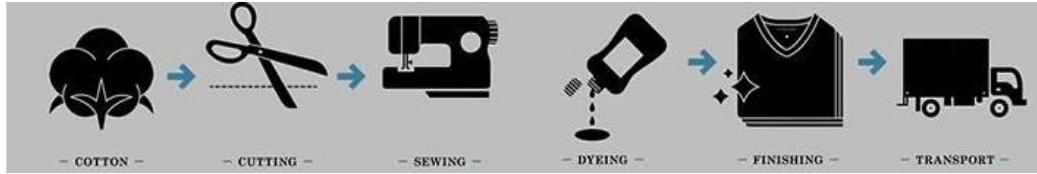
Table 1.1: Field Evidence (Study 1). Units Sold on a Daily Basis, by Transparency Condition. Treatment variable subsumed by Fixed Effects Estimation. Fixed Effect Coefficients withheld to protect confidential company information. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$  Two-Tailed. Robust Newey West standard errors, accounting for autocorrelation and heteroskedasticity within colors, shown in parentheses.

	(1)	(2)	(3)	(4)	(5)
	Treatment Sales %	Treatment Sales %	Treatment Sales %	Treatment Sales %	Treatment Sales %
Post	0.126*** (0.047)	0.117** (0.048)	0.117** (0.049)	0.138*** (0.051)	0.157*** (0.052)
Treatment: Visits		0.002 (0.007)	0.004 (0.008)	0.004 (0.008)	0.001 (0.008)
Treatment: Visits <sup>2</sup>		-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Control: Visits		-0.008** (0.003)	-0.006* (0.004)	-0.005 (0.004)	-0.005 (0.004)
Control: Visits <sup>2</sup>		0.000* (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Treatment: No visits %			0.120 (0.130)	0.131 (0.130)	0.181 (0.131)
Control: No visits %			0.136 (0.095)	0.140 (0.095)	0.128 (0.093)
Treatment: Lagged no visits %				-0.070 (0.116)	-0.021 (0.116)
Control: Lagged no visits %				0.142 (0.095)	0.149 (0.096)
Treatment: Lagged no sales %					-0.169 (0.105)
Control: Lagged no sales %					-0.077 (0.068)
Constant	0.662*** (0.030)	0.713*** (0.058)	0.641*** (0.074)	0.608*** (0.085)	0.681*** (0.090)
Observations	92	92	92	92	92
Adjusted R-squared	0.063	0.114	0.129	0.134	0.165



Table 1.2: Field Evidence Supplemental Analysis (Study 1). Daily Percentage of Wallet Sales Attributed to the Treatment Condition (As a Percentage of All Wallets Sold). \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$  Two-Tailed. Robust standard errors shown in parentheses.

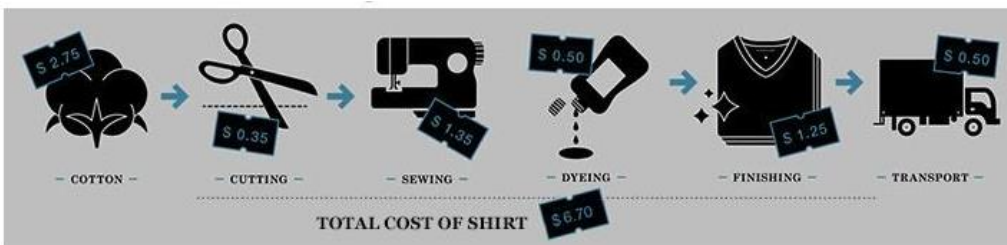
a.



b.



c.



d.

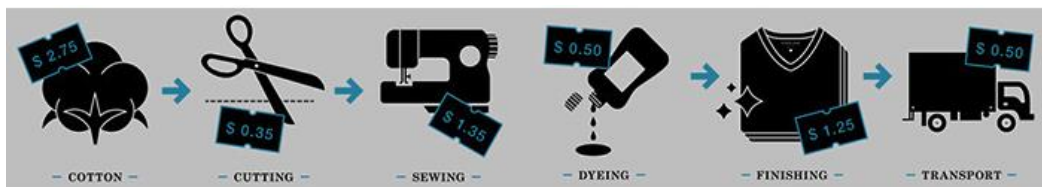


Figure 1.1: Operational transparency (a), aggregated cost transparency (b), full cost transparency (c), and disaggregated cost transparency (d) and as operationalized in our studies with the t-shirt stimuli.

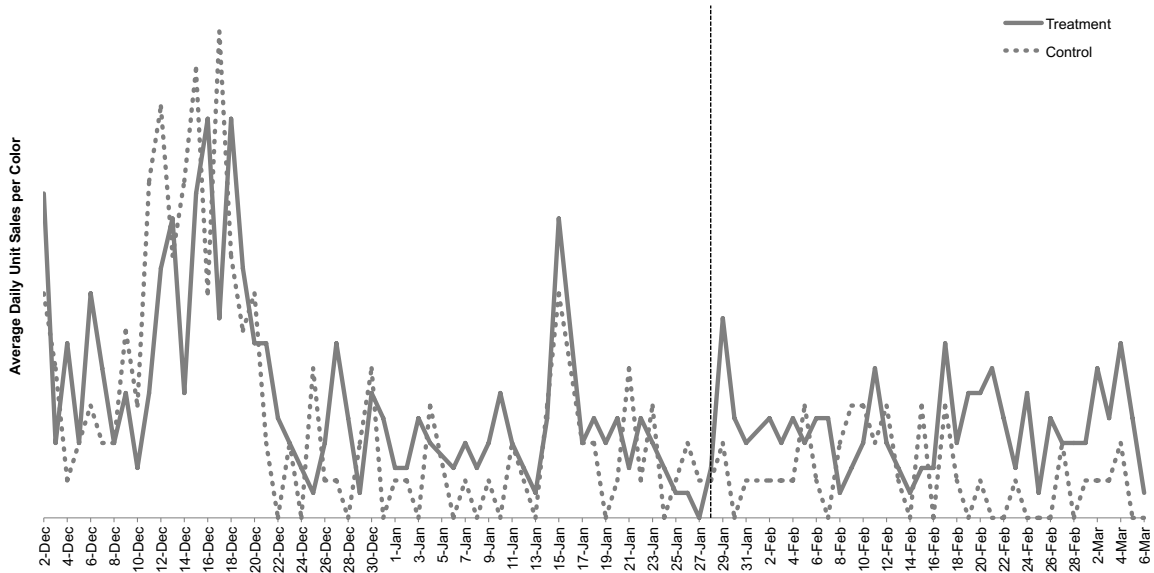


Figure 1.2: Field Evidence (Study 1). Average Daily Unit Sales per Treatment and Control Color.

Note: Dashed vertical line indicates the date the infographic was added. Values withheld to protect confidential company information.

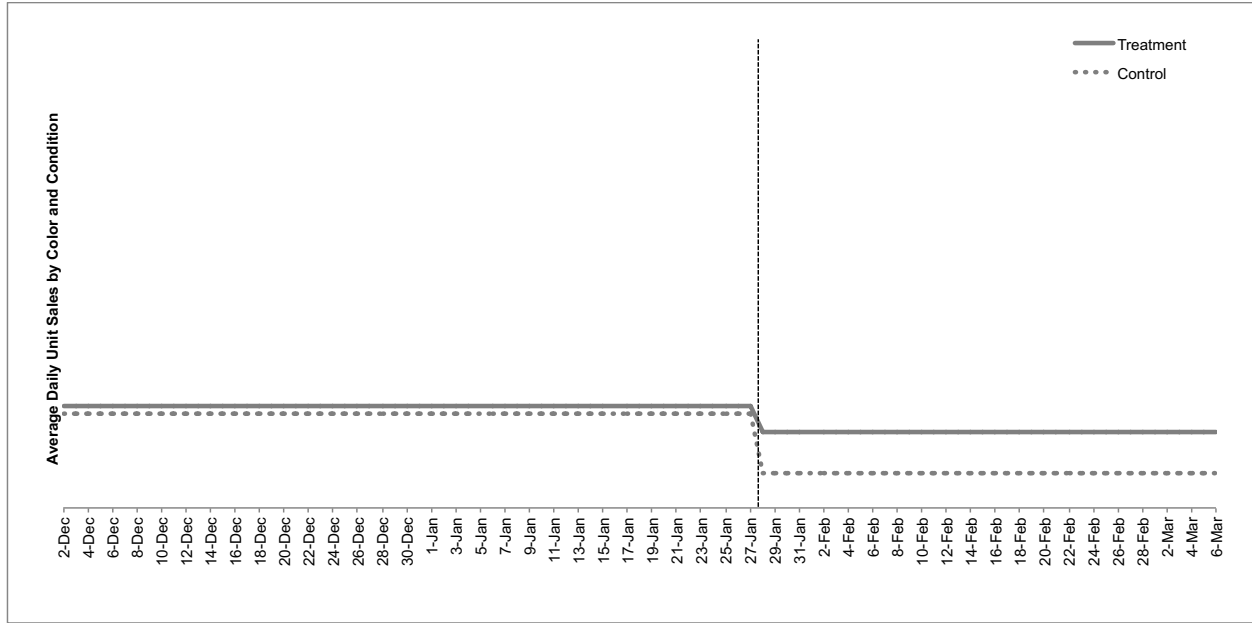
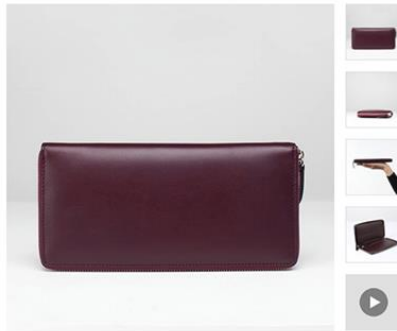


Figure 1.3: Field Evidence (Study 1). Average Daily Unit Sales per Treatment and Control Color Collapsed Across Pre and Post Periods.

Note: Values withheld to protect confidential company information.

CONTROL

COST TRANSPARENCY



The Slim Zip Wallet  
\$115

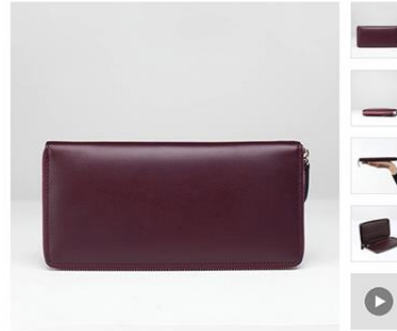
COLOR  
BURGUNDY

ADD TO BAG

Free Shipping

**STYLE DETAILS**  
An elegant, streamlined exterior and functional interior

- 100% Italian leather
- Handmade in Spain
- Matte silver zipper
- To clean, wipe dry with a damp cloth; condition leather when needed



The Slim Zip Wallet  
\$115

COLOR  
BURGUNDY

ADD TO BAG

Free Shipping

**STYLE DETAILS**  
An elegant, streamlined exterior and functional interior

- 100% Italian leather
- Handmade in Spain
- Matte silver zipper
- To clean, wipe dry with a damp cloth; condition leather when needed

What goes into the production of The Slim Zip Wallet?

MATERIALS	HARDWARE	LABOR	DUTIES	TRANSPORT
\$11.00	\$3.60	\$38.00	\$4.21	\$1.00

Figure 1.4: Wallet condition screens, Study 2.

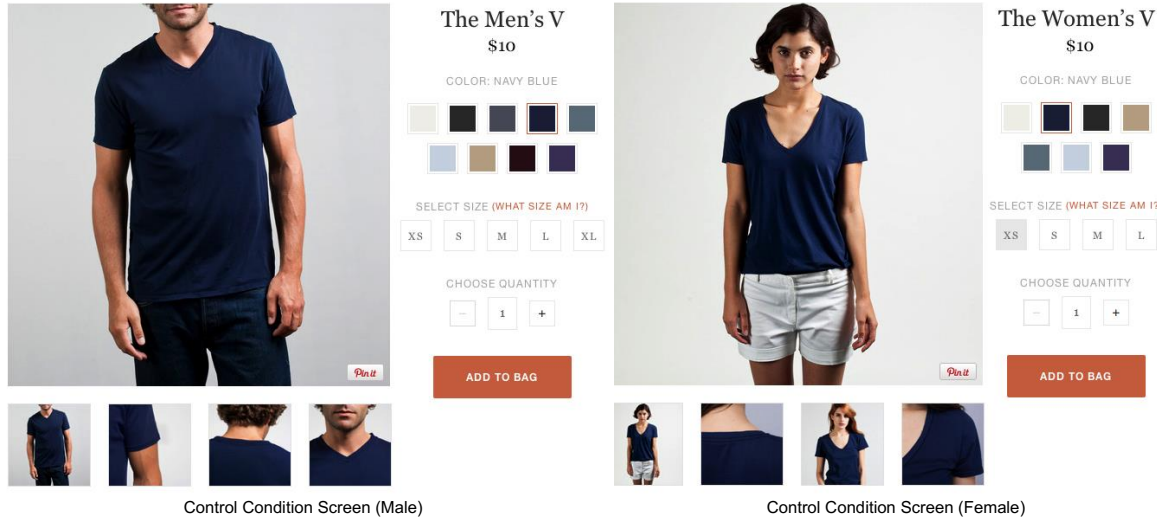


Figure 1.5: Control Condition Screens

Note: Screens were matched to the gender of the participant.

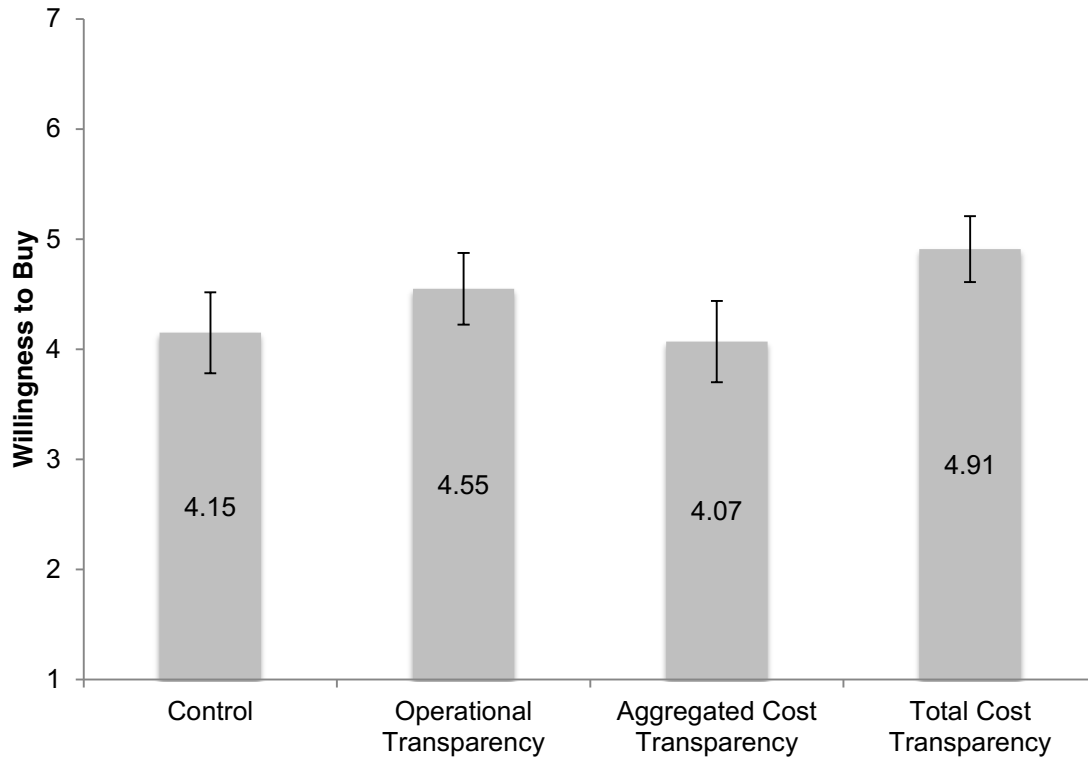


Figure 1.6: Study 3 Results.

FRONT

BACK (CONTROL)

BACK (TRANSPARENT)



Figure 1.7: Chocolate infographics for Study 5.



## Paying Up for Fair Pay:

### Consumers Prefer Firms with Lower CEO-to-Worker Pay Ratios

BHAVYA MOHAN

TOBIAS SCHLAGER

ROHIT DESHPANDÉ

MICHAEL NORTON

Starting on January 1, 2017, public companies in the United States will be required to disclose their pay ratios: the ratio of the total compensation of the CEO to the average annual compensation of all other employees. Prior research examining consumer expectations of equity has not addressed such wage fairness. Pilot field data and seven experiments show that pay ratio disclosure affects purchase intentions of consumers via perceptions of wage fairness. The disclosure of high pay ratios can diminish firm performance and decrease willingness to pay (Pilot Study and Study 1). Lower pay ratios improve consumer purchase intentions across a range of products at different price points (Study 2). High pay ratios can diminish purchase intention even when consumers encounter such information embedded in subtle, naturalistic contexts such as media coverage or product labels (Studies 3A and 3B). Finally we explore risks of disclosing low pay ratios: signaling firm incompetence, alienating particular consumers on the basis of ideology, and signaling high prices (Studies 4A, 4B and 5).

*INTRODUCTION*

On August 5, 2015, the United States Securities and Exchange Commission (SEC) adopted section 953(b) of the Dodd Frank Act as a formal rule. As a result, starting on January 1, 2017, public companies in the United States will be required to disclose the ratio of the total compensation of the CEO to the average annual compensation of all other employees in registration statements, proxy statements, and annual reports.

As the mandatory disclosure of pay ratios looms large in the United States, an open question remains: what will happen when consumers learn about the pay ratios of firms that they frequent? Upon disclosure, a company's pay ratio could ostensibly be publicized to consumers by the news media, activist groups, politicians, and competitors – even if not by firms themselves. Indeed, a natural experiment offers initial evidence that awareness of such pay ratios is of interest not only to investors and employees, but also to consumers. Two years prior to the finalized rule, the SEC posted a request for public comment on the proposed pay ratio disclosure rule. Over 60 days, the SEC received thousands of public comments, which were classified into eight letter types and posted online (“Comments on Pay Ratio Disclosure,” 2013). Comment Letter Type A, for instance, contained the following text: “Knowing which corporations heap riches upon their executives while squeezing struggling employees also will be a useful factor for me when considering which businesses to support with my consumer and investment dollars.” Notably, this letter explicitly includes the term “consumer” in addition to the term “investor.” By our analysis, based on the posted letter counts for the eight comment letter types, of the 126,861 comment letters received in response to the proposed pay ratio disclosure rule, over a quarter - 32,899 – use the term “consumer” in addition to the term “investor.”

This evidence motivates our prediction that CEO-to-worker pay ratios might influence both consumers' attitudes and their purchase behavior. We propose that consumers perceive a high pay ratio as indicating low wage fairness whereas they perceive a low pay ratio as indicating high wage fairness. Drawing on fairness theory (Folger 1994, 1998), we propose that perceived wage fairness drives both consumer purchase behavior (i.e., willingness to buy, willingness to pay) as well as consumer attitudes toward retailers (i.e., a retailer's warmth and competence). A pilot study using field data and seven experiments support this proposition, revealing that disclosing pay ratios influences consumers' perceptions of wage fairness, which in turn influences their attitudes and behaviors.

### *THEORETICAL BACKGROUND*

#### *CEO-to-Worker Pay Ratio and Wage Fairness*

We propose that disclosing high pay ratios will have a marked impact on consumer behavior, given that previous research shows that consumers believe pay ratios are far lower than they actually are (Kiatpongsan & Norton, 2014). In fact, consumers' ideal ratio of CEO pay to average unskilled worker pay is 4.6 to 1, while their estimated actual ratio of CEO pay to average unskilled worker pay is 10 to 1 (Kiatpongsan & Norton, 2014). Note that these estimated ratios are far lower than the average pay ratio across US firms, which is estimated to be approximately 331 to 1 (AFL-CIO, 2014). Importantly, even large firms operating and competing in the same industry have dramatically different pay ratios: for instance, Wal-Mart's ratio, excluding stock compensation, has been estimated to be close to 1000 to 1, whereas Costco comes in closer to 60 to 1 (Bach, 2013; PayScale, 2013). A recent investigation conducted by the

*New York Times* estimated that the pay ratio of total CEO compensation to median employee wage at an American public company was as high as 2,238 to 1 in 2014 (Morgensen, 2015).

We ground our predictions in research on the general effects of ratios, which shows that large ratios – intuitively – have a greater impact on individuals’ perceptions than small ratios (Kwong and Wong 2006). Moreover, ratios between perceived quality and perceived price have been shown to influence consumers’ judgments of product value (Monroe 1990). Indeed, people often prefer relative over absolute representations when considering factors such as price and quality (Saini and Thota 2010). Various perspectives draw on the relative nature of ratios. For instance, equity theory (Adams 1965) uses the construct of “relative ratios” to explain when and how distributions among partners are considered equitable. Specifically, according to equity theory, individuals judge fairness by “assessing the ratio of their outcomes... and inputs [...] against the outcome/input ratio of a comparison other” (Huseman, Hattfield, and Miles 1987, p. 222). We thus expect that presenting consumers with pay ratios will have a direct effect on their fairness judgments. In short, we hypothesize that the disclosure of high (versus low) CEO-to-worker pay ratios will impact consumer perceptions and spur wage fairness considerations. We predict that consumers perceive a high CEO-to-worker pay ratio as conveying low wage fairness and a low CEO-to-worker ratio as conveying high wage fairness.

### *The Consequences of Wage Fairness*

Perceived fairness plays a critical role in shaping consumer behavior; generally speaking, individuals prefer equitable to inequitable distributions of outcomes (Adams 1965). Fairness increases individuals’ happiness (Tabibnia, Satpute, and Lieberman 2008) and reduces negative affect (Sanfey et al. 2003). The marketing literature has predominantly examined fairness in

relation to the distribution of profits between buyers and sellers (Darke and Dahl 2003; Huppertz, Arenson, and Evans 1978; Oliver and Swan 1989). When consumers believe that a firm is making an unfairly large profit on them relative to a prior reference, they perceive prices as less fair, and in turn are less willing to buy (Kahneman, Knetsch, and Thaler 1986; Xia, Monroe, and Cox 2004).

Moreover, principles of fairness can influence consumer behavior even when consumers' direct welfare is unaffected. For instance, research examining the effects of an ethical supply chain suggests that consumers evaluate and respond to fairness between firms and third parties (their workers); at least a subset of consumers are willing to pay a premium to ensure the fair treatment of a firm's workers (Cramer and Ross 1996, Hiscox, Broukhim, and Litwin 2011; Paharia, Vohs, and Deshpandé 2013; Pelozo, White, Shang 2013; Prasad, Kimeldorf, Meyer, and Robinson 2004). For instance, in one study, consumers are willing to pay a 23% premium for coffee labeled 'Fair Trade' (Hiscox, Broukhim, and Litwin 2011), and signage ensuring good working conditions leads some consumers to pay up to 40% more for these labeled products (Prasad, Kimeldorf, Meyer, and Robinson 2004). We note that such indicators of firm social responsibility cue that workers are not treated badly. On the other hand, a high CEO-to-worker pay ratio does not necessarily mean that the average worker is treated poorly or paid a low absolute wage. However, fairness theory (Folger 1994, 1998) holds that individuals' fairness judgments rely on commonly accepted principles, and predicts that consumers should prefer high over low wage fairness *independent* of whether employees receive an absolute high or low wage. Thus, we hypothesize that the CEO-to-worker pay ratio spurs fairness considerations and that consumers perceive high pay ratios to be less fair than low ratios. Perceived wage fairness, in

turn, will drive consumers' purchase intentions and willingness to pay, thus mediating the effect of high (vs. low) CEO-to-worker pay ratios on consumers.

#### *Possible Downsides of Pay Ratio Disclosure*

Disclosing pay ratios is not without potential downsides to the firm. Consumers might ignore the “relative” framing of the ratio and focus instead on potential quality signals conveyed by underlying “absolute” values, such as the wage paid to the CEO. Previous research has shown that observable expenditures that do not directly provide information about a product's quality – such as expensive advertising or a shop in a high-rent location – can still increase perceptions of product quality (Milgrom & Roberts, 1986). Akin to the finding that chocolate fudge tastes better to consumers when they are told that it was produced using an expensive machine (Chinander & Schweitzer, 2003), consumers may feel that certain products produced by firms with “expensive” versus “cheap” CEOs are superior in quality. To assess this potential downside, we explore whether the effect of a retailer's pay ratio disclosure on consumer intention to buy holds for a wide variety of products across a range of listed retail prices, and for products whose quality is outside of that retailer's purview – such as gift certificates to different retailers (Okada, 2005). We predict that the negative wage fairness inferences resulting from a high pay ratio outweigh any possible positive quality inferences resulting from high CEO pay.

Second, prior research has shown that the manner in which a company distributes its profits can affect consumer purchase intention. Indeed, consumers are less willing to buy identical products made by non-profit versus for-profit firms, precisely because non-profits are seen as more warm and less competent (Aaker, Vohs, & Mogilner, 2010). Given that a company's corporate structure can affect how competent it appears to consumers, it is plausible

that a firm is seen as less competent if it pays its CEO a very low wage. To examine this possible downside risk of disclosing a low pay ratio, we assess this possible tradeoff between warmth and competence at different pay ratio levels.

Third, it is possible that while disclosing low pay ratios enhances firm perceptions among some consumer segments, it may backfire with others. Individual-specific factors influence whether company initiatives in the context of corporate social responsibility affect consumer purchase behavior (Sen and Bhattacharya 2001). Political ideology is an important individual-specific factor: for instance, liberal Americans are more likely to support increases in the minimum wage (and therefore lower pay ratios) than conservative Americans (Kuziemko Norton, Saez, and Stantcheva 2015). Given the central role that political ideology plays in consumption (Crockett & Wallendorf, 2004; Hirschman, 1993) and the fact that different persuasive messages have different appeal to different political affiliations (Kidwell, Farmer, & Hardesty, 2013; Winterich, Zhang, & Mittal, 2012), it is possible that while low pay ratios may appeal to liberal consumers, they may serve as a deterrent to more conservative consumers. At the same time, both liberal and conservative Americans' ideal pay ratios are far lower than current actual pay ratios (Kiatpongsan & Norton, 2014), suggesting the possibility that low pay ratios may improve company perceptions more broadly in spite of political affiliation. We explore whether political ideology moderates the impact of pay ratios on perceived wage fairness and willingness to buy.

Finally, a low CEO-to-worker pay ratio may appear less appealing if consumers believe that product prices have been substantially increased to subsidize the higher wages of well-paid employees. Because price perceptions are sensitive to contextual factors (Janiszewski and Lichtenstein 1999), we explored whether a low pay ratio may be less palatable to consumers if

they believe that the higher wages paid to employees result in higher product prices; by the same logic, a high pay ratio firm may become relatively more attractive due to the perceived downward pressure on prices. We assess whether presenting consumers with information explaining the repercussions of pay ratios on prices will moderate the effect of pay ratios on perceived wage fairness and willingness to buy.

### *PILOT STUDY*

We use field data to offer an initial examination of the relationship between CEO-to-worker pay ratios and consumers' purchase decisions, leveraging discussions in Switzerland regarding legislation limiting pay ratio. In 2009, Swiss citizens started a public initiative (called "1:12 – für gerechte Löhne") aiming to hold a referendum for a legally mandated pay ratio cap in Switzerland, limiting the salary of CEOs to 12 times that of the lowest paid worker (Hooper 2013). This initiative was accepted for a public referendum in mid-2011 (Schweizerische Bundeskanzlei 2011). About 2.8 million Swiss citizens (53% of the eligible voters) voted on this legislation at the end of 2013. While the referendum did not pass, nearly 35% of Swiss voters voted for the cap. Most importantly, the time course of this referendum allows a preliminary examination into how increased public awareness regarding CEO-to-worker pay ratios might predict consumers' purchase decisions.

#### *Method*

*Data and Procedure.* We obtained data on the CEO-to-worker pay ratio of 27 Swiss companies for the period between 2004 and 2014 from Travail Suisse ([www.travailsuisse.ch](http://www.travailsuisse.ch)), an



institute that examines wage-related issues. In addition, we obtained data on the sales of the same 27 companies in the Swiss market for the period between 2004 and 2014 from the companies' annual reports. For four of these 27 companies we obtained the financial statements from the companies' public relations departments, because these companies were not publicly listed. For the 27 companies, we used 11 years of data each (factoring in one lag), which resulted in 297 cases. For 43 cases, we were not able to obtain Swiss sales data (e.g., since sales data was not available for the Swiss market for specific years), so this reduced the final sample from 297 to 254 cases.

We used several measures to capture public awareness of the referendum. First, we obtained the number of articles referencing the pay ratio initiative published in the top three Swiss newspapers. In addition, as validation measures, we crawled data of the public Facebook profile of the referendum (e.g., number of comments, likes, and posts) and obtained the number of yearly Google search results for the initiative.

### *Results*

To examine the interactive effect of pay ratio and the initiative on company performance, we used the companies' sales as the dependent variable, the CEO-to-worker pay ratio as the independent variable, and the number of newspaper articles published on the initiative (as proxy for public awareness) as the moderator. We lagged the ratio and the number of articles by one year, as the ratio is published along with the annual report in the subsequent period. We used a mixed effects model nested in companies with an AR1 error correction (to account for autocorrelation of the time series-structured data).

[Insert Figure 2.1 About Here]

The results supported our intuition: companies with a low pay ratio performed significantly better than companies with a high pay ratio when more newspapers published more articles about the referendum ( $\beta_{PayRatio} = 0.030, SE = 0.027, t = 1.138, p = 0.26, \beta_{NumberArticles} = -0.008, SE = 0.013, t = -0.640, p = .52, \beta_{PayRatio \times NumberArticles} = -0.048, SE = 0.023, t = -2.145, p < 0.05, SD_{Random\_Intercept} = 0.001$ ). From 2009 (start of referendum) to 2014, the sales of companies with a low pay ratio (yearly median split) increased by a yearly average of 8.75%, whereas the one of companies with a high ratio increased by a yearly average of 6.28%. To test the robustness of the results, we used the companies' industry (e.g., food retailer) and a dummy for the period between 2008 and 2009 (i.e., the years of the financial crisis) as controls. The results again supported our prediction ( $\beta_{PayRatio} = 0.030, SE = 0.027, t = 1.108, p = 0.27, \beta_{NumberArticles} = -0.009, SE = 0.013, t = -0.679, p = 0.50, \beta_{PayRatio \times NumberArticles} = -0.048, SE = 0.023, t = -2.143, p < 0.05, SD_{Random\_Intercept} = 0.001$ ).

To further validate the robustness of these results, we used the measures obtained from Google and Facebook – two additional measures of public awareness of the initiative – as moderators, which resulted in similar results. The interaction between the companies' pay ratio and the proxy was significant for the number of Google search hits ( $\beta = -0.031, SE = 0.015, t = -1.992, p < 0.05$ ), the number of comments on the initiative's Facebook site ( $\beta = -0.044, SE = 0.021, t = -2.074, p < 0.05$ ), the number of likes ( $\beta = -0.046, SE = 0.021, t = -2.117, p < 0.05$ ), and the number of posts ( $\beta = -0.053, SE = 0.021, t = -2.533, p < 0.05$ ).

### *Discussion*

The pilot study provides preliminary evidence that a low versus high CEO-to-worker pay ratio may positively impact company sales – especially when such information is publicized. Of

course, while these correlational results are consistent with our theorizing, they preclude a causal interpretation; for instance, it is possible that employees may exhibit increased motivation when learning that their firm has relatively low pay ratios, leading to improved firm performance. In the studies that follow, we therefore test the effect of a high versus low pay ratio using experimental methodology, which allows us to assess underlying psychological processes.

### *OVERVIEW OF STUDIES*

We examine when and why consumers reward or punish companies for their CEO-to-worker pay ratios. In seven experiments, we test the effect of disclosure of pay ratio on product and brand desirability. We first examine whether disclosure of a high pay ratio versus a low pay ratio leads to decreased willingness to pay in an incentive compatible context (Study 1). We next assess these effects across a range of different goods, while controlling for possible inferences about different product types and different prices of the retailer's goods (Study 2). We then explore whether pay ratio disclosure diminishes purchase intention when embedded in varied, subtle contexts, such as news media and product labeling (Studies 3A and 3B). Next, we explore possible risks of pay ratio disclosure, examining whether disclosure of a low pay ratio negatively affects consumer perceptions of company competence (Study 4A), and whether pay ratio disclosure alienates certain consumers due to ideological differences (Study 4B). Finally, we examine whether creating a salient link between low pay ratios and high product prices can decrease the attractiveness of low pay ratio firms and increase the attractiveness of high pay ratio firms (Study 5). Across all studies, we assess perceived wage fairness in order to demonstrate its mediating role on consumer decisions. Finally, we discuss the implications of these findings for

companies and policy-makers, and directions for future research.

### *STUDY 1: PAY RATIOS AND ACTUAL WILLINGNESS TO PAY*

In Study 1 we used an experimental paradigm to examine whether the disclosure of pay ratios can causally affect consumers' willingness to pay in a controlled, incentive-compatible context. We sold gift cards to two retailers that vary in their real-world pay ratio – Urban Outfitters (with a pay ratio of 15:1) and GAP (with a pay ratio of 456:1; AFL-CIO 2014) – and varied the presence of pay ratios by revealing this information or not.

#### *Method*

*Design and Procedure.* Participants ( $N = 232$ ,  $M_{Age} = 35.63$ , 42% male) completed this online experiment in exchange for \$0.50. To increase the realism of this study, we developed an online platform that closely resembled that of an actual online auction platform ([www.ricardo.ch](http://www.ricardo.ch)). In addition to the base payment, we endowed participants with \$0.50 that they were free to either keep or use to bid on a product. Next, participants received personal login information to the online auction platform. The first page of the online platform introduced participants to the bidding task: participants were presented with a description of a \$5 gift card to one of two retailers. They subsequently indicated the amount of money they wished to bid on the gift card (see Figure 2.2). Participants were randomly assigned to four conditions of the 2(Pay Ratio: High vs. Low)  $\times$  2(Ratio Presence: Absent vs. Revealed) between subjects online experiment. We chose two apparel retailers with considerably different estimated pay ratios for this experiment. The retailer with the low pay ratio was Urban Outfitters (with a pay ratio of

15:1) and the retailer with a high pay ratio was GAP (with a pay ratio of 456:1; AFL-CIO 2014). When revealing the pay ratio of the target firm, we also revealed the underlying wages of the CEO and average employee, the pay ratio of the competing firm, and the industry average.

[Insert Figure 2.2 About Here]

Participants were informed that they would be bidding against one other randomly assigned participant. After participants placed their bid, we revealed whether they won the auction or not. The winner either received the gift card or an equivalent amount of money while the loser received the amount she or he did not bid.

*Dependent Measures.* We used the log of the amount participants bid on the gift cards. The transformation was due to a bimodal distribution: several participants bid the highest possible amount for the product. We also measured perceived wage fairness: participants indicated their perception of wage fairness on a 7-point scale (1: *Not at all fair*, 7: *Very fair*): “How fair do you think the wages that this retailer pays its employees are?”

All subsequent experiments concluded with voluntary demographic questions. Participants indicated their gender, age, highest level of education completed (some high school, high school graduate, some college, college graduate, postgraduate), and combined annual household income. We set the desired number of participants at the outset of each experiment and did not analyze the data until that number was reached. We report all manipulations and measures.

## *Results*

*Amount Bid.* We conducted a 2(Ratio Presence: Absent vs. Revealed) by 2(Pay Ratio: High vs. Low) ANOVA on the amount participants bid on the gift card (Figure 3;  $M_{High\_Absent} =$

1.734,  $SD = 1.35$ ,  $M_{High\_Revealed} = 1.667$ ,  $SD = 1.42$ ,  $M_{Low\_Absent} = 1.525$ ,  $SD = 1.31$ ,  $M_{Low\_Revealed} = 2.244$ ,  $SD = 1.25$ ;  $F(3,228) = 3.028$ ,  $p < 0.05$ ). While the two main effects were not significant (Ratio Presence:  $F(1,228) = 3.106$ ,  $p < 0.10$ ; Pay Ratio:  $F(1,228) = 1.003$ ,  $p = 0.32$ ), the interaction effect was significant (Pay Ratio  $\times$  Ratio Presence:  $F(1,228) = 4.975$ ,  $p < 0.05$ ). A linear regression with an interaction between both factors showed that revealing the pay ratio lowers the amount participants bid for the gift card when the pay ratio was high relative to when the pay ratio was low ( $\beta_{Low} = -0.153$ ,  $t = -0.833$ ,  $p = 0.41$ ;  $\beta_{Revealed} = -0.049$ ,  $t = -0.272$ ,  $p = 0.79$ ;  $\beta_{Revealed \times Low} = 0.579$ ,  $t = 2.230$ ,  $p < 0.05$ ).

[Insert Figure 2.3 About Here]

*Perceived Wage Fairness.* We conducted a 2(Ratio Presence: Absent vs. Revealed) by 2(Pay Ratio: High vs. Low) ANOVA on perceived wage fairness ( $M_{High\_Absent} = 4.281$ ,  $SD = 1.19$ ,  $M_{High\_Revealed} = 3.328$ ,  $SD = 1.81$ ,  $M_{Low\_Absent} = 3.947$ ,  $SD = 1.11$ ,  $M_{Low\_Revealed} = 4.648$ ,  $SD = 1.57$ ,  $F(3,228) = 8.798$ ,  $p < 0.001$ ). The main effect of ratio presence was not significant ( $F(1,228) = 0.701$ ,  $p = 0.40$ ), whereas both the pay ratio as well as its interaction with ratio presence were significant (pay ratio:  $F(1,228) = 7.122$ ,  $p < 0.01$ ; Pay Ratio  $\times$  Ratio Presence:  $F(1,228) = 18.571$ ,  $p < 0.001$ ). A linear regression with an interaction between both conditions showed that perceived wage fairness was lower for the company with the low pay ratio, but that this effect was attenuated when the ratio was revealed ( $\beta_{Low} = -0.218$ ,  $t = -1.220$ ,  $p = 0.22$ ;  $\beta_{Revealed} = -0.623$ ,  $t = -3.587$ ,  $p < 0.01$ ;  $\beta_{Revealed \times Low} = 1.081$ ,  $t = 4.309$ ,  $p < 0.01$ ).

*Mediation by Wage Fairness.* We used moderated mediation to examine whether perceived wage fairness mediated the effect of high and low pay ratio on the amount participants bid on the gift card depending on pay ratio presence. We included pay ratio as the independent variable, wage fairness as the mediator and ratio presence as the moderator of the effect of pay

ratio on wage fairness. A 5,000-sample bootstrap analysis revealed that the 95% bias-corrected confidence interval for the indirect effect did not exclude zero when no pay ratio was revealed [-0.01, 0.16] but excluded the zero when the pay ratio was revealed [-0.60, -0.48]. The direct effect of the pay ratio (high vs. low) became insignificant [-0.44, 0.26], indicating moderated mediation (Preacher and Hayes 2008).

### *STUDY 2: PAY RATIOS ACROSS CATEGORY AND PRICE*

Study 1 demonstrated the possible benefits of revealing low pay ratios on consumers' willingness to pay for gift cards. Study 2 has two primary goals. First, we test whether the effect of disclosing a high pay ratio holds across different products and price points – ranging from a \$3.99 box of cereal to a \$499.99 flat screen television. Second, in Study 1 we revealed not only the pay ratio of the retailers, but also specific CEO wage and employee wage paid out by the firm and information about industry averages and competitors' wages; in Study 2, we test whether the effect of pay ratios holds in the absence of this additional information.

#### *Method*

*Design and Procedure.* Participants ( $N = 151$ ,  $M_{Age} = 30.2$ , 66.2% male) completed this online experiment in exchange for \$0.30. We used a 2(Pay Ratio: High vs. Low) by 10(Product Category: Stereo Headphones, 50" Flat Screen Television, Book, 16-Quart Cooking Pot, 80 Pack of Garbage Bags, 5' Floor Lamp, 21 oz. Cereal Box, 8 AA Battery Pack, 2 Bath Towels, Bag-Less Upright Vacuum Cleaner) mixed factorial design. All participants were told: "Imagine you are shopping at a well-known general merchandise retailer, with both retail locations and an

online store.” Pay ratio was manipulated between subjects. To inform the high pay ratio value, we used publicly available data for a national retailer, compiled by a firm that collects employee wage data at Fortune 100 firms (Bach, 2013). In the high ratio condition, participants were told, “You learn that at this retailer, the ratio of the CEO’s salary to the average employee’s salary is 1000 to 1.” In the low ratio condition, participants were told, “You learn that at this retailer, the ratio of the CEO’s salary to the average employee’s salary is 5 to 1.” Each participant subsequently assessed their willingness to buy products from 10 different product categories. For each product, participants were told, “You come across [product], at a price that is below your budget.” Each product was assessed on a separate page, and the 10 products were presented in random order. The products represented a wide range of categories and prices: listed retail prices for similar products sold online ranged from \$3.99 (cereal box) to \$499.99 (flat screen television).

*Dependent Measures.* To gauge willingness to buy, participants were asked: “Given the opportunity, how likely are you to purchase the towel set from this retailer?” on a 7-point scale (1: *Not at all likely*, 7: *Very likely*). Subsequently, participants indicated their perception of wage fairness using the same measure as Study 1.

## *Results*

*Willingness to Buy.* We conducted a 2(Pay Ratio: High vs. Low) by 10 (Product Category: stereo headphones, 50” flat screen television, book, 16-quart cooking pot, 80 pack of garbage bags, 5’ floor lamp, 21 oz. cereal box, 8 AA battery pack, 2 bath towels, bag-less upright vacuum cleaner) mixed ANOVA, with willingness to buy as the dependent variable. Unsurprisingly, we observed a significant within-subjects main effect of product category on



willingness to buy ( $F(9,141) = 5.05, p < 0.01$ ), denoting that some products were preferable to others. As predicted, we observed a significant main effect of disclosing a high pay ratio on willingness to buy ( $F(1,149) = 5.43, p < 0.05$ ). Importantly, the interaction between pay ratio and product category was not significant ( $F(9,141) = 0.66, p = 0.75$ ), indicating that the impact of pay ratio disclosure was not significantly different across product categories. As can be seen in Figure 2.4, for every product category, the trend was such that willingness to buy was higher for the low-pay-ratio retailer.

[Insert Figure 2.4 About Here]

Moreover, these results suggest that merely disclosing the pay ratio of a retailer –without specific CEO and employee wages or information about industry averages and competitors’ wages – is sufficient to shape consumer perceptions.

### *STUDY 3A: PAY RATIOS IN CONTEXT*

Studies 1 and 2 tested the impact of pay ratio disclosure presented in two contexts – alongside a product description in an auction, and directly accompanying a product description. Study 3A offers an initial test of the effect of disclosing a high versus low pay ratio on consumer purchase intention presented in a context highly relevant to retailers – news media. In everyday consumption situations, consumers would be more likely to learn about a firm’s pay ratio from the news media alongside other financial facts about the firm (Ross Sorkin 2015); indeed, our Pilot Study shows that learning about pay ratios from media sources predicts the impact of those ratios on sales. As in Study 1, we also assess perceptions of wage fairness to examine whether

these perceptions track with pay ratios, and mediate the influence of high versus low pay ratios on purchase intentions.

### *Method*

*Design and Procedure.* Participants ( $N = 149$ ,  $M_{Age} = 30.6$ , 64.4% male) completed this online experiment in exchange for \$0.30. Participants were randomly assigned to one of 2 experimental conditions (Pay Ratio: High vs. Low) between subjects.

Participants were first told: “Please read the following short segment from a news article. The article recaps a large retailer’s financial performance over the last year.” Subsequently, participants were given the following news snippet, entailing six statements about a firm: “A retailer operates retail stores in various formats under various banners. Its operations comprise of multiple business segments. This retailer made \$484 billion dollars in revenue. The firm had an operating income of \$27 billion dollars. The firm had a net income of \$17 billion dollars. Earnings per share were \$5.05.” These statements were informed by publicly available revenue data for a large retailer (“Wal-Mart Stores Inc.,” 2015). In the high ratio condition, one additional sentence was added to the news snippet: “The CEO made 1000 times more than the average employee.” This pay ratio (\$24,000,000 to \$22,400, or 1000 to 1) reflects the estimated CEO cash compensation, median employee cash compensation, and resulting pay ratio of a large American retailer – Walmart, although participants were not informed of this fact (PayScale, 2013). In the low ratio condition, the following sentence was added: “The CEO made 5 times more than the average employee.” To create the low pay ratio, we held median employee pay constant at \$22,400, and calculated the CEO salary to reflect the ideal pay ratio of a CEO’s wage

to the average worker's wage of approximately 5 to 1 demonstrated in previous research (Kiatpongsan & Norton, 2014).

Subsequently, on the next screen, participants were told to envision replacing their towel and told the listed retail price for the towel (a similar product sold online by a major American retailer informed the product description and retail price): "Now, imagine that you are looking to purchase a new set of towels, to replace a worn out set you currently have. You find a set of 2 high-quality, 100% Turkish cotton towels that you like, at a price point that is below your budget. Imagine that the towels are sold by the retailer you read about in the previous news article segment."

*Dependent Measures.* Participants answered the same willingness to buy measure as in Study 2, and the same wage fairness measure as in Study 1.

### *Results*

*Willingness to Buy.* Willingness to buy from the high-pay-ratio retailer was significantly lower than from the low-pay-ratio retailer ( $M_{Low} = 5.54, SD = 1.47$  vs.  $M_{High} = 4.61, SD = 1.71$ ;  $t(147) = 3.55, p < 0.01$ ).

*Wage fairness.* Perceived wage fairness was significantly lower in the high compared to the low pay ratio condition ( $M_{Low} = 4.38, SD = 1.79$  vs.  $M_{High} = 2.63, SD = 1.30$ ;  $t(147) = 6.84, p < 0.01$ ).

*Mediation by Wage Fairness.* Pay ratio condition predicted both willingness to buy ( $\beta = -0.93, p < 0.01$ ), as well as wage fairness ( $\beta = -1.75, p < 0.01$ ). When wage fairness and pay ratio condition were both included in the regression model predicting willingness to pay, the effect of wage fairness remained significant ( $\beta = 0.49, p < 0.01$ ), but the effect of pay ratio condition

became insignificant ( $\beta = -0.07, p = 0.78$ ), providing support for mediation. A 5,000-sample bootstrap mediation analysis revealed that the 95% bias-corrected confidence interval for the size of the indirect effect excluded zero  $[-0.53, -0.43]$ , suggesting a significant indirect effect (Preacher and Hayes 2008).

### *STUDY 3B: PAY RATIOS AND ENERGY EFFICIENCY*

In Study 3B we test the impact of disclosing pay ratios in another naturalistic setting – the labels on a product’s package. Specifically, we model our stimuli on EnergyGuide labels, which are required to be placed alongside on all appliances sold both online and in-store (Federal Trade Commission, 2010). The labels show the estimated yearly operating cost for a given product model, and contextualize where this cost falls within the cost range of similar models (Federal Trade Commission, 2015). Thus, in Study 3B, we developed a pay ratio label, inspired by the EnergyGuide label design (see Figure 2.5).

#### *Method*

*Design and Procedure.* Participants ( $N = 500, M_{Age} = 32.8, 61.6\%$  male) completed this online experiment in exchange for \$0.30. Participants were randomly assigned to one of 4 experimental conditions in a 2(Pay Ratio: High vs. Low) by 2(Energy Cost: High vs. Low) between-subjects design.

All participants were told: “Imagine that you are looking to purchase a new flatscreen television. You come across the television shown below, which is sold both in-store and online by a retailer.” Across all conditions, participants saw a baseline product website interface

depicting a photo of a flatscreen television, and key product specifications, including screen size and resolution and price (\$429.99). The product specifications and price were similar to other 50-inch flat screen televisions sold online at the time of the experiment. In addition to the baseline product interface, each of the conditions included a label depicting energy usage, and a label depicting the retailer's pay ratio (Figure 2.5).

[Insert Figure 2.5 About here]

The design of both labels was modeled after mandatory yellow EnergyGuide labels (Federal Trade Commission, 2015). In the low energy use condition, the energy label stated that the fridge has an annual estimated energy cost of \$22, that the cost range of similar models is \$21-\$61, and that the estimated yearly electricity use is 188 kilowatt hours. In the high energy use condition, the energy label stated that the fridge has an annual estimated energy cost of \$57, that the cost range of similar models is again \$21-\$61, and that the estimated yearly electricity use is 570 kilowatt hours. In the low pay ratio condition, the pay ratio label stated that the pay of the average employee at this retailer is \$24,000, the total pay of the CEO is \$112,000, and that the estimated ratio of CEO-to-worker pay is 5/1. In the high pay ratio condition, the pay ratio label stated that the pay of the average employee at this retailer is \$24,000, the total pay of the CEO is \$22,400,000, and that the estimated ratio of CEO-to-worker pay is 1000/1.

*Dependent Measures.* Participants then completed the same willingness to buy and perceived wage fairness measure as in Study 3A.

## *Results*

*Willingness to Buy.* We conducted a 2(Pay Ratio: High vs. Low) by 2(Energy Costs: High vs. Low) ANOVA on willingness to buy (Figure 6). We observed a significant effect of energy

costs ( $F(1,496) = 16.38, p < 0.01$ ). Importantly, we again observed a significant effect of pay ratios ( $F(1,496) = 9.12, p < 0.01$ ). The television with low energy costs and a high pay ratio was rated similarly to a television with high energy costs and a low pay ratio ( $M_{High\_LowCost} = 4.33, SD = 1.57; M_{Low\_HighCost} = 4.19, SD = 1.55; t(250) = 0.73, p = 0.47$ ) (Figure 2.6).

[Insert Figure 2.6 About here]

There was no significant interaction between disclosing energy costs and pay ratios ( $F(1,496) = 0.19, p = 0.66$ ), indicating that the effect of ratios on preferences held for regardless of whether energy costs are high or low.

*Wage fairness.* Perceived wage fairness was significantly lower in the high compared to the low pay ratio condition ( $M_{Low} = 3.74, SD = 1.68$  vs.  $M_{High} = 3.30, SD = 1.00; t(498) = 3.08, p < 0.01$ ).

*Mediation by Wage Fairness.* Pay ratio condition predicted both willingness to buy ( $\beta = -0.48, p = 0.02$ ), as well as wage fairness ( $\beta = -0.42, p = 0.02$ ). When wage fairness and pay ratio condition were both included in the regression model predicting willingness to pay, the effect of wage fairness remained significant ( $\beta = 0.40, p < 0.01$ ), but the effect of pay ratio condition became insignificant ( $\beta = -0.29, p = 0.12$ ), providing support for mediation. A 5,000-sample bootstrap mediation analysis revealed that the 95% bias-corrected confidence interval for the size of the indirect effect excluded zero  $[-0.31, -0.07]$ , suggesting a significant indirect effect (Preacher and Hayes 2008).

#### STUDY 4A: PAY RATIOS, WARMTH, AND COMPETENCE

In Study 4A, we explore a potential risk for firms that disclose a low pay ratio. Prior research shows that consumers assess firms across the dimensions of warmth and competence, with warm brands being perceived as less competent (Aaker et al., 2010). Thus, in Study 4A, we test whether disclosing a low pay ratio lowers consumers' perceptions of firm warmth versus firm competence – specifically, whether being perceived as treating employees in a fair manner increases perceptions of being warm – but comes at the expense of appearing incompetent.

### *Method*

*Design and Procedure.* Participants ( $N = 402$ ,  $M_{Age} = 31.5$ , 66.8% male) completed this online experiment in exchange for \$0.30. Participants were randomly assigned between subjects to one of 2 experimental conditions (Pay Ratio: High vs. Low). Participants were again told to envision a variation of the towel context used in Study 3A, with the identical focal product description. In the high ratio condition, participants were told: “You learn that the retailer pays the average employee \$22,400. The retailer pays the CEO \$48,000,000. The ratio of the CEO’s salary to the average employee’s salary is 2000 to 1.” In the low ratio condition, participants were told: “You learn that the retailer pays the average employee \$22,400. The retailer pays the CEO \$112,000. The ratio of the CEO’s salary to the average employee’s salary is 5 to 1.”

*Dependent Measures.* The willingness to pay and wage fairness measures were administered as in Study 3A. We excluded one outlier participant, who stated a willingness to pay above \$240. In addition, we included measures to gauge perceptions of firm warmth and perceptions of firm competence (Aaker et al. 2010). Participants were asked, “To what extent do you believe that this retailer is \_\_\_\_?” on a set of 6 traits, presented in randomized order. We summed and averaged the participants’ responses to create two indexes. Three traits comprised

the warmth index (warm, kind, generous; Cronbach's  $\alpha = 0.93$ ) and three traits comprised the competence index (competent, effective, efficient; Cronbach's  $\alpha = 0.88$ ).

### *Results*

*Willingness to Pay.* Replicating our prior studies, willingness to pay in the high ratio condition was significantly lower than in the low ratio condition ( $M_{High} = 25.84, SD = 13.27$  vs.  $M_{Low} = 28.83, SD = 12.79$ ;  $t(399) = 2.28, p < 0.05$ ).

*Wage Fairness.* Perceived wage fairness was significantly lower in the high ratio condition than in the low ratio condition ( $M_{High} = 2.65, SD = 1.61$  vs.  $M_{Low} = 3.64, SD = 1.78$ ;  $t(399) = 5.96, p < 0.01$ ).

*Retailer Warmth and Competence.* Most importantly for the current study, we observed no tradeoff between warmth and competence for low-pay-ratio retailers. Perceived retailer warmth was significantly lower in the high ratio condition than in the low ratio condition ( $M_{High} = 2.76, SD = 1.35$  vs.  $M_{Low} = 3.66, SD = 1.41$ ;  $t(399) = 6.57, p < 0.01$ ), as was perceived retailer competence ( $M_{High} = 4.25, SD = 1.38$  vs.  $M_{Low} = 4.66, SD = 1.20$ ;  $t(399) = 3.10, p < 0.01$ ).

*Mediation by Wage Fairness.* Again, the pay ratio condition predicted both willingness to pay ( $\beta = -2.99, p = 0.02$ ) and wage fairness ( $\beta = -0.99, p < 0.01$ ). When wage fairness and pay ratio condition were both included in the regression model predicting willingness to pay, the effect of wage fairness remained significant ( $\beta = 1.65, p < 0.01$ ), but the effect of pay ratio condition became insignificant ( $\beta = -1.36, p = 0.31$ ), again providing support for mediation. A 5,000-sample bootstrap mediation analysis revealed that the 95% bias-corrected confidence interval for the size of the indirect effect excluded zero  $[-2.58, -0.60]$ , suggesting a significant indirect effect (Preacher and Hayes 2008).



*STUDY 4B: PAY RATIOS AND POLITICAL BELIEFS*

In Study 4B, we explore another potential risk for firms that disclose a low pay ratio – the risk that such disclosure will alienate some consumers due to ideological differences. Prior research shows that people have varying perceptions of inequality; for instance, Republican voters desire relatively less equal distributions of wealth than their Democratic counterparts (Norton & Ariely, 2011). In Study 4B we test whether a low pay ratio will alienate certain customer subgroups, such that even if disclosure increases positive perceptions of some customers, it may harm the firm in the eyes of other customers.

*Method*

*Design and Procedure.* Participants ( $N = 253$ ,  $M_{Age} = 33.5$ , 57.7% male) completed this online experiment in exchange for \$0.30. We again varied the pay ratio: participants were randomized to see a product sold by a retailer with either a low (5 to 1) pay ratio or a high (2000 to 1) pay ratio.

Participants were again told to envision a variation of the towel context used in Study 3A, with the identical focal product description. In the high ratio condition, participants were told: “You learn that the retailer pays the average employee \$22,400. The retailer pays the CEO \$48,000,000. The ratio of the CEO’s salary to the average employee’s salary is 2000 to 1.” In the low ratio condition, participants were told: “You learn that the retailer pays the average employee \$22,400. The retailer pays the CEO \$112,000. The ratio of the CEO’s salary to the average employee’s salary is 5 to 1.”

*Dependent Measures.* Participants then completed the same willingness to buy and wage fairness measures as in Study 3. In addition, participants were asked to report their political party from four choices (Democrat, Republican, Independent, Other). Of the participants, 18.6% identified as Republican, 44.3% identified as Democrat, 32.8% identified as Independent, and 4.3% identified as “Other.” Finally, as a secondary measure, participants stated their positions regarding inequality: “Differences in income in the United States are too large (*strongly agree, agree, neither agree nor disagree, disagree, strongly disagree*)” (International Social Survey Programme 2009). When participants were asked if differences in income in the United States are too large, 38.7% indicated “*strongly agree*,” 39.9% indicated “*agree*,” 10.7% indicated “*neither agree nor disagree*,” 7.9% indicated “*disagree*,” and 2.4% indicated “*strongly disagree*.”

## *Results*

*Willingness to Buy.* Willingness to buy from the high-pay-ratio retailer was again significantly lower than from the low-pay-ratio retailer ( $M_{Low} = 5.22$ ,  $SD = 1.53$  vs.  $M_{High} = 4.17$ ,  $SD = 1.89$ ;  $t(251) = 4.84$ ,  $p < 0.01$ ).

*Political Beliefs.* We conducted a 2(Pay Ratio: High vs. Low) by 4(Political Beliefs: Republican, Democrat, Independent, Other) ANOVA on willingness to buy. We observed a significant effect of pay ratio ( $F(1,245) = 20.36$ ,  $p < 0.01$ ), political belief ( $F(3,245) = 14.87$ ,  $p < 0.01$ ), and importantly, a significant interaction between political beliefs and pay ratio ( $F(3,245) = 8.59$ ,  $p < 0.05$ ). In three of the four subgroups by political beliefs (Democrat, Independent, Other) there were significant differences in willingness to buy between the high-pay-ratio and the low-pay-ratio retailer, with participants more willing to buy in the high pay ratio condition

( $p$ 's < 0.02; Figure 2.7). In the Republican subgroup, there was no significant difference in willingness to buy between the high-pay-ratio and the low-pay-ratio retailer ( $M_{Low} = 5.36$ ,  $SD = 1.32$  vs.  $M_{High} = 5.36$ ,  $SD = 1.68$ ;  $t(45) = -0.01$ ,  $p = 0.99$ ).

[Insert Figure 2.7 About here]

*Beliefs about Inequality.* We conducted a 2 condition (Low Ratio, High Ratio) ANOVA on willingness to buy, including perceived differences in income as a covariate. There were main effects of condition ( $F(1,248) = 23.34$ ,  $p < 0.01$ ) and perceived difference in income ( $F(1,248) = 22.85$ ,  $p < 0.01$ ), and a significant interaction ( $F(1,248) = 8.59$ ,  $p < 0.01$ ). Participants who strongly agreed or agreed reported greater willingness to buy from low-pay-ratio retailer ( $p$ 's < 0.01); participants who neither agreed nor disagreed, disagreed, or strongly disagreed reported no difference in willingness to buy ( $p$ 's > 0.05).

*Wage Fairness.* We conducted a 2(Pay Ratio: High vs. Low) by 4(Political Beliefs: Republican, Democrat, Independent, Other) ANOVA on perceived wage fairness. We observed a significant effect of pay ratio ( $F(1,245) = 23.79$ ,  $p < 0.01$ ), political party ( $F(3,245) = 6.69$ ,  $p < 0.01$ ), and a marginally significant interaction between political party and pay ratio ( $F(3,245) = 2.55$ ,  $p = 0.06$ ). In three of the four subgroups by political affiliation (Democrat, Independent, Other) there were significant differences in perceptions of wage fairness between the high pay ratio and the low pay ratio retailer ( $p$ 's < 0.05). In the Republican subgroup, there was no significant difference in perceived between the high pay ratio and the low pay ratio retailer ( $M_{Low} = 4.28$ ,  $SD = 1.57$  vs.  $M_{High} = 3.59$ ,  $SD = 2.11$ ;  $t(45) = 1.28$ ,  $p = 0.21$ ).

*Mediation by Wage Fairness.* We conducted a separate mediation analysis for participants with each set of political beliefs. Pay ratio condition predicted both willingness to buy ( $p$ 's < 0.05, as well as wage fairness ( $p$ 's < 0.05 in three of the four subgroups by political

affiliation (Democrat, Independent, Other). For participants in the Republican subgroup, pay ratio condition predicted neither willingness to buy ( $p = 0.99$ ) nor wage fairness ( $p = 0.21$ ). When wage fairness and pay ratio condition were both included in the regression model predicting willingness to pay, the effect of wage fairness remained significant for Democrats and Independents ( $p$ 's  $< 0.01$ ), but the effect of pay ratio condition became insignificant ( $p$ 's  $> 0.14$ ), providing support for mediation. The mediation model did not hold for those who selected 'other', likely due to the small sample size ( $N = 11$ ). A 5,000-sample bootstrap mediation analysis revealed that the 95% bias-corrected confidence interval for the size of the indirect effect excluded zero for both Democrats [-1.11, -0.35] and Independents [-1.44, -0.44], suggesting a significant indirect effect (Preacher and Hayes 2008).

#### *STUDY 5: PAY RATIOS, EMPLOYEE WAGES, AND PRODUCT PRICE*

In Study 5, we explore a potential moderator of the effect of a high versus low pay ratio on consumers' willingness to buy. We assess whether drawing attention to the potential link between higher employee wages (and lower pay ratios) and higher product prices moderates the attractiveness of high versus low pay ratio firms. Specifically, we predict that when consumers are led to believe that higher employee wages result in higher product prices, price perceptions of high ratio firms (who pay lower wages and therefore have lower prices) will become more positive, and the preference for high versus low pay ratio firms would be decreased.

#### *Method*

*Design and Procedure.* Participants ( $N = 220$ ,  $M_{Age} = 36.9$ , 47.3% male) completed this

online experiment in exchange for \$0.50 and were randomly assigned to one of the four conditions of the 2(Pay Ratio: High vs. Low) by 2(Employee Wage Explanation: Absent vs. Present). Participants were led to believe that they were participating in two – ostensibly unrelated – studies. In the “first” study, we asked participants to read a page from a newspaper with several articles and follow-up questions to examine their level of textual understanding. Between subjects, we manipulated the text of one article on the page. Participants either read an article explaining that high employee salaries result in retailers setting higher product prices, or a neutral newspaper article about an unrelated subject (see Figure 2.8).

[Insert Figure 2.8 About here]

In the “second” study, participants received a login to the same online auction platform as in Study 1 with a hypothetical bidding task. We used the same two companies as in Study 1, but modified the salaries: Urban Outfitters (with a pay ratio of 20:1) and GAP (with a pay ratio of 60:1). This time, regardless of the condition, the CEO payment was kept constant at \$1,246,740. Thus, the pay ratio was manipulated by varying employee salary.

*Dependent Measures.* Participants completed the same wage fairness measures as in Study 1. Participants’ willingness to buy was measured on a slider scale (0: *Very unlikely to buy the product*, 100: *Very likely to buy the product*). In addition, participants were asked to report price perceptions on a 7-point scale (1: *Not attractive*, 7: *Attractive*): “How do you perceive the price of this retailer?” (Janiszewski and Lichtenstein 1999).

As a manipulation check, we asked participants to indicate the extent to which they agreed with the statement “The more employees earn, the more expensive a product” on a slider scale (0: *Strongly disagree*, 100: *Strongly agree*).

## Results

*Manipulation Check.* We conducted a 2(Pay Ratio: High vs. Low) by 2(Employee Wage Explanation: Absent vs. Present) ANOVA to examine whether the manipulation was successful. Those who read the article about wages were more likely to agree with the statement, regardless of ratio condition ( $M_{NoExplanation} = 57.67$ ,  $M_{Explanation} = 69.41$ ,  $t(218) = 3.47$ ,  $p < 0.001$ ), indicating that the manipulation was effective.

*Willingness to Buy.* We conducted a 2(Pay Ratio: High vs. Low) by 2(Employee Wage Explanation: Absent vs. Present) ANOVA on willingness to buy ( $M_{High\_NoExplanation} = 15.65\%$ ,  $SD = 19.19$ ,  $M_{High\_Explanation} = 25.82\%$ ,  $SD = 25.97$ ,  $M_{Low\_NoExplanation} = 37.63\%$ ,  $SD = 26.11$ ,  $M_{Low\_Explanation} = 30.03\%$ ,  $SD = 26.41$ ). We observed a significant effect of pay ratio ( $F(1,216) = 15.24$ ,  $p < 0.001$ ), a non-significant effect of the wage explanation ( $F(1,216) = 0.07$ ,  $p = 0.79$ ), and, importantly, a significant interaction between pay ratio and wage explanation ( $F(1,216) = 7.11$ ,  $p < 0.001$ ). The difference between the high and the low pay ratio conditions that did not see the wage explanation was highly significant ( $t(105) = 4.98$ ,  $p < 0.001$ ), replicating the earlier studies. However, as predicted, the difference between the high and the low pay ratio conditions that saw the wage explanation was not significant ( $t(111) = 0.85$ ,  $p = 0.39$ ). Those in the high pay condition that saw the explanation were significantly more willing to buy than those who saw no explanation ( $t(103) = 2.30$ ,  $p < 0.05$ ; Figure 2.9).

[Insert Figure 2.9 About here]

*Wage Fairness.* We conducted a 2(Pay Ratio: High vs. Low) by 2(Employee Wage Explanation: Absent vs. Present) ANOVA on wage fairness ( $M_{High\_NoExplanation} = 2.25$ ,  $SD = 1.49$ ,  $M_{High\_Explanation} = 3.22$ ,  $SD = 1.81$ ,  $M_{Low\_NoExplanation} = 5.38$ ,  $SD = 1.32$ ,  $M_{Low\_Explanation} = 5.43$ ,  $SD = 1.51$ ). We observed a significant effect of pay ratio ( $F(1,216) = 168.5$ ,  $p < 0.001$ ), a significant

effect of the explanation ( $F(1,216) = 5.44, p < 0.05$ ), and a significant interaction effect of pay ratios and the explanation of the relationship between employee pay and the explanation ( $F(1,216) = 4.90, p < 0.05$ ). The difference between the high and the low pay ratio conditions were significant for those participants who did not see the wage explanation ( $t(105) = 11.47, p < 0.001$ ) as well as for those who saw the explanation ( $t(111) = 7.07, p < 0.001$ , though mirroring our results for willingness-to-buy, the size of the effect was smaller in the wage explanation conditions.

*Price Perception.* We conducted a 2(Pay Ratio: High vs. Low) by 2(Employee Wage Explanation: Absent vs. Present) ANOVA on price perception ( $M_{High\_NoExplanation} = 2.38, SD = 1.62, M_{High\_Explanation} = 3.06, SD = 1.70, M_{Low\_NoExplanation} = 3.58, SD = 1.50, M_{Low\_Explanation} = 3.37, SD = 1.57$ ). We observed a significant effect of pay ratio ( $F(1,216) = 12.34, p < 0.001$ ), a non-significant effect of the explanation ( $F(1,216) = 0.99, p = 0.32$ ), but a significant interaction effect of pay ratios and the explanation of the relationship between employee pay ( $F(1,216) = 4.25, p < 0.05$ ). Moreover, the difference between the high and the low pay ratio condition was significant for those that did not see the wage explanation ( $t(105) = 3.96, p < 0.01$ ), but not for those who saw the explanation ( $t(111) = 0.99, p = 0.32$ ). Informing participants that employee pay increased product prices increased the attractiveness of prices at high pay ratio firms. Price perceptions significantly increased for the high pay ratio retailer (who paid lower wages) when participants were reminded that higher wages lead to higher product prices, relative to no explanation ( $t(103) = 2.10, p < 0.05$ )

*Mediation Analyses.* We conducted a moderated mediation analysis, with pay ratio as the independent variable, wage fairness as the mediator, and willingness to buy as the dependent variable. The manipulation of consumers' belief about the relationship between employee wages

and product prices served as the moderator of the effect of pay ratio on wage fairness.

Corroborating the findings of the prior studies, a 5,000-sample bootstrap analysis revealed that the 95% bias-corrected confidence interval of the indirect effect excluded the zero for both participants who did not receive an explanation of the employee payment-price relationship [-26.60, -11.15], and for those who received the explanation [-18.85, -8.01]. The test of the equality of both indirect effects was significant as the 95% bias-corrected confidence interval excluded zero [0.61, 12.27], indicating that the indirect effect was attenuated by the manipulation of the employee payment-price relationship. The direct effect of the pay ratio (high vs. low) was non-significant [-5.71, 10.66], indicating a (moderated) full mediation.

In addition, we conducted two regression analyses with wage fairness and price perceptions as predictor of buying intention. For participants who did not read the wage explanation, both predictors were significant ( $\beta_{Wage} = 0.25, t = 4.12, p < 0.01$ ;  $\beta_{Price} = 0.70, t = 11.49, p < 0.01, R^2 = 0.66$ ). However, for participants who read the wage explanation, the effect of wage fairness on willingness to buy became insignificant, while the effect of price perception on purchase intention remained significant ( $\beta_{Wage} = 0.13, t = 1.85, p > 0.05$ ;  $\beta_{Price} = 0.69, t = 9.96, p < 0.01, R^2 = 0.55$ ).

Taken together, these analyses suggest that when consumers are told that they will be paying for increased wages via higher product prices, negative price perceptions outweigh positive perceptions of wage fairness. As a result, the significant difference between high versus low pay ratio disclosure on purchase intention is attenuated.

## *GENERAL DISCUSSION*



We opened by noting that Section 953(b) of the Dodd-Frank Act will require that all public companies in the United States disclose the ratio of CEO to median employee pay in their annual financial statements beginning in 2017. Our Pilot Study and seven experimental studies show that pay ratio disclosure can affect purchases and purchase intention of consumers, driven by perceptions of wage fairness. Examining data from Switzerland, firms with low pay ratios perform better than firms with high pay ratios when pay ratios are discussed in the media (Pilot Study). In an incentive compatible auction context, consumers are willing to pay more for gift cards when a firm's relatively low pay ratio is revealed (Study 1). Lower pay ratios improve consumer purchase intentions across a range of products (Study 2). High pay ratios continue to diminish purchase intention when consumers encounter such information embedded in news articles and product labels (Studies 3A and 3B). We explore two risks of pay ratio disclosure: signaling firm incompetence, and alienating certain consumer segments. Importantly, low pay ratios increase consumers' ratings of both firm warmth and firm competence (Study 4A). Moreover, pay ratios enhance perceptions of Democrats and Independents without alienating Republican consumers (Study 4B). Finally, when consumers believe that higher employee wages result in higher product prices, a low pay ratio no longer significantly decreases consumers' willingness to buy relative to a high pay ratio (Study 5). Across our studies, we find that perceptions of wage fairness mediate the effect of revealing pay ratio consumer purchase decisions.

Prior research examining consumer expectations of equity and price fairness has not addressed wage fairness. As a result, our investigation contributes to both streams of literature (Darke and Dahl 2003; Xia et al. 2004), by demonstrating that consumers care about the distribution of wages a firm pays its employees. Our results also contribute to a broader literature

on the benefits of firm transparency – of firms literally showing consumers what happens behind the scenes (R. W. Buell & Norton, 2011; Chinander & Schweitzer, 2003). In Buell and Norton (2011), for example, consumers preferred websites that revealed the process by which they searched for results – in one study, revealing the process through which it looked for potential dates for online dating users. At the same time, however, not all forms of disclosure proves to be beneficial: when websites reveal their processes only to suggest poor options (e.g., an unattractive date), consumers react more negatively. Despite the differences between dating and compensation, our results also suggest that pay disclosure can have benefits: firms with low pay ratios stand to garner improved consumer perceptions via disclosure relative to firms with high ratios.

While this research examined the consequences of CEO-to-worker pay ratio on consumers, other stakeholders are likely to be influenced by this ratio. The pay ratio of CEOs to average workers has long been a question of interest to both employees and investors. Management guru Peter Drucker, for example, argued that the ratio of a CEO's salary to average worker salary should be capped at 25 to 1, and that greater disparity would lead to employee resentment and decreased morale, negatively affecting company performance (McGregor, 2013). By examining implications of pay ratio disclosure on consumer behavior, we bring a new perspective to prior management literature that examines how CEO compensation affects firm performance (Carpenter and Sanders 2002; Chang, Dasgupta and Hilary 2010)

Given our results, it is plausible that other types of disclosures that signal wage unfairness could also affect consumer purchase intentions. One possible context for further exploration is gender inequality in wages within a company. Unlike pay ratio disclosure, there is no pending legislation requiring companies to disclose whether men and women doing the same

job within a firm are receiving equal pay. However, in 2014, the ratio of women's to men's median weekly full-time earnings was 82.5 percent (Hegewisch, Ellis, & Hartmann, 2015). While some statistics comparing salaries by gender at the firm level are available, they are both limited and self-reported (Glassdoor, 2014). In 2014, Sony Pictures was hacked, and confidential internal data was made public; among other salacious findings, it was discovered that a female executive made 65% of the salary of a male executive with the same title and position (Roose, 2014). If such information represented the gender wage gap of the whole firm, would consumers care? Thus, future research could further explore the repercussions of ratio disclosure on consumer behavior in other wage-related contexts.

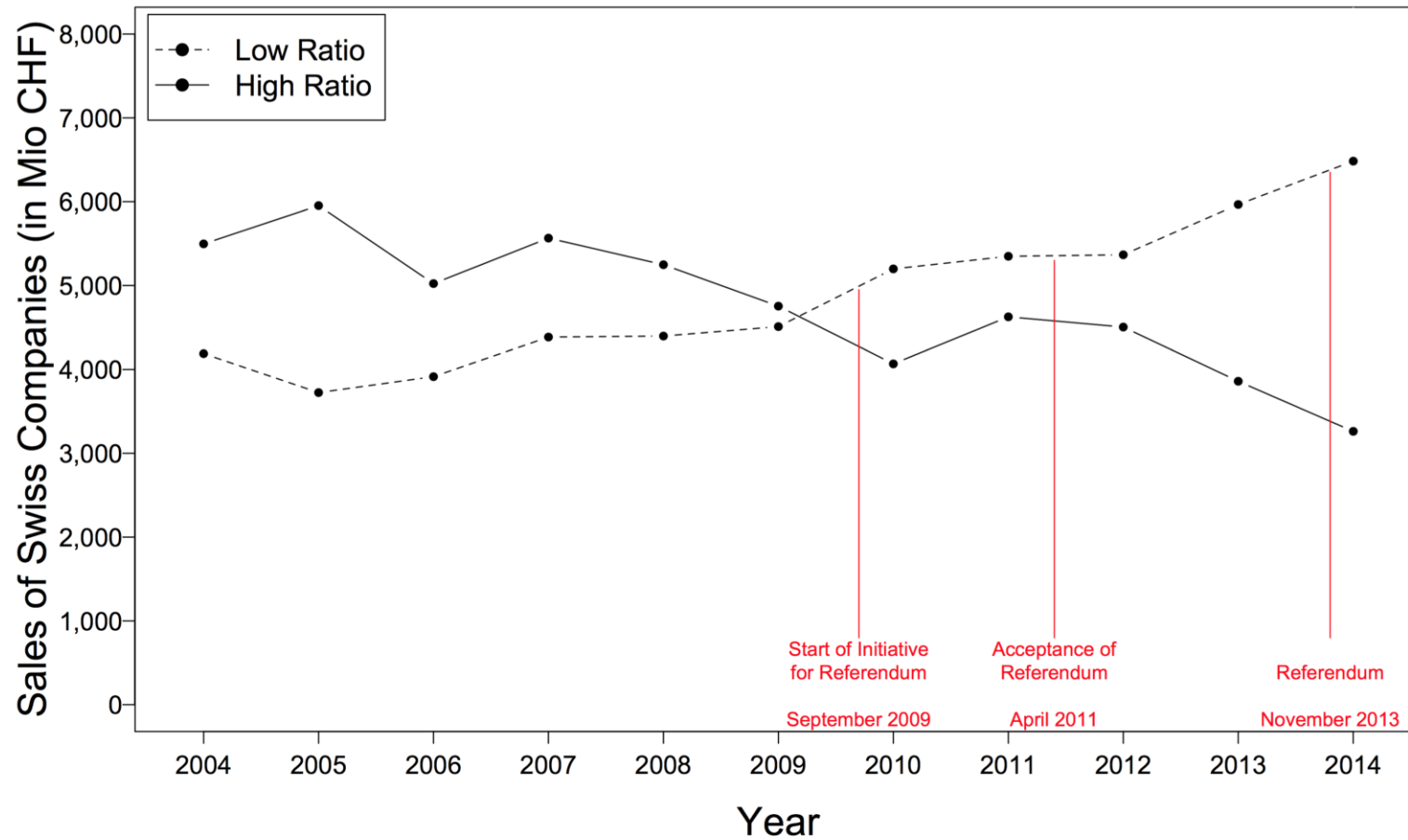


Figure 2.1: A publicized low pay ratio has a positive effect on sales, whereas the opposite applies for a high pay ratio.

(A)

## Gift Card of Urban Outfitters (\$5)

### Product Details

- Valid for all Products in all Stores
- Value: \$5

Urban Outfitters Inc. is an American clothing corporation. Urban Outfitters' **CEO-to-worker pay ratio**, indicating by **how many times the CEO's annual salary exceeds the average worker's salary**, is **15:1** (meaning that the CEO of Urban Outfitters earns 15 times the amount an average worker earns).



Current Price

\$ 0.50

Remaining Balance  
(Bid on Top of Current Price)

\$ 0.50

Minimum Amount  
To Bid

\$ 0.01

Bid Now

Your Bid  
(in  
Cents):

\$0.00 \$0.05 \$0.10 \$0.15 \$0.20 \$0.25 \$0.30 \$0.35 \$0.40 \$0.45 \$0.50

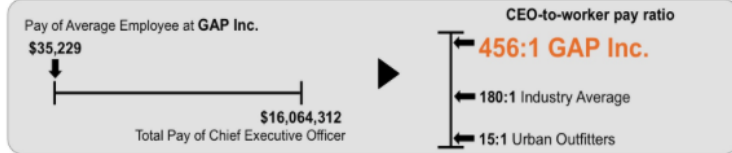
(B)

## Gift Card of GAP Inc. (\$5)

### Product Details

- Valid for all Products in all Stores
- Value: \$5

GAP Inc. is an American clothing corporation. GAP Inc.'s **CEO-to-worker pay ratio**, indicating by **how many times the CEO's annual salary exceeds the average worker's salary**, is **456:1** (meaning that the CEO of GAP Inc. earns 456 times the amount an average worker earns).



Current Price

\$ 0.50

Remaining Balance  
(Bid on Top of Current Price)

\$ 0.50

Minimum Amount  
To Bid

\$ 0.01

Bid Now

Your Bid  
(in  
Cents):

\$0.00 \$0.05 \$0.10 \$0.15 \$0.20 \$0.25 \$0.30 \$0.35 \$0.40 \$0.45 \$0.50

Figure 2.2: Screenshots of the online auction for the (A) high ratio retailer and for (B) the low ratio retailer (Study 1).

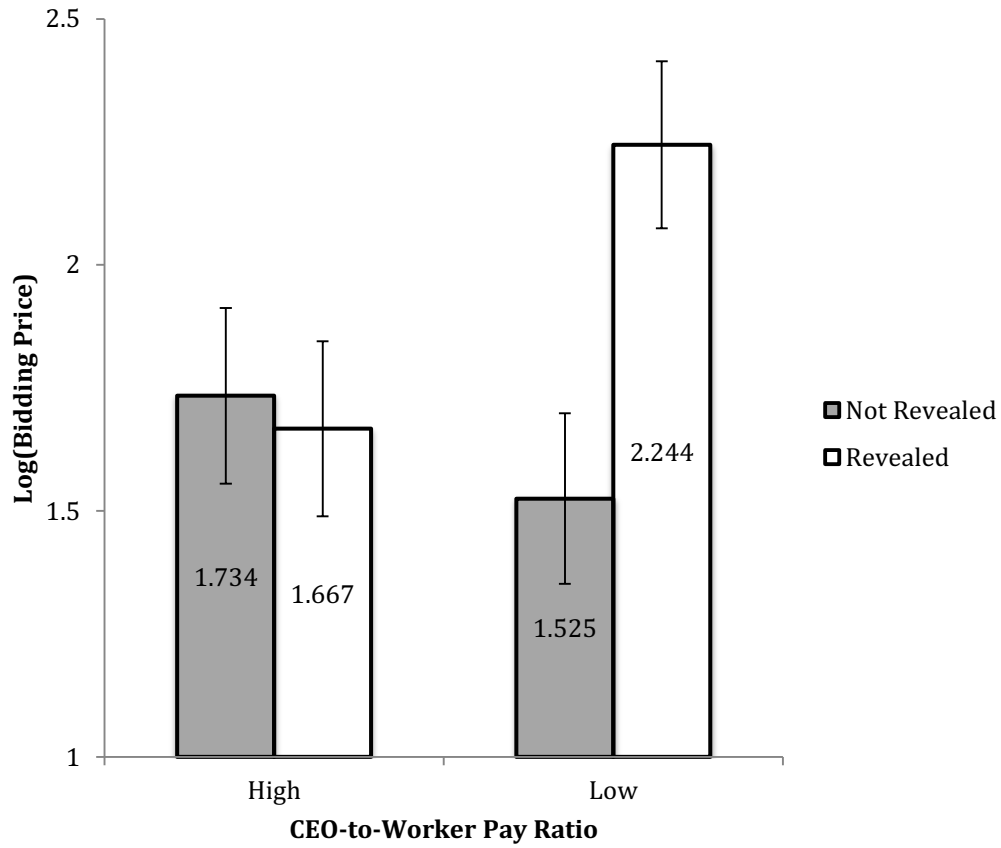


Figure 2.3: Consumers' bidding price for the gift card depends on the pay ratio and whether it was revealed (Study 1).

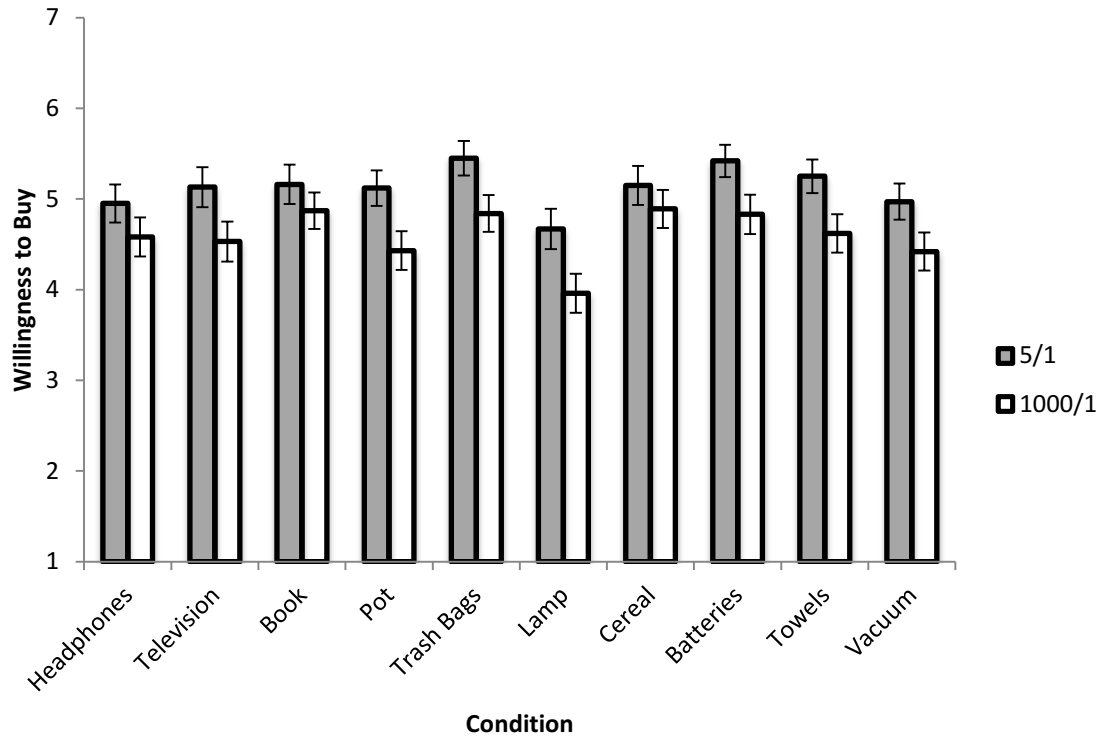


Figure 2.4: Consumers are more willing to buy a wide range of products from retailers with low pay ratios (Study 2).

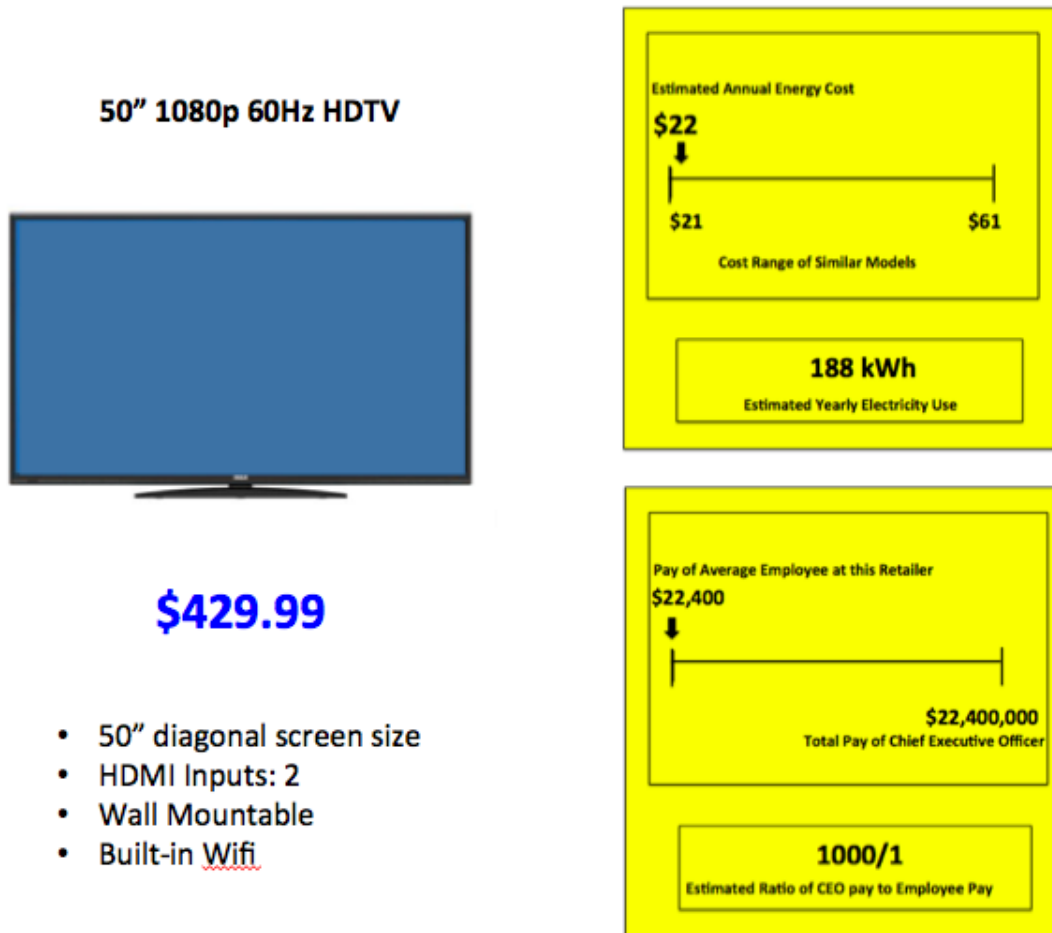


Figure 2.5: Condition screen for low energy cost high pay ratio condition (Study 3B).



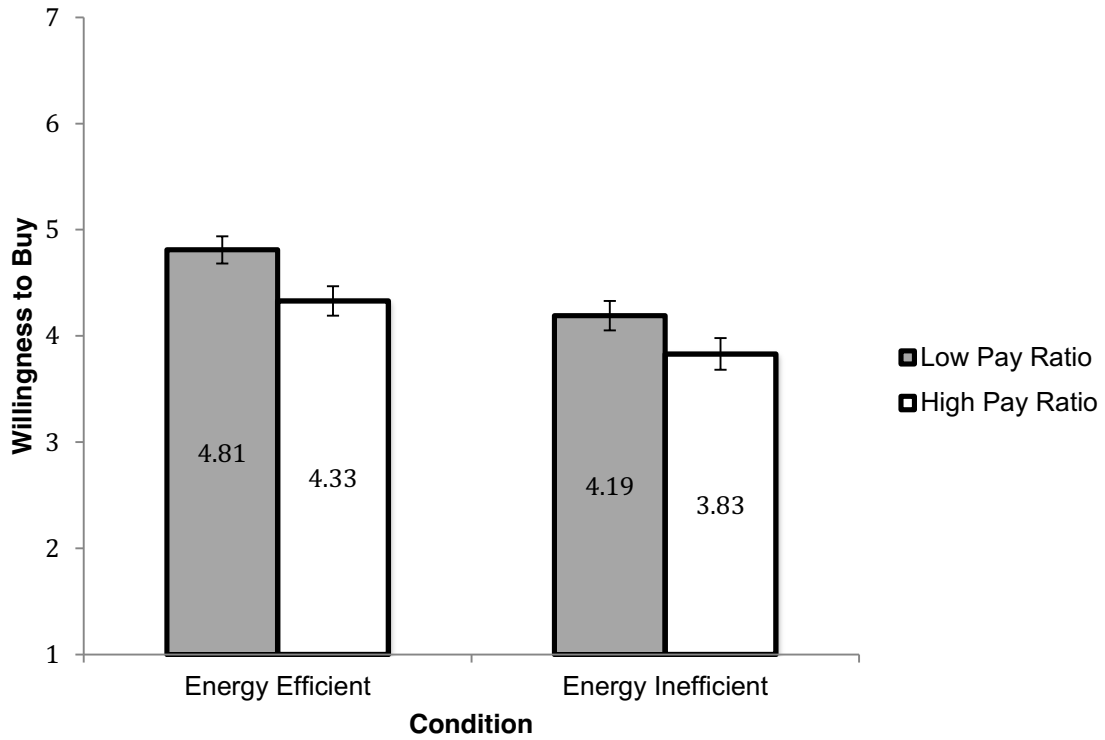


Figure 2.6: A television with low energy costs and a high pay ratio is rated the same as a television with high energy costs and a low pay ratio (Study 3B).

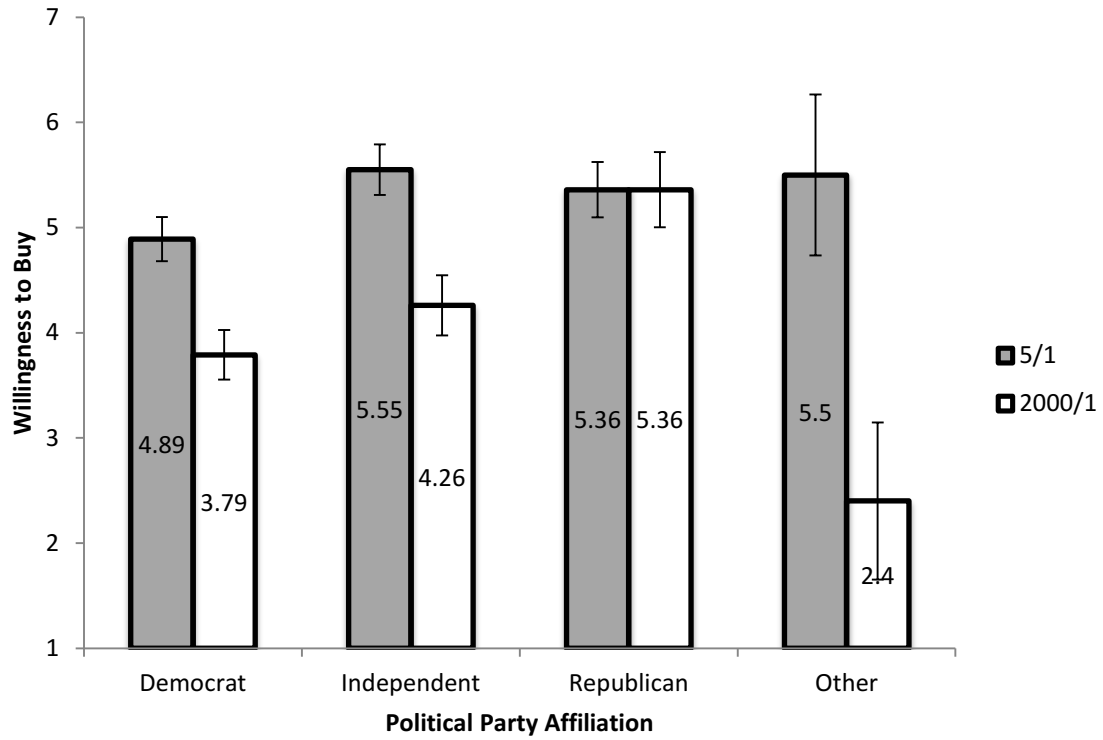


Figure 2.7: Most consumer subgroups, by political party, are more willing to buy from retailers with low pay ratios (Study 4B).

(A)

**Business Science Weekly**

28 FEB 2016

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**Consumers pay up for employees' salaries**

By CHRISTIAN HILDEBRAND

Do high wages translate into high prices? A research group from a prominent research university recently published important findings answering these questions.

Consider Costco and Wal-Mart's Sams Club, which operate in the same market. Among warehouse retailers, Costco with 67,600 full-time employees in the United States is number one, accounting for about 50% of the market. Sams Club with 551 stores and 110,200 employees in the United States is number two, with about 40% of the market.

Though the businesses are competitors, a remarkable disparity shows up in their wage structures. The average wage at Costco is \$17 an hour. A full-time worker at Wal-Mart Sams Clubs makes about \$10 an hour on average. At \$17 an hour, Costcos average pay is 72% higher than Sams Clubs \$9.86 an hour. However, upon examination, Costcos average prices are higher than those of Sams Club.

Do wages really affect the product prices that consumers pay? Based on data collected from more than 2,000 US companies, the recent research provides a surprising answer: higher wages directly result in higher prices. This is particularly true in the apparel business, where 95% of product prices are result directly from the wages paid to employees.

The researchers conclude that paying employees higher salaries can significantly increase a retailers costs, which are subsequently passed on to consumers. As a result of these increased employee wages, consumers have to pay more for their products, and companies may risk going out of business if their costs-and prices-

become too high.

**International Moose Count Underway**

By BOB O'BOBSTON

The UN-sponsored International Moose Census got off to a flying start today with hopes for an increase in the worldwide moose population compared to last year's disappointing figures. Among the traditional early reporters were Egypt, returning figures of six moose, a twenty percent increase on 2011's figures of five, and Uruguay whose moose population remains stable at eleven.

According to Robbie McRobson, head of the UN Moose Preservation Council, worldwide moose numbers are expected to grow markedly on last year due to the traditional moose strongholds of Canada and the United States, with the larger developing moose ecologies also poised to make gains. The largest percentage increase in moose will likely come from China", says McRobson, The Chinese government has invested heavily in moose infrastructure over the past decade, and their commitment to macrofauna is beginning to pay dividends". Since 2004 China has expanded moose pasture from 1.5% of arable land to nearly 3.648% and moose numbers are expected to rise to 60,000 making China a net moose exporter for the first time. This is good news for neighbouring Mongolia, a barren moose-wasteland whose inhabitants nonetheless have an insatiable desire for the creatures. The increase in Beijing-Ulanbataar trade is anticipated to relieve pressure on the relatively strained Russian suppliers, but increase Mongolia's imbalance of trade with its larger neighbour.

Historically the only competitor to China in the far eastern moose

markets has been Singapore but the tiny island nation is set to report a net loss, expecting a decrease of more than five percent on last year's 50,000 moose counted. The head of Singapore's Agency for Agriculture, Jing-Feng Lau, explained to an incredulous Singaporean parliament yesterday that bad weather had contributed to this season's poor showing, most notably when a cargo of 150 moose were swept out into the Indian ocean in a monsoon.

Yet again the global demand for moose will be met largely by the US and Canada. The recession-hit States is taking comfort in its moose growth figures with gross production expected to break 700,000 and net exports to grow by 2%. The worldwide dominance of Canada shows no signs of abating though with this year's moose population expected to match last year's record figures of one hundred million billion.

Europe's rise as an international moose power will slow slightly this year as a response to the European Union's move towards standardising the European moose. Stringent quality controls are holding back the development of the eastern european populations compared to last year when they contributed significantly to europe's strong growth figures. Norway, which is not an EU member but has observer status, strengthened in numbers relative to the Euro area with numbers of Norwegian moose, known locally as elk" expected to rise for the tenth consecutive year, particularly thanks to a strong showing in the last quarter.

As moose season reaches its close, researchers world wide are turning to science in an attempt to boost next year's figures. NASA studded the scientific community today with the announcement of their discovery that the moon is significantly smaller than previously believed. This conclusion, which is the conclusion of a ten-year collaborative project, will have profound implications for the moose

(B)

**Business Science Weekly**

28 FEB 2016

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**New York City Announces Deal on Carriage Horses in Central Park**

By MICHAEL GRYNBAUM

The Central Park horse carriage industry would be significantly reduced and its horses confined to a new stable within the park under a deal announced late Sunday by the de Blasio administration.

The agreement, which would shrink the industry from about 220 horses today to 95 horses by 2018, would ease a longtime political headache for Mayor Bill de Blasio, who promised campaign supporters that he would eliminate the industry on the first day of his administration.

The City Council must approve the deal, described by the mayors office as an agreement in concept, and a hearing could occur as soon as this week, officials said. The horse carriage industry is a longtime target of animal-rights activists who supported Mr. de Blasio's 2013 mayoral campaign, spending about \$1 million to attack his chief Democratic opponent, Christine C. Quinn. The activists remain big donors to Mr. de Blasio's political efforts.

But New Yorkers broadly opposed Mr. de Blasio's effort to end the industry, and some supporters of the mayor said they hoped he would abandon the cause.

City officials did not elaborate on Sunday evening about the financing for the stable, which would be Central Park's first for commercial carriage horses, according to park historians. People briefed on the deal, who requested anonymity to describe discussions meant to be private, said that an area near the park's 86th Street Transverse would be the most likely

site.

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By BOB O'BOBSTON

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Figure 2.8: Manipulation of (A) consumers' belief about effect of employee pay on product prices and (B) control condition (Study 5).

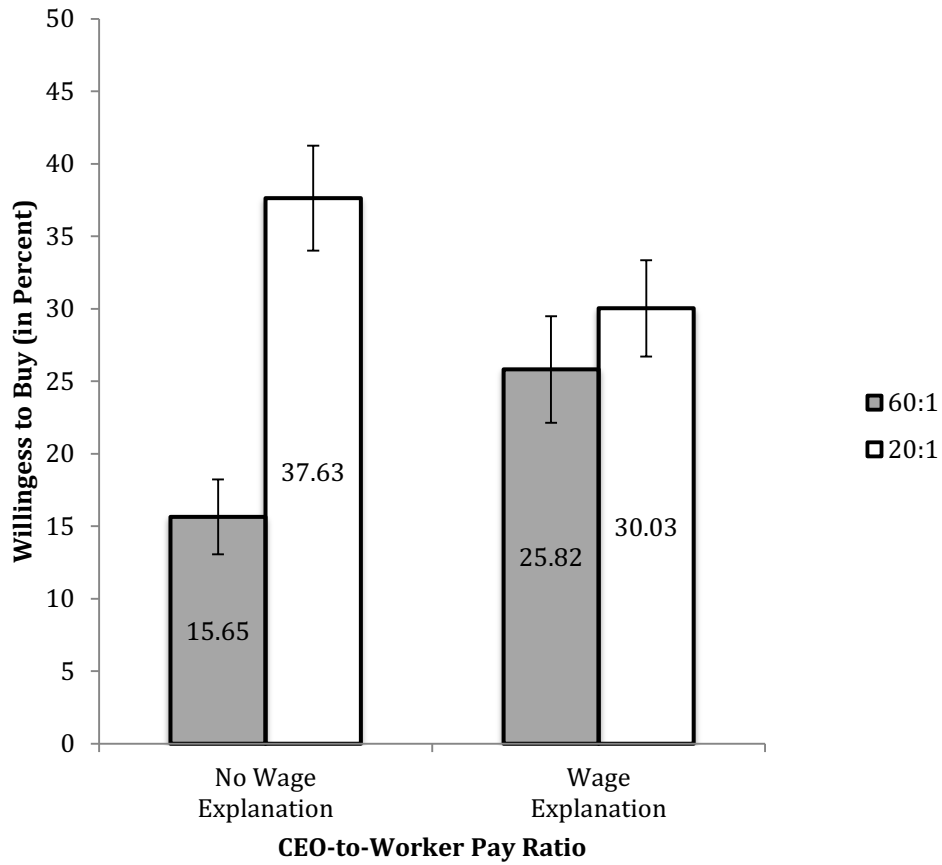


Figure 2.9: The explanation about the employee payment-price relationship attenuates the effect of pay ratio on buying intention.

Percentage cost discounts always beat percentage benefit bonuses: Helping consumers evaluate nominally equivalent percentage changes

BHAVYA MOHAN

PIERRE CHANDON

JASON RIIS

Abstract

Marketing offers that are framed as a “percentage change” in consumer cost vs. benefit can have highly non-linear impacts in terms of actual value for consumers. Even though two offers might appear identical, we show that consumers are better off choosing the offer framed as a percentage cost change over one framed as the opposite percentage benefit change, regardless of whether the net result is a gain (e.g., 50% less cost is better than 50% more benefit) or a loss (e.g., 50% less benefit is worse than 50% more cost) and regardless of whether costs or benefits are in the nominator or denominator of the standard rate (cost/benefit or benefit/cost). Three lab studies and one field experiment show that a majority of consumers (and particularly those with low numeracy) fail to accurately recognize the superiority of percentage cost changes over percentage benefit changes across various tasks and contexts. Even highly numerate consumers are prone to error. However, the provision of salient standard rates can reduce consumer error.

Marketing communications frequently contain percentage information about increases in consumer benefits (e.g., “15% more product per dollar,” “20% more miles per gallon”) or decreases in consumer costs (“15% lower price per package,” “20% fewer gallons per mile”). We show that while consumers generally perceive nominally equivalent offers to be of equal value, in fact, a percentage cost decrease is always a better value for the consumer than a nominally equivalent benefit increase.

Consider, for example, a consumer who is looking to upgrade her car. The current car’s engine gets a rate of 40 miles/gallon (MPG). There are two possible upgrade options. The first car’s engine is described as “needing 50% fewer gallons per mile.” This is a percentage reduction in “cost” since gallons are what the consumer must pay for. The second car’s engine is described as “leading to 50% more miles driven per gallon.” This is a percentage increase in “benefit,” since miles driven are what the consumer gets in return for her expenses. These offers might seem identical at the outset because the nominal percentage change is the same. However, the percentage cost reduction (50% fewer gallons) is better for the consumer, resulting in a 100% improvement in terms of miles per gallon (from 40 MPG to 80 MPG). In contrast, the percentage benefit increase (50% more miles) leads to only a 50% improvement in miles per gallon (from 40 MPG to 60 MPG). It turns out that it is hard for consumers to see the superiority of cost reductions (Chen et al. (2012), potentially to their great disadvantage.

In the current paper, we provide evidence as to why consumers have trouble perceiving the superiority of cost reductions, and we show how to address the issue to provide consumer protection. We begin by briefly demonstrating the generality of the cost reduction superiority, and by showing that it matters more in some circumstances than others. We then discuss the theoretical basis for consumers’ failure to recognize the cost reduction superiority, and we

suggest three important moderators of consumers' perceptions – the magnitude of the actual cost reduction superiority, the availability of underlying rate information, and the individual consumer's level of numeracy. Finally, we present four studies to test our hypotheses, across several contexts and in a task with real incentives.

### *CONCEPTUAL FRAMEWORK*

#### *Percentage Change and the Superior Value of Cost Reductions*

Before addressing the extent to which consumers detect the superior value of cost reductions over benefit increases, we must make a few notes about the superiority itself. First, even in the simple car engine example mentioned earlier, one may actually have to “do the math” to see that the 50% cost reduction leads to better fuel efficiency (80 MPG) than does the 50% benefit increase (60 MPG). In Table 1, we show the arithmetic for this example, and for several variations of it. The table shows that the cost reduction is better than a nominally equivalent benefit increase regardless of a) whether one is considering benefit/cost (e.g., MPG) or cost/benefit (e.g., GPM, as recommended by Larrick and Soll (2008)), and b) whether the nominal percentage change is large or small. The table also shows that the superiority of the cost reduction is much bigger in cases where the percentage changes are themselves large (i.e., 50% vs. 10%).

[Insert Tables 3.1 and 3.2 about here]

The superiority of cost reductions generalizes in one more way. The examples in Table 3.1 all deal with cases where the percentage change results in an improvement or a “gain” for the consumer. The fuel efficiency always gets better in these cases. But sometimes consumers have

to consider cases when fuel efficiency gets worse. For example a consumer may consider a powerful new car that uses 10% more fuel per mile driven or that drives 10% fewer miles per gallon of fuel used. Table 3.2 shows that in this loss domain as well, the consumer is always better off taking the cost change (in these cases a cost increase) over the nominally equivalent benefit change (in these cases a benefit decrease).

The focus of this paper will be the gain domain because it is percentage gains that are more commonly promoted to consumers, but it is important to note that what we have called the “superiority of cost reduction” (in the gain domain) is really a more general effect of “superiority of cost change” (generalizing to the loss domain as well).

[Insert Figure 3.1 about here]

In Figure 3.1 we show that the phenomenon generalizes beyond our engine example to any case where percentage changes in cost and benefits are compared. The figure shows graphically that percentage cost changes are always better than nominally-equivalent percentage benefit changes, regardless of whether one is considering their cost/benefit or benefit/cost impact, and that their superiority rises hyperbolically with the magnitude percentage change itself. The caption below the figure provides the mathematical reasoning to explain this relationship further.

### *Consumer Perceptions of Percentage Change Offers*

We do not expect consumers to detect the superiority of cost changes when comparing nominally equivalent cost and benefit changes. Prior research has shown that consumers are often swayed by nominal versus actual values, in contexts ranging from inflation to exchange rates (Shafir, Diamond and Tversky 1997, Raghurir and Srivatsava 2002, Wertenbroch et al



2007). Consumers are particularly prone to these types of biases when assessing percentage promotions (Heath, Chatterjee and France 1995, Chen and Rao 2007, Kruger and Vargas 2008). Chen et al. (2012) showed that consumers are typically indifferent toward nominally equivalent percentage changes in price and quantity (e.g., 50% off the base price of \$10 vs. 50% more product on a base of 8 oz.) for familiar consumer goods, failing to recognize that 50% off is better than 50% more, provided that consumers can choose the quantity level that they desire.

This leads to our first two hypotheses, which extend and generalize prior work:

**H1.** Consumers erroneously believe that nominally equivalent percentage changes in cost and benefits are equally valuable. This will be true a) whether the nominal percentage change is large or small, b) whether the effect is considered for a cost/benefit rate or benefit/cost rate and c) whether the changes result in an ultimate gain for the consumer or a loss for the consumer.

**H2.** The magnitude of the superiority of the percentage cost change will moderate the size of consumers' evaluation errors. Specifically, when the superiority of the percentage cost change is large, consumers will display the biggest errors in evaluating the relative value of cost changes and benefit changes.

### *Effects of Standard Rate Information Availability and Type on Perceptions of Cost Change*

#### *Superiority*

A ratio, which connotes the relationship between two measurements, is a construct that is easily understood, even by children (Sophian 2000). A rate is a specialized ratio, a comparison of two measurements that have different units. Rates are often expressed in a standardized way with a consistent value for the denominator; we call this a "standard rate." For example, when printer companies advertise the speed of their products, rates are often framed in terms of pages printed

per minute. Thus, consumers can compare varying levels of efficiency by comparing the numerators across standard rates for different printers. In some retail contexts, prices are translated into unit price, connoting price per unit of product. Indeed, such standard rates have been shown to ease cross-brand comparison of retail prices; and as a result, they are mandatory in some brick and mortar shopping contexts (Manning et al. 2003; Russo 1977). Bagchi and Davis (2012) have shown that consumers are unlikely to compute standard rates if the computation is challenging.

Thus far, none of the existing studies of consumer response to offers framed as a percentage change have examined the effects of providing standardized rate information on perceptions of those percentage changes (Chen et al. 2012; Hardesty and Bearden 2003; Mishra and Mishra 2011). Table 1 showed the difficulty applying percentage cost and benefit change to rates without the help of a calculator. We therefore expect that providing standard rate information will help consumers recognize the superiority of percentage cost changes.

This leads to the following hypothesis:

**H3.** Consumers will be more likely to detect the superiority of percentage cost changes over percentage benefit changes when each percentage change is accompanied by a standard rate.

While we expect that providing rate information will reduce consumer error, there is one additional nuance in the relationship between rate frame and gains/losses that leads to an additional prediction. When we compare two nominally equivalent percentages – i.e. using 50% more fuel versus driving 50% fewer miles – the resulting rates can be framed as a cost/benefit (gallons/mile) or as a benefit/cost (miles/gallon). The choice of rate frame can make the superiority of a percentage cost change more salient. This is perhaps most easily seen with

another look at Tables 1 & 2. As shown in Table 2, in the loss domain, the difference between 50% more cost and 50% less benefit offers is more apparent when framed as a cost/benefit rate (Offer 8: 37.5 gallons/1000 miles vs. 50 gallons/1000 miles) versus a benefit/cost rate (Offer 6: 26.7 miles/gallon vs. 20.0 miles/gallon). As shown in Table 1, this result is reversed in the gain domain: the difference between 50% less cost and 50% more benefit offers is more apparent when framed as a benefit/cost rate (Offer 2: 80 miles/gallon vs. 60 miles/gallon) versus a cost/benefit rate (Offer 4: 12.5 gallons/1000 miles vs. 16.7 gallons/1000 miles).

This example can be further generalized by looking at the gap between the dotted lines (cost changes) and the solid lines (benefit changes) in the top and bottom panels of Figure 1. The top chart shows that cost change superiority in the gain domain is particularly salient when the standard rate is framed as a benefit/cost. Choosing a 50% cost decrease over a 50% benefit increase magnifies the gain in terms of cost/benefit by a factor of 2 (from 50% to 100%). In contrast, the corresponding gain in terms of cost/benefit is only a factor of 1.5 (from -33% to -50%). The bottom chart shows that the opposite is true in the loss domain. There, cost change superiority is particularly salient when the standard rate is framed as a cost/benefit. In this case, choosing a 50% benefit decrease over the 50% cost increase magnifies the loss in terms of cost/benefit by a factor of 2 (from 50% to 100%) but magnifies the loss in terms of benefit/cost by a factor of 1.5 (from -33% to -50%).

We expect that consumers will find this pattern difficult to detect. Extending this analysis, we expect that the availability of a rate will be particularly helpful to consumers when the rate (cost/benefit or benefit/cost) makes the economic difference between nominally equivalent percentage changes as salient (large) as possible (Miyazaki et al. 2000; Pandelaere et al. 2011; Russo 1977). This leads to the following hypothesis:

**H4a:** In the gain domain, consumers will be better able to detect the superiority of percentage cost reductions (over benefit increases) when they are provided standard rate information in the benefit/cost frame than in the cost/benefit frame.

**H4b:** Conversely, in the loss domain, consumers will be better able to detect the superiority of percentage cost increases (over benefit decreases) when they are provided standard rate information in the cost/benefit frame than in the benefit/cost frame.

#### *Individual Differences in Numeracy*

Beyond the availability and type of rate information, we expect that numeracy will moderate people's ability to correctly choose between percentage cost and benefit offers. Numeracy is the ability to process basic probability and numerical concepts such as percentages (Lipkus et al. 2001). Numeracy has been shown to affect performance on judgment and decision tasks across a wide variety of domains (Dickert et al. 2011; Fagerlin et al. 2007; Weller et al. 2012). Highly numerate individuals are more able to avoid framing effects involving percentages; for instance, they are more likely to interpret a test score of "74% correct" as equivalent to a test score of "26% incorrect" than less numerate individuals (Peters et al. 2006). We therefore predict that numeracy is negatively associated with the likelihood of focusing on the nominal percentage value rather than on cost-benefit rates. The cognitive reflection test (CRT, Frederick 2005) - a three question test which captures an individual's ability to engage in more effortful and deliberate mathematical computation and suppress the spontaneous and intuitive response - is highly correlated with other numeracy measures (Kelley 2009; Weller et al. 2012). Chen et al. (2012) found that people who know how to calculate compound interest make fewer mistakes when choosing between bonus packs and price discounts with the same

percentage change value. However, they found no statistically significant effect for CRT in the two studies in which they measured it.

Given the theoretical support for expecting an effect of numeracy, and the always-present possibility of type-2 error in previous results, we re-examine the effects of numeracy across multiple studies. We expect a main effect of numeracy – as measured by both the three-item cognitive reflection test and the eight-item abbreviated numeracy scale - even after varying the types of cost and benefits (beyond quantity bonus vs. price discount), the valence of the outcome (gain vs. loss), task (choice vs. evaluation vs. ranking), and the product category.

This leads to the following hypothesis:

**H5.** Failure to detect the superiority of percentage cost changes will be moderated by consumer numeracy, such that consumers with better numeracy will be less prone to make errors in evaluation of percentage offers.

### *Overview of studies*

In study 1, we ask participants to estimate the impact of percentage changes in cost (fuel used) or benefit (miles driven) on an engine's efficiency (framed as either cost/benefit or benefit/cost). We find, as per H1, that consumers generally do not detect the superiority of percentage cost changes over nominally equivalent percentage benefit changes. We also find that consumers make larger errors when the difference between the percentage cost change and the percentage benefit change is larger, as per H2.

In study 2, we test H1 and H3 using percentages that are not nominally equivalent, a different task (ranking), and context (comparing printer speed). We ask participants to rank the efficiency of four printers after either a percentage change in cost (time needed to print a page)

or a percentage change in benefit (pages printed for a unit of time). We find, as per H3, that the availability of standard rate information (pages/minute) can lead consumers to more accurately recognize the superiority of a percentage cost change over a percentage benefit change.

In study 3, we test H1, H3 and H4 by asking participants to choose between and then rate the difference between a percentage cost (price) and percentage benefit (minutes) change to a wireless internet provider's service. We find, as per H4, that rate information can work particularly well when framed in a way that makes the consequences of a percentage change more salient. Consumers are better able to detect the superiority of a percentage cost increase (over a benefit decrease) when they are provided standard rate information in the cost/benefit frame than in the benefit/cost frame.

In study 4, we test H1 and H3 in an incentive-compatible context. We pay Mechanical Turk workers a set wage per word found on a word search puzzle. We then give them the opportunity to complete a bonus puzzle for which they must choose between two nominally equivalent wage offers: a 50% benefit change (more pay per word found) or a 50% cost change (finding fewer words for the same pay). As per H1, we find that participants do not recognize that a percentage cost change is superior to a nominally equivalent percentage benefit change. We also find that participants are more likely to choose the superior cost change when provided a standard rate (wage/word), as per H3.

Finally, across all four studies, we examine how numeracy moderates our effects. We find, as per H5, that more numerate consumers are generally less likely to make errors when assessing a superior percentage cost change versus a percentage benefit change. Together, these findings allow us to examine practical solutions to help consumers better choose between

percentage cost and benefit offers. They also offer novel implications from both the marketing and consumer protection perspectives.

### *STUDY 1: ASSESSING ABILITY TO EVALUATE PERCENTAGE CHANGES*

In this study, we quantify the errors consumers make when assessing the impact of percentage changes in the context of engine upgrades and fuel efficiency. We test hypothesis 1, by measuring whether people accurately estimate the impact of nominally equivalent percentage changes. Participants estimate the impact of percentage changes that are both large and small, both in the domain of gains and losses, and expressed both as cost/benefit rates and benefit/cost rates. We test hypothesis 2 by examining whether consumers make larger errors when the difference between the percentage cost change and the percentage benefit change is greater. Finally, by measuring participant numeracy, we also test whether more numerate consumers are less prone to error when assessing a superior percentage cost change or a percentage benefit change (hypothesis 5).

#### *Method*

We asked 606 Americans recruited via an online panel to evaluate changes to a car engine. We used a 2 (outcome: loss vs. gain) by 2 (rate framing: cost/benefit vs. benefit/cost) by 2 (change type: cost vs. benefit) by 3 (percentage change level: 17% vs. 33% vs. 50%) mixed factorial design. While outcome valence, rate framing, and change type were all manipulated between subjects, percentage change level was manipulated within subjects. The specific instructions varied by condition, but as an example, participants who evaluated a cost change in

the gain domain, with cost/benefit rate framing were told: “Imagine you own a car which currently uses 25 gallons per 1000 miles. Upgrading your engine will lead to 17% fewer gallons used per 1000 miles. By what percentage will your GPM (gallons per 1000 miles) decrease?” The response was selected on a sliding scale, from 1 to 100%. As another example, participants who evaluated a benefit change in the loss domain, with benefit/cost rate framing were told: “Imagine you own a car which currently gets 40 miles per gallon. A problem affecting your engine will lead to 17% fewer miles driven per gallon. By what percentage will your MPG (miles per gallon) decrease?” using the same sliding scale from 0 to 100%.

Thus, participants solely assessed either the impact of a percentage reduction in costs (gallons) or a percentage increase in benefit (miles), and they did so either in the gain or loss domain and for either a cost/benefit (GPM) or benefit /cost (MPG) change. Each participant assessed changes at all 3 percentage change levels (17%, 33%, and 50%), in random order. Table 3.3 shows the exact instructions given in each condition, as well as the correct responses. The table reveals that the degree of cost change superiority depends on the percentage change level itself (e.g. it is lower for 17% than for 50%), the framing of the rate (as cost/benefit or benefit/cost), and the valence of the offer (a gain versus a loss).

[Insert Table 3.3 about here]

In addition to completing the estimation task, all participants also completed both the 3 question cognitive reflection test (Frederick 2005), as well as the 8 question abbreviated numeracy scale (Weller et al., 2013).

## *Results*



We had predicted that the superiority of percentage cost changes over nominally equivalent percentage benefit changes would not be detected by consumers, as per hypothesis 1. To better visualize our data, we plotted the average participant guess relative to the actual percentage cost reduction or benefit increase (see Figure 2). The figure shows that participants generally (and erroneously) estimated that the nominal percentage of the offer would be its actual impact on MPG (miles per gallon) or GPM (gallons per 1,000 miles), thus, in aggregate, failing to detect the superiority of the cost change, consistent with hypothesis 1.

To test our hypotheses more directly, we coded our data to capture the extent to which the percentage cost change was superior to the nominally equivalent percentage benefit change. To test hypothesis 1, we first coded whether participants were in one of the four between-subjects conditions where the correct response was not the percentage of the offer (see Table 3). For instance, using 17% less gas for every 1000 miles actually leads to a 20% improvement in miles driven per gallon. In this example, there is a discrepancy between the percentage mentioned in the offer (17%) and its actual impact (20%).

To test hypothesis 2, we coded whether participants were in one of the four between-subjects conditions where the superiority of the percentage cost change appears especially large compared to the nominally equivalent percentage benefit change (see Table 3). As discussed in the introduction, the degree of cost change superiority depends upon the percentage change level, framing of the rate (as cost/benefit or benefit/cost), and the valence of the offer (a gain versus a loss). This can also be seen in the current context in Table 3. This coding allowed us to test for whether the size of the superiority of the percentage cost change would moderate the size of consumers' evaluation errors.

We conducted a 2 (nominal discrepancy: yes vs. no) by 2 (large cost change superiority: yes vs. no) by 2 (outcome valence: loss vs. gain) by 2 (rate framing: cost/benefit vs. benefit/cost) by 2 (percentage change type: cost vs. benefit) by 3 (percentage change level: 17% vs. 33% vs. 50%) mixed ANOVA, with participant accuracy as the dependent variable. To capture participant accuracy, we subtracted each participant estimate from the correct value and used the absolute value as the dependent variable.

We first examined the between-subjects effects. We found a significant main effect on accuracy of whether the nominal percentage change was correct,  $F(1,594) = 69.64, p < 0.01$ . Thus, as per hypothesis 1, consumers did not detect the difference between nominally equivalent percentage changes. With respect to hypothesis 2, we also found a significant main effect of large cost change superiority on accuracy  $F(1,594) = 24.98, p < 0.01$ . The interaction between the two was also significant  $F(1,594) = 30.86, p < 0.01$ . Thus, the size of the superiority of the percentage cost change moderated the size of consumers' evaluation errors.

Examining the multivariate tests, we also found a significant within-subjects main effect of the percentage change level on accuracy,  $F(2, 596) = 72.19, p < 0.01$ . Participants had a higher average error at the 50% level ( $M = 20.52, SE = 1.06$ ) than at the 17% level ( $M = 6.22, SE = .44$ ). This also supports hypothesis 2, that consumer errors will be bigger in cases where the superiority of the cost change is bigger.

We found no significant main effect of outcome valence (gain versus loss), rate framing (MPG versus GPM), or percentage change type (change in miles travelled versus change in gallons used), in all cases,  $p > 0.14$ . This indicates that none of these manipulations had any effect on participants' accuracy beyond their hypothesized effects. For example, it did not matter whether the offer was a gain or a loss or whether people were asked to consider MPG or gallons

per 1,000 miles. What mattered for accuracy was whether the actual impact of the offer was the nominal rate or not (in which case they were wrong), the size of the percentage, and the magnitude of the gap between the nominal and the actual impact.

Finally, we examined the effects of numeracy. When we included our measure of numeracy in our model as a covariate, we found a significant main effect of the 8-measure abbreviated numeracy score,  $F(1, 597) = 76.217$ . When we instead included CRT in our model as the measure of numeracy, the effect was also significant,  $F(1, 595) = 36.21$ . Participants' score on the 8-measure numeracy test and the 3-measure CRT measure were highly correlated ( $r = .74, p < 0.01$ ). We dichotomized participants between those who answered all three CRT questions incorrectly and those who had at least one correct answer. The average error of those with a CRT score of zero ( $M = 26.33, SE = 2.25$ ) was higher than those with a CRT score above zero ( $M = 18.52, SE = 1.17$ ). All of these results support hypothesis 5.

### *Discussion*

In Study 1, we find that consumers make large errors when assessing nominally equivalent percentage changes in cost and benefit because they assume that the nominal percentage change offered always corresponds to the actual effect on value, whereas this is true in only some conditions. As per hypothesis 1, these errors exist even after controlling for whether the effect is considered for a cost/benefit or benefit/cost rate, or whether or not the percentage changes lead to gains or to losses for the consumer. Consistent with hypothesis 2, these errors increase with the size of the cost change. Specifically, when the superiority of the cost reduction is large, consumers display the biggest errors in evaluating the relative value of

cost changes versus benefit changes. Finally, as per hypothesis 5, more numerate consumers are less prone to error, regardless of the numeracy measure used.

In the next study, we examine how participants assess percentage changes in costs versus benefits which are not necessary nominally equivalent (e.g. a 35% cost reduction vs. a 50% benefit increase). We test our third hypothesis, which is that the availability of standard rate information can lead consumers to more accurately recognize the superiority of a percentage cost change over a percentage benefit change. We also again examine whether individual differences in numeracy can moderate the propensity to make errors.

#### *STUDY 2: RATE AVAILABILITY, OUTCOME VALENCE, AND COGNITIVE ABILITY*

In this study, we test hypothesis 3 and examine whether standard rate information can moderate the propensity to make errors assessing nominal percentage changes. A cost change can be even more beneficial to a consumer than a benefit change with a higher nominal value (e.g., a 35% cost reduction leads to a better price per unit than a 50% increase in benefit). For instance, Chen et al. (2012) showed that consumers prefer nominally higher but economically inferior percentage changes (e.g., 50% more quantity over 40% less price) and that these errors occur regardless of whether consumers are comparing two gains or two losses. We test whether consumers are more likely to detect the superiority of percentage cost changes over percentage benefit changes when each percentage change is accompanied by a “standard rate” (in this case, pages/minute), as per hypothesis 3. We therefore build upon prior research on the effects of information availability in shopping contexts, such as the provision of rate information in the form of unit price (Manning et al. 2003; Russo 1977).

### *Method*

We used a 2 (outcome valence: loss vs. gain) by 2 (standard rate information: present vs. absent) between subjects design. We recruited 400 American adults via an online panel to rank printers by speed but excluded 5 of them due to duplicate IP addresses. We told participants to imagine that they must purchase a new printer to replace their previous one, which printed 25 pages per minute (ppm). We manipulated outcome valence by telling participants that all the printers available were either slower (loss condition) or faster (gain condition) than their previous one. We then asked them to rank the four printers in order of speed and to complete the three-question cognitive reflection test (Frederick, 2005).

We chose this printer scenario for a specific reason; as in the fuel context, there is a normatively correct response. With some products (specifically packaged goods) consumers may not only seek to get the best rate (e.g., price per unit). Instead, they may prefer a particular package size (e.g., 8 ounce jar of pasta sauce) or a specific price point (e.g., nothing more than \$6). With printers, however, people can print as many pages as they want; there is no downside to printing more pages per minute or taking less time to print a page.

As shown in Table 3.4, the change in printing efficiency was described as the percentage change in the number of pages printed per minute (benefit change) for two of the printers and as the percentage change in the time needed to print one page (cost change) for the other two printers. In the rate information present condition, a standard rate was also included alongside the percentage changes. The standard rate comprised speed described in pages per minute (ppm).

[Insert Table 3.4 here]

## Results

We analyzed whether or not participants correctly ranked the four printers with a binomial logistic regression with standard rate availability and outcome valence as categorical factors, CRT as a continuous variable, and all interactions. Consistent with hypothesis 3, the effect of providing standard rate information was strongly significant ( $\beta=3.23$ ,  $SE=.41$ ,  $p<.001$ ). When rate information was present, 57% of the participants provided the correct ranking (see Figure 3). When standard rate information was absent, only 8% of the participants ranked the printers accurately, which is not statistically higher than chance (given the 24 possible rankings,  $M_{random} = 1/24 = 4.2\%$ ,  $z = -1.45$ ,  $p = .15$ ). This supports hypothesis 3, that consumers are more likely to detect the superiority of percentage cost changes over percentage benefit changes when each percentage change is accompanied by a standard rate. The main effect of outcome valence was not statistically significant ( $\beta=0.43$ ,  $SE=.41$ ,  $p=.30$ ), replicating the effects of Study 1 and the general findings of Chen et al. (2012) that outcome valence on its own does not affect accuracy.

[Insert Figure 3.3 about here]

We also measured the effects of numeracy. As in Study 1, CRT was strongly associated with accuracy ( $\beta=.59$ ,  $SE=.16$ ,  $p<.001$ ). To provide a more intuitive understanding of the effects of numeracy, we dichotomized in Figure 3.3 participants into two groups: those who answered all three CRT questions incorrectly and those who had at least one correct answer. 21% of the participants with a zero CRT score found the correct order vs. 41% among those with a nonzero score. None of the interactions were statistically significant ( $p's>.10$ ), including the interaction between CRT and rate information ( $\beta=-.33$ ,  $SE=.31$ ,  $p=.29$ ), indicating that providing rate

information helped participants regardless of their cognitive ability. Thus, our results supported hypothesis 5, that consumers with better numeric abilities are less prone to error.

*Choice of fastest printer.* For a more generalized analysis of accuracy, we examined which printer was identified as the fastest printer. The majority (66%) of participants incorrectly chose the printer with the best nominal percentage change (printer #1: described as “prints 50% more pages per minute”). Only 19% of the participants chose printer #2 (described as “takes 45% less time to print each page”), which was the fastest but whose speed was measured with a nominally lower percentage cost change. The rest chose one of the two dominated, inferior printers, thus providing additional support for our prediction in hypothesis 1 that consumers misinterpret the nominal value of percentages. As in the ranking results, rate information significantly increased the proportion of participants choosing the correct printer (respectively, from 19% to 65%,  $\chi^2(1,395)=83, p<0.001$ ), consistent with hypothesis 3. Moreover, as per hypothesis 5, numeracy reduced consumer error, which was 30% among participants with a zero CRT score vs. 51% among those with a nonzero score,  $\chi^2(1,395)=17, p<0.001$ ).

### *Discussion*

Study 2 showed that a majority of people focused on nominal percentage information when assessing printer speed and therefore erroneously chose the option described in terms of percentage benefit change instead of the better option described in terms of percentage cost change, as per hypothesis 1. Most importantly, as per hypothesis 3, providing standard rate information (in this case, a benefit/cost rate) helped correct consumer errors, regardless of their numeracy. The availability of standard rate information (framed as pages/minute) led the

majority of consumers to more accurately recognize the superiority of a percentage cost change over a percentage benefit change, though some participants disregarded the rate information. Study 2 also demonstrated once again that more numerate consumers, as measured by cognitive reflection ability, are generally more accurate (consistent with hypothesis 5).

In the next study, we further test our hypotheses by looking at a task that ought to be easier for consumers, compared to the task they did in studies 1 and 2. In this study, participants are simply asked to choose between two nominally equivalent percentage changes (vs. rate individual percentage changes or rank four changes). We examine their choices in a context that is frequently encountered by consumers – a context where price is the cost and service quantity the benefit (specifically, we ask them to choose between differently priced internet access plans). In addition to testing hypotheses 1, 2, 3, and 5, this study also allows a clean test of hypothesis 4: specifically we can test whether standard rate information can work particularly well when framed in a way that makes the consequences of a percentage change more salient. We specifically manipulate the salience of standard rate information by manipulating the framing as cost/benefit vs. benefit/cost.

### *STUDY 3: MANIPULATING THE TYPE OF RATE INFORMATION*

In this study, we first ask consumers to evaluate the superiority of a percentage cost change relative to a percentage benefit change. We test hypothesis 4 by measuring whether framing of a standard rate as a cost/benefit versus a benefit/cost better enables consumers to detect the superiority of a percentage cost increase over a nominally equivalent percentage benefit decrease. To better understand the implications of rate framing, we examine the



interaction between outcome valence (gain vs. loss) and the type of standard rate provided (cost/benefit vs. benefit/cost). Specifically, we examine whether, in the gain domain, consumers will be better able to detect the superiority of percentage cost reductions (over benefit increases) when they are provided standard rate information in the benefit/cost frame than in the cost/benefit frame. Conversely, in the loss domain, we examine whether consumers will be better able to detect the superiority of percentage cost increases (over benefit decreases) when they are provided standard rate information in the cost/benefit frame than in the benefit/cost frame.

It is also important to note that in study 3, we use a scenario where the cost and benefit associated with a service (wireless internet access) have identical numerical values. Chen et al. (2012) suggest that errors occur because people neglect the differences in base values – i.e. price and quantity - associated with percentages. We suggest that neglecting rate information (comprised of a cost/benefit or benefit/cost ratio) matters more to consumers than neglecting the base value differences associated with a percentage. Consider, for example, the case of identical base values for the cost and benefit associated with a service. If “pay as you go” internet access costs 100 cents for 100 seconds, the base values associated with the cost and benefit are both 100. In this case, we expect that even consumers who consider base values will be incorrectly indifferent between a given percentage cost change (e.g., “50% price off for the same time online”) and the opposite percentage benefit change (“50% more time online for the same price”). This would occur due to a failure to notice that these promotions lead to different benefit/cost rates: 200 minutes per 100 cents for the cost change (50% less cost) vs. 150 minutes per 100 cents for the benefit change (50% more benefit). The same promotions can be framed as a cost/benefit rate as well: 50 cents/100 seconds (50% less cost) versus 67 cents/100 seconds

(50% more benefit). Finally, we again measure CRT, to examine how numeracy moderates our effects as per hypothesis 5.

### *Method*

We used a 2 (outcome valence: loss vs. gain) by 3 (standard rate availability: none vs. cost/benefit vs. benefit/cost) between-subjects design. We asked 606 American adults recruited via an online panel to evaluate wireless internet service providers (we excluded one participant who did not answer all the questions). We told participants to imagine that they had used an airport's wireless internet provider while waiting for a plane delay and asked them to choose between two "pay as you go" wireless providers or to indicate that the offers were equivalent. Table 4 provides the full stimuli descriptions. Those who chose one of the two providers were then asked to rate the difference between the two offers on a 5 point scale ranging from 'no difference' to a 'very big difference.' We used participant ratings of the cost or benefit offer to create a continuous scale from -4 to 4. Those who stated that the offers were equivalent were coded as a zero. Those who stated that the percentage benefit change was better had negative values (up to -4). Those who stated that the percentage cost change was better had positive values (up to +4). Thus, positive values were more accurate than negative values. This rendering of the evaluation data allowed us to subsequently test how the availability and type of standard rate information influenced whether participants detected the superiority of the percentage cost change.

Our standard rates were framed as either cents/100 seconds or seconds/100 cents. In the loss condition, as per Figure 1, the superiority of the 50% percentage cost change is more salient when the standard rate is framed as a cost/benefit rate (150 cents/100 seconds vs. 200 cents/100

seconds) versus a benefit/cost rate (50 seconds online/100 cents vs. 67 seconds online/100 cents). Choosing a 50% benefit decrease over the 50% cost increase magnifies the loss in terms of cost/benefit by a factor of 2. As shown in table 3.4, the opposite is true in the gain condition. Participants finally completed the cognitive reflection test.

[Insert Table 3.4 about here]

### *Results*

The evaluation data were regressed on rate condition, outcome valence, CRT, and all interactions. To test hypothesis 3 and 4, and examine the effects of the three-level rate intervention (no rate, benefit/cost rate, cost/benefit rate), we created two binary variables via Helmert coding and conducted a regression of the evaluation data on these two binary variables, outcome valence, CRT, and all interactions. This allowed us to estimate the effects of providing any standard rate information (vs. no rate) and the difference between the two rate types.

[Insert Figure 3.4 about here]

As shown in Figure 3.4, rate information significantly increased the evaluation of percentage cost change superiority ( $\beta=.80$ ,  $SE=.14$ ,  $p<.001$ ), consistent with hypothesis 3. Moreover, as per hypothesis 4, we found the expected interaction between outcome valence and ratio type ( $\beta=.97$ ,  $SE=.34$ ,  $p<.01$ ), indicating that the cost/benefit rate was more effective than the benefit/cost rate in the loss condition than in the gain condition. Finally, the main effect of CRT was statistically significant ( $\beta=.13$ ,  $SE=.06$ ,  $p=.02$ ); people with a higher CRT evaluated the percentage cost change more positively than the percentage benefit change option, consistent with hypothesis 5. None of the other 2-way or 3-way interactions (between ratio, outcome valence and CRT) were significant ( $p>.20$ ).

*Discussion*

Study 3 demonstrates the robustness of the effects of standard rate information availability and cognitive ability, even for the simpler task of comparing two nominally equivalent percentages when the base rates (100 cents or 100 seconds) are identical, providing support for hypotheses 3 and 5. The study also provides support for hypothesis 4, and demonstrates that standard rates framed using a cost/benefit format can be particularly effective in the loss domain, when people erroneously prefer a quantity decrease to the more efficient, but more aversive, monetary cost increase. Thus, we find partial support for hypothesis 4: the framing of a standard rate as a cost/benefit versus a benefit/cost better enable consumers to detect the superiority of a percentage cost increase over a nominally equivalent percentage benefit decrease.

However, we do not show the opposite in the gain domain, that consumers will be better able to detect the superiority of percentage cost reductions (over benefit increases) when they are provided standard rate information in the benefit/cost frame than in the cost/benefit frame. Participants appeared to have a general inclination for choosing decreases in price over increases in time, and a preference for highlighting money over time in the numerator. Research has shown that people are more sensitive for changes in money than for changes in time (Chandran and Morwitz 2006; Leclerc et al. 1995; Okada and Hoch 2004; Prelec and Loewenstein 1998; Shampanier et al. 2007). This suggests that percentage changes in time and money may not be psychologically identical, even if the results are economically equivalent. Since this does not replicate the results of study 2 and those of Chen et al. (2012), who found no effect of outcome

valence on the assessment of percentage changes, these effects can be more carefully examined in future research.

In our next study, we test whether a standard rate can help work and wage decisions in an incentive-compatible field experiment.

#### *STUDY 4: PERCENTAGE FRAMING AND WORK CHOICES*

In this study, we examine both the propensity to make errors as well as the effectiveness of standard rate information in an incentive-compatible context. We shift our focus to a new real world context: the framing of bonuses and incentives. We ask Mechanical Turk workers to complete a job (a word search puzzle). They are subsequently paid a wage for every task completed (every word found). We then give them the opportunity to complete a second job, and choose between two nominally equivalent ‘bonus structures’: a 50% “benefit” change (more pay per word found), or a 50% “cost” change (finding fewer words for the same pay), thereby testing hypothesis 1. Finally, we examine how providing a standard rate (wage/task) and individual differences in numeracy moderate our findings, thereby testing hypotheses 3 and 5.

#### *Method*

We hired 150 workers from Mechanical Turk and paid them 6 cents for each word with at least three letters that they could find in less than one minute in an 8 by 8 letter word search puzzle. After completing the first puzzle, we gave them the option to earn more money by solving an additional word search puzzle. If they decided to continue, they had to choose between two payment options, one framed as a benefit increase and the other as a cost decrease.

Participants were then randomly assigned to one of 2 conditions (rate information: present vs. absent). In the rate information present condition, the benefit increase option was described as “you will make 50% more money per correct word that you find (this means that you will be paid 9 cents per word)” and the cost decrease option was described as “you will need to find 50% fewer correct words to make as much money as you earned in the first task (this means that you will be paid 12 cents per word).” Note that, in this study, cents per word is a benefit/cost rate. The information in parentheses was omitted in the rate information absent condition. In both conditions, the instructions stated “as in the first word search game, your goal will be to find as many words as possible in 1 minute,” thus indicating that consumers would be paid for every word found. This wording was included to ensure that those who chose to find 50% fewer correct words to make as much money did not curtail their effort. Thus, participants were able to choose both their workload and wage. Participants finally completed the cognitive reflection test (Frederick, 2005).

### *Results*

We excluded two participants because of duplicate IP addresses. In the first puzzle, participants found on average 4 words and were paid \$0.24. Of the 148 participants who chose to do the bonus puzzle, only 7% chose the more advantageous percentage cost decrease payment scheme (fewer words) when rate information was absent vs. 49% when rate information was present ( $\chi^2(1,144)=31.2, p<.001$ ). Thus, as per hypothesis 1, participants did not detect the superiority of a percentage cost change over the nominally equivalent percentage benefit change. However, as per hypothesis 3, participants were more likely to detect the superiority of

percentage cost changes over percentage benefit changes when each percentage change was accompanied by a standard rate.

Participants worked equally hard on the bonus puzzle regardless of their payment scheme choice ( $M_{Benefit}=6.17$ ,  $SE=.19$  vs.  $M_{Cost}=5.73$ ,  $SE=.37$ ,  $t(254)=1.6$ ,  $p=.11$ ). Thus, participants did not work harder on the second puzzle as a function of choosing the superior cost change payment scheme. This also indicates that those who chose the inferior percentage benefit change did not curtail their effort as a result of question wording. As a result, participants who chose the percentage cost change offer earned a bonus of \$0.69 each, a 23% premium for doing the same amount of work over those who chose the percentage benefit change. Because there were no differences in the total number of words that were found across the two conditions, this higher wage was entirely driven by the choice of the correct payment scheme.

Finally, the proportion of participants choosing the more advantageous cost payment scheme rose from 23% among participants who missed all three CRT questions to 39% for those who answered all three questions correctly ( $\chi^2(3,144)=6.9$ ,  $p=.07$ ), providing further evidence for hypothesis 5.

### *Discussion*

Overall, study 4 replicated our findings in an incentive-compatible field experiment involving work choices, providing additional support for hypothesis 1, 3 and 5. In the absence of a standard rate, most participants chose the percentage benefit change over the superior percentage cost change. Providing standard rate information led to significantly more participants choosing the percentage change in cost (words) versus benefit (wage). Participants worked equally hard, regardless of the bonus payment scheme chosen. Some participants might

have had a hard time understanding the cost payment scheme. Working 50% less for the same amount of money is not a typical way of offering bonus payments, and the description could have come across as disfluent. Thus, particular framings may be more natural to certain contexts, and this could skew preferences. In our field study, we also had a marginally significant effect of cognitive reflection, providing further evidence to support hypothesis 5. Participants with higher cognitive reflective abilities were more likely to choose the percentage cost change over the percentage benefit change.

### *GENERAL DISCUSSION*

Findings from three laboratory studies and one incentive-compatible field experiment show that most consumers cannot correctly evaluate marketing offers communicated as a percentage change in cost or in benefit because they assume that the nominal value of percentages represents their true impact on value. This result replicates across a variety of tasks and domains. We show that providing standard rates can help correct these errors and lead the majority of consumers from making the wrong choice to making the correct one. This occurs because consumers fail to consider the impact of percentages in terms of cost/benefit (or benefit/cost).

#### *Implications for Consumers*

We find that rate information helps all groups of consumers equally but that some, particularly those with low cognitive ability, persist in choosing the wrong percentage benefit option even after being provided with a rate. For instance, even after providing rate information to participants in study 4, 51% of the online workers still did not choose the superior cost change



over the benefit change. Similarly, even after providing standard rate information in study 2, 43% of consumers still did not rank printers correctly. Future work could more systematically explore how different types of costs versus benefits affect how consumers assess percentages.

Moreover, future research could examine whether less numerate people with a low CRT score disregard rate information or if they are just unable to process even simple ratios (Viswanathan et al. 2005). In most retail contexts, rate information resulting from a percentage promotion is not calculated out for consumers. At the moment, unit prices are not mandatory for retail distribution in the majority of US states, and are never mandatory when accompanying percentage promotions or for consumers shopping online (U.S. Department of Commerce 2013). Thus, in the absence of a unit ratio provided by a retailer, consumers would need to calculate the resulting rate from a promotion themselves. Thus, future research could examine whether other aids – such as the provision of a calculator – would help consumers derive rates and assess percentages more accurately.

We also show in study 3 that price/minute rate information is relatively more effective than minute/price information to help consumers make correct decisions in the loss domain. Future research is necessary to determine whether this result would hold for benefits and costs other than time and money.

### *Implications for Marketers*

Unlike a consumer, a marketer would likely seek to maximize the cost/benefit ratio resulting from a sale (or conversely, minimize the benefit/cost given to the consumer). Thus, from the marketer's perspective, cost or benefit changes should always be framed as a percentage change in benefit as opposed to a percentage change in cost. Marketers should frame their offers as a

percentage benefit change, which produces the largest nominal percentage, rather than by the equivalent percentage cost change. From a profit-focused perspective, marketers would be better off framing a promotion as a 40% bonus pack as opposed to a 40% price discount, because they will receive a higher price per unit sold.

While potentially beneficial to businesses, framing offers as a percentage benefit change rather than a percentage cost change could lead consumers to sub-optimal choices, where they might be unknowingly paying more for a unit of a good than expected. Future work could further explore the tension between the marketer and consumer perspectives, and how each party could benefit from greater transparency around percentage promotions.

Our results suggest that if a firm intends to be truly transparent, marketing offers promoting a percentage change in cost or benefit should focus on how the underlying rate changes, taking into account any local conventions. This means, for example, that percent increases in fuel efficiency should be described as increases in MPG in the United States and as decreases in GPM (or, rather, liters per 100 kilometer) in other parts of the world.

However, there might be scenarios where both marketers and consumers would benefit from the actual percentage change in cost or benefit being highlighted. Consider for example a new technology, which leads to a 33% improvement in terms of liters per 100 km. Our results show that it would be more effective to frame this improvement as “50% more km driven per liter” than “33% fewer liters needed per 100 km.” Thus, building on prior work by Larrick and Soll (2008), our results suggest that for countries around the world that rate fuel efficiency in liters per 100 km driven (the vast majority), fuel efficiency improvements should be framed as a percent benefit increase rather than the nominally lower percent cost decrease.

In certain retail shopping contexts, government intervention might help further mandate and standardize how percentage and rate information is presented to consumers, since these laws vary by state and channel (U.S. Department of Commerce 2013). Although less frequent than promotional offers, marketing communication can also convey percentage losses to the consumer, such as cost increases or benefit decreases. In Brazil for example, consumer protection laws mandate that percentage reductions in product quantity due to downsizing be displayed on the front of packages (Neves and Itacarambi 2008). However, our research suggests that a percentage reduction of a package might be difficult to interpret without an accompanying unit price ratio. Thus, our findings have important implications for public policy. They affirm the effectiveness of providing unit price information in conjunction with percentage promotions, and support the case that rate information should be made mandatory across different retail domains.

Of course, the tension between the perspective of the consumer and the perspective of the marketer would only exist if marketers know that the highest nominal framing is best for them, but do they? In a short follow-up study, we distributed a questionnaire to 75 MBA students at a top business school (average GMAT score = 703). All participants were asked to assess a question from the marketer perspective: “Marketing offers often feature a percentage. Percentage offers can be framed as a benefit change (‘X% more product’, ‘X% more miles per gallon’) or as a cost change (‘X% lower price’, ‘X% fewer gallons per mile’). These offers can both be gains for the consumer, for example: 20% more products vs. 20% less price. They can also both be losses for the consumer, for example: 40% fewer miles per gallon vs. 40% more gallons per mile. Imagine marketers are seeking to maximize their unit margin. From the marketer's perspective, which of these sentences is true?”

Participants were given two minutes to answer the question and the order of the five choices was randomized.

- (1) An 'X% cost change' is always better than a '-X% benefit change'
- (2) An 'X% benefit change' is always better than a '-X% cost change'
- (3) An 'X% cost change' is as valuable as a '-X% benefit change'
- (4) The best offer depends on whether we are dealing with gains or losses to the consumer

- (5) There is no general solution. You have to do the math depending on the value of X

We found that only 34% of participants chose the correct response (2). However, this proportion was only marginally statistically superior from the chance level ( $M=20%$ ,  $z=-1.8$ ,  $p=.08$ ). Thus, even highly trained MBA students could not identify the normative solution from the marketer's perspective. This suggests that many marketers may not even recognize consumers' misperceptions of percentage offers.

original framing	original engine capability	offer name	offer description	offer type	arithmetic	new engine capability	relative superiority of cost change for consumer
benefit / cost	40 miles / gallon	offer 1 (cost)	"Use 10% less fuel for each mile driven"	10% less cost	$(40) / (1 \times 90\%)$	44.4 miles / gallon	+0.4 miles/gallon
		offer 1 (benefit)	"Drive 10% more miles for each gallon of fuel"	10% more benefit	$(40 \times 110\%) / (1)$	44.0 miles / gallon	
		offer 2 (cost)	"Use 50% less fuel ..."	50% less cost	$(40) / (1 \times 50\%)$	80.0 miles / gallon	+20.0 miles/gallon
		offer 2 (benefit)	"Drive 50% more miles ..."	50% more benefit	$(40 \times 150\%) / (1)$	60.0 miles / gallon	
cost / benefit	25 gallons / 1000 miles	offer 3 (cost)	"Use 10% less fuel ..."	10% less cost	$(25 \times 90\%) / (1000)$	22.5 gallons / 1000 miles	-0.2 gallons/1000 miles
		offer 3 (benefit)	"Drive 10% more miles ..."	10% more benefit	$(25) / (1000 \times 110\%)$	22.7 gallons / 1000 miles	
		offer 4 (cost)	"Use 50% less fuel ..."	50% less cost	$(25 \times 50\%) / (1000)$	12.5 gallons / 1000 miles	-4.2 gallons/1000 miles
		offer 4 (benefit)	"Drive 50% more miles ..."	50% more benefit	$(25) / (1000 \times 150\%)$	16.7 gallons / 1000 miles	

*Table 3.1:* The table shows that for any two nominally equivalent percentage offers that are beneficial for the consumer (e.g., offer 1a and offer

1b), the cost reduction is always a better deal for the consumer than the benefit increase. This is true regardless of whether the offers are applied to a benefit/cost frame or to a cost/benefit frame. It is also true for both small percentage offers (10%) and for large percentage offers (50%), although the superiority of the cost reduction is larger when the percentage offer is larger.

original framing	original engine capability	offer name	offer description	offer type	arithmetic	new engine capability	relative superiority of cost change for consumer
benefit / cost	40 miles / gallon	offer 5 (cost)	"Use 10% more fuel for each mile driven"	10% more cost	$(40) / (1 \times 110\%)$	36.4 miles / gallon	+0.4 miles/gallon
		offer 5 (benefit)	"Drive 10% less miles for each gallon of fuel"	10% less benefit	$(40 \times 90\%) / (1)$	36.0 miles / gallon	
		offer 6 (cost)	"Use 50% more fuel ..."	50% more cost	$(40) / (1 \times 150\%)$	26.7 miles / gallon	+6.7 miles/gallon
		offer 6 (benefit)	"Drive 50% less miles ..."	50% less benefit	$(40 \times 50\%) / (1)$	20.0 miles / gallon	
cost / benefit	25 gallons / 1000 miles	offer 7 (cost)	"Use 10% more fuel ..."	10% more cost	$(25 \times 110\%) / (1000)$	27.5 gallons / 1000 miles	-0.3 gallons/1000 miles
		offer 7 (benefit)	"Drive 10% less miles ..."	10% less benefit	$(25) / (1000 \times 90\%)$	27.8 gallons / 1000 miles	
		offer 8 (cost)	"Use 50% more fuel ..."	50% more cost	$(25 \times 150\%) / (1000)$	37.5 gallons / 1000 miles	-12.5 gallons/1000 miles
		offer 8 (benefit)	"Drive 50% less miles ..."	50% less benefit	$(25) / (1000 \times 50\%)$	50.0 gallons / 1000 miles	

*Table 3.2:* The table shows that even in the domain of losses (i.e., where all of the percentage changes involve "offers" that leave the consumer worse off), the cost change is still superior to the benefit change.

Starting Rate	Outcome	Type	Context	Question	Actual rate impact			Statistical coding	
					x = 17%	x = 33%	x = 50%	Nominal Discrepancy	Large % Cost Superiority
Cost/Benefit  (Imagine you own a car which currently uses 25 gallons per 1,000 miles)	Gain	Cost	Upgrading your engine will lead to X% fewer gallons needed per 1000 miles.	By what percentage will your GPM (gallons per 1000 miles) decrease?	17%	33%	50%	No	No
	Gain	Benefit	Upgrading your engine will lead to X% more miles per gallon.		14%	25%	33%	Yes	No
	Loss	Cost	A problem affecting your engine leads to X% more gallons needed per 1000 miles.	By what percentage will your GPM (gallons per 1000 miles) increase?	17%	33%	50%	No	Yes
	Loss	Benefit	A problem affecting your engine leads to X% fewer miles per gallon.		20%	50%	100%	Yes	Yes
Benefit/Cost  (Imagine you own a car which currently gets 40 miles per gallon)	Gain	Cost	Upgrading your engine will lead to X% fewer gallons needed per 1000 miles.	By what percentage will your MPG (miles per gallon) increase?	20%	50%	100%	Yes	Yes
	Gain	Benefit	Upgrading your engine will lead to X% more miles driven per gallon.		17%	33%	50%	No	Yes
	Loss	Cost	A problem affecting your engine leads to X% more gallons needed per 1000 miles.	By what percentage will your MPG (miles per gallon) decrease?	14%	25%	33%	Yes	No
	Loss	Benefit	A problem affecting your engine leads to X% fewer miles driven per gallon.		17%	33%	50%	No	No

Table 3.3: Study 1 stimuli summary, with correct responses and coding.

Naïve speed rank (based on nominal percentage value)	Actual speed rank (based on rate)	Gain condition	Loss condition
1	3	Printer 1: Prints 50% more pages per minute than your previous printer (37.5 ppm).	Printer 1: Prints 33% fewer pages per minute than your previous printer (16.8ppm).
2	1	Printer 2: Takes 45% less time to print each page than your previous printer (41.2 ppm).	Printer 2: Takes 35% more time to print each page than your previous printer (18.5 ppm).
3	4	Printer 3: Prints 40% more pages per minute than your previous printer (35 ppm).	Printer 3: Prints 38% fewer pages per minute than your previous printer (15.5 ppm).
4	2	Printer 4: Takes 35% less time to print each page than your previous printer (38.5 ppm).	Printer 4: Takes 40% more time to print each page than your previous printer (17.8 ppm).

Table 3.4: Study 2: Stimuli description. Speed rate (in pages per minutes) was only provided in the rate information present condition.



Rate condition	Wireless internet provider	Loss condition	Gain condition
	Initial provider	Spent \$6.00 in 10 minutes with the initial provider's introductory rate.	Spent \$6.00 in 10 minutes with the initial provider.
Control (no rate information)	Provider Y (percentage benefit change)	Costs 50% more for the same time online compared to the initial provider.	Costs 50% less for the same time online compared to the initial provider.
	Provider X (percentage cost change)	Offers 50% less time online for the same price compared to the initial provider.	Offers 50% more time online for the same price compared to the initial provider.
	Initial provider	Spent \$6.00 in 10 minutes with the initial provider's introductory rate of 100 cents/100 seconds online.	Spent \$6.00 in 10 minutes with the initial provider (rate of 100 cents/100 seconds online).
Cost/benefit rate information	Provider Y (percentage benefit change)	Costs 50% more for the same time online compared to the initial provider (rate of 150 cents/100 seconds online).	Costs 50% less for the same time online compared to the initial provider (rate of 50 cents/100 seconds online).
	Provider X (percentage cost change)	Offers 50% less time online for the same price compared to the initial provider (rate of 200 cents/100 seconds online).	Offers 50% more time online for the same price compared to the initial provider (rate of 67 cents/100 seconds online).

	Initial provider	Spent \$6.00 in 10 minutes with the initial provider's introductory rate of 100 seconds online/100 cents.	Spent \$6.00 in 10 minutes with the initial provider (rate of 100 seconds online/100 cents).
Benefit/cost rate information	Provider Y (percentage benefit change)	Costs 50% more for the same time online compared to the initial provider (rate of 67 seconds online/100 cents).	Costs 50% less for the same time online compared to the initial provider (rate of 200 seconds online/100 cents).
	Provider X (percentage cost change)	Offers 50% less time online for the same price compared to the initial provider (rate of 50 seconds online/100 cents).	Offers 50% more time online for the same price compared to the initial provider (rate of 150 seconds online/100 cents).

Table 3.5: Study 3: Stimuli description.

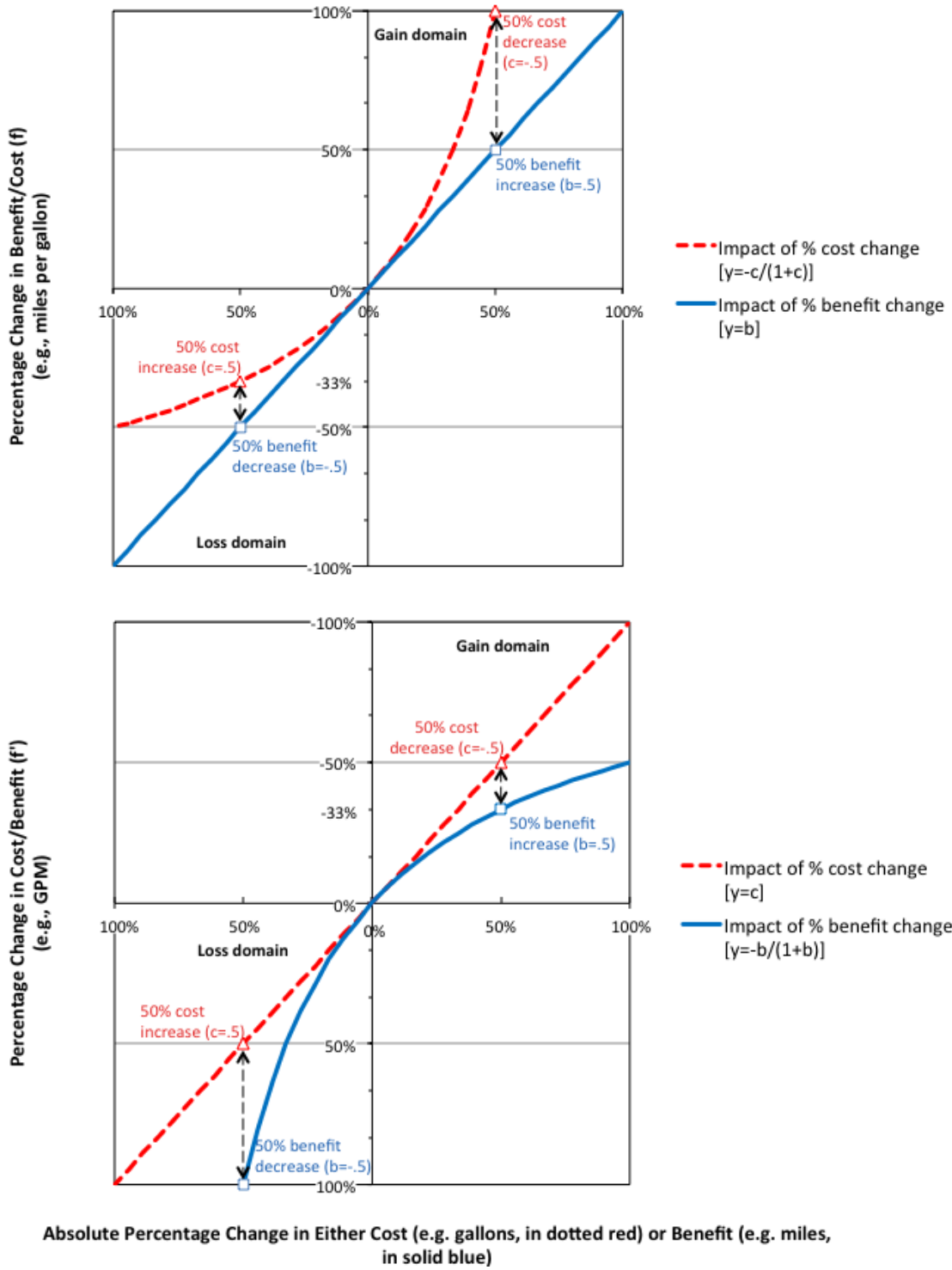


Figure 3.1: Effects of Percentage Changes in Benefits and Cost on Percentage Changes in Benefit/Cost (top) or Cost/Benefit (bottom). For any % change in cost or benefit, the dotted % cost change line is

always above (i.e. more beneficial for the consumer) than the solid % change benefit line. As shown in the top chart, a  $b\%$  change in benefit leads to the same  $b\%$  change in benefit/cost whereas a  $-c\%$  change in cost leads to a hyperbolic  $[c/(1-c)]\%$  change in benefit/cost. If  $c$  and  $b$  are equal,  $[c/(1-c)]\%$  is always larger than  $b\%$ . Thus, the cost change is always better for the consumer because it leads to more benefit per unit of cost. The bottom chart shows that it is the same when rates are expressed as cost per unit of benefit. Finally, the cost change superiority grows larger as the outcome (cost/benefit or benefit/cost) becomes most positive, as indicated by the double arrows.

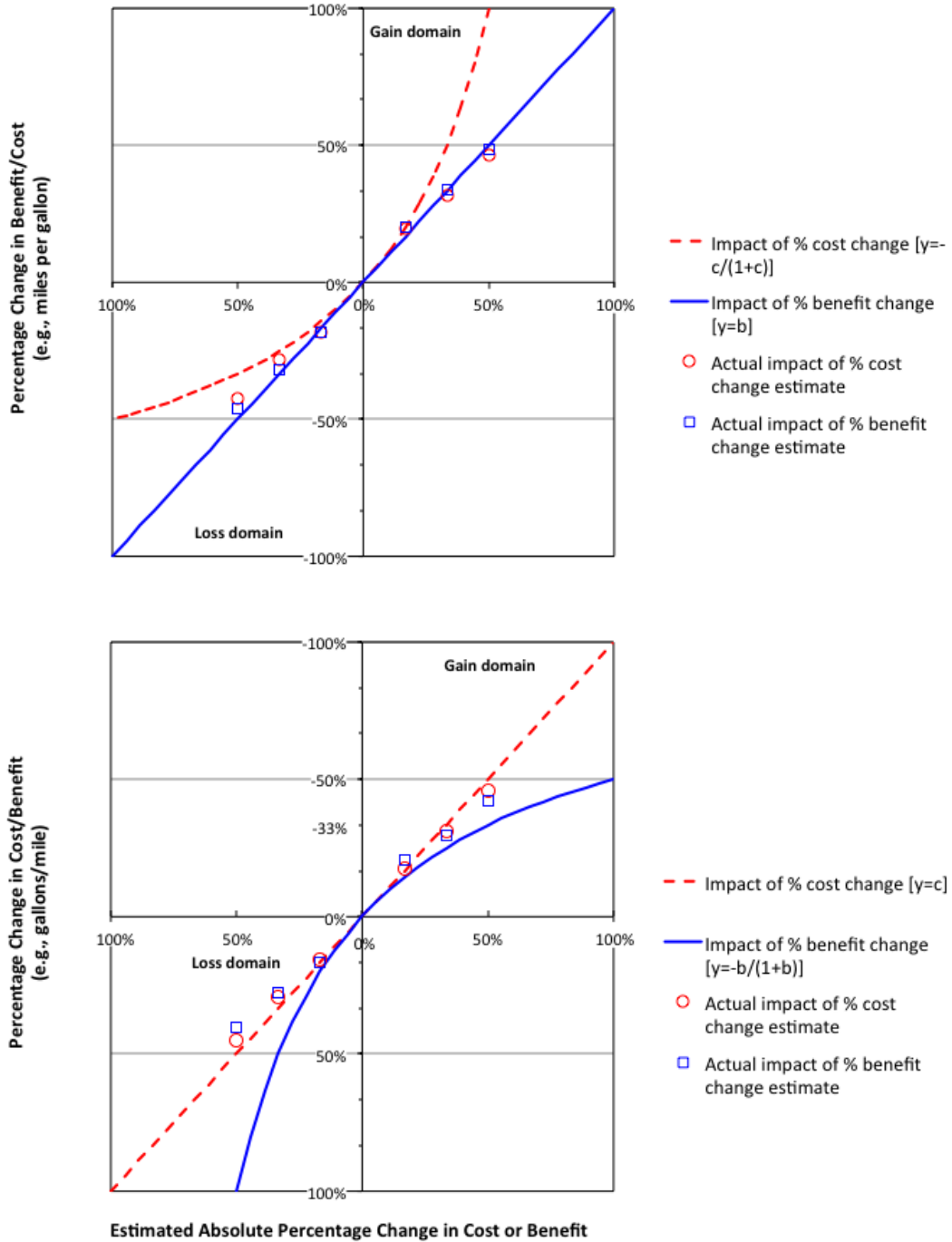
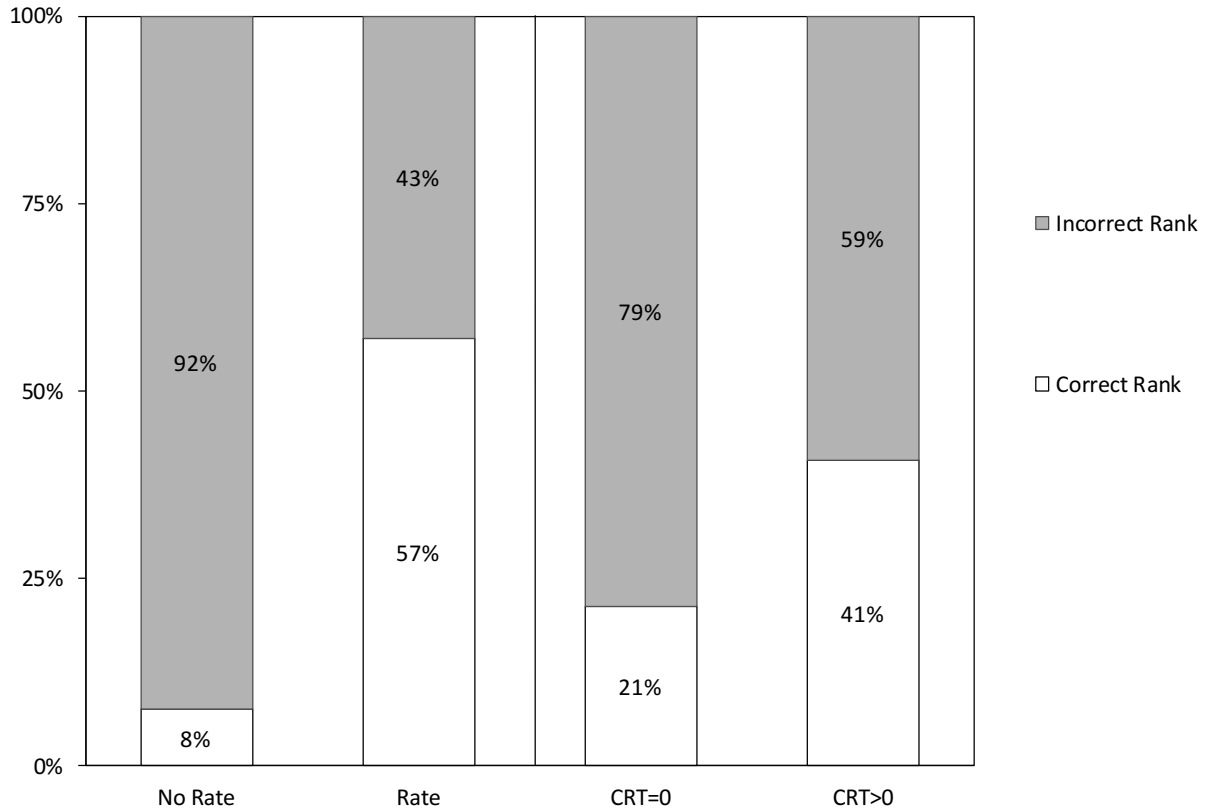
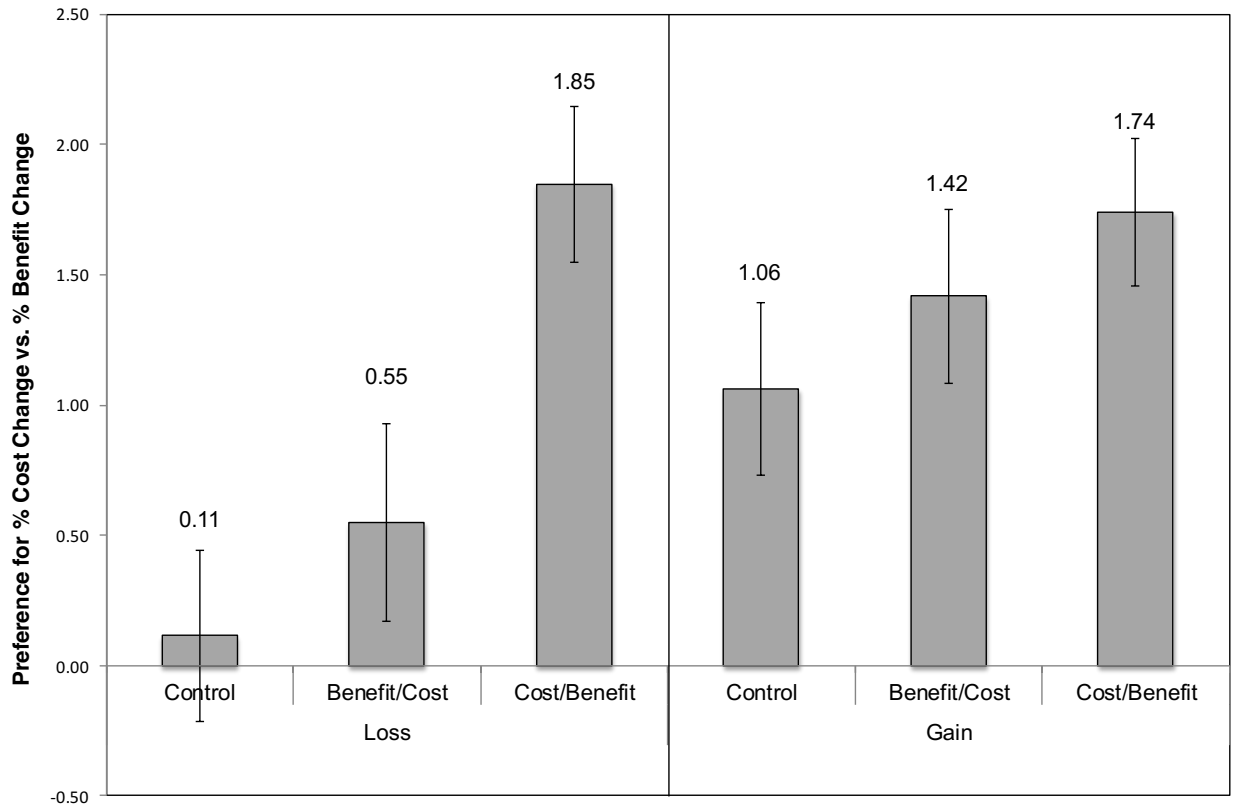


Figure 3.2: Study 1: Actual and estimated impact of 17%, 33%, and 50% changes in cost (gallons consumed) and benefit (miles driven). Participants failed to recognize the non-linear

effects of some of the changes and hence did not infer the superiority of the percentage cost change over the nominally equivalent percentage benefit change. The magnitude of their errors increased with the size of the change.



*Figure 3.3:* Study 2: Rate information and cognitive ability improve the odds of correctly ranking four printers by order of speed, when their speed is described either in terms of percentage changes in benefits (e.g., “50% more pages per minute”) or in cost (e.g., “35% less time to print each page”) over a benchmark printer. Rate information was provided as pages per minute. CRT = Cognitive Reflection Test.



*Figure 3.4:* Study 3: Rate information most improves the evaluation of the most cost-efficient “percentage cost” offer, especially in the loss condition. Cost/benefit rate information is more effective than benefit/cost information in the loss than in the gain condition. More positive values on the Y-axis indicate a positive evaluation of a cost change of 50% as opposed to the opposite benefit change of 50%.



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