

Price Promotions, Beneficiary Framing, and Mental Accounting

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Abstract: We introduce the idea of beneficiary framing to the promotion response literature. Two large-scale field experiments (total $N = 73,010$) in the context of print-at-home coupons show that framing a savings message as affecting a beneficiary (i.e., “save for X”) increases coupon printing and redemptions. This beneficiary framing effect is equivalent to an incremental \$0.05 (3.4%) of coupon value. We report eight additional studies (total $N = 3,052$) to investigate the mechanism, replicability, and role of salience. We find evidence supporting a mental accounting explanation: participants with beneficiary-related budget categories rate beneficiary-framed coupons as more account-relevant and more valuable than non-beneficiary-framed coupons. The experiments further suggest that the beneficiary framing effect (i) holds across multiple common budgeting domains, (ii) cannot be fully explained by an affect transfer mechanism, (iii) is not solely attributable to the visual salience of the beneficiary framing treatment, and (iv) exhibits a boundary condition in which beneficiary framing may reduce promotion response when the recipient does not have a relevant beneficiary. Overall, the results suggest a low-cost approach that some marketers can use to increase promotion uptake.

Keywords: price promotions, coupons, beneficiary framing, field experiments, mental accounting

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Price promotions vary in their framing and design elements. For example, promotions can display product images or specify whom the offer benefits. Framing and design elements do not impact the economic value of a promotion, but they may affect consumer decisions and purchases.

A small but growing literature examines how promotion designs affect consumer behaviors in digital environments (Aribarg and Schwartz 2020, Aydini et al. 2014, Fong 2017, Fong et al. 2019, Sahni et al. 2018). However, promotion design often covaries with offer size and terms, and both may be driven by unobservable market conditions, making it difficult to separate how design elements affect promotion response from other factors.

Beneficiary framing has not previously been studied in the context of price promotions. We conducted two large-scale field experiments utilizing print-at-home coupons for a well-known brand in collaboration with two partner firms. Consumers find print-at-home coupons online at dedicated brand websites and multi-brand portals (e.g., Coupons.com, Redplum, Retailmenot). Once printed, coupons can be redeemed at traditional retail stores that accept paper coupons.¹ The two field experiments leverage the format's flexibility for testing different combinations of non-economic coupon design features (i.e., beneficiary framing, call to action, product image alterations) as well as traditional economic variables (i.e., offer value) in a baby-related product category.

In the first field experiment, we found that making a beneficiary-framed savings statement (i.e., “save for your baby”) significantly increased coupon printing and redemptions and was equivalent to an incremental \$0.05, or a 3.4% increase in coupon value: a sizable

¹ Print-at-home coupons reduce coupon distribution costs, maintain paper-based auditing safeguards against misredemptions and fraud, are compatible with most point-of-sale systems, and help preserve coupons' ability to price discriminate between consumer segments by imposing a pre-shopping effort cost.

increase given the ease of adding the short beneficiary savings statement. The promoting firm subsequently adopted the novel stimulus and tested the stimulus in a second field experiment six months later, replicating the increased printing rate under somewhat different conditions. Additionally, a subset of users who were previously exposed to the beneficiary framing treatment and who returned to the website via a promotional email continued to be positively influenced by beneficiary framing, suggesting the result is not due to the novelty of the beneficiary framing message.

The field data provide causal evidence that beneficiary framing reliably increases promotion uptake, but they do not reveal why this occurs. Identifying possible mechanisms is both academically interesting and managerially relevant: deeper understanding can help predict when the effect might or might not replicate in other categories. We had no ex-ante reason to expect a single mechanism could fully explain the effect, so we hypothesized three potential mechanisms and tested them using seven additional online experiments.

First, we hypothesized that specifying a beneficiary can increase the probability that an offer is coded as directly affecting a budget earmarked for the beneficiary (e.g., “save for your baby” activates a mental budget for children). This mental accounting explanation is consistent with previous work that has studied how consumers group funds into different categories or mental accounts (Thaler 1999; Zhang and Sussman 2018). Across three online experiments, we find that (1) a beneficiary savings statement increased the likelihood a coupon was classified as belonging to a target budget category and (2) the presence of a beneficiary-related budget category increased the perceived value of beneficiary-related offers but did not alter the perceived value of offers unrelated to the beneficiary category. Together, these links provide evidence that mental accounting contributes to the effect uncovered in the field experiments.

Second, we hypothesized that beneficiary framing might lead consumers to project their emotions for the beneficiary onto the coupon. This affect transfer mechanism would increase the perceived value of beneficiary-framed offers compared to control offers when the beneficiary is positively perceived (likely the case in the field studies where the beneficiary is a baby) and would decrease the value of beneficiary-framed offers when the beneficiary is negatively perceived (MacKenzie et al. 1986; Sweldens et al. 2010). To test affect transfer, we manipulated affect to a beneficiary (i.e., induced a positive or negative affect) and then asked online participants to rate coupons with or without beneficiary framing. The results indicate that beneficiary framing increases the coupon's desirability, even when a negative affect toward the beneficiary is activated. Hence, affect transfer does not appear to explain the field results.

One limitation of the field experiments was that they did not manipulate the prominence of the beneficiary framing message, leaving the role of salience unclear. Of course, some degree of salience is needed for any message to be perceived and have an effect. However, if the mere presence of the beneficiary framing message increased attention to the coupon, then the result might be due simply to increasing the time consumers spend thinking about the coupon and lead to an increased printing and redemption. We addressed this potential concern in two additional experiments where we varied both the content and visual salience of a message, or only the message's visual salience. The results suggest that message salience alone cannot explain the beneficiary framing effect we observe in the field experiments, but we remain mindful that beneficiary framing effect sizes may depend on numerous factors including context, beneficiary, relationship, product category, and promotion design, among others.

Next, we review the relevant literature including academic studies of coupons, framing effects, and digital promotions. The following two sections present the field experiment context,

methods, and results for the two large scale field experiments that find beneficiary framing increases promotional uptake. Thereafter, we present online experiments exploring mental accounting, affect transfer, and visual salience. The final section concludes and discusses managerial implications.

Related Literature

Coupon and Promotion Literature

Academic studies of coupon effects date back to Kuehn and Rohloff (1967), who proposed the first model relating brand purchase shares to coupon availability and face value. The literature has grown to study a wide variety of topics such as new coupon response models (Reibstein and Traver 1982, Neslin 1990, Raju et al. 1994, Bawa et al. 1997, Dhar and Raju 1998), promotion profitability (Neslin and Shoemaker 1983, Leone and Srinivasan 1996, Dhar et al. 1996), the trade-off between shelf price and coupon offer value (Anderson and Song 2004, Kumar et al. 2004), purchase timing (Neslin et al. 1985, Neslin and Shoemaker 1989, Papatla and Krishnamurthi 1996), consumer factors (Mittal 1994, Gonul and Srinivasan 1996), retailer promotions (Krishnan and Rao 1995), expiration date (Krishna and Zhang 1999) and mere-exposure effects (Venkatesan and Farris 2012).

Coupons and temporary price promotions have been the focus of numerous experimental studies, both in the laboratory and in the field. Among lab studies, Raghubir (1998) showed that the magnitude of a coupon's discount can lead inexperienced consumers to infer a higher shelf price for the product, partially offsetting the coupon's ability to promote sales to new consumers. LeClerc and Little (1997) offered the first evidence that coupons' non-economic terms could influence redemption: participants receiving a brand advertisement with a coupon indicated

greater purchase intentions and willingness to clip the coupon than the coupon-only control group. Guimond et al. (2001) manipulated coupon offer value and presentation, finding that non-deal-prone consumers were more sensitive to coupon value than deal-prone consumers. Raghubir et al. (2004) review and summarize the literature, proposing that coupons can have economic, informational and affective effects on consumers, and suggest that firms should maximize nonmonetary informational and affective elements in order to optimize economic profits.

Outside of the laboratory, couponing is one of the oldest applications of field experiments in marketing. Chapman (1986) published the first field experiment in direct mail coupons, in which restaurant coupons were randomly mailed to households. Coupon reception significantly predicted purchase incidence and, based on a sales response model, it was estimated that coupon profitability was about 5% of normal gross margins in the absence of a promotional effort. Bawa and Shoemaker (1987) followed a similar design, mailing to households either a low, medium, or high value coupon for a mature brand in a frequently purchased category. They found that coupons increased purchase probability among both frequent brand users and also among non-users. Bawa and Shoemaker (1989) further examined the characteristics of the responding households, showing that incremental sales increased with education, homeownership, and household size. Venkatesan and Farris (2012) proposed a conceptual model for how retailer-customized coupon campaigns affect purchases where revenue was influenced by both exposure to campaign effects and coupon redemptions. In a quasi-experimental setting, they found that mere exposure to customized campaigns more strongly contributes to campaign returns than coupon redemptions, suggesting exposure itself can act as an important marketing tool.

Our study is related to efforts to estimate behavioral effects in the field, as has often been called for (e.g., McGrath and Brinberg 1983, Cummings et al. 2015, Lynch et al. 2015). Sudhir et

al. (2016) exemplify this literature: they randomized advertising copy in a direct mail appeal to potential donors for a nonprofit and found results consistent with several sympathy biases found in lab studies. These results, when combined with statistically and economically significant differences in donations between experimental conditions, helped practitioners optimize mail promotions and increase donations.

Recent digitization has lowered the cost of tracking promotion delivery and response at the consumer level, leading more firms and researchers to experiment with digital promotions. Sahni et al. (2017) reported the results of 70 field experiments run at a large ticket resale platform. They found that email promotions increased revenues significantly, but 90% of incremental gains came from non-promoted products, suggesting that targeted promotions can have important spillover effects on primary demand for live entertainment tickets. Relatedly, Fong (2017) and Fong et al. (2019) showed in a series of field experiments that promotions that are precisely tailored to consumers' tastes led to less consumer search outside of the promoted categories, suggesting a possible downside to narrowly targeted promotions. Moreover, Sahni et al. (2018) found personalizing an email advertisement with the name of the recipient increased the likelihood the recipient opened the message, thereby increasing sales leads and reducing the likelihood the recipient unsubscribed from the email campaign. Danaher et al. (2015) sent consumers coupons via SMS text messaging, finding that the location and time of coupon reception influenced redemptions.

The work here shares the empirical context of McGranaghan et al. (2019), which reported the results of two field experiments that randomized the value of initial promotions on an online coupon website. They found that high-value initial offers causally increased the printing and redemption of identical subsequent offers. In contrast, the central question of this paper entails

understanding the effects of the *non-economic* coupon manipulations of beneficiary framing on coupon response. The first field experiment in this paper reports the results of four novel non-monetary treatments that were conducted at the time of McGranaghan et al. (2019)’s first field study. In order to quantify the monetary value of the non-economic treatments, we also utilized the variation in offer value, which was the only attribute reported by McGranaghan et al. (2019). In other words, we exploit the exogenous variation in a coupon’s economic value to compute the compensating variation of the relevant non-economic treatments. In addition to this field experiment, we replicate the effects of non-economic treatments in a second field experiment which was not shared by McGranaghan et al. (2019), and we conduct several additional studies to explore the mechanisms that underlie the field evidence.

Framing Effects Literature

A separate literature has found that the semantics with which information is presented can have profound effects on choice, a finding often referred to as framing effects (Tversky and Kahneman 1981; Keren 2011; Teigen 2015). For example, Ganzach and Karsahi (1995) report the results of a field experiment where loss-framed benefit messages (e.g., highlighting potential losses when not utilizing a product) were more likely to increase credit card uptake compared to gain-framed benefit messages. A sizeable portion of the framing literature has addressed how differences in price framing can influence consumer choice. For example, Guha et al. (2018) found that framing discounts such that the discount depth is compared to the sales price rather than the original price increased consumer perceptions of the discount depth and purchase intentions. Furthermore, previous work investigated how framing discounts on bundled products influenced purchase rates and found that which product in a bundle is discounted can impact consumer choice (Khan and Dhar 2010; Janiszewski et al. 2004). Additional work on the “money

illusion” has found that the perceived price in a foreign currency is influenced by its nominal value (Shafir et al. 1997), suggesting that changing the price metric (e.g., from dollars to yen) alters the perceived price. Consistent with this, the units with which numerical quantities are framed (e.g., one year versus 12 months) has been found to alter consumer judgments (Ülkümen et al. 2008; Monga and Bagchi 2012; Ülkümen and Thomas 2013). Overall, the work highlighted here suggests that the framing of price information can impact consumer purchasing decisions.

Additional work has investigated whether framing decisions as primarily benefiting oneself versus others influences choice. Much of this literature has been conducted in the domain of charitable giving where donations can be viewed as intrinsic (i.e., primarily benefiting oneself in the “warm glow” of altruism) or extrinsic (i.e., primarily benefiting the charity’s mission); however, the findings are somewhat mixed. For example, research has found that appeals framed as benefitting oneself can be more (Holmes et al. 2002) or less (Pessemier et al. 1977; Fisher et al. 2008) successful than those framed in terms of benefitting others. Other work has found that the effectiveness of such appeals varies depending on whether donations are public or private in nature (White and Peloza 2009). Overall, there is clear evidence suggesting that non-economic beneficiary framing could influence consequential choices, though the possibility remains untested and unexplained in the commonplace context of coupons.

Contribution

The current paper contributes to the existing literature by testing novel treatments related to beneficiary framing within the long tradition of field experiments in the coupon literature. We study a popular means of coupon distribution—print-at-home coupons—that has not received much attention in the scholarly literature, and we propose novel experimental treatments based on previous work in psychology and marketing. Specifically, this paper is the first to demonstrate

that beneficiary framing increases promotion uptake, to explain why that occurs, and to estimate the compensating variation of non-economic coupon elements.

Field Experiment 1: The Beneficiary Framing Effect

Method

Empirical context. The field experiments were conducted on a website which offers print-at-home coupons to consumers and ran for a prespecified duration of 34 days, during which 36,634 consumers visited the site and were exposed to the treatments described below. When a consumer navigated to the site, they were required to first log in with an email address or standard social-network profile.² Afterwards, they were presented with more than 10 coupon offers, one per product, for products sold by many different brands. Consumers could not view coupon offers without logging in.

The coupon website displayed three coupon offers per row in multiple rows. The experiment applied a single random treatment to the three coupon offers displayed on the top row. The three treated coupon offers were for three products sold by a single brand in an unspecified baby-related category. Non-treated coupon offers were displayed on subsequent rows and were for products across a range of different household product categories, most of which were not related to babies. Figure 1 illustrates a similar layout. Most consumer devices displayed at least the first two rows of coupon offers without manual scrolling. Non-treated coupon offers did not vary in position or value throughout the entire experiment and included a

² The brand and coupon website did not use any paid or owned media to drive traffic to the coupon website during the sample period of the study. The brand did not use any other means to distribute digital coupons at the times of the experiments reported in this paper.

product image, made a savings statement (e.g., “Save \$X.XX”), and described the specific coupon offer (e.g., “on any one [brand name] [product name]”).

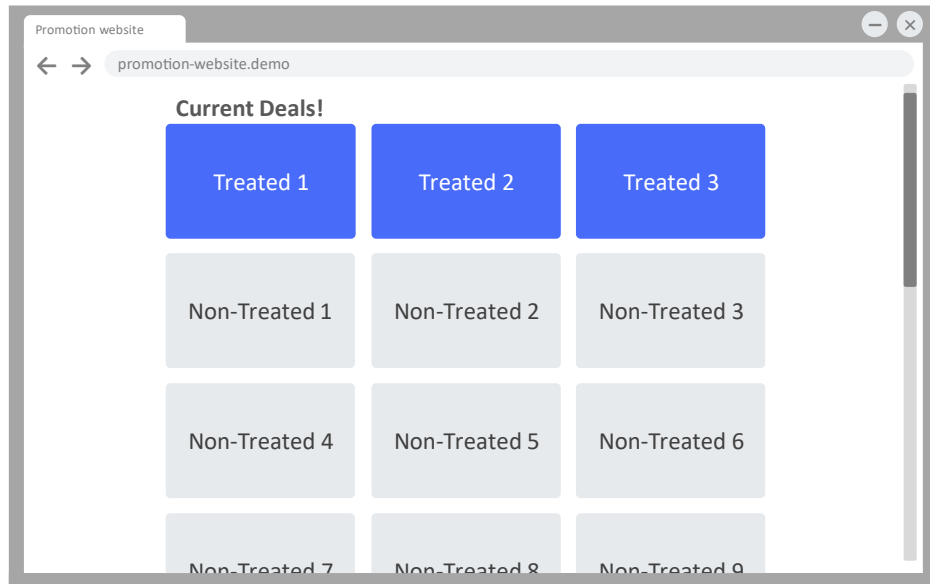


Fig. 1 Mock-up of coupon website. Treated coupons were always in the top row and followed by Non-Treated coupons. The attributes, positions and values of Non-Treated coupons were held constant across all experimental conditions.

The three treated products differed in price level and the age of the baby they were intended to serve. For illustration purposes only, we use a brand of baby shoes, Robeez, but Robeez was not involved in the research. We omit names of the manufacturer, brand, products, product category, coupon website, and offer values to preserve partners’ anonymity as required by a non-disclosure agreement. We refer to the three treated products as P1, P2, and P3.

Each coupon offer displayed a small check-box in the upper-left-hand corner. Consumers first clicked the check-box for any offer they wanted to print, then clicked a “print” button in the webpage header. After clicking the “print” button, the coupon website sent them a series of images for printing. These images looked like traditional paper coupons; each coupon included a product image, an expiration date, a coupon value and terms, a machine-readable barcode, a

Quick Response (QR) code, and a lengthy legal passage stating permissible use.³ The coupon website's technology did not allow the printed coupons to vary with experimental attributes other than offer value. Thus, only pre-print coupon offers displayed the non-economic treatments.

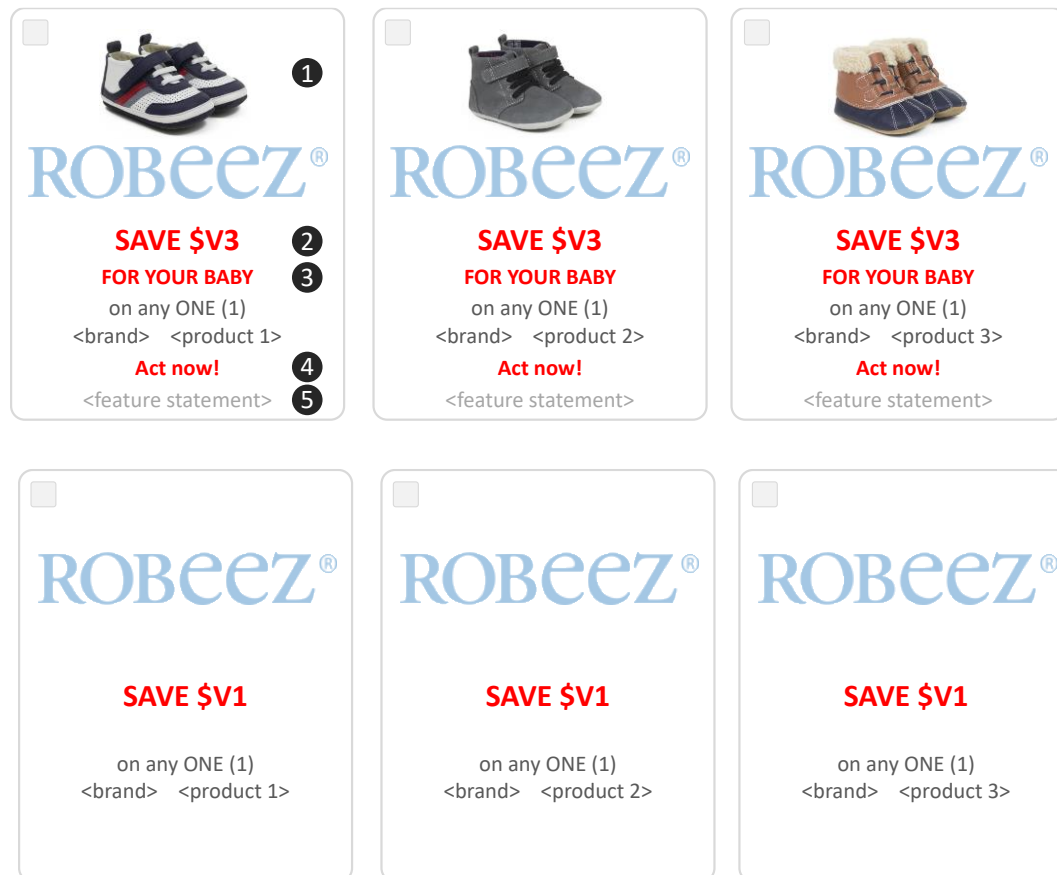


Fig. 2 Illustration of the experiment design. The top left coupon illustrates the 5 experimental attributes that were randomly varied using a full factorial design with 192 total cells, each with equal probability: (1) Image attribute with three levels; (2) Coupon value attribute with four levels; (3) Beneficiary framing attribute with two levels; (4) Legacy Call to Action framing attribute with two levels; and (5) Feature statement attribute with four levels. The top middle and top right coupon examples are included for completeness to illustrate consumers in the experiment viewed three treated coupons and highlight the fact that coupons were for different products. Each consumer viewed all three coupons with same combination of experimental attributes, as illustrated in the top row. The bottom row illustrates a treatment consisting of a combination of control levels for each of the five attributes. Brand name Robeez included for illustration purposes; Robeez was not involved in the research.

³ All coupons expired two weeks after printing. The website limited each logged-in consumer to two print requests per coupon in any two-week period; a third request was met with a “Too many prints” error message.

Consumers could redeem printed coupons when purchasing the specified product at any retail store that accepted paper coupons.⁴ After use, retailers delivered paper coupons to professional auditing and clearing firms. Retailers were reimbursed and the coupon website database was updated several weeks later, tying individual-level coupon redemptions to the specific offers that were viewed on the website.

Experimental Design. We randomized five attributes of coupon offers in a balanced, full factorial design as illustrated in Figure 2:

(1) *Image Framing:* All coupon offers displayed a prominent brand logo beneath one of three image conditions with equal probability: a picture of a baby; an image of the product, whose packaging featured a picture of a baby; or no image (control).

(2) *Coupon Value:* The coupon's economic value was randomly treated with one of four value points with equal probability. We refer to experimental coupon values in ascending order as V1, V2, V3, and V4. The highest value treatment, V4, was double the lowest treatment, V1. V2, which represented the historical average offer value on the website, and V3 were spaced evenly between V1 and V4. Note that this feature was the only one that varied the coupon's economic value, and we only use this to compute the compensating variation of the non-economic treatments.

(3) *Beneficiary Framing:* Half of all coupon offers framed available savings by saying, "SAVE \$X.XX FOR YOUR BABY." The control condition used no framing, saying only "SAVE \$X.XX," as the website had done prior to the experiment.

⁴ Retailers require consumers to print manufacturers' print-at-home coupons because auditors required the paper coupons. This policy differed from digital-only coupons that may be displayed on a mobile device at a retail check-out counter. Digital-only coupons tend to be offered by retailers (rather than manufacturers) or in vertically integrated channels. Such settings reduce concerns about coupon misredemptions and fraud.

(4) *Call to Action Framing*: With 50% probability, each coupon offer either contained a Legacy Call to Action (“Act Now!”; abbreviated “LCTA” hereafter) or contained no call to action (Control). We manipulated LCTA presence because we were unsure how the total information presented might affect coupon uptake, and as a point of comparison for evaluating the beneficiary framing effect. However, it is important to note that the experiment was not designed explicitly to test LCTA effects, the LCTA stimulus was less prominent than the beneficiary framing stimulus, and that the particular LCTA treatment was chosen simply because it had been used previously and we wanted to avoid introducing too many unfamiliar elements to returning consumers. Hence, we only consider the LCTA effect as a contextual point of reference.

(5) *Feature Statement*: With equal probability, each coupon offer displayed one of four conditions: a product-specific feature statement, a product-specific feature specifically framed as benefitting the baby (“for your baby”), a product-specific feature statement framed in comparison to a competing product, or no feature statement (control). To preview some of the results, none of these feature statements had any detectable main effects or interactions with other treatment attributes. In retrospect, we speculate that this was because the feature statements were printed in a smaller, lighter gray font at the bottom of the coupon offers. Given this, they were far less prominent than the other treatment attributes. However, the null effects of feature statements suggest that simply varying the quantity of information by altering the number of words within each coupon offer is not sufficient to explain some of the main results reported below.

Each attribute was manipulated independently using JavaScript code executed in the consumer’s browser. Upon each consumer’s first visit to the website during the sample period, random numbers were drawn to determine the coupon offer treatment attributes. The treatments

selected were held constant for all subsequent visits by the same consumer during the sample period, to avoid exposing users to multiple treatments.

The same draw of five treatment attributes (Image, Value, Beneficiary Framing, Legacy Call to Action, and Feature) was applied to all three focal coupon offers on the top row of the website. We did this with the goal of minimizing the “design distance” within the treated coupons.⁵ The positions and attributes of all subsequent non-experimental coupon offers were held constant throughout the sample period to eliminate confounding variation (see Figure 1).

Outcome measures. The data record offer printing and redemption for each consumer-offer combination. We report effects of experimental treatments on both behaviors. Additionally, since P1, P2, and P3 were intended for babies of different sizes, a consumer responding to a treatment is likely to print a promotion for the product that corresponds to her baby’s current size. It is therefore important to note that consumers self-selected into coupon offers like P1, P2 and P3. Hence, in addition to analyzing the printing of each product, we also analyze a response behavior of printing any focal brand coupon, which we denote as “any P1/P2/P3” and is defined as the union of P1, P2, and P3 prints.

Responding to a coupon offer with a print request is a nearly-instant response to an experimental stimulus. Differences in printing rates across treatments are causal effects. Across all treatments, 40.2% of coupon site visitors printed experimental coupons.

Based on conversations with executives at the partner firms, brand managers believed that coupon printing itself might benefit the brand directly, in addition to serving as an intermediate step required for redemption. Printing is a tangible marker of brand engagement, which may result in a more favorable brand attitude and increased brand purchase intention.

⁵ Note that although we attempted to minimize the “design distance” within treated coupons, we did not alter untreated coupons. Hence, we were not able to entirely eliminate design differences between all coupons.

These changes in attitudes may produce “halo effects,” which increase purchase probability even in the absence of coupon redemption. This reasoning corresponds strongly to the “mere-exposure” results of coupon distribution found by Venkatesan and Farris (2012). We want to emphasize that these were unproven speculations in this empirical setting, but we believe they accurately reflect the views of the career professionals responsible for brand promotions.

Coupon redemption behavior is important but sparser than printing, reducing statistical power and the likelihood of detecting true differences between conditions. Experimental treatments could influence several choices prior to a redemption decision, such as store choice or purchase timing, muddying the direct effects of treatment on redemption behavior within each experimental cell. There are also many non-experimental factors that influence coupon redemptions more than coupon prints, such as store choice, on-shelf promotions, advertising, competitors’ marketing strategies, or other unobserved shocks encountered within a retail store. Overall, 13.3% of coupon site visitors redeemed experimental coupons. Moreover, given that printed coupons did not display framing effects, the impact of a non-economic treatment on redemptions is likely to be diminished.

In contrast with coupon printing, redemption may or may not be profitable for the manufacturer. Redemptions impose the direct cost of coupon payments to retailers. They may also cannibalize loyal consumers’ purchases that may have otherwise occurred at a higher price point. The overall profitability of coupon promotions is quite difficult to estimate causally—see Neslin and Shoemaker (1983) for a full accounting of all relevant factors, which include dynamic behaviors such as purchase timing and stocking up. Therefore, it is not possible for us or our partner firms to accurately assess coupon profitability with the available data. For these reasons, we report results for both coupon printing and redemptions, but focus more on printing behavior

as it permits a more reliable explanation of the mechanisms that drive differences in non-economic treatments.

Statistical power. The power of the experiment to detect the causal effects of the treatment attributes depends on the number of levels per attribute. For two-level attributes (Savings Beneficiary, Call to Action), given a sample of 36,634 consumers and overall coupon printing rate of 40.2%, the experiment's minimum detectable effect (for 80% power and 95% confidence) is 2.6%.⁶ For a three-level attribute (Image), the experiment's minimum detectable effect is 3.1%. For four-level attributes (Value, Feature), the minimum detectable difference between cells is 3.6%.

It is substantially less likely that we will detect effects of coupon offers on coupon redemptions, because redemptions occur only one-third as often as printing. 13.3% of exposed consumers redeemed experimental coupons, so the minimum detectable effects are 5.4% for two-level attributes, 6.7% for three-level attributes, and 7.7% for four-level attributes.

Consumer Characteristics. The website's database has several historical variables that can be used as "demographics" of site visitors. An earlier version of the coupon site asked new registrants to optionally indicate if they had a newborn, a baby, and/or a toddler at home, and whether they would like to be notified of future coupon offers by email.⁷ These questions were not displayed prominently during the registration process, their completion was not mandatory, and there was no requirement or clear incentive for consumers to complete them, so they are

⁶ All minimum detectable effects are relative differences between conditions, not absolute differences.

⁷ Opting in to receive notifications only indicates consumers' stated willingness to receive email promotions. The coupon website did not send any emails during the sample period of the first field experiment.

sparsely populated. Still, they help to predict consumer printing and redemptions, so we include them as control variables in the regressions reported below to reduce noise.⁸

Randomization checks. We performed several randomization checks. First, we reviewed the in-browser JavaScript randomization code prior to implementation. Second, we checked the data carefully after the experiment and found that the cells were well balanced. Third, we used the four user characteristics referenced in the previous paragraph (i.e., *Newborn*, *Baby*, *Toddler*, and *Email*) to examine the validity of the experimental randomization. We tested 20 null hypotheses, each one indicating whether one of the four observable consumer characteristics is distributed independently of one of the five treatment attributes. Table 1 reports the *p*-values of these 20 randomization checks and Online Appendix Table A1 reports the proportion in each treatment-characteristic cell. As would be expected due to random chance alone, one test indicated non-independence at a 95% confidence level, and another test indicated correlation at a 90% confidence level.⁹ These results are exactly in line with expected rates of Type 1 error, supporting that the experimental randomization code worked as expected. Moreover, the size of any identified difference is relatively small.

Table 1 Randomization Checks

	Beneficiary Framing	Image	Offer Value	Legacy Call to Action	Feature
<i>Newborn</i>	0.939	0.813	0.018**	0.978	0.982
<i>Baby</i>	0.547	0.158	0.816	0.084*	0.515
<i>Toddler</i>	0.797	0.653	0.330	0.567	0.632
<i>In Email</i>	0.467	0.464	0.743	0.163	0.758

Each cell reports the *p*-value of a Pearson's chi-square test of the null hypothesis that the row variable and column variable are independently distributed. **p* < 0.10, ***p* < 0.05, ****p* < 0.01.

Results

⁸ We also have checked for whether they interact with treatment variables to predict consumer response, using both parametric and nonparametric approaches, but found no evidence of heterogeneous treatment effects.

⁹ We are further informed by the fact that the two hypothesis tests that are significant at the 90% level are associated with different experimental treatment variables.

Overview. Table 2 presents p -values from a series of chi-square tests, each of which indicates whether a set of non-economic treatment variables influenced coupon printing and redemption. Beneficiary framing has significant effects on all coupon printing variables and some coupon redemption variables. Image and LCTA significantly influence some coupon printing variables, but they do not significantly influence redemption behavior. Feature has no significant effects on any printing or redemption variables.

Table 2 Independence Tests

	Beneficiary Framing	Image	Legacy Call to Action	Feature
<i>P1 Coupon Printed</i>	0.024**	0.061*	0.212	0.325
<i>P2 Coupon Printed</i>	0.031**	0.250	0.162	0.184
<i>P3 Coupon Printed</i>	0.065*	0.056*	0.061*	0.263
<i>Any P1/P2/P3 Coupon Printed</i>	0.014**	0.089*	0.108	0.552
<i>P1 Coupon Redeemed</i>	0.683	0.996	0.178	0.644
<i>P2 Coupon Redeemed</i>	0.046**	0.650	0.490	0.815
<i>P3 Coupon Redeemed</i>	0.142	0.644	0.352	0.527
<i>Any P1/P2/P3 Coupon Redeemed</i>	0.084*	0.358	0.806	0.871

Tests of independence for main effects. Each cell reports the p -value of a Pearson's chi-square test of the null hypothesis that the row variable and the column variable are unrelated. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We first present figures showing the average treatment effects for treatment variables that had some statistically significant effects. Figures showing non-significant average treatment effects are provided in Online Appendix Figures A1-A3. Raw counts of prints and redemptions by treatment are reported in Online Appendix Table A2. Additionally, we estimate a series of regressions to estimate treatment effects simultaneously while controlling for observable user characteristics.

Effects of non-economic treatments on coupon printing and redemptions. To address whether the four non-economic treatments had an impact on relevant consumer behavior, we tested whether the effect of all non-economic treatments on the printing and redemption of coupons were jointly equal to zero. To do this, we pool the observations into a single regression

and control for outcome-coupon indicators (e.g., an indicator for the dependent variable being prints for P1, an indicator for the dependent variable being redemptions for P2, etc.), outcome-coupon indicators x treatment indicators (e.g., an indicator for whether beneficiary framing was present, an indicator for whether a call to action was present, etc.), and cluster standard errors by user.¹⁰ This allows individual treatment effects for every coupon to appear in a single coefficient vector, which allows for a Wald test for their joint equality with zero. The p -value on this test is 0.005, which indicates that the non-economic treatments influenced choices. The remainder of the analysis decomposes the total effect using experimental features and response behavior data.

Figure 3a shows the effect of beneficiary framing on printing rates for each of the three product coupons, and on the fraction of consumers who print any coupon for the brand (i.e., “Any P1/P2/P3”). On average, beneficiary framing increased the printing rate by 3.6% for P1 ($\chi^2(1) = 5.09, p = 0.024$), by 3.2% for P2 ($\chi^2(1) = 4.66, p = 0.031$), by 2.6% for P3 ($\chi^2(1) = 3.40, p = 0.065$), and by 3.2% for the brand as a whole ($\chi^2(1) = 6.02, p = 0.014$).¹¹ These changes are estimated with substantial precision: three effects are significant at the 95% confidence level, and one is significant at the 90% confidence level.

Figure 3b shows that the increased printing rate led to positive average changes in redemption rates. However, the possibility that redemption increased due to random chance alone cannot be ruled out for P1 ($\chi^2(1) = 0.17, p = 0.683$) or P3 ($\chi^2(1) = 2.16, p = 0.142$). For P2, the effect of beneficiary framing significantly increased the redemption rate by 8.1% ($\chi^2(1)$

¹⁰ This analysis pools the baby and product image conditions into a single treatment to account for the presence of any image on the coupon.

¹¹ We conducted an additional analysis on the total prints for these coupons, accounting for users who may have printed a coupon more than once. We found that beneficiary framing increased the total prints in addition to the likelihood of printing any coupon reported below. Specifically, consumers in the beneficiary framing treatment printed 1.61 coupons (SD = 2.28) and those in the control printed 1.56 coupons (SD = 2.25; $t(36,632) = 2.12, p = .034$). A Poisson regression of total prints on beneficiary framing yielded a similar result ($\beta = .031, p < .001$). This suggests that beneficiary framing affected printing behavior on the extensive margin.

= 3.97, $p = 0.046$). For the brand as a whole, the average redemption rate increased with beneficiary framing by 4.7% ($\chi^2(1) = 2.99$, $p = 0.084$) but the effect is only significant at the 90% confidence level.

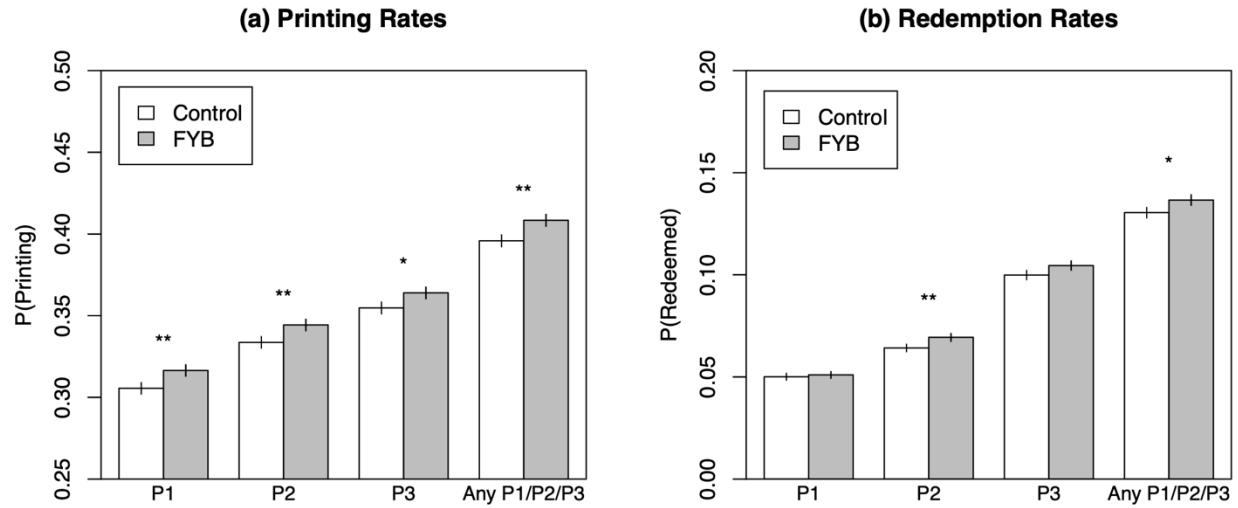


Fig. 3 Effect of savings beneficiary on (a) coupon printing and (b) redemption rates. Error bars are standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 4 displays the effects of coupon offer image on printing rates for the three products and the brand as a whole. Across all three products, and the brand as a whole, the prominent baby image led to higher printing rates than the control (no image), but the difference between the conditions was never significant (P1: $\chi^2(1) = 0.20$, $p = 0.655$; P2: $\chi^2(1) = 1.40$, $p = 0.236$; P3: $\chi^2(1) = 0.40$, $p = 0.529$; Any P1/P2/P3: $\chi^2(1) = 1.75$, $p = 0.186$). The product image (which itself contained an image of a baby on the packaging) had mixed effects for different products, but led to an overall effect for the brand that was positive and significant when compared to the control ($\chi^2(1) = 4.77$, $p = 0.029$), but was not statistically distinguishable from the baby image ($\chi^2(1) = 0.73$, $p = 0.393$).¹² Neither image treatment had any significant effects on coupon redemptions, as depicted in Online Appendix Figure A1.

¹² More specifically, the product image led to a lower print rate for P1 and a higher print rate for P3. We suspect that these two products are purchased by different types of consumers and that the product image may have had different effects on each segment. Further research would be needed to explain the different effect signs.

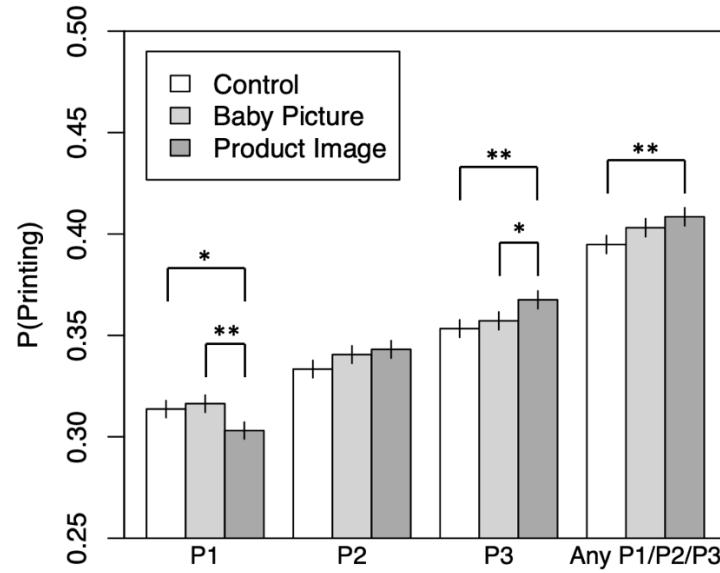


Fig. 4 Effect of image on coupon printing. Error bars are standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 5 shows the experimental effect of the Legacy Call to Action on coupon printing. The average effect of telling the consumer to “Act Now!” was positive for all three product coupons, but only significant at the 90% level for P3 (P1: $\chi^2(1) = 1.56$, $p = 0.212$; P2: $\chi^2(1) = 1.96$, $p = 0.162$; P3: $\chi^2(1) = 3.52$, $p = 0.061$; Any P1/P2/P3: $\chi^2(1) = 2.59$, $p = 0.108$). Online Appendix Figure A2 shows that the LCTA did not significantly affect redemptions.

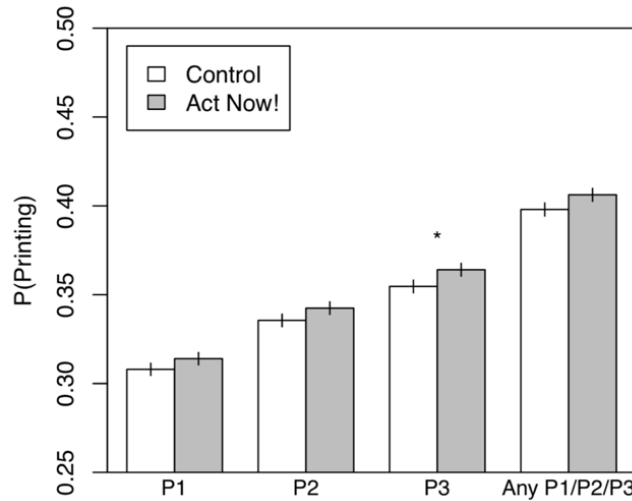


Fig. 5 Effect of the Legacy Call to Action on coupon printing. Error bars are standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Finally, as referenced earlier, Online Appendix Figure A3 reports that none of the feature statements had any effects on coupon printing or redemptions. The remainder of the paper focuses on beneficiary framing due to its novelty and connection to the rich literature on framing effects.

Comparing beneficiary framing with economic coupon value. In order to more directly compare beneficiary framing to traditional coupon elements, we estimated a series of econometric models. We considered a range of models, including Binary Probit, Zero-Inflated Poisson, and Negative Binomial. For simplicity, Tables 3 and 4 present Binary Probit estimates of the experimental attributes and control variables on coupon printing and redemption rates. Online Appendix Tables A3 and A4 present a linear probability model that finds similar results to the tables reported in the main text.

Table 3 Experimental Attribute Effects on Coupon Printing Rates

	P1 Coupon Prints		P2 Coupon Prints		P3 Coupon Prints		Any Coupon Prints	
Beneficiary Framing								
For Your Baby	0.009	**	0.009	*	0.007		0.011	**
	(0.005)		(0.005)		(0.005)		(0.005)	
Coupon Image								
Baby Image	0.003		0.007		0.004		0.008	
	(0.006)		(0.006)		(0.006)		(0.006)	
Product Image	-0.011	*	0.010	*	0.014	**	0.014	**
	(0.006)		(0.006)		(0.006)		(0.006)	
Coupon Value								
V2	0.107	****	0.118	****	0.118	****	0.124	****
	(0.006)		(0.006)		(0.006)		(0.007)	
V3	0.188	****	0.209	****	0.221	****	0.233	****
	(0.006)		(0.006)		(0.007)		(0.007)	
V4	0.292	****	0.312	****	0.319	****	0.324	****
	(0.007)		(0.007)		(0.007)		(0.007)	
Legacy Call to Action								
"Act Now!"	0.007		0.009	*	0.011	**	0.010	**
	(0.005)		(0.005)		(0.005)		(0.005)	

Notes: Binary probit estimates of the experimental attributes on coupon printing rates. $N = 36,634$. All specifications include user characteristic fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Table 4 Experimental Attribute Effects on Coupon Redemption Rates

	P1 Coupon Redeems	P2 Coupon Redeems	P3 Coupon Redeems	Any Coupon Redeems
Beneficiary Framing				
For Your Baby	0.000 (0.002)	0.004 * (0.003)	0.003 (0.003)	0.005 (0.003)
Coupon Image				
Baby Image	0.000 (0.003)	0.001 (0.003)	0.004 (0.004)	0.006 (0.004)
Product Image	0.000 (0.003)	-0.002 (0.003)	0.003 (0.004)	0.002 (0.004)
Coupon Value				
V2	0.022 **** (0.002)	0.030 **** (0.002)	0.039 **** (0.003)	0.055 **** (0.004)
V3	0.046 **** (0.003)	0.063 **** (0.003)	0.082 **** (0.004)	0.108 **** (0.004)
V4	0.094 **** (0.003)	0.121 **** (0.004)	0.182 **** (0.005)	0.213 **** (0.005)
Legacy Call to Action				
"Act Now!"	-0.003 (0.002)	-0.001 (0.003)	0.004 (0.003)	0.002 (0.003)

Notes: Binary probit estimates of the experimental attributes and control variables on coupon redemption rates. $N = 36,634$. All specifications include user characteristic fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$

Tables 3 and 4 enable direct comparison of effect sizes between elements. We focus on the final column as it measures response at the brand level. Table 3 shows that the point estimate of the beneficiary framing effect of 0.011 was slightly larger than the LCTA point estimate (0.010), but within one standard error.¹³ Comparing beneficiary framing to economic offer terms, we find that its effect on printing is comparable in magnitude to increasing the value of the coupon by about \$0.05, equivalent to increasing the coupon value by 3.4%. We find this figure by calculating the change in economic value that the model predicts to have the equivalent

¹³ Table 4 shows that the non-economic elements did not significantly impact redemptions, and again does not allow for a precise differentiation between the size estimates of the non-economic elements. We interpret these non-findings as consistent with the lower statistical power of the experiment to find treatment effects in redemption data, the partner firms' inability to display treatment attributes on printed coupons, and the non-immediacy of treatment and redemption behaviors.

predicted effect on printing as removal of the beneficiary framing statement. There are several benchmarks against which we can compare this effect size; we consider the most natural benchmark to be the manufacturer unit margin, which we deduce (based on industry reports) to be in the \$1-5 range. Thus, the compensating variation of \$0.05 is approximately 1-5% of the manufacturer unit margin. We believe this to be non-negligible, especially since this effect is obtained from adding just three words, “for your baby,” to a coupon offer. It gives the appearance of a “free lunch” for the manufacturer. Extrapolating from this empirical setting into others, consider that the subsample of consumers who voluntarily navigate to a print-at-home coupon website are likely to be substantially more economically motivated than the general population of consumers. If behavioral triggers can influence the behavior of this self-selected subsample, we would speculate that the effects on the broader market may be more pronounced.

Field Experiment 2: The Beneficiary Framing Effect Replicates in an Additional Study

Method

We report here a field study that was designed primarily to investigate the importance of feature statements in non-baby-related product categories, but also included randomized beneficiary framing vs. control (i.e., no framing) on coupon offers for the same three products tested in the first experiment. We were interested in examining the efficacy of a beneficiary framing treatment in this conceptual replication at a later date.

36,376 consumers were exposed to the treatments over a 38-day period approximately six months after the previous experiment. The beneficiary framing from the original field experiment had been used intermittently on the coupon website, so it was no longer a novel stimulus for repeat users. The replication study design was focused primarily on feature

statements for several non-baby-related products. Specifically, it varied feature statements for several products that target older adults who are unlikely to have young children at home. These feature statements were fairly unobtrusive. We do not identify the brands or products involved to preserve partners' anonymity.

The baby-related product coupons were not the primary focus of this field study. They were slotted in less prominent positions on the page. P1 was displayed in the third position on the first row, and P2 and P3 coupon offers were on the second row, "below the fold" meaning the typical consumer had to scroll down in their browser to view the P2 and P3 coupon offers. This was an important difference in prominence from the first experiment, in which P1, P2 and P3 were displayed in the top three positions on the page. Critically, the conceptual replication manipulated the same beneficiary framing statement used in the previous experiment, as shown in Figure 2.

A sizeable amount of traffic to the coupon website during the second field experiment was generated through earned media. Specifically, a brand in the test's focal non-baby-related product category sent promotional emails to contacts in its Customer Relationship Management (CRM) system identified as interested in the products targeted toward older adults. Promotional emails generated about two-thirds of the traffic in the sample period, yielding consumers who were far less likely to print baby-related product coupons, as will be shown below. We refer to these consumers as "CRM-generated traffic," to distinguish them from the regular organic visitors, which accounted for the remaining one-third of the sample.

Finally, the baby-related coupon values were set at the V1 level, at the low end of the historical range, and executives at the coupon website believe there are large seasonal factors that have historically led to less organic website traffic in the winter season of this experiment

than in the summer season of the previous experiment. As a result of all four factors—reduced prominence, lower print rates among CRM-generated traffic, lower coupon value and negative seasonal effects—far fewer people printed and redeemed baby-related product coupons during the conceptual replication. Therefore, average treatment effects are not directly comparable to the first experiment and are estimated with substantially less precision.¹⁴

Results

Figure 6 reports the average treatment effects of savings beneficiary framing in the full sample. Beneficiary framing led to a higher average print rate for the P1 coupon offer ($\chi^2(1) = 5.96, p = 0.015$). This demonstrates that the beneficiary framing result is replicable. The average effects were positive for P2 and P3 coupons, the less prominent offers, but not precise enough to rule out random noise as an alternate explanation (P2: $\chi^2(1) = 1.30, p = 0.254$; P3: $\chi^2(1) = 0.39, p = 0.532$).¹⁵ This is not surprising given that these may not have been viewed frequently given their placement. Indeed, McGranaghan et al. (2019) shows that coupon consideration decreases substantially as one moves from the top to the bottom of a webpage.

¹⁴ It is important to note that none of these factors influenced the random assignment of consumers between treatment and control cells, so the experimental manipulation remains valid. A randomization check relating treatment assignment to CRM-generated versus organic traffic supported the hypothesis of random assignment ($\chi^2(1) = 1.15, p = 0.283$).

¹⁵ Perhaps due to the substantially lower print rates, redemption data showed no meaningful differences between treatment and control cells (P1: $\chi^2(1) = 0.36, p = 0.549$; P2: $\chi^2(1) = 0.01, p = 0.935$; P3: $\chi^2(1) = 0.51, p = 0.477$).

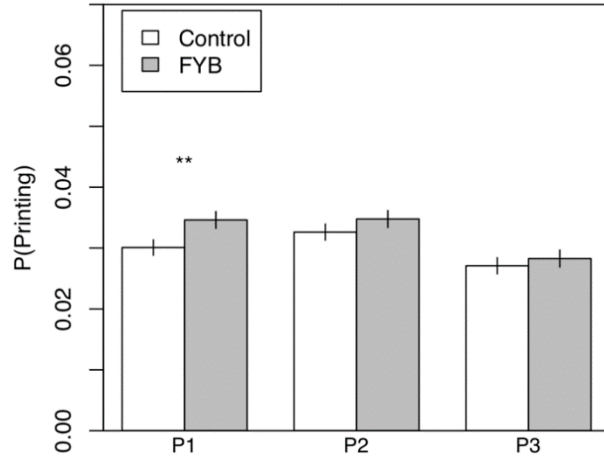


Fig. 6 Effect of savings beneficiary on coupon printing rate in Field Experiment 2. Error bars are standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 7 breaks out the average treatment effects by traffic source: CRM-generated vs. organic. Although CRM-generated traffic printed fewer baby-related product coupons overall, the larger sample size for this subsample allowed increased precision when estimating treatment effects. The average treatment effect of savings beneficiary framing was positive and statistically significant for P1 in the CRM sample ($\chi^2(1) = 4.84, p = 0.028$), but not significant in the organic traffic sample ($\chi^2(1) = 1.19, p = 0.276$). Additionally, the below-the-fold products did not have significant effects when split by traffic source (CRM P2: $\chi^2(1) = 2.36, p = 0.124$; CRM P3: $\chi^2(1) = 1.18, p = 0.277$; Organic P2: $\chi^2(1) = 0.00, p = 0.955$; Organic P3: $\chi^2(1) = 0.04, p = 0.851$).

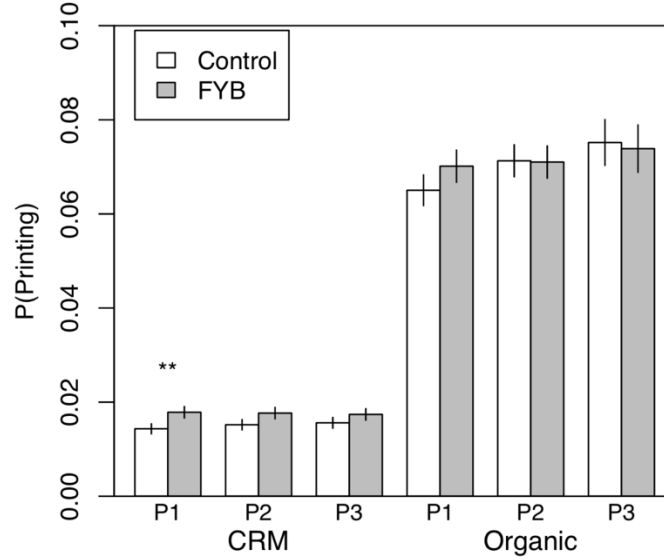


Fig. 7 Effect of savings beneficiary on coupon printing rate by traffic source in the A/B test. Error bars are standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Finally, we test the long-run effects of beneficiary framing. For consumers who visited the website during both experiments, we can examine the beneficiary framing effect in the conceptual replication within each of the first experiment's treatment cells. Although this self-selected subsample of consumers is small—just 6.7% of the total sample size received savings beneficiary treatments in the prior field experiment—the results suggest beneficiary framing can increase coupon prints even after repeated exposure to the stimulus. Figure 8 shows the effects of the beneficiary framing treatment on printing in the conceptual replication among the 1,210 consumers who previously were treated with beneficiary framing in the first experiment. The figure pools data across P1, P2, and P3, showing the average tendency to print at least one coupon of the focal baby-related brand. We find that, among consumers treated in the first experiment, CRM-Generated traffic ($N=348$) was more than twice as likely to print a coupon when they received the same beneficiary framing treatment in the conceptual replication ($\chi^2(1) = 5.76, p = 0.016$). Among organic traffic ($N=858$), beneficiary framing treatment had no significant effect ($\chi^2(1) = 0.02, p = 0.885$). Pooling across traffic sources washes out the effect

from the CRM subset ($\chi^2(1) = 1.91, p = 0.167$). Online Appendix Figure A4 shows that the treatment in the conceptual replication did not have any statistically significant effects on printing among the 1,243 consumers who received the control in the first experiment.

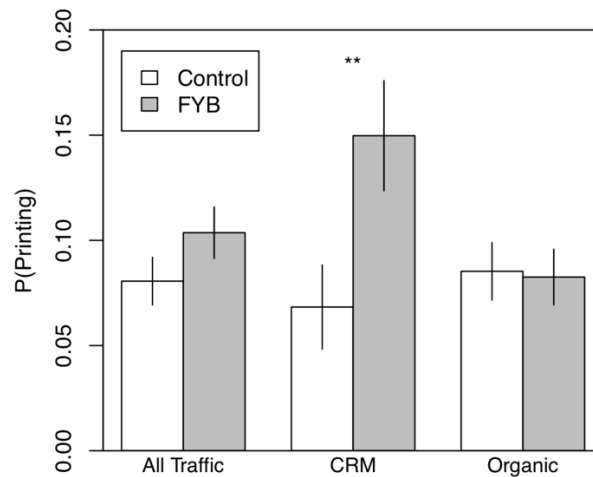


Fig. 8 Effect of savings beneficiary on coupon printing rate by traffic source in the A/B test among those consumers who previously received the beneficiary framing treatment. Error bars are standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Overall, the conceptual replication showed that the positive beneficiary framing effect on printing can be replicated in the field and that its effects can persist with repeated exposures. Given the positive results, despite the increased statistical noise involved in the replication, the evidence reported so far suggests that the main beneficiary framing result is reasonably robust.

Mechanisms: Mental Accounting

The two field studies suggest beneficiary framing can increase offer printing and redemptions; however, they do not address why this occurs, which may result from one or multiple mechanisms. In the following three sections, we investigate three potential mechanisms that could drive the beneficiary framing effect: (1) mental accounting: beneficiary framing influences the budget category to which a coupon is classified, (2) affect transfer: consumers project their positive feeling about the beneficiary onto the coupon, and (3) salience: the visual

prominence of the beneficiary savings statement attracts attention and this attention positively impacts coupon desirability. We focus on these three because they were frequently raised in our discussions with colleagues, domain experts, and throughout the peer review process, but we acknowledge that other mechanisms also could contribute to the beneficiary framing effect.

Mental accounting research has addressed how consumers organize, track, and evaluate their financial activities (Thaler 1999; Zhang and Sussman 2018). One important component of mental accounting involves the propensity to group funds into different categories or accounts. For instance, expenses one incurs at the movies or a concert could be grouped together in a single category that is specific to entertainment. Previous work has found that expenses can be assigned to an account based on judgments of similarity and categorization (Heath and Soll 1996). Moreover, a common method to categorize funds is by their intended use. Households set budgets for specific expenses and treat funds within each budget as distinct and imperfectly substitutable (Hastings and Shapiro 2013; Heath and Soll 1996; Thaler 1985). An additional component of mental accounting involves unpacking how individuals segregate and track the allocation and use of funds against different accounts with spending limits or budgets. For example, survey research has found that approximately half of individuals have a household budget (Lin et al. 2016). These budgets, in turn, can shape demand for various products and services.

In order for mental accounting to be implicated in the beneficiary framing effect from the field studies, three factors must be at work. First, consumers should have budgets that constrain their spending and, in particular, they should have a budget intended for the beneficiary. In the context of the field experiments, this means parents should have some budget targeted for their children. Given that properly budgeting for one's baby is common advice for parents who are

expecting or currently have young children (Choudhri 2015; Farmer and Ling 1990; Jones and Lannelli 2009), parents who use the coupon website and have an interest in baby products are likely to act as if they have a mental account specific to their child's expenses.

Second, people with beneficiary-related mental accounts will categorize beneficiary framed coupons as more likely to affect the beneficiary-related budget category than coupons without beneficiary framing. In other words, the likelihood a coupon is categorized as belonging to a budget category is altered by beneficiary framing. The presence of a beneficiary statement (e.g., "for your baby") on an offer should increase the probability that a consumer classifies a coupon as belonging to a budget category relating to that beneficiary (e.g., their baby). Relatively little work in mental accounting has focused on the factors that influence the categorization of funds (Zhang and Sussman 2018), although some work has found that savings can be increased when affected by sealed envelopes and visual reminders (Soman and Cheema 2011) or earmarked for responsible uses (Sussman and O'Brien 2016). We propose that beneficiary framing is one technique that affects the formation and categorization of funds. To test this link, we conducted an experiment where we manipulated a coupon's beneficiary framing and asked participants to classify the coupon into a budget category. We find, across a range of product categories, that framing increased the likelihood a coupon was classified as belonging to a target category. This experiment is fully detailed below as Mental Accounting Study 1.

Third, people who have a beneficiary-related mental account should perceive beneficiary-related coupons as more valuable than people without a beneficiary-related mental account, all else equal. A direct prediction of mental accounting is that budgets can shape the perceived value of coupons and other price promotions. In this context, a beneficiary-related mental account increases the subjective value of beneficiary-related funds. Since the creation of the budget

category introduces a monetary constraint, a marginal dollar spent in that budget category may seem marginally greater than a dollar spent on an uncategorized expense. To test this link, we conducted two experiments where we varied the composition of participants' budgets.

Participants either had a categorized budget containing a beneficiary-related budget category or had an uncategorized budget. We then asked participants to rate the desirability of coupons to test how budget existence and composition influenced coupon desirability. In line with the theory, the presence of a beneficiary-related budget category increased the value of beneficiary-related offers but did not alter the value of offers that were unrelated to the beneficiary category. These experiments are fully detailed below as Mental Accounting Studies 2 and 3.

Together, these three links suggest that the beneficiary framing from the field studies increases coupon desirability by increasing the likelihood that a consumer with a baby-specific mental account classifies a baby-related promotion as relevant to her baby-specific account. However, the studies we conduct are general enough to suggest the framing effect from the field is likely to extend to other contexts where a consumer has a budget for the beneficiary (e.g., pets, entertainment, food and dining). We draw on previous literature suggesting the first link above holds, and explicitly test the second and third links in the remainder of this section.

Mental Accounting Study 1: Categorization

This study was designed to address whether beneficiary framing alters the account to which a coupon is classified. To do this, we asked online participants to choose which budget category a coupon best applies as coupons were presented either with beneficiary framing (treatment) or without beneficiary framing (control). If beneficiary framing affects coupon choice through a mental accounting mechanism, then the likelihood of a coupon being

categorized in a particular budget category should increase when the beneficiary framing applies to that category compared to the control condition.

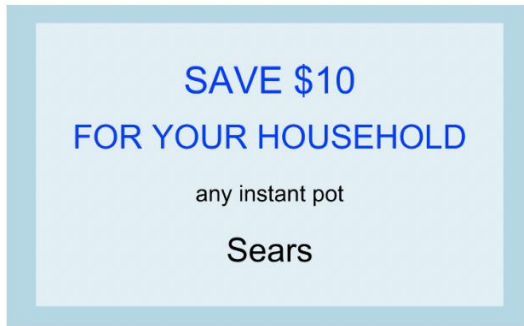
Method. 250 participants were recruited from Prolific and paid \$0.60 for their participation (preregistration: <http://aspredicted.org/blind.php?x=mz487k>). After providing consent, participants were asked to imagine a scenario in which they were in charge of how much their family spent on goods and services, with four people in their family: themselves, their partner, their 3-year-old child, and their 1-year-old child. They were then told that they recently met with a financial adviser to help with their financial planning. Based on the meeting, they created a budget with seven budget categories: (1) Baby / Kids, (2) Car / Travel, (3) Entertainment, (4) Food / Dining, (5) Health / Wellness, (6) Household / Personal Goods, and (7) Utilities.

Next, participants were presented with nine coupon offers, displayed one at a time in a random order, and asked to categorize each offer into a budget category from the list of seven categories above. Participants were informed that there were “no right or wrong answers, and whatever decision you make is a matter of personal preference.”

Importantly, participants were randomly assigned to a beneficiary framing or no-beneficiary-framing condition (Figure 9). In the beneficiary framing condition, all offers for coupons contained the phrase “for your X” or “for X” where X was a single target category selected from one of the given budget categories.¹⁶ In contrast, participants in the no-beneficiary-framing condition viewed coupons that omitted this phrase. Participants remained in the same treatment when facing all nine coupon decisions.

¹⁶ The decision to frame a coupon as “for X” or “for your X” depended on which phrasing was more natural. For example, “for your baby” is more common than “for baby” but “for utilities” is a more natural phrasing than “for your utilities.” Of the categories given in the task, only “entertainment” and “utilities” appeared without “your.”

(a) Beneficiary Framing Treatment



(b) Control (No Framing) Treatment

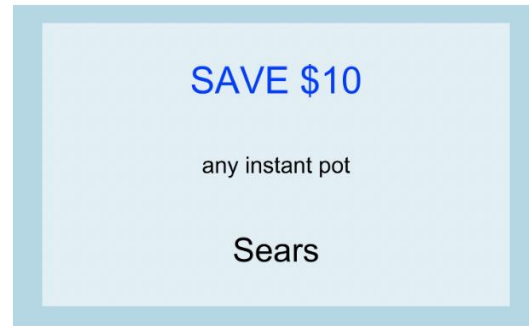


Fig 9. Example coupons for the mental accounting categorization study. Subjects viewed coupons either (a) with beneficiary framing or (b) without beneficiary framing.

Note that, given the budget categories and offer types, it is possible that an offer can apply to more than one category. For example, an offer for baby wipes can apply to the Baby/Kids or the Household/Personal Goods category. We asked participants to select only one category for each coupon in order to understand how the beneficiary framing alters the primary classification of coupons.

Results. In the beneficiary framing condition, participants classified 5.02 (SD = 1.88) of the offers as belonging to the target category while participants in the no beneficiary framing condition only classified 3.21 (SD = 1.14) offers as belonging to the target category ($t(204.2) = 9.20, p < 0.001$). This is consistent with the categorization component of the mental accounting mechanism: beneficiary framing increased the likelihood of a coupon applying to a target account.

In additional analyses, we examined the effect at the individual coupon level which is reported in Table 5. Overall, beneficiary framing statistically increased the likelihood a coupon was classified as belonging to a target category for eight of the nine coupons. Notably, the beneficiary framing manipulation was successful across a wide variety of target categories and

types of items, including across items that had sizeable differences in price and those that were offered through subscription-based services.

Table 5 Coupons Used in the Mental Accounting Categorization Study

Target Category	Item	Retailer	Treatment	Control	<i>p</i> -value
Baby / Kids	Baby Wipes	Target	0.86	0.50	< 0.001
Baby / Kids	Prepackaged Food	Walmart	0.31	0.00	< 0.001
Baby / Kids	Clothing Item	The Children's Place	0.94	0.90	0.235
Entertainment	New iPhone	Best Buy	0.54	0.42	0.058
Health	Hydro Flask	Amazon	0.50	0.37	0.040
Household	Instant Pot	Sears	0.82	0.66	0.006
Utilities	One Month of Netflix	Netflix	0.19	0.01	< 0.001
Household	Playroom Furniture	Wayfair	0.58	0.34	< 0.001
Travel	Lunchbox	LL Bean	0.26	0.01	< 0.001
All Coupons	All Coupons	All Coupons	5.02	3.21	< 0.001

The beneficiary framing category appears on the left-hand column. The treatment and control columns give the fraction of decisions when a coupon was classified as belonging to the target category, with the *p*-value from a t-test reported on the right column. The last row reports the result summed over all nine coupons.

Altogether, the results here suggest beneficiary framing can have substantial effects on the budget category to which one perceives a coupon to apply, supporting the second link of the mental accounting mechanism.

Mental Accounting Study 2: Baby Budgeting

While the previous study found that beneficiary framing alters the budget category to which a coupon is assigned in general, this second study was designed to test whether the presence of a budget category specifically for children increases the desirability of baby-related coupons. To do this, we asked online participants to rate the desirability of coupons for different products (i.e., one baby product and one non-baby product) while the categories that composed their household budgets were varied (i.e., baby budget category versus no baby budget category).

Method. 250 participants were recruited from Prolific and paid \$0.50 for their participation (preregistration: <http://aspredicted.org/blind.php?x=w95tw9>).¹⁷ After providing consent, participants were asked to imagine that they oversaw a hypothetical family's finances. They were informed that their family consisted of themselves, their partner, their 3-year-old child, and their 1-year-old child. Additionally, participants were told they recently met with a financial advisor and based on the advisor's advice and their income/spending patterns, they were given a budget of \$2,000 per month for their personal expenses, excluding rent or mortgage.

Critically, we manipulated the construction of the budget the participant was given. Participants were randomly assigned to one of two budget conditions: baby/kids category or control. In the baby/kids category treatment, participants were told that they decided to categorize their expenses based on whether or not they applied to a baby/kids category. In other words, they had a specific budget category for their baby/kids. Participants in the control condition were given the same overall budget, but the budget was not broken down into separate categories.

Next, participants viewed two coupons, displayed one at a time in a random order, and were asked to provide a rating for each coupon's desirability on an integer scale from 0 (as undesirable as possible) to 100 (as desirable as possible) with a rating of 50 as the default. One of the coupons was for diapers, which was intended for young children. The other coupon was for a digital movie purchase, which we viewed as less likely to apply to young children.

¹⁷ As the mechanism tests require additional process data that would be infeasible to collect in a field setting with the partner firm, we opted to test the mechanisms using data collected through Prolific, a commonly used experimental study platform. Although the participant pool in the mechanism experiments differs from the field experiment setting, we believe there are two advantages that make Prolific a useful test environment. First, Prolific participants, like participants from the field experiment setting, are likely to be income constrained and may positively respond to price promotions. Second, while Prolific participants are also a self-selected sample, they did not select into a coupon website which permits testing in a potentially more generalizable population.

Results. We first analyzed how the two budget conditions impacted ratings of the baby-related coupon. Those in the baby/kids category budget condition valued the baby coupon 15.4% more than those in the control budget condition ($M_{\text{baby}} = 78.1$, $SD_{\text{baby}} = 24.4$; $M_{\text{control}} = 67.7$, $SD_{\text{control}} = 32.4$; $t(230.7) = 2.87$; $p = 0.005$). Furthermore, there was no difference in the desirability of the non-baby coupon across budget treatments ($M_{\text{baby}} = 31.3$, $SD_{\text{baby}} = 29.5$; $M_{\text{control}} = 30.0$, $SD_{\text{control}} = 29.9$; $t(248.0) = 0.34$; $p = 0.735$). Together, these results support the third condition for a mental accounting explanation, namely that a beneficiary-related budget category increases the desirability of beneficiary-related offers.

Mental Accounting Study 3: Food and Dining Budgeting

This study was designed to test whether the findings from the Mental Accounting Study 2 replicate in another common budgeting domain – food and dining. If so, this suggests the link is not specific to baby products but might be extended to other choice environments, including those outside of the choosing-for-others paradigm.

Method. This study is identical to the previous study with the following two caveats. First, rather than participants being placed into a baby/kids category treatment, they were instead placed into a food/dining category treatment. Second, rather than viewing a coupon for diapers, participants viewed a coupon for cereal which could be naturally applied to a food/dining category. The study was preregistered (<http://aspredicted.org/blind.php?x=6a7wk4>).

Results. We analyzed how the two budget conditions impacted participants' coupon ratings. Those with the food/dining category budget valued the food/dining coupon 10.0% more than those in the control budget ($M_{\text{food/dining}} = 76.2$, $SD_{\text{food/dining}} = 21.7$; $M_{\text{control}} = 69.3$, $SD_{\text{control}} = 27.6$; $t(236.7) = 2.18$; $p = 0.030$). Furthermore, there was no difference in the desirability of the non-food coupon across budget treatments ($M_{\text{food/dining}} = 25.0$, $SD_{\text{food/dining}} = 27.1$, $M_{\text{control}} = 27.9$,

$SD_{\text{control}} = 28.6$; $t(247.6) = 0.80$; $p = 0.423$). These results support the hypothesis that a beneficiary-related budget category increases the desirability of beneficiary-related offers in an additional product category to the above study.

A supplemental study in Online Appendix B augments these results, showing that beneficiary framing can decrease coupon attractiveness when participants lack a beneficiary-linked budget category. In this study, participants were given a scenario where they either had a pet or did not have a pet, and then rated coupons that either had beneficiary framing (i.e., “for your pet”) or did not have beneficiary framing (i.e., control). We found that beneficiary framing increased the coupon’s desirability rating when participants were informed they had a pet and decreased the coupon’s desirability rating when they were informed they did not have a pet. This provides an interesting boundary condition where “mistargeting” a coupon might be costly to the firm.

Mechanisms: Affect Transfer

Affect transfer hypothesizes that beneficiary framing amplifies coupon desirability as consumers project their emotions for the beneficiary onto their subjective valuation for a coupon. When the affect towards the beneficiary is positive, as is likely the case when one’s young child is the beneficiary, this increases the coupon’s value compared to not specifying a beneficiary as in the latter case the coupon does not receive the additional beneficiary value projection. This mechanism is suggested by previous work that has found that brand attitudes and ad evaluations can be influenced by affect transfer (Sweldens et al. 2010; MacKenzie et al. 1986; Goldbert and Gorn 1987; Dunn and Hoegg 2014; Mitchell and Olson 1981).

Notably, affect transfer and mental accounting both predict the beneficiary framing effect from the two field studies. However, the two theories make different predictions when one's affect to the beneficiary frame is negative or neutral. When the beneficiary frame is perceived as negative, affect transfer predicts that one's negative emotions towards the beneficiary should be projected to the valuation of a coupon which will decrease the coupon's value, compared to a no framing condition. When the beneficiary frame is neutral, affect transfer predicts that one's neutral emotions towards the beneficiary should be projected to the valuation of a coupon which should have no effect on the coupon's value, compared to a no framing condition. In contrast, mental accounting predicts that regardless of one's affect towards the beneficiary, beneficiary framing should increase the desirability of the coupon as long as one has a budget category to which the coupon applies. For example, even if one dislikes their utility company or their bank, a coupon framed as benefitting one of those categories (e.g., "for your electricity bill" or "for your savings") would be more successful than a coupon without framing.

To test this, we gave online participants a scenario that induced either positive or negative affect toward a pet. We then presented a coupon that was applicable for the pet either with or without beneficiary framing (i.e., "for your pet").

Method. 500 participants were recruited from Prolific and paid \$0.70 for their participation (preregistration: <http://aspredicted.org/blind.php?x=ni5549>). After providing consent, participants were asked to imagine a similar scenario to the previously described mental accounting experiments. Specifically, participants were asked to imagine they were in charge of their family's expenses and that there were a total of four people in their family: themselves, their partner, their 3-year-old child, and their 1-year-old child.

Next, participants were informed that they recently decided to get a new dog named Spot. In order to stay on budget with this addition to their family, they decided to categorize their expenses based on whether or not they applied to a pet category. Participants were then asked to provide a rating for one coupon's desirability on an integer scale from 0 (as undesirable as possible) to 100 (as desirable as possible) with a rating of 50 as the default.

We randomized participants to be in both an affect condition (i.e., positive or negative) and a beneficiary framing condition (i.e., "for your pet" or control). In the positive affect condition, participants were told that Spot is easy to care for and behaves positively; in the negative affect condition, participants were told that Spot is difficult to care for and behaves negatively.¹⁸ In the "for your pet" framing condition, the coupon participants viewed included the pet beneficiary statement (i.e., similar in nature to Figure 9); however, the control condition included no framing. Finally, we asked participants to rate their feelings for Spot on a 100-point scale (0 = very unfavorably, 50 = neutral, 100 = very favorably) to test whether the affect manipulation was successful.

Results. We first examined whether beneficiary framing had an effect in the positive affect condition. We found those in the positive affect condition and "for your pet" framing condition rated the coupon significantly higher than those in the positive affect condition and control framing condition ($M_{\text{FYP}} = 57.6$, $SD_{\text{FYP}} = 31.3$; $M_{\text{control}} = 36.5$, $SD_{\text{control}} = 30.1$; $t(221.4) =$

¹⁸ Specifically, the positive affect treatment read "After 1 month with Spot, he has proven easier to care for than you imagined. For example, Spot is already house trained and clearly lets you know when he needs to be taken outside. Spot only barks to alert you about danger and is very well behaved on walks. He loves playing games (like fetch or tug) with you and also loves to cuddle. Overall, you view adopting Spot as a wonderful decision." The negative affect treatment read, "After 1 month with Spot, he has proven more difficult to care for than you imagined. For example, despite your best attempts, you have been unable to house train Spot so you are constantly cleaning up his waste. Additionally, Spot barks loudly at people who walk by your house and has even pulled you to the ground on walks when trying to chase a squirrel, resulting in some minor cuts and bruises. He does not seem to enjoy playing games (like fetch or tug) with you and mostly wants to be left alone. Overall, you are beginning to regret your decision to adopt Spot."

5.14, $p < 0.001$). This conceptually replicates the main finding from the field experiments in a different domain, but notably is also consistent with both the mental accounting and affect transfer mechanisms.

Next, we examined the beneficiary framing effect in the negative affect condition. As in the positive affect condition, those in the negative affect condition and “for your pet” framing condition rated the coupon significantly higher than those in the negative affect condition and control framing condition ($M_{\text{FYP}} = 53.2$, $SD_{\text{FYP}} = 30.4$; $M_{\text{control}} = 31.6$, $SD_{\text{control}} = 27.6$; $t(271.5) = 6.19$, $p < 0.001$). This finding opposes the affect transfer prediction explained above.

Finally, we found that the scenario successfully manipulated affect to the pet as those in the positive affect condition rated their feelings towards Spot as significantly more favorable than those in the negative affect condition ($M_{\text{positive}} = 93.4$, $SD_{\text{positive}} = 12.6$; $M_{\text{negative}} = 44.2$, $SD_{\text{negative}} = 25.0$; $t(421.8) = 28.52$, $p < 0.001$). A supplemental study in Online Appendix C provides additional evidence that affect transfer is unlikely to explain the beneficiary framing effect.

Mechanisms: Message Content and Salience

The field experiment design displayed the beneficiary framing treatment prominently, in a red font and central location. Is the beneficiary framing effect solely attributable to the high visual salience of the savings message?

Surely, salience must play some role: if the beneficiary framing message was printed in a light text in a tiny font, it seems likely no consumer would see it, and it would consequently have no effect. This is our speculation for why the feature statement treatments did not produce any detectable changes in printing or redemption. Therefore, we conducted two online experiments that deliberately manipulated the salience of the beneficiary framing to understand how salience

may change the beneficiary framing effect. The first experiment manipulates salience and beneficiary framing in a one-coupon stimulus, and the second experiment manipulated both variables in a multi-coupon setting that resembled the field experiment context.

Method. 625 participants were recruited from Amazon Mechanical Turk and paid \$0.40 for their participation (preregistration: https://aspredicted.org/Z41_84S). We told participants they were in a scenario identical to the previous Affect Transfer study and asked them to rate the desirability of a coupon, but with the following changes to the design. First, we did not manipulate affect to the dog. Second, in addition to a “for your pet” beneficiary-framed message, we included a second message, “waste your money” (abbreviated WYM), to evaluate whether the content of the message influenced coupon desirability. Third, we randomly displayed beneficiary framing treatments as either red and bold (i.e., high salience) or as grey and light (i.e., low salience – where the message appeared in a similar background color to the coupon) to test whether the visual salience of the coupon’s message amplified results. Example stimuli appear in Online Appendix Figure D1. Overall, participants were randomized into one of five treatment groups where the first four were highlighted by a combination of message and salience (i.e., FYP-HighSalience, FYP-LowSalience, WYM-HighSalience, WYM-LowSalience) and the final group was a control with no beneficiary framing message.

Results. As preregistered, we first pooled the high and low salience groups with the “for your pet” framing into a single group and compared this with the control to evaluate whether there was an effect of the beneficiary framing message. Indeed, we found that beneficiary framing increased the desirability of the coupon ($M_{\text{control}} = 33.4$, $SD_{\text{control}} = 29.5$; $M_{\text{FYP}} = 40.2$, $SD_{\text{FYP}} = 30.0$; $t(256) = 2.08$, $p = .038$). However, we found no significant difference between the high salience beneficiary framing condition and the low salience beneficiary framing condition

($M_{\text{FYP-HighSalience}} = 38.5$, $SD_{\text{FYP-HighSalience}} = 29.2$; $M_{\text{FYP-LowSalience}} = 41.9$, $SD_{\text{FYP-LowSalience}} = 30.8$; $t(249) = .88$, $p = .377$) which suggests that message salience alone cannot fully explain the beneficiary framing effect. Instead, the message seemed to play an important role as WYM reduced coupon ratings compared to the control condition ($M_{\text{control}} = 33.4$, $SD_{\text{control}} = 29.5$; $M_{\text{WYM}} = 21.3$, $SD_{\text{WYM}} = 24.8$; $t(218) = 3.99$, $p < .001$), but there was again no significant difference between the high and low salience WYM messages ($M_{\text{WYM-HighSalience}} = 20.6$, $SD_{\text{WYM-HighSalience}} = 24.3$; $M_{\text{WYM-LowSalience}} = 22.0$, $SD_{\text{WYM-LowSalience}} = 25.3$; $t(248) = .43$, $p = .669$). Finally, we found a significant effect of the message when holding message salience constant for the high salience treatment ($t(242) = 5.29$, $p < .001$) and low salience treatment ($t(241) = 5.59$, $p < .001$).

These results suggest the content of the message played an important role in affecting coupon desirability, but that salience alone could not fully explain that effect. Note that in this study, participants rated the desirability of a single coupon; however, one open question is whether salience has a different effect when consumers must choose among multiple coupons as in the field experiments. To investigate this, we conducted an additional study where participants saw a display with multiple coupons where the top row received either a high-salience beneficiary savings statement, a low-salience beneficiary savings statement, or no savings statement (control). We report the results of this study in Online Appendix E. Overall, we find that both the high and low-salience beneficiary savings statement increased the selection of top row coupons, relative to the control condition. Moreover, there was no significant difference between the high and low-salience conditions in the likelihood of selecting a top row coupon. The findings from this additional study are consistent with salience alone not fully explaining the beneficiary framing effect.

Discussion

We adapted a beneficiary framing stimulus from the charitable giving literature and tested how it affects promotion response. We designed a novel field experiment to investigate how beneficiary framing impacts coupon printing and redemption behaviors and how such treatment effects compare to more traditional coupon elements like offer value. We found the beneficiary framing effect size was comparable to an incremental \$0.05 of coupon value. Moreover, we provide a conceptual replication of the qualitative result in a second large field experiment. Evidence from multiple preregistered online experiments support a mental accounting explanation: beneficiary framing increases the probability that an offer is coded as affecting a budget earmarked for beneficiary expenses which, in turn, increases a coupon's desirability. Further experiments do not indicate that affect transfer or message salience can solely explain the beneficiary framing effect.

The results of these studies shed light on several types of inexpensive activities that may influence consumer choices, each of which presents many opportunities for further research. First, because the individuals who self-select into print-at-home coupon portals are likely to be more economically motivated than the typical consumer, and because coupon printing and redemption actions serve their economic self-interest, it is striking that a non-economic attribute (beneficiary framing) has a strong and replicable influence on behavior. We suspect that the effects of beneficiary framing would be even stronger in a more representative consumer population, an idea consistent with Guimond et al. (2001). This result may suggest that savings beneficiary framing could be tested within other coupon delivery contexts, such as direct mail, email, apps, or Free-Standing Inserts. Additionally, the beneficiary framing effect might suggest

that marketers could more explicitly communicate how savings can free up money for additional purchases within the same budget category.

Second, by identifying mental accounting as an underlying mechanism for the effect, one can make a more informed speculation regarding when beneficiary framing is likely to hold across different contexts and when it can act as a promising managerial tool. Specifically, framing offers as benefitting particular existing consumer budget categories is a promising avenue to increase promotion uptake and can potentially affect high-stakes decision contexts such as medicine, finance, education, philanthropy or senior housing. Through this lens, this work contributes to an important open question in mental accounting regarding better understanding how external forces may alter the categorization of funds (Zhang and Sussman 2018).

Whereas the field experiments found a positive beneficiary framing effect in a baby-related product category, a natural question is whether this effect is replicable across product categories and, relatedly, the boundary conditions involved in the effect. Notably, identifying mental accounting as a mechanism underlying the main effect is critical in addressing this question as a mental accounting mechanism posits an effect when a consumer has a mental account that includes the beneficiary. Given this mechanism, one can reasonably expect such an effect when the consumer has a budget for the beneficiary but should not anticipate an effect when a budget category is not present. For example, one might not expect a beneficiary framing effect if the offer is for events one might not plan for, such as gifts for weddings or for service providers. Moreover, the effect replicates regardless of one's affect (i.e., positive or negative) towards the beneficiary. Finally, the effect can hold across a number of different groups and product categories. The experiments that were conducted on Prolific support these claims from

the mental accounting mechanism and provide managerially relevant tools to utilize in additional contexts.

More generally, the beneficiary framing results speak to different ways that marketers could explain promotion savings to consumers. Consumers make many consequential decisions partly or entirely on behalf of other people, such as financial investments, medical care, education and gifts. It would be interesting to understand when, whether, and how reminders of decision-makers' mental accounts might influence the quality and frequency of particular consumer choices.

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