

PRICES AND THE REVERSE PLACEBO EFFECT

David Karp
Department of Economics
McMaster University

Daniel Rondeau
Department of Economics
University of Victoria
342 Business and Economics Building
Victoria BC, Canada
V8W 2Y2
Voice: 250 472 4423
Fax: 250 721 6214
rondeau@uvic.ca

ABSTRACT

In *Placebo Effects of Marketing Actions*, Shiv, Carmon, & Ariely (2005) find that a product's price can have a placebo effect. In other words, raising a product's price, holding everything else constant, can improve how well the product works. In this paper, the authors replicate Shiv, Carmon, & Ariely's experiment, and find the reverse outcome: lower prices lead to better product efficacy. The authors discuss the importance of properly incorporating individual determinants of performance in the statistical analysis, and other confounding factors such as product reputation, consumer expectations and sampling error. The results suggest that the placebo effect of prices may be weak.

KEYWORDS: Placebo effect, prices, behavioral economics, consumer choice, optimal pricing strategy.

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In medical trials, patients who are treated with inactive substances often show improvements in their condition. These “placebo effects” are understood as the result of brain processes triggered by the patient’s beliefs that they are being treated and the expectations of improvements that they form.

In economics, it is well accepted that higher quality products are on average more expensive to produce. It would be natural, therefore, that they would fetch a higher price on the market. If we consider the possibility that consumers have incomplete information about the quality of alternative products, they may use the price of a good as a signal of quality and come to believe and expect that higher-priced goods are of higher quality or more effective. If the beliefs and expectations borne out of the price of a product are strong enough, it is conceivable that the price of the product could trigger a placebo effect and actually influence how effective the product is. This is referred to in recent literature as the “placebo effect of prices.”¹ It refers to the ability of prices to affect the physiological benefits a person receives from consuming a product.

Shiv, Carmon, & Ariely (2005) perform the first experiment examining the placebo effect of prices. They run a series of experiments with the intention of determining whether the price of an energy drink, SoBe Adrenaline Rush, affects the efficacy of the drink. The packaging of many energy drinks, including SoBe Adrenaline Rush, claims that the product increases mental acuity. Hence, Shiv and his co-authors use the number of word puzzles that participants solved over a period of thirty minutes after consuming the drink as their measure of the good’s efficacy. They run three main treatments: a first group of subjects pays the retail price for the drink, a second group purchases the drink at a discounted price, and a third group does not purchase or consume

the drink. Shiv, Carmon, & Ariely find that subjects in the discount treatment complete fewer puzzles on average than subjects who purchase the drink at the regular price.

Other research has found evidence of a similar placebo effect of prices. Waber (2008) and Waber et al. (2008) find that subjects who consume a sugar pill that they believe is a painkiller can tolerate more pain if the pill is described as an expensive drug than when it is presented as inexpensive. Plassmann et al. (2008) find that reporting a higher price for a wine increases consumers' experienced pleasantness from drinking the wine. Almenberg & Dreber (2009) find a similar placebo effect of the price of wine, but find it only occurs for more expensive wines and only when subjects are given price information before consumption.

The existence of a placebo effect of prices has serious implications for a range of issues, since it generally puts upward pressure on prices. The manufacturers of many products would have incentives to increase the price they charge. We should expect new products, in particular, to have higher than anticipated introductory prices (Bagwell & Riordan, 1991). The Waber et al. (2008) result could discourage policies that support discount drugs for low-income individuals or change the cost-benefit analysis of extending patent protection. A strong placebo effect of prices could justify any number of other policies that encourage, or do not inhibit, higher prices, with both efficiency and distributional consequences.

In this paper, we perform an experiment similar to the one performed by Shiv, Carmon, & Ariely (2005). Our purpose is twofold: first, we attempt to replicate their findings that higher prices lead to higher product efficacy. Second, we examine whether consumer choice alters the placebo effect. In Shiv, Carmon, & Ariely (2005) and Waber et al. (2008), participants are randomly assigned either a high or low price for a good. In reality, consumers have preferences for products of differing prices and can choose to purchase discounted or regular priced goods at dif-

ferent retail outlets. Our hypothesis is that once appropriate control for product quality are in place (that is, when consumer can reasonably infer that they can purchase the same good at two different prices), consumers will choose the low price and the placebo effect will be neutralized since the price can no longer act as a signal of quality (as long as there are no doubts that the discounted product is spoiled). In effect, we hypothesize that the placebo effect of price works as a quality signal, as discussed above. Hence, if subjects assume that quality does not vary with the price, the placebo effect should disappear.

Contrary to the results of Shiv, Carmon, & Ariely (2005), we find that higher prices lead to lower product efficacy. This effect is statistically significant despite a relatively small sample size and occurs regardless of whether consumers can exercise choice over the price or not. These results raise questions regarding the strength of the placebo effect of prices and the conditions under which it arise. The analysis also points to the importance of statistically controlling for individual characteristics, since we show that despite random assignment of subjects to treatments, a subject's level of experience with word games (e.g. Scrabble) significantly alters the results.

In what follows, we first describe the experimental procedure, present the key findings and then conclude with a brief discussion.

EXPERIMENTAL PROCEDURE

Seventy-nine subjects were selected from a database of students interested in participating in economics experiments at the University of Victoria (Canada). Because the experimental task involved solving English word puzzles, the only criterion applied to this selection was the requirement that the subject be a native English speaker. This criterion was implemented to minim-

ize the variance in the number of word puzzles solved that would be associated with subjects who had different native language. Subjects were otherwise randomly assigned to one of three treatments: i) regular retail price; ii) discount price; or iii) choice of retail or discount.

Six experimental sessions were conducted (two of each treatment) between March 12 and March 18, 2009. Each session with up to 15 subjects was held in the economics laboratory at the University of Victoria, which is equipped with computer terminals, privacy screens around each station and a video projector. Upon arrival, subjects were informed the experiment involved consuming an energy drink and were invited to leave if they did not wish to participate. Subjects who stayed signed a consent form. The experimenter read instructions aloud, while subjects followed along in a text version on their computer terminal. The instructions stressed the mental benefits the energy drink (Red Bull) claims to offer.

Subjects purchased a 250 ml can of Red Bull at a price determined by their treatment. The price was CA\$2.50 in the retail-price treatment and \$1 in the discount-price treatment. In the choice treatment, subjects were given the option of buying either a retail drink or a discounted drink. Subjects in the discount and choice treatments were informed their drinks were discounted because the experimenters received a bulk discount on some of their purchases. The purchase price of the drink was deducted from subjects' participation payment of \$18.

After subjects finished consuming their drink, they watched a 10-minute video to allow the effects of the beverage to take hold. Subjects then had 30 minutes in which to solve as many six-letter scrambled word puzzles as possible; there were 80 puzzles available in total. The word puzzles and experiment instructions were provided using Z-Tree and Z-Leaf software (Fischbacher, 2007). After time expired, participants completed a questionnaire on demographic information and their prior experience with word games.

Four potentially significant differences between the protocol of Shiv, Carmon, & Ariely (2005) and our own are worth mentioning. First, and most significantly, Shiv and his co-authors used SoBe Adrenaline Rush as the energy drink. We used a different drink, Red Bull, because the SoBe beverage was taken off the market in Canada prior to our experiment. Red Bull is a close substitute; both products make similar claims of increasing mental functioning. Second, Shiv, Carmon, & Ariely (2005) have sub-treatments with either psychological prompts or no psychological prompts (e.g. asking subjects to rate how well the product improves mental performance prior to completing puzzles and changing the strength of claims about the product in the experiment instructions). Since we are only interested in the price effect, we do not to employ psychological prompts. Third, we added the choice treatment to examine whether consumer choice negates the placebo effect as hypothesized above. Finally, we did not implement a control group where subjects did not consume an energy drink before solving puzzles. Shiv and his co-authors did not detect differences in the performance of their no drink and retail price subjects leading them to conclude that the effect was a negative one (low priced goods perform less well than regular price or no drink). In our experiment, this placebo effect can still be detected.

It is worth mentioning that although we closely followed the design of Shiv, Carmon, & Ariely (2005) in order to facilitate comparisons, some aspects of their design are not desirable. In particular, a number of word puzzles solved in 30 minutes is a very coarse measure, not ideal for statistical analysis beyond basic comparisons of distributions. The measure is discrete, which creates less differentiation between subjects than a continuous measure would. In addition, solving word puzzles is a specialized skill prone to naturally high variance due to subjects' differing

abilities. Nevertheless, the ease of comparison created by using an identical metric outweighs its downsides.

RESULTS

The results of our experiment are provided in Table 1. F-tests for equality of the mean number of word puzzles solved between treatments are not statistically significant at the 10% level, indicating that there is no statistically significant placebo effect.

~~Insert Table 1 about here~~

However, while F-tests for the equality of the means of each treatment are a good starting point, they fail to account for possible differences in word puzzle-solving ability between subjects. Thus, we also regress the number of word puzzles solved against treatment dummy variables. We follow the two-step negative binomial quasi-generalized pseudo-maximum likelihood estimation procedure outlined in Cameron & Trivedi (1986). Since our dependent variable (number of word puzzles solved) is a count variable, standard regression techniques such as ordinary least squares (OLS) are not appropriate. OLS is designed for estimating dependent variables that are continuous and can be negative or positive. However, our dependent variable is not continuous (i.e. one cannot solve a fraction of a word puzzle) and is strictly positive.²

This method results in identical coefficient estimates to the Poisson method, but with larger standard errors to account for overdispersion. Our basic model specification is provided in Equation 1.

$$(1) \quad \text{SCORE}_i = e^{(C+d_1\text{RETAIL}_i+d_2\text{CHOICE}_i)}$$

SCORE is the number of word puzzles solved, C is a constant, RETAIL is a dummy variable that takes a value of 1 if subjects are in the retail-price treatment and 0 otherwise, and CHOICE is a dummy variable that takes a value of 1 if subjects are in the choice treatment and 0 otherwise.

The resulting coefficient estimates are provided in Table 2. Contrary to the findings of Shiv, Carmon, & Ariely (2005), the negative coefficient for the retail-price dummy variable indicates that the retail-price treatment performs worse than the discount treatment. However, the difference between the two treatments is not statistically significant. The choice treatment also appears to have little or no effect on performance.

~~~ Insert Table 2 about here ~~~

A close inspection of the data actually reveals a number of outlier test scores associated with players who report substantial experience with word games. Thus, we control for individual abilities in solving scrambled word puzzles by using data from our questionnaire. Subjects provided how often they play Scrabble and do Jumble puzzles on a 5-point scale, bounded by “never” and “often.” We use this information to generate dummy variables SCRABBLE and JUMBLE. Each variable takes a value of 1 if a subject responded that he played a game regularly (4) or often (5), and a value of 0 otherwise. Equation 2 provides the new model specification.

$$(2) \quad \text{SCORE}_i = e^{(C+d_1\text{RETAIL}_i+d_2\text{CHOICE}_i+d_3\text{JUMBLE}_i+d_4\text{SCRABBLE}_i)}$$

The coefficient estimates from this equation are provided in Table 3.

~~~ Insert Table 3 about here. ~~~

With the added controls for ability, the RETAIL coefficient is statistically significantly negative, suggesting a “backwards” placebo effect in which individuals who paid the high retail price solved fewer puzzles than those who paid the discounted price. Subjects in the choice treatment did not solve a statistically different number of puzzles as those in the discount group.

CONCLUSION

Our findings raise doubts about the power of the placebo effect of prices. In particular, we are unable to replicate the findings of Shiv, Carmon, & Ariely (2005). A number of possible explanations for the discrepancy can be put forward.

First, we have demonstrated the importance of accounting for confounding factors, such as word solving ability or, in the wine price experiments, prior experience with wine tasting. Such factors are not controlled for in the previous literature. Misleading conclusions can be reached if the exogenous source of variance in the dependent variable is not evenly distributed across treatments. Only Waber et al. (2008) avoid this problem by testing the same subjects’ pain tolerances before and after consuming a sugar pill.

Second, the placebo effect may be sensitive to knowledge of, or experience with, a product. In a famous example, Klopfer (1957) reports the history of a cancer patient who made substantial progress after being injected a drug, only to get worse when he learned that the drug’s effectiveness was being questioned. He then received placebo injections under the false pretense that he was getting an improved version of the drug. His health substantially improved again un-

til he learned that the entire family of drugs was useless at fighting cancer, at which point he died within days.

Thus, perhaps the level of experience with a product directly interacts with the price of the product in giving rise (or not) to differences in effectiveness. One of the most significant differences between our experiment and that of Shiv, Carmon & Ariely (2005) is that we use Red Bull while they use SoBe Adrenaline Rush. These had achieved different levels of familiarity on the marketplace (SoBe was withdrawn in Canada for lack of commercial success). The signaling mechanism that we theorized as the potential source of any placebo or price effect should not be expected to function when consumers are familiar with the product they are buying. They might still question why a good is being sold at discount but to the extent that they purchase their goods from reputable retailers or retailers they trust not to sell tainted products, it is difficult to conceive of a significant placebo effect in consumers who know the product they are buying.

It might be possible, in fact, that consumers who are familiar with the product and receive a discount get excited by the prospect of obtaining the good at a great price and go on to experience greater benefits from the product. This would be consistent with our own findings and a finding of Shiv, Carmon, & Ariely (2005) that “on average, participants who had consumed SoBe before solved more puzzles.” We find that subjects who had consumed Red Bull before the experiment indeed had a positive effect on the number of word puzzles solved, but the effect was not statistically significant at the 10% level.

Third, it is possible that subjects’ expectations about how well Red Bull works are responsible for the backwards placebo effect. Irmak, Block, & Fitzsimons (2005), for example, find that the placebo effect arises among participants who want to experience the benefits of an energy drink, and is absent in those who do not.³ As such, it is possible the subjects in the dis-

count and choice treatments of our experiment wanted Red Bull to work more than subjects in the retail treatment better because of some reassuring feeling they obtain when receiving a discount.

Given that the existing body of literature on the placebo effect of prices is limited, and that our results are in conflict with existing findings, additional examination of the placebo effect of prices seems necessary. It is puzzling, for instance, that most of the evidence thus far suggests a negative placebo effect (i.e. lower-price goods result in worse performance than higher-price goods), and not a positive one (i.e. higher-priced goods do not increase performance relative to a control group). Future research opportunities exist for determining through what aspects of a product's price the placebo effect functions. For example, in our experiment, Shiv, Carmon, & Ariely (2005) and Waber et al. (2008), subjects in the cheaper product treatments received an explanation that the good was discounted. No control is done to determine whether the placebo effect is a result of the low price, or is really a placebo effect resulting from the notion that the product is discounted. It is also unclear what aspects of a good's price cause the placebo effect. Additional efforts should also attempt to test the signaling theory. It might not only help explain discrepancies between existing experiments, but also help determine whether it is a long-lived effect, or simply one associated with the uncertainty regarding the quality and effectiveness of new or unknown products only. This could be a critical element to help producers and retailers price both new and familiar products optimally.

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FOOTNOTES

¹ The seminal paper by Shiv, Carmon, & Ariely (2005) uses the term “placebo effect of price discounts.” We prefer the more general term “placebo effect of prices.”

² The Poisson method is frequently used to estimate regressions involving count data. However, our data suffers from overdispersion, which causes “spuriously small standard errors” of the coefficient estimates using Poisson (Cameron & Trivedi, 1986).

³ It should be noted that the authors study a standard placebo effect, not a placebo effect of prices.

TABLES

TABLE 1: NUMBER OF WORD PUZZLES SOLVED

| Treatment | Choice | Discount | High |
|---------------------------|--------|----------|-------|
| Observations | 23 | 28 | 25 |
| Mean | 16.00 | 14.86 | 13.80 |
| Median | 14 | 13.5 | 12 |
| Maximum | 47 | 37 | 48 |
| Minimum | 2 | 6 | 3 |
| Standard Deviation | 10.87 | 6.75 | 10.01 |

TABLE 2: BASIC REGRESSION RESULTS

| Variable | Coefficient |
|-----------------|----------------------|
| C | 2.6985***
(.1177) |
| RETAIL | -.0738
(.1720) |
| CHOICE | .0741
(.1747) |

Standard errors provided in brackets

*significant at 10% level

** significant at 5% level

*** significant at 1% level

TABLE 3: REGRESSION RESULTS WITH CONTROLS FOR EXPERIENCE

| Variable | Coefficient |
|----------|----------------------|
| C | 2.6607***
(.0943) |
| RETAIL | -.2940**
(.1438) |
| CHOICE | -.0052
(.1420) |
| JUMBLE | .7179**
(.2828) |
| SCRABBLE | .6932***
(.1927) |

Standard errors provided in brackets

*significant at 10% level

** significant at 5% level

*** significant at 1% level