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How Large Are Non-Budget-Constraint Effects of Prices on Demand?†

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Elementary consumer theory assumes prices affect demand only because they affect the budget constraint (BC). Alternative models, and some evidence, suggest prices can affect demand through other, non-BC channels (e.g., by signaling quality). This paper uses a lab and a field experiment to disentangle BC from non-BC effects of prices on demand. In the lab, we find that although prices positively affect stated willingness to pay, non-BC price elasticities are considerably smaller than BC price elasticities, are often statistically insignificant, and do not increase with product uncertainty. We do not detect any non-BC effects in our field experiment. (*JEL* C93, D12, M31)

In elementary consumer theory, prices affect demand only because they affect the budget constraint (BC). This assumption plays a crucial role in a wide range of economic applications, from tax incidence analysis to the evaluation of the welfare consequences of price changes. However, several models suggest that prices can affect demand through channels other than the BC. Most prominently, in the presence of incomplete information, consumers may infer a good's quality from its price (Tibor Scitovsky 1945; Benjamin Klein and Keith B. Leffler 1981; Asher Wolinsky 1983; Paul Milgrom and John Roberts 1986; Kyle Bagwell and Michael H. Riordan 1991). Another possibility is that goods may, to some extent, be "valued for their values" and not only for their intrinsic consumption benefit (Yew-Kwang Ng 1987).[1]

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[1] We do not examine other channels through which prices may affect demand, such as conspicuous consumption (Thorstein Veblen 1899), notions of fairness (Daniel Kahneman, Jack L. Knetsch, and Richard Thaler 1986), and customer antagonism (Eric T. Anderson and Duncan I. Simester 2008).

How large are such non-BC effects of prices on demand? In this paper, we set out to measure these effects, in the lab and in the field, and assess their importance relative to (standard) BC effects.

The marketing literature suggests that non-BC effects may be extremely important, especially for unfamiliar products. Numerous studies have found that prices can positively affect self-reported perceptions of product quality (see Akshay R. Rao and Kent B. Monroe 1989 for a review). Further, dating back at least to Harold J. Leavitt (1954), studies have found that when people are asked to state which of two unknown brands they would buy, under certain circumstances many say they would buy the more expensive one.[2] Recently, it has been demonstrated in the lab that prices can affect aspects of the consumption experience that extend beyond self-reported intentions and quality judgments. Thus, Baba Shiv, Ziv Carmon, and Dan Ariely (2005) show that participants who are charged a "regular" retail price of $1.89 for an energy drink subsequently perform better in solving puzzles than participants who pay a discounted $0.89. In another study, Rebecca L. Waber et al. (2008) find positive price effects on analgesic response to placebo pills. Participants who are informed that a new drug has "a regular price of $2.50 per pill" are more likely to show a reduction in reported pain after taking the pill compared with participants who are informed that the price has been "discounted to $0.10 per pill." A particularly intriguing result is reported by Hilke Plassmann et al. (2008), who measure brain activity as subjects taste wines identified by their price. Their results indicate that increasing the price associated with a given wine (from $5 to $45 and from $10 to $90) increases not only self-reported pleasantness but also brain activity in an area associated with experienced pleasantness.

These findings, as well as the economic models cited above, suggest the possibility of positive non-BC effects of prices on demand. Indeed, given the well-established sensitivity of consumer behavior to situational factors (see Lee Ross and Richard E. Nisbett 1991), promotions (Meghan R. Busse, Simester, and Florian Zettelmeyer 2007), and even to arbitrary "anchors" (Ariely, George Loewenstein, and Drazen Prelec 2003), the possibility of strong non-BC effects seems very plausible.[3] However, neither theory nor existing evidence is informative regarding the magnitude of such non-BC effects.

If non-BC effects are empirically important, standard estimates of price elasticities may be picking up a compound effect, consisting of a negative BC effect and a positive non-BC effect. This means that the overall size of the price effect may vary with context. For example, if consumers infer quality from price then the demand response to a given change in price may depend on whether the change in price stems from a tax hike or from firms' pricing decisions. This can have important implications. For example, it means that we cannot base our tax incidence analyses

[2] Indeed, a standard textbook approach posits that consumers have an "acceptable price range." In particular, there are prices that are unacceptable because they are considered too low (e.g., Monroe 2002; see André Gabor and C. W. J. Granger 1966 for an early exploration of the price thresholds idea).

[3] Notice that positive effects of sale signs (Anderson and Simester 2001) do not necessarily reflect non-BC effects, as sales typically affect the consumer's intertemporal BC.

on elasticities estimated from price variations that may signal variations in quality.[4] It may also imply that in some contexts (e.g., severe uncertainty), non-BC effects could be large enough to generate upward-sloping regions in the demand curve. Finally, if large non-BC price effects are due to consumers valuing goods "for their values" then, as Ng (1987) points out, taxes can be "burden-free."[5]

This paper uses a lab and a field experiment to identify and measure non-BC price effects on the demand for certain food products. In our lab experiment, a consumer chooses between two unfamiliar goods. We vary, independently, their relative prices in the consumer's BC (i.e., we vary the choice set); and the price stickers attached to them (which can, in our setup, affect demand only through non-BC channels). We further vary the information available to consumers (by having them make choices before and after tasting the goods) to examine the interaction between the two price effects and consumer uncertainty.

Our lab experimental design contains four elements that shed light on hitherto unanswered questions. First, we look at actual choices. This allows us to directly estimate demand effects (rather than perceptions and aspects of behavior that may or may not translate to demand effects). Second, we vary the amount of product information available to consumers. This allows us to measure the extent to which the estimated non-BC effects are consistent with the notion that uncertain consumers infer a good's quality from its price (notice that, in contrast, uncertainty is not required in Ng's 1987 model).

Third, to verify the validity of our experimental treatments and to make our findings more comparable with the marketing literature, we also measure self-reported evaluations. Comparing these with actual choices allows us to study the relationship between the two, and to measure the extent to which the familiar effects on perceptions are associated with effects on actual choices.

Fourth, we decompose standard price elasticity into its BC and non-BC components. This is our main contribution. It allows us to compare the magnitude of non-BC price effects on demand with that of standard BC effects. As discussed above, the comparison is crucial for assessing economic implications. Even from a commercial perspective, a pharmaceutical company considering pricing a pill at \$2.50 instead of at \$0.10 presumably weighs the (possible) positive non-BC demand effects suggested by the findings above against the negative BC effect of such a dramatic (2,400 percent) price increase. Disentangling the two and measuring them in a comparable way is a challenge that requires a special design since, in most cases, a change in price (necessary for estimating any non-BC price effects) also changes the BC.

[4] That is, the demand elasticity relevant to a tax hike may be higher (in absolute value) than the standardly-estimated elasticity, as the tax hike may generate less of the offsetting effect of prices as signals of quality. In a similar vein, a commodity tax levied on consumers may not be equivalent to a tax levied on producers if consumers are less aware of the source of price increase in the latter case and partly associate it with higher quality. Notice the difference between these implications and the finding by Raj Chetty, Adam Looney, and Kory Kroft (2009) that even the same tax may have a stronger (negative) effect on demand if it is included in posted prices.

[5] In Ng's (1987) model, utility from diamond goods is a function of expenditures on (rather than quantities of) these goods. A tax on such goods reduces the quantities consumed, but since (tax inclusive) expenditures remain the same, there is no welfare loss.

Consistent with the marketing literature, we find that variations in sticker prices affect subjective evaluations of the goods, and especially stated willingness to pay (SWTP). These effects are quite remarkable since they take place after much of the relevant uncertainty has been removed (after tasting the goods). However, when it comes to actual choices, we find non-BC effects of sticker prices that are significantly smaller than BC effects. Our point estimates of the non-BC price elasticity of demand are, depending on specification and information conditions, between 0.09 and 0.18, and are often statistically insignificant. Furthermore, they do not increase with product uncertainty. By comparison, our estimate of BC price elasticity is around -1.02, and is highly statistically significant in all specifications.

Moving to a natural economic environment, our field experiment is designed to complement the lab experiment by measuring non-BC effects in a real-world setting. The experiment exploits a pricing scheme commonly used in restaurants. Specifically, restaurants often offer a set menu meal, allowing diners to choose from several main course options while paying a fixed meal price. Since the choice set of a diner who chooses to order the meal is fixed, one can, in principle, vary the à la carte prices of the main courses, thus varying prices without affecting the diner's BC.

We implement this idea at a restaurant in Tel Aviv. The restaurant's location (relatively isolated), character (relatively luxurious), and price policy minimize the chances for selection bias (i.e., selection into set menu) and intertemporal BC effects, and ensure that most customers are unfamiliar with the specific entrées offered. We use two types of treatments to look for non-BC effects. First, we examine the effect of increasing the salience of the à la carte prices of the main courses offered in the set menu. This is done by printing prices in parentheses next to the main course options listed on the set menu. This alerts customers to the fact that, e.g., one course costs over \$20 if ordered à la carte while another costs \$14. Second, and more importantly, we systematically vary the à la carte prices themselves. Specifically, over the course of 14 weeks, we make weekly changes of almost 20 percent in the parenthesized prices of two main courses.

The results are striking. None of these non-BC price treatments has any detectable effect on consumer demand. Indeed, we do not find a single entrée for which demand is appreciably affected by the parenthesized prices. Hence, our findings from the lab and from the field complement each other. Together, they suggest that while non-BC price effects on demand may exist and may be experimentally detectable, their magnitude in at least some real-world settings might be too small to matter. While the price treatments in our field experiment are rather aggressive—price hikes of roughly 20 to 50 percent are more than what many a market will bear—they may be well below the hundreds-to-thousands percent hikes that are needed for detectable effects to arise in the lab.

Our findings appear consistent with recent field evidence from other price studies. Tanjim Hossain and John Morgan (2006) examine bidding on eBay when the total offer price is fixed while the product price component varies against the shipping and handling component. They do not detect positive (non-BC) demand effects of the product price component. Indeed they find that a higher product price leads to *fewer* bidders. Nava Ashraf, James Berry, and Jesse M. Shapiro (2007) examine the possibility of non-BC ("psychological") effects of prices on product use. They find no evidence that, conditional on purchase, higher prices increase actual usage. Using the same methodology, Jessica Cohen and Pascaline Dupas (2008) find no evidence, though with wide confidence intervals, of such "psychological" effects of either higher prices or the act of paying.

The next section discusses the lab experimental design, and Section II reports lab results. Section III discusses the field experimental design, and Section IV reports field results (our main lab and field results are presented, respectively, in Sections IIC and IV). Section V concludes.

I. Lab Experiment: Design

We conduct an experiment in which participants are presented with two unfamiliar goods (caramel and peanut candies), complete six choice tasks where they choose between bundles of the two goods, and fill out a questionnaire. They then roll a die to determine which of the six bundles they have chosen during the experiment they will actually receive, making their choices incentive compatible.[6]

The experiment proceeds as follows. Participants are presented with two unfamiliar products in their original packages.[7] They may inspect the closed packages. A standard price sticker, clearly indicating both the retail price of the package and the implied unit price, is attached to each package. Participants complete three choice tasks, taste each of the two products, and then complete three additional choice tasks. Having completed the choice tasks (choosing six bundles overall), they answer product evaluation and demographic questions. The former include quality ratings and SWTP. Finally, participants roll the die, receive one of the bundles they chose and a show-up fee of 10 New Israeli Shekel (NIS), the official currency of Israel, (roughly $2.50 at the time of the study), and leave the room.

To rule out social interaction during the experiment, participants are seated alone where they cannot see other participants' choices. They are asked to remain silent throughout the experiment and on their way out. A session typically lasts less than 15 minutes.

A. *Price Variations*

When encountered on a store shelf, the price sticker attached to a product may affect demand through the BC channel (because it indicates the cost that enters the consumer's BC) and through the non-BC channel (e.g., as a quality signal). Our experiment is designed to disentangle the two by varying sticker prices independently

[6] A quiz administered after reading the instructions and prior to making any decision verifies that participants understand the rules that determine what they will receive at the end of the experiment (the quiz and all other instruments used in the experiment are available in the Web Appendix).

[7] The two products are: *Publix "peanut butter bars,"* in a 30-unit (7 oz.) package; and *Goetze's "Caramel Creams,"* in a 30-unit (12 oz.) package. Choice bundles consist of combinations of candy units. The experiment takes place in Israel, where neither brand is available and where the products are unfamiliar. Peanut butter bars are virtually nonexistent in Israel and caramels are relatively rare (verified through online catalogues of supermarket chains and by physical inspection of the campus supermarket). Both are imported especially for the experiment. Unfamiliarity with the products (discussed further in section IB) is verified in the questionnaire participants fill out at the end of the experiment. We denote the peanut bars by x and the caramels by y.

from choice sets (so that sticker prices no longer enter the BC). We now explain how this is done.

As mentioned above, each participant makes six choices. The choice tasks are titled Choice No. 1 through Choice No. 6. A sample choice task is provided below.

Choice No. 1

Which of the following combinations would you like to receive? Please circle one of the following options.

a) 5 units of caramel candy (A) and 0 units of peanut candy (B)
b) 4 units of caramel candy (A) and 2 units of peanut candy (B)
c) 3 units of caramel candy (A) and 4 units of peanut candy (B)
d) 2 units of caramel candy (A) and 6 units of peanut candy (B)
e) 1 unit of caramel candy (A) and 8 units of peanut candy (B)
f) 0 units of caramel candy (A) and 10 units of peanut candy (B)

The list of bundles in each choice task is generated by a standard linear budget constraint as follows. Denote a bundle of the two goods by (x, y), where x and y are the quantities of peanuts and of caramels, respectively. Denote the normalized *BC price*—the price that enters the BC—of good $i \in \{x, y\}$ by p_i^{BC}. Both total expenditure and the BC price of good y are held constant across all choice tasks. Specifically, the BC for all choices is given by

$$p_x^{BC} x + p_y^{BC} y = I, \quad \text{where} \quad p_y^{BC} = 1 \text{ and } I = 5.$$

This means that the bundle $(0, 5)$ is feasible in all choices. In other words, option (a) in the sample choice task above is identical in all choice tasks. The rest of the options, however, differ between choice tasks, due to variations in the BC price of x. Specifically, we let

$$p_x^{BC} \in \{0.5, 1, 2\}.$$

The sample task above, for example, corresponds to $p_x^{BC} = 0.5$.[8] Each participant makes three choices under the three different (randomly ordered) values of p_x^{BC}, respectively, before tasting the products, and three more choices after tasting the product. This variation in p_x^{BC} allows us to estimate, for each participant, the BC price effects on the demand for x. Furthermore, for each participant, we can estimate, separately, both a pre- and a post-tasting demand curve.

In addition to (and independently of) varying p_x^{BC} for each participant, we vary the price on the sticker attached to good x across participants. These variations are within the range of prices charged for such products in the United States. Participants are informed that the goods "were purchased from a large supermarket chain in

[8] Notice that in the sample task above, no bundle consists of equal amounts of the two goods. The experiment's parameters are chosen to ensure that this is the case in all choice tasks, to avoid a bias toward a "50-50" option.

the United States" and it seems reasonable to assume that they perceive the sticker prices as actual (equilibrium) store prices (for evidence, see Section IIA). The sticker price of y is, again, held constant. Specifically, denoting by p_i^S the normalized sticker price of good $i \in \{x, y\}$, we set

$$p_y^S = 1$$

for all participants, while we let

$$p_x^S \in \{0.5, 2\}.$$

This is done by randomly assigning participants to either the $p_x^S = 0.5$ treatment, where the sticker attached to the package of good x reads *"30 units, \$1.49, 1 unit = 5¢,"* or the $p_x^S = 2$ treatment, where the sticker reads *"30 units, \$5.99, 1 unit = 20¢."* The sticker attached to good y is held fixed and reads *"30 units, \$2.99, 1 unit = 10¢"* in all treatments.[9] In order to avoid sticker-design confounds, uniform stickers are produced for the experiment. They are identical in all but the prices they indicate.

B. *Consumer Uncertainty and Intertemporal Substitution*

Before tasting the products, the informational content available to participants (through inspection of the sealed packages) is similar to that typically available to consumers browsing through store shelves. However, participants are uncertain regarding how good these unfamiliar products taste.[10] We refer to the pre-tasting informational benchmark as *uncertainty condition*, and to the post-tasting choices as made under a *certainty condition*.

Hence, our experimental design is a 3 (BC prices) $\times$ 2 (certainty conditions) $\times$ 2 (sticker prices). Each participant makes 3 $\times$ 2 choices, and is randomly assigned to one of 2 sticker price treatments.

Before we turn to the results, it is important to note that although BC prices are varied independently of sticker prices, the latter could, in principle, affect a participant's *intertemporal* BC. To see why, suppose a participant has a preference for diversity in the amounts of the two goods that she consumes over time (i.e., her preferences are strictly convex). Suppose, further, that she is uncertain about the retail price of x, and takes its sticker price p_x^S as informative about the price she would have to pay for the good if she were to purchase it outside the experiment. Then, for a given p_x^{BC}, it may be optimal to choose more of x in the experiment (and less outside the experiment) the higher is p_x^S. Therefore, to correctly measure non-BC effects, the consumer's decision needs to be one-shot. This is achieved in our experiment by

[9] Dollar price stickers pose no evaluation issues in Israel, where some goods and services (e.g., housing) are routinely priced in dollars.

[10] The centrality of taste to choice is supported by our results. See the discussion (and footnote 15) on page 180.

using two goods that are unavailable in the participants' country, and are unlikely to be faced again.[11]

II. Lab Results

One hundred eighty-six Hebrew University students were recruited. Of these, 183 completed the experiment: 94 in the $p_x^S = 0.5$ treatment and 89 in the $p_x^S = 2$ treatment.[12] Table 1 reports descriptive statistics. As the top panel shows, both treatment groups consist of about 55 percent females and have similar age distributions centered around 23 years old. The groups are also generally similar in terms of study major, except that the $p_x^S = 2$ group has a higher proportion of economics, accounting, and business students. In the analysis below, we report results with and without demographic controls.

A. *Subjective Measures*

We begin by examining the effect of sticker prices on subjective quality ratings and stated willingness to pay (SWTP). By replicating findings from the marketing literature (e.g., Rao and Monroe 1989; William B. Dodds, Monroe, and Dhruv Grewal 1991; R. Kenneth Teas and Sanjeev Agarwal 2000), we verify that participants perceive and react to the sticker prices as expected.

Consider first participants' SWTP. For each of the two goods, participants were asked: "If the product were offered for purchase in Israel, what would be the maximum price (in NIS) that you would be willing to pay for a package of 30 units like the one in front of you? You may write any price from 0 to 100 NIS."

As seen in the second panel of Table 1, mean SWTP for good x (peanut candy) is 11.88 NIS under $p_x^S = 0.5$, and increases to 12.29 under $p_x^S = 2$. For good y (caramel candy), whose sticker price is constant across treatments, mean SWTP decreases from 13.12 NIS under $p_x^S = 0.5$ to 11.63 under $p_x^S = 2$. Both changes are in the expected direction but, interestingly, the sticker price effect is larger for the good whose sticker price did not vary. This rules out the concern that participants answer the SWTP question by simply translating the dollar sticker price to NIS. Rather, it suggests that relative sticker prices affect participants' evaluations of the goods.

Although neither of the above changes in SWTP is statistically significant when viewed in isolation, the joint effect of sticker prices on SWTP is unambiguous and robust. To see this, the first four columns in Table 2 report OLS regressions, where the dependent variable is the difference in SWTP between x and y. The main explanatory variable is a dummy for the high sticker price treatment, $p_x^S = 2$. The first

[11] The use of goods that are not available locally helps alleviate an additional minor concern, namely that participants might seek to "return" the good to a local store and receive its price in cash. This concern is further minimized by the fact that participants do not receive the products in their original unopened package but rather receive a bundle consisting of single units.

[12] The assignment process was as follows. Individual experimental sessions were carried out in ten sets, over the course of three days. All sessions in a given set had the same sticker price, which alternated between sets. Participants were recruited on campus by RAs and directed to the lab. The RAs did not know for which treatment they were recruiting, and participants could only see the products and stickers once their individual session began.

Table 1—Lab Experiment Descriptive Statistics (*by treatment*)

	$p_x^S = 0.5$		$p_x^S = 2$		Difference	
	Mean	(SD)	Mean	(SD)	Mean	[SE]
Panel A. Demographics						
Female (percent)	0.56	(0.50)	0.55	(0.50	−0.01	[0.07]
Age	23.33	(3.70)	23.38	(4.09)	0.06	[0.58]
At least one study-major in:						
Humanities	0.29	(0.45)	0.31	(0.47)	0.03	[0.07]
Economics, accounting, business	0.31	(0.46)	0.48	(0.50)	0.17**	[0.07]
Social sciences (w/o economics)	0.44	(0.50)	0.39	(0.49)	−0.04	[0.07]
Panel B. Subjective evaluations						
SWTP:						
Peanut (x)	11.88	(6.81)	12.29	(6.23)	0.42	[0.96]
Caramel (y)	13.12	(7.05)	11.63	(6.71)	−1.48	[1.02]
Quality:						
Peanut (x)	4.22	(1.74)	4.25	(1.78)	0.03	[0.26]
Caramel (y)	4.32	(1.75)	3.92	(1.58)	−0.40	[0.25]
Panel C. Quantities of x (peanut) demanded						
$p_x^{BC} = 0.5$:						
Uncertainty	3.94	(2.95)	4.45	(3.13)	0.51	[0.45]
Certainty	4.21	(3.41)	5.26	(3.77)	1.05*	[0.53]
$p_x^{BC} = 1$:						
Uncertainty	2.11	(1.20)	2.25	(1.38)	0.14	[0.19]
Certainty	2.32	(1.72)	2.66	(1.85)	0.34	[0.26]
$p_x^{BC} = 2$:						
Uncertainty	0.96	(0.76)	1.03	(0.78)	0.08	[0.11]
Certainty	1.09	(0.80)	1.17	(0.87)	0.08	[0.12]
Observations	94		89			

Notes: Standard deviations are in parentheses, standard error of differences are in brackets. Age data are missing for two observations in the $p_x^S = 0.5$ group, and quality data are missing for one observation in the $p_x^S = 2$ group. Study-major dummies = 1 if at least one of the participant's study fields is in the indicated category. Since most Israeli undergraduate students major in two fields of study, a subject may be counted in up to two of the study-major categories. Subjects who are counted in none of the reported three categories include nonstudents as well as students in law, the life sciences, the natural sciences, and other small groups. SWTP is reported in the NIS. Quantity is reported on a 1 to 7 scale. See text for details.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

column shows the effect in the entire population, with no controls: the high sticker price increases the difference in SWTP in favor of good x by 1.90 NIS. Controlling for demographics as well as date fixed effects (column 2) increases the estimated effect to 2.42 NIS. Columns 3 and 4 show that these results are not driven by outliers. Excluding observations with SWTP difference that is more than two standard deviations away from the mean does not alter the results (statistical significance increases due to smaller standard errors).

Next, consider participants' quality ratings of the two goods. The wording of the relevant question was: "Please rate the quality of each of the products on a scale of 1 to 7. (1 means 'terrible' and 7 means 'excellent.')"

Since this is an ordinal variable, a useful way of summarizing the data is to look at whether good x was judged to be of higher quality, same quality, or lower quality than good y. The difference in perceived quality yields a variable taking values in

Table 2—The Effect of Sticker Prices on Subjective Product Evaluations
(*Dependent variable = difference between peanut* (x) *and caramel* (y) *in SWTP and sign of difference in product quality perceptions*)

	Difference in SWTP (OLS)				Sign of difference in perceived quality (ordered logit)	
	(1)	(2)	(3)	(4)	(5)	(6)
$p_x^S = 2$	1.90**	2.42***	1.94***	2.42***	0.48*	0.54*
	(0.86)	(0.92)	(0.72)	(0.76)	(0.28)	(0.31)
Female		0.36		0.23		−0.17
		(0.98)		(0.83)		(0.33)
Age		−0.04		−0.13		−0.04
		(0.19)		(0.16)		(0.07)
Age^2		−0.00		0.00		−0.01
		(0.01)		(0.01)		(0.01)
Humanities		0.64		1.30		0.18
		(1.05)		(0.86)		(0.35)
Economics, accounting, business		−1.54		−1.09		−0.15
		(1.02)		(0.85)		(0.33)
Social sciences (w/o economics)		0.23		0.20		−0.10
		(0.93)		(0.77)		(0.31)
Constant	−1.24**	−1.53	−1.50***	−2.12**		
	(0.60)	(1.16)	(0.49)	(0.97)		
Other controls	No	Yes	No	Yes	No	Yes
Outlier observations dropped	No	No	Yes	Yes	No	No
Observations	183	183	174	174	182	182
R^2	0.03	0.06	0.04	0.09		
Pseudo R^2					0.01	0.02

Notes: Standard errors are in parentheses. All results are post choosing and post tasting. OLS regression estimates are reported in columns 1–4. Dependent variable = difference in SWTP (in NIS) between peanut and caramel candy (peanut − caramel). Ordered logit regression (OLR) estimates are reported in columns 5 and 6. Dependent variable = sign of difference in perceived quality (on a 1–7 scale) between peanut and caramel (three ordered categories). "$p_x^S = 2$" dummy = 1 if sticker price of peanut candy is double that of caramel ($p_x^S - 2p_y^S$), and 0 otherwise ($p_x^S = 0.5\, p_y^S$). Age variables are centered around the median (i.e., age = subject age − 23). "other controls" include date fixed effects and an "age missing" dummy. Where "outlier observations dropped" = yes, observations where the dependent variable is more than two standard deviations away from the mean are dropped.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

$\{-1, 0, 1\}$. In columns 5 and 6, we regress this variable on the dummy for the $p_x^S = 2$ treatment, using an ordered logit model. The results suggest that the likelihood that participants associate a good with a higher quality goes up with its relative price. This effect is significant at only the 8 percent level, but is consistent with the SWTP effect in columns 1–4 and with the marketing literature. As with SWTP, the effect is mainly driven by differences in the quality ratings of good y.[13]

These positive effects are rather remarkable since SWTP and quality ranking in our experiment are reported shortly after inspecting the packages and actually consuming the goods, whereas in most of the marketing studies where positive price

[13] A Mann-Whitney test rejects equality of the distributions of y's quality ratings across the two sticker price treatments at the 8 percent level. We cannot reject equality of these distributions for x.

effects are obtained, subjects know very little about the products (sometimes only brand names, sometimes not even that, and sometimes other features such as country of origin or the store where they were purchased).

B. *Quantities Demanded and BC Effects*

We now turn to participants' actual choices. Figure 1 shows the distribution of quantities of x demanded across all 183 subjects, under the three possible p_x^{BC}'s. The row titled "uncertainty" shows pre-tasting distributions, and the row titled "certainty" shows post-tasting distributions. For ease of reference, the bottom row (titled "budget sets") illustrates the choices available to participants in each p_x^{BC} condition given that total expenditure is held constant at five. Thus, when $p_x^{BC} = 0.5$ (left-most column) participants can choose up to 10 units of x, while when $p_x^{BC} = 1$, they can choose up to 5 units. Finally, because participants cannot choose fractions of candies, when $p_x^{BC} = 2$, they can choose up to 2 units.

As the histograms make clear, prior to tasting, when choice is based only on visual inspection of the unfamiliar products, most participants are found at interior solutions. Thus, the uncertainty row shows distributions that have a clear mode in the interior of the BC, with frequencies decreasing more or less monotonically in both directions toward the two corner solutions. However, the certainty row shows that having tasted both goods, many subjects shift to corner solutions.[14] This response to the new information is intuitive. Subjects diversify under uncertainty, but once they know exactly what they are getting, many relinquish diversification in favor of the better-tasting product.[15]

Further exploring subject heterogeneity, Figure 2 shows the distribution, across the 183 participants, of the sensitivity of individual demand to BC price, by certainty condition. We measure sensitivity to BC price by averaging, for each individual in each certainty condition, the two observed arc elasticities (of the demand for x): one calculated at $p_x^{BC} = 0.5, 1$; and the other at $p_x^{BC} = 1, 2$.[16]

Figure 2 shows, first, that positive demand responses to BC price are rare. In the uncertainty condition, only five subjects exhibit positive response, and this number

[14] The variance in quantities demanded is higher under certainty for any given p_x^{BC}. The difference is statistically significant with p-values of 0.001 and 0.02 for $p_x^{BC} = 0.5$ and $p_x^{BC} = 1$, respectively. For $p_x^{BC} = 2$, we cannot reject equality of variances.

[15] This interpretation is supported by responses to the questionnaire at the end of the experiment. Specifically, participants are asked the following question: "What were the factors that influenced your choices *before you tasted the products*? Please indicate the importance of each of the following factors on a scale of 0 to 10, where 0 means not important at all and 10 means very important. A. Expected quality of the product; B. Expected price of the product; C. Expected taste of the product; D. Nutritional ingredients; E. Attractive Packaging; F. Desire to receive maximum units of candy; G. Desire for variation in types of candy; H. Other (describe); I. Other (describe)."

By far the most important factor turned out to be C (expected taste), which scored 8.9, on average, with a standard deviation of 1.9. Second in importance was A (expected quality) with a mean score of 6.5 (SD = 3.1), and third was G (desire for variety) with a mean score of 5.5 (SD = 3.0). All other factors scored less than 4.4 on average.

[16] BC price senstivity is distributed similarly in the two p_x^S treatments: Mann-Whitney tests for equality of distribution across p_x^S yield p-values of 0.78 and 0.18 for the uncertainty and certainty conditions, respectively. Similarly, equality of means across p_x^S cannot be rejected (t-test $p = 0.92$, 0.23, respectively). Variations in price sensitivity under certainty are not significantly associated with any of our demographic variables (used in Table 2). However, pre-tasting, males are significantly more sensitive to price.

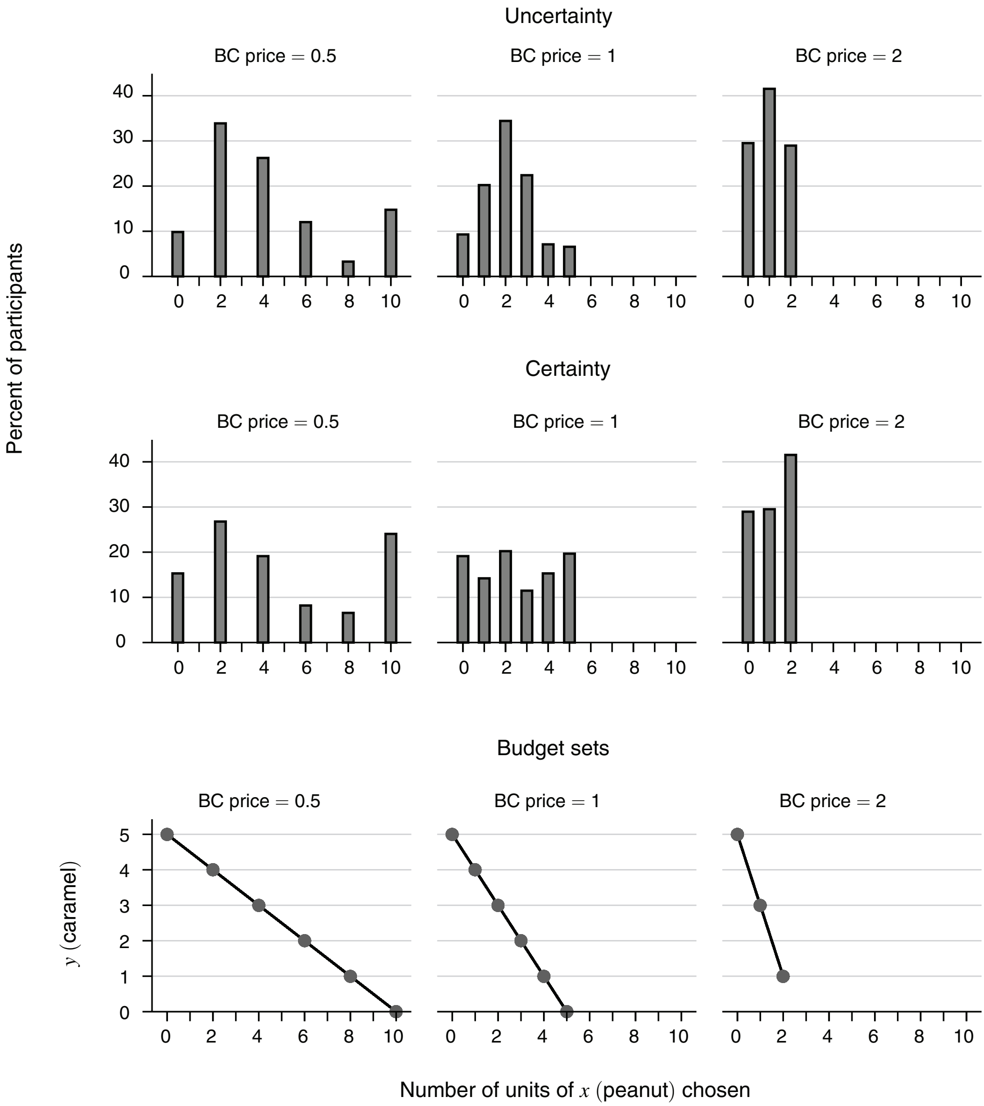

FIGURE 1. DISTRIBUTIONS OF PARTICIPANTS' CHOICES BY CERTAINTY CONDITION AND BY p_x^{BC}

Note: Based on 183 participants.

drops to one in the certainty condition. Nonetheless, there is considerable cross-subject variation in the magnitude of the response. While almost 10 percent of subjects are perfectly inelastic in the uncertainty condition, roughly 22 percent have a negative BC price sensitivity of 1.5 or more.[17] After tasting, however, the proportion of subjects exhibiting such extreme sensitivity to BC price reduces to 10 percent,

[17] Responses to the questionnaire at the end of the experiment suggest that subjects are quite conscious of their pattern of choice. The stronger the observed price sensitivity, the higher the importance that subjects assign to the "desire to receive maximum units of candy" before tasting. Correlation between the importance score given

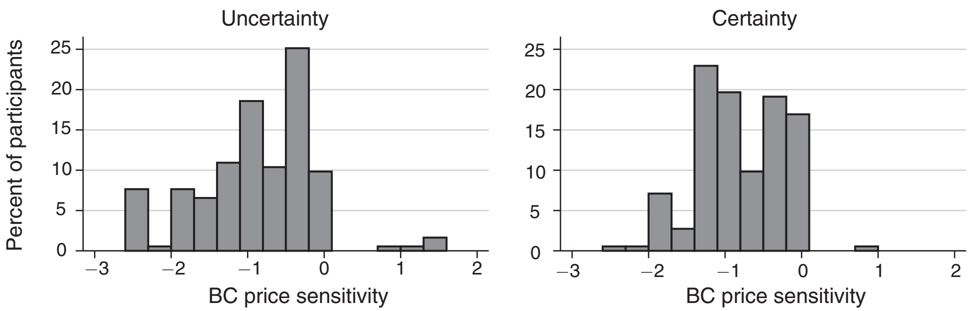

Figure 2. Distributions of Participants' Sensitivity to p_x^{BC} by Certainty Condition

Notes: Based on 183 participants. BC price sensitivity measure: average of the two arc elasticities calculated at $p_x^{BC} = 0.5, 1$, and at $p_x^{BC} = 1, 2$, respectively.

while that of perfectly inelastic subjects increases to 17 percent. This complements our finding (see Figure 1), that more subjects switch to corner solutions after tasting. Overall, the variation in price sensitivity decreases somewhat after tasting, with the standard deviation dropping from 0.76 to 0.56 (the difference is statistically significant at the 0.001 level). We cannot reject equality of mean sensitivity across certainty conditions.

Finally, before turning to non-BC effects, it is worth noting that individual choices after tasting are strongly correlated with the subjective measures (also taken after tasting) studied in Table 2. The correlation between the mean quantity of x chosen under certainty and either the difference in SWTP or the sign of the difference in perceived quality is remarkably high (0.71 and 0.85, respectively). Thus, our results below should not be construed as evidence that consumers fail to choose what they report they like (Christopher K. Hsee and Reid Hastie 2006). Rather, self-reports seem quite consistent with choice behavior. At the same time, non-BC effects on choice behavior may be small relative to BC effects. We turn to this question next.

C. *The Relative Magnitude of BC and Non-BC Effects*

This section presents our main results.

We start with Figure 3, which depicts the data reported in the bottom panel of Table 1. The left panel shows choice under uncertainty. It plots, for each p_x^{BC} and each p_x^S, the mean quantity demanded and its 95 percent confidence interval. This yields two 3-point demand curves: one for the low sticker price treatment $p_x^S = 0.5$ (diamonds), and one for the high sticker price treatment $p_x^S = 2$ (triangles). As expected, the demand curve under the high sticker price treatment lies to the right of the demand curve under the low sticker price treatment. The difference between the curves, however, is surprisingly small given participants' unfamiliarity with the

to this factor and observed BC price sensitivity under uncertainty (in absolute value) is 0.30. By way of comparison, correlation of this score with observed sensitivity after tasting is 0.05. See footnote 15 for precise phrasing.

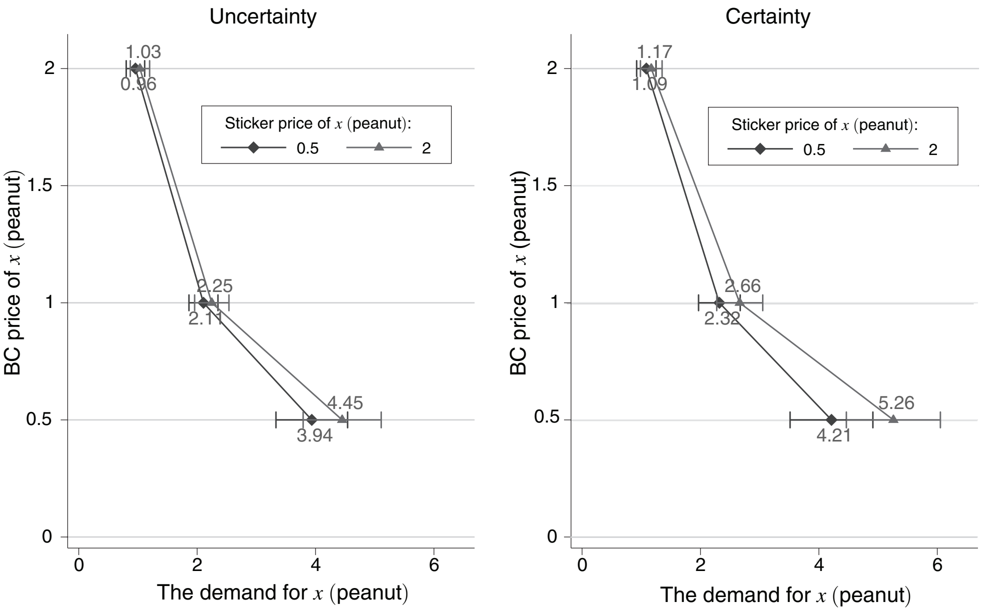

FIGURE 3. BC DEMAND CURVES FOR x BY STICKER PRICE AND BY UNCERTAINTY CONDITION

Notes: Based on 183 participants. Capped ranges indicate 95 percent confidence intervals.

products. And, as the confidence intervals suggest, the means are not significantly different across sticker prices.[18]

The right panel of Figure 3 shows post-tasting choice. Again, the demand curve under the high sticker price lies to the right of the demand curve under the low sticker price. And, again, the difference between the curves is rather small. Nonetheless, and quite unexpectedly, the difference between the curves is slightly larger (but not statistically significantly so) than under uncertainty. While we still cannot reject equality of either means or distributions across sticker prices for $p_x^{BC} = 1, 2$, equality is rejected at the 6 percent level for $p_x^{BC} = 0.5$.

Finally, our experiment allows us to estimate price elasticities, and to decompose standardly estimated elasticity into its BC and non-BC components. As discussed in the introduction, disentangling and comparing the two components is crucial to understanding the economic implications of any non-BC price effects. To this end, consider only the choices made under $p_x^{BC} \in \{0.5, 2\}$. Since, in typical situations, sticker price and BC price move in tandem, a standard estimate of the effect of price on demand would rely on the difference between observed choices under the conditions

$$p_x^{BC} = p_x^S = 0.5$$

[18] Testing for equality of distributions (rather than means) with Mann-Whitney and Wilcoxon signed-rank tests does not change the picture. For any of the three BC prices, we cannot reject the hypothesis that the distributions of quantities demanded are equal across sticker prices, while equality is strongly rejected ($p < 0.0001$) across any two BC prices.

and

$$p_x^{BC} = p_x^S = 2.$$

Table 3, column 1 reports this estimate for the uncertainty condition. The column reports the OLS-estimated effect of the price of x on the quantity of x demanded.[19] The regression uses only observations for which one of the above conditions is satisfied (i.e., only observations for which $p_x^{BC} = p_x^S$). The only explanatory variable is an indicator for a high "standard price" of x, denoted by p_x (namely $p_x = p_x^{BC} = p_x^S = 2$). An increase in the price of x from 0.5 to 2 is estimated to reduce the quantity demanded by 1.93 units. The implied price elasticity (evaluated at the mean) is reported in the bottom panel and is estimated to be -0.94. Controlling for demographics and date fixed effects in column 2 does not alter these estimates.

In columns 3 and 4, we decompose this effect by adding the observations in which $p_x^{BC} \neq p_x^S$. This allows us to separately estimate the BC and non-BC effects of prices. As column 3 shows, when controlling for sticker price, the BC price effect is slightly higher—though not statistically significantly so—than the estimated effect in column 1. That is, an increase in BC price alone reduces demand by 2.13 units, and the implied price elasticity is -1.03. Thus, while failing to control for the non-BC effect might yield an underestimate of the BC effect of prices, the bias we find is statistically indistinguishable from zero.

At the same time, a high sticker price is estimated to increase quantity demanded by (statistically insignificant) 0.20 units for a given BC. The point estimate of the sticker price elasticity is 0.09, with a 95 percent confidence interval $[-0.06, 0.25]$. Comparing magnitudes, the non-BC elasticity is around one-twelfth the (correctly estimated) BC elasticity, with a confidence interval $[-0.06, 0.24]$ around the ratio (sticker price elasticity/$-$ BC price elasticity). In summary, while we cannot distinguish the non-BC elasticity from zero, we can reject the hypothesis that it is greater than 0.25, either in absolute magnitude or as a fraction of BC elasticity. Results in column 4 are similar.

Columns 5–8 in Table 3 repeat the above procedure for choices in the certainty condition. The standard estimate of price elasticity is about -0.9 (columns 5 and 6), while the price elasticity controlling for the non-BC effect is -1.02. As was seen in Figure 3, the non-BC effect does not shrink in the certainty condition (indeed it increases, although the difference is not statistically significant). The estimated elasticity with respect to sticker price is between 0.16 and 0.18, with 95 percent confidence intervals $[-0.01, 0.33]$ without controls (column 7) and $[0.001, 0.36]$ with controls (column 8). In other words, non-BC elasticity is now marginally different from zero and—because confidence intervals around the non-BC-to-BC elasticity ratio are again almost identical to those around the point estimates—we can confidently reject that the ratio is greater than roughly one-third.[20]

[19] We report OLS rather than negative binomial model estimates mainly for ease of interpretation. Results are similar in the two models (sticker price elasticities and their standard errors are very slightly smaller with negative binomials). See footnote 20 for other robustness checks.

[20] Since each subject makes three sequential choices in randomized order (both pre- and post-tasting), one can estimate Table 3 using only each subject's first choice pre-tasting and first choice post-tasting, thus estimating

TABLE 3—PRICE ELASTICITIES OF DEMAND
(*Dependent variable = quantity of peanut candy (x) demanded*)

	Uncertainty (pre-tasting)				Certainty (post-tasting)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
p_x (standard price)	−1.93***	−1.92***			−2.03***	−1.92***		
	(0.21)	(0.24)			(0.24)	(0.26)		
p_x^{BC} (BC price)			−2.13***	−2.13***			−2.40***	−2.40***
			(0.14)	(0.15)			(0.15)	(0.15)
p_x^S (sticker price)			0.20	0.19			0.38*	0.43*
			(0.16)	(0.17)			(0.21)	(0.22)
Female		−0.17		−0.38		−0.23		−0.38
		(0.38)		(0.29)		(0.43)		(0.36)
Humanities		0.73*		0.29		0.2		0.13
		(0.38)		(0.29)		(0.44)		(0.38)
Economics, accounting, business		0.30		0.23		−0.12		0.01
		(0.40)		(0.29)		(0.42)		(0.36)
Social sciences (w/o economics)		−0.08		−0.16		−0.14		−0.05
		(0.33)		(0.25)		(0.40)		(0.34)
Constant	4.90***	4.47***	5.01***	5.04***	5.23***	5.17***	5.46***	5.52***
	(0.41)	(0.56)	(0.35)	(0.46)	(0.47)	(0.66)	(0.41)	(0.56)
Other controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	183	183	366	366	183	183	366	366
R^2	0.31	0.33	0.34	0.36	0.27	0.29	0.33	0.34
Implied price elasticities of demand:								
Standard price	−0.94***	−0.94***			−0.91***	−0.87***		
	(0.06)	(0.08)			(0.06)	(0.08)		
	[0.00]	[0.00]			[0.00]	[0.00]		
BC price			−1.03***	−1.03***			−1.02***	−1.02***
			(0.04)	(0.04)			(0.02)	(0.03)
			[0.00]	[0.00]			[0.00]	[0.00]
Sticker price			0.09	0.09			0.16*	0.18**
			(0.08)	(0.08)			(0.09)	(0.09)
			[0.30]	[0.45]			[0.03]	[0.09]

Notes: Robust standard errors are in parentheses. In columns 3, 4, 7, and 8 they are clustered by individual participant. OLS regression estimates are reported in the top panel. Dependent variable = quantity of peanut candy demanded, holding implicit total expenditure and caramel prices (both BC and sticker) constant. "Other controls" include date fixed effects, "age," "age^2," and an "age missing" dummy. Elasticities, calculated at mean price/quantity, are reported in the bottom panel. p-values in square brackets (bottom panel) are based on randomization inference (see details in text).

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Finally, in addition to the previous analysis, the bottom panel of Table 3 reports, in square brackets, p-values based on randomization inference (e.g., Guido W. Imbens and Jeffrey M. Wooldridge 2009, section 4). To compute these p-values, we randomly divide the ten experimental session sets into two groups of five, and (falsely)

both BC and non-BC elasticities from only between-subject variations. This does not change the main results. BC price elasticities decrease from 1.02–1.03 to 0.89–1.02 in absolute value but remain large and highly statistically significant. Non-BC price elasticities become statistically indistinguishable from 0 (and are never larger than 0.12; some even become negative).

reassign the groups to two $\tilde{p}_x^S$ treatments (0.5 and 2). We also randomly reassign each individual to false (permuted) $\tilde{p}_x^{BC}$'s. Repeating this 10,000 times, we estimate p-values by counting the resulting elasticities that are at least as large as those reported in Table 3. This does not change our main results. Our reported (simulated) p-values for sticker price elasticities (0.30, 0.45, 0.03, and 0.09) are fairly close to those computable from the reported coefficients and standard errors (respectively, 0.23, 0.26, 0.07, and 0.05). As for standard price and BC price elasticities, simulated and computed p-values are virtually identical (at 0.00).

To sum up, we find that, consistent with marketing evidence, prices positively affect SWTP. However, when examining actual demand, non-BC price elasticities are considerably smaller than BC price elasticities and are often statistically insignificant. We estimate the non-BC-to-BC elasticity ratio at roughly one-twelfth to one-sixth, and can confidently reject the hypothesis that it is much greater than one-quarter (pre-tasting) to one-third (post-tasting). Interestingly, we find no evidence that non-BC effects increase with uncertainty.

III. Field Experiment: Design

As with many lab experiments, one worries that results obtained in the lab may not carry over to natural economic settings (Glenn W. Harrison and John A. List 2004; Steven D. Levitt and List 2007). One aspect of the lab environment that could be particularly important is that participants are acutely aware of the fact that their choices are being recorded and subsequently scrutinized. They may feel compelled to behave differently from the way they would behave in a typical market setting. For example, knowing that they are participating in an economics experiment, participants may behave more "economically" than they would otherwise. While our lab results benefit from a clean identification in a controlled setting, it is clearly important to examine behavior in the field.

A. *The Restaurant*

The field experiment took place from June 2007 to October 2007 at a restaurant in the metropolitan area of Tel Aviv. The restaurant was chosen for several reasons. First, it offers, simultaneously, both à la carte and set-menu dinner options. Second, as detailed below, the decision of which menu to order from (à la carte or set menu) is made at the table level. This reduces the potential for selection bias.[21] Third, every diner's order at the table is promptly entered into a point-of-sale computer system by the waitperson, hence, data are automatically collected. Fourth, the restaurant owner agreed to implement our experiment by printing five different versions of the menu and using them on different weeks.

[21] Selection bias could arise if individuals self-selected into the group of set-menu diners. This potential is reduced due to the fact that individual diners have only limited influence over the choice of menu (à la carte or set). The influence of the average diner is decreasing with table size (but is not entirely eliminated; we return to this point later).

Fifth, the location of the restaurant (relatively isolated from both residential clusters and high-rise office buildings), as well as its price level (relatively high, with the average diner's check around $25), suggest that the typical diner is unlikely to be a daily or even a weekly "regular." Minimizing the chance of repeat customers, who frequently return to the restaurant, enhances internal validity because it minimizes the chance that diners will be familiar with the entrées; will remember the prices of entrées, and order without looking at the menu; will be subject to a treatment while still remembering prices from a previous treatment; or will be influenced by intertemporal BC considerations. Anecdotal evidence based on conversations with staff suggests that most diners at the restaurant seem to be there for the first time (or for the first time in a long time).

In spite of these advantages (and as expected in the field), the restaurant setting remains less controlled than the lab setting. For example, the potential for selection bias, mentioned above, is easily ruled out in the lab. We postpone the discussion of this and other concerns to Section IIID, where we offer ways to address them. First, we detail the experimental setup and design.

B. *The Set Menu and Diners' Choice*

The set menu (reproduced in Appendix A) is the first page of the dinner menu that is handed to diners once they are seated (the rest of the pages form the à la carte menu). It offers, for a fixed price, a three-course meal consisting of a selection of first course options, a main course, and a dessert. Diners decide, jointly at the table level, whether to order à la carte or from the set menu. The restaurant does not allow for "mixed" tables, with some diners opting for the set menu and others ordering à la carte (children are excluded). In other words, either all (adult) diners at a table order à la carte, or they all order from the set menu.

Diners at tables that opted for the set menu are charged a fixed price, and are served four shared first courses, one out of five main course options per diner, and one out of three dessert options per diner. For an additional "upgrade" cost, diners may change the main course options and, instead of choosing one of the five main course options on the set menu, they may choose any main course from the à la carte menu. Finally, diners may add to their meal any item from the à la carte menu (e.g., a drink) for an additional charge.[22]

Diners are involved in two decisions. First, the whole table chooses jointly between ordering à la carte and ordering from the set menu. Second, each diner orders individually from the chosen menu. Our experiment focuses on the second choice of diners at tables that opted for the set menu. Specifically, it focuses on their choice of a main course. *Within this choice*, the individual prices of main courses do not affect the choice set (= main courses listed on the menu) or the cost of a meal (= fixed set-menu cost, possibly with a fixed upgrade cost added). However, if

[22] The fixed price of a set-menu meal was 115 NIS when our experiment began, and changed to 120 NIS midway through our experiment. The "upgrade" cost was 15 NIS throughout (at the time of the experiment US $1 was roughly equal to 4 NIS). A small fraction (in our data, less than 2 percent) of the set-menu diners pay for their meal with a set-menu gift certificate, and a small fraction of upgraders are effectively waived the upgrade cost. Excluding either the former or the latter (or both) does not affect our findings below.

prices have a positive non-BC effect on demand, the demand for a given main course should, nonetheless, increase with its price. Our experiment is designed to detect such demand effects by varying the prices of main courses.

C. *The Experiment*

The experiment is conducted as follows. Five versions of the set menu are used, on different weeks, constituting five treatments.[23] Of the five, four versions include prices, in parentheses, that are printed next to each one of the five main course options. A fifth version does not include printed parenthesized prices. We refer to the former as the *price* treatments, and to the latter as the *no-price* treatment. The parenthesized prices in the price treatments are the actual prices diners would have to pay for each of the main courses if ordered à la carte rather than from the set menu. Waitpersons are informed of this fact and are instructed to explain it to diners who inquire regarding the parenthesized prices.

Neither diners nor waitpersons are aware that they are participating in an experimental study. When the experiment begins, management explains to the waitstaff that in subsequent weeks management will explore different menu versions (with and without parentheses) and different prices.

The five main course options on the set menu are listed below along with their price-treatment parenthesized prices:

Shrimp gnocchi in tomato & cream sauce (82 *NIS*)
Pork shank in herbs (78 *NIS*)
Red mullet fillet in roasted pepper & cream sauce [(67 *NIS*) *or* (80 *NIS*)]
House sausages (56 *NIS*)
Stuffed artichoke in lemon sauce [(67 *NIS*) *or* (80 *NIS*)]

Three of these options—shrimp, pork, and sausages—are also listed in the à la carte menu. Their parenthesized prices replicate their à la carte prices and are held constant in all four price treatments. The other two options (stuffed artichoke and red mullet) are listed only in the set menu (a potential concern regarding this point is addressed below). The parenthesized price of each of these two entrées alternates between 67 NIS and 80 NIS, forming four (artichoke price, mullet price) combinations, hence four price treatments: (67, 67), (67, 80), (80, 67), and (80, 80). In contrast, as mentioned above, in the no-price treatment, no such price information is available on the set menu (the parentheses above are entirely absent).

By the nature of the setup and the menu items, diners choose under a fair amount of uncertainty regarding product qualities (the size, taste, quality, etc., of such entrées vary widely across restaurants). This makes choices in the restaurant more comparable with lab choices in the uncertainty (pre-tasting) condition than in the certainty (post-tasting) condition. If non-BC effects are important in such a context,

[23] We could not vary menus across tables on a given night for practical reasons (waitpersons during rush hour might mix menus), and because it could be perceived as discriminatory. The assignment process of treatments is detailed in Table 4.

the economics models and the marketing findings cited suggest two different effects that are expected in our experiment. First, increases in the parenthesized prices of mullet and artichoke should increase their demand. Notice that our price variations are rather large: a price change from 67 NIS to 80 NIS amounts to a 19.4 percent increase. At the same time, the varied prices remain within the range of prices at the restaurant. We refrain from using excessively large price variations—of the scale often used in the lab—to retain external validity.[24]

Second, the very existence of parenthesized prices gives salience to the fact that sausages are the cheapest main course option offered, while shrimp, at the other extreme, are the most expensive (and are a substantial 46.4 percent more expensive than sausages). This fact is not apparent to diners in the no-price treatment (although, with some effort, they could find the prices of shrimp and sausages in the à la carte menu). Consequently, merely adding parenthesized prices should increase demand for shrimp (and decrease demand for sausages).

D. *Potential Biases*

In this section, we discuss four concerns regarding our design, and how they are addressed. The first two arise from economic theory. The last two are technical implementation issues. Before discussing them one by one, we stress that the biggest three concerns, if valid, would bias our estimates towards an *overestimated* non-BC effect of price on demand. In other words, they would potentially magnify, rather than attenuate, any other positive price effects. Hence, one could jointly address these concerns by interpreting our estimates as upper bounds on, rather than point estimates of, the potential size of positive non-BC price effects. The fourth (lesser) concern is addressed directly.

We start with selection bias. As mentioned above, parenthesized prices could potentially affect the joint table decision regarding which menu (à la carte or set) to order from. For example, diners who like artichokes may be more likely to prefer the set menu when artichoke's parenthesized price is higher: given that they order an artichoke, ordering it à la carte would cost more. The proportion of artichoke lovers in the set-menu population may therefore increase with artichoke price, contributing to a positive price effect on demand.

The likelihood of this bias is somewhat reduced due to the fact that selection is done at the table level (see footnote 21). Moreover, in Appendix B, we present evidence suggesting that such bias did not occur in our data. Regressing a set-menu dummy on treatment and control variables (reported in Table B1), we find no effect of treatments on table selection into set menu.

Our next concern is intertemporal substitution which, again, was ruled out by design in our lab experiment. In the restaurant, in contrast, a changed parenthesized price on the set menu could affect the intertemporal BC. Thus, for example, if the price of mullet is high, diners may infer that mullet is, in general, an expensive entrée

[24] See, for example, Nicholas M. Kiefer, Thomas J. Kelly, and Kenneth Burdett (1994), who conduct a field experiment in a restaurant to estimate (standard) price elasticities. They argue that price increases of 6.1 to 22.3 percent for a menu item are "reasonably large price changes."

and would remain so in the future. Hence, they may be more likely to choose it now from the set menu, again, contributing to a positive effect of price on demand. While this possibility cannot be ruled out, our choice of restaurant renders it unlikely. Most diners do not seem to return to the restaurant frequently, and the prices of main courses are only informative to a limited extent regarding future prices elsewhere.

Moving to technical concerns, one may worry that our design artificially "pushes" diners to base their choices on parenthesized prices. Unaccustomed to seeing parenthesized prices on set menus, diners might assume that the former are printed there for a reason (for example, to signal size or quality). Since this may bias diners toward overreacting to price treatments, this potential bias is addressed (similarly to the above two) by interpreting our estimated effects as upper bounds.

Finally, a minor technical concern is that since artichoke and mullet are not listed in the à la carte menu, diners may *mistakenly* view their parenthesized prices as cheap talk in spite of the fact that, as stated above, these prices are what diners would actually pay if they ordered the two entrées à la carte. One should keep in mind, however, that the absence of the two entrées from the à la carte menu is far from salient to diners. Indeed, only an unusually scrupulous diner would systematically scan the multipage menu to notice that two of the set menu options are not listed anywhere else. Moreover, this potential credibility issue is irrelevant when examining the effect of adding parenthesized prices on the demand for shrimp and sausages.

IV. Field Results

The experiment lasted 14 weeks. We restrict our data by keeping only diners who sat at tables of two to ten diners between 4 PM and midnight.[25] With these restrictions, the system recorded orders from 6,225 individual diners (or 2,078 tables) during the experiment.

Table 4 shows the assignment of treatments by weeks, and reports the number of diners (and, in parentheses, tables) recorded each week. Week numbers refer to the ordinal number of the week in 2007.[26] As seen in Table 4, the five treatments were implemented in two cycles (with gaps), with each treatment covering two or three weeks in total. The bottom row reports the overall allocation of diners (tables) to treatments.

In interpreting the data, we assume that an individual's decision to dine at the restaurant is unaffected by treatments. Diners, unaware of the experiment, are unlikely to know prior to sitting at the restaurant which version of the menu is used (indeed, their decision to sit there is unlikely to be affected by treatments even if they were expected). Similarly, we assume that ancillary decisions such as the size of the party

[25] Regarding table size, the set menu is not offered to single diners. Excluding single diners, 98.9 percent of the tables had ten diners or less. Regarding hours, the dinner menu is not officially offered before 4 PM (although diners may request it earlier). Orders after midnight are rare.

[26] Treatment blocks always started on a Sunday, lasted one or two weeks, and ended on a Saturday. Notice that while 14 full weeks include 98 days, our data cover only 95 days. The three missing days include two days during which the restaurant was closed due to Jewish high holidays, and one day the kitchen was unprepared to serve stuffed artichoke due to a mistake.

Table 4—Diners (and Tables) by Treatment and Week

Week number	Treatment				
	No price	(67, 67)	(67, 80)	(80, 67)	(80, 80)
26	0	455 (144)	0	0	0
27	0	0	426 (139)	0	0
28	0	0	441 (147)	0	0
29	0	0	0	535 (177)	0
30	0	0	0	506 (170)	0
31	0	0	0	0	584 (199)
33	556 (177)	0	0	0	0
34	0	573 (187)	0	0	0
36	0	0	303 (98)	0	0
37	0	0	0	287 (95)	0
38	0	0	0	0	281 (108)
39	0	0	0	0	340 (121)
40	506 (175)	0	0	0	0
41	432 (141)	0	0	0	0
Total: 6,225 (2,078)	1,494 (493)	1,028 (331)	1,170 (384)	1,328 (442)	1,205 (428)

Notes: "Week number" refers to the ordinal number of the week in 2007, with week number 1 starting on the first Sunday of the year. Treatment (p_1, p_2) denotes a week during which the parenthesized price of stuffed artichokes was p_1 NIS and that of red mullet fillet was p_2 NIS. Figures in cells report number of diners in two-to-ten-diner tables placing orders between 4 PM and midnight during that week. Parenthesized figures in cells report numbers of tables in which diners were grouped.

(i.e., the number of diners per table), the time and day of the dinner, etc., are unaffected by treatments. At the same time, since treatment effects cannot be identified separately from effects of the specific weeks during which each treatment was implemented, our analysis below includes time trends and other control variables.

Table 5 summarizes some of these control variables. It reports means or percentages for the no-price treatment, and the difference between this treatment and each of the four price treatments. The reported characteristics (average number of diners per table, percent of tables who dined during "dinner rush hour," 8 PM to 11 PM, and percent during weekend) sometimes vary across treatments. For example, the (67, 67) group might be slightly skewed toward busy days and busy hours. Since they may be correlated with diners' behavior, these characteristics are controlled for in the analysis below.

Main Course Selection of Set-Menu Diners.—Out of the 2,078 tables with 6,225 diners recorded during the experiment, 198 tables with 572 diners (9.2 percent of all diners) chose the set menu. We analyze this choice in Appendix B, and find that it was not affected by treatments in any meaningful way. In this section, we analyze the second, individual choice, namely main course selection by set-menu diners.

Of the 572 diners sitting at set-menu tables, 547 ordered a set-menu meal, and we have data on the main course selection for 544 of them.[27] Table 6 reports these selections, with set-menu main courses sorted by popularity. The table shows that

[27] Diners sitting at a set-menu table without ordering a set-menu meal are mostly children. Other rare exceptions include, for example, individuals who sit with others but do not order food. Additionally, three main course selections (out of 547) were not properly entered into the system, possibly due to waitperson errors.

Table 5—Diner Characteristics by Treatment

	Control group	Difference between treatment group and control group			
	No price	(67, 67)	(67, 80)	(80, 67)	(80, 80)
Number of diners per table	3.03	0.08	0.02	−0.03	−0.22**
	(0.07)	(0.12)	(0.11)	(0.10)	(0.10)
Percent between 8 PM and 11 PM	0.60	0.06*	0.04	0.04	0.08**
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Percent during Israeli weekend (Thursday–Saturday)	0.54	0.07**	0.0	0.01	−0.04
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Number of tables	493	331	384	442	428

Notes: The leftmost ("Control group") column reports each statistic's mean value and standard error (in parentheses), for a control group with no price treatment. The rest of the columns report each statistic's difference in means from the control, and the standard error of the difference. Asterisks denote the result of t-tests on the equality of means between each treatment group and the control group. Treatment (p_1, p_2) denotes the parenthesized prices of (artichoke, mullet) in NIS. Total number of observations (or tables) in each treatment group is reported in the bottom row.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

mullet, which was ordered by one-third of the diners, was the most popular main course. Next in popularity were pork and shrimp, each ordered by almost a quarter of the diners. Artichoke and sausages were substantially less popular. Finally, slightly more than one-tenth of diners chose to "upgrade" their main course, ordering a main course from the à la carte menu and being charged accordingly.

Remember that, as was discussed in Section IIID, the hypothesis of positive non-BC effects gives rise to two predictions regarding our experiment. First, the demand for artichoke and for mullet should rise with their parenthesized price. Second, the demand for shrimp should rise (and the demand for sausages should fall) when parenthesized prices are present. Our main result is illustrated in Figure 4.

The figure depicts the percentage of set-menu diners choosing the various main courses, by treatment. It shows that none of the above two predictions is borne out in our data. In contrast with the first prediction, the top chart shows no systematic (and never statistically significant) effect of any of the treatments on the demand for artichoke and mullet. While the sign of the differences in the demand for artichoke is in the predicted direction—demand is slightly higher in the $(80, 67)$ and $(80, 80)$ treatments than it is in the $(67, 67)$ and $(67, 80)$ treatments—the opposite is true for mullet. Here differences in demand are in the direction opposite to that predicted (demand is slightly lower in the $(67, 80)$ and $(80, 80)$ treatments).

In a similar vein, the bottom chart shows none of the predicted non-BC effects on either shrimp or sausages. Opposite to the second prediction, the demand for shrimp, pooled across all four price treatments, slightly falls compared with the no-price treatment (the difference is not statistically significant). The demand for sausages remains virtually unchanged.

These results are reflected in Table 7. The table reports estimated coefficients from diner-level regressions of main course selection on treatment and control variables. The top panel presents estimates from separate OLS regressions, and the

TABLE 6—MAIN COURSE SELECTION OF SET MENU DINERS

Main course	Number of diners	Percent
Mullet	182	33.5
Pork	125	23.0
Shrimp	123	22.6
Artichoke	33	6.1
Sausage	19	3.5
Upgrade (not in set menu)	62	11.4
Total	544	100.0

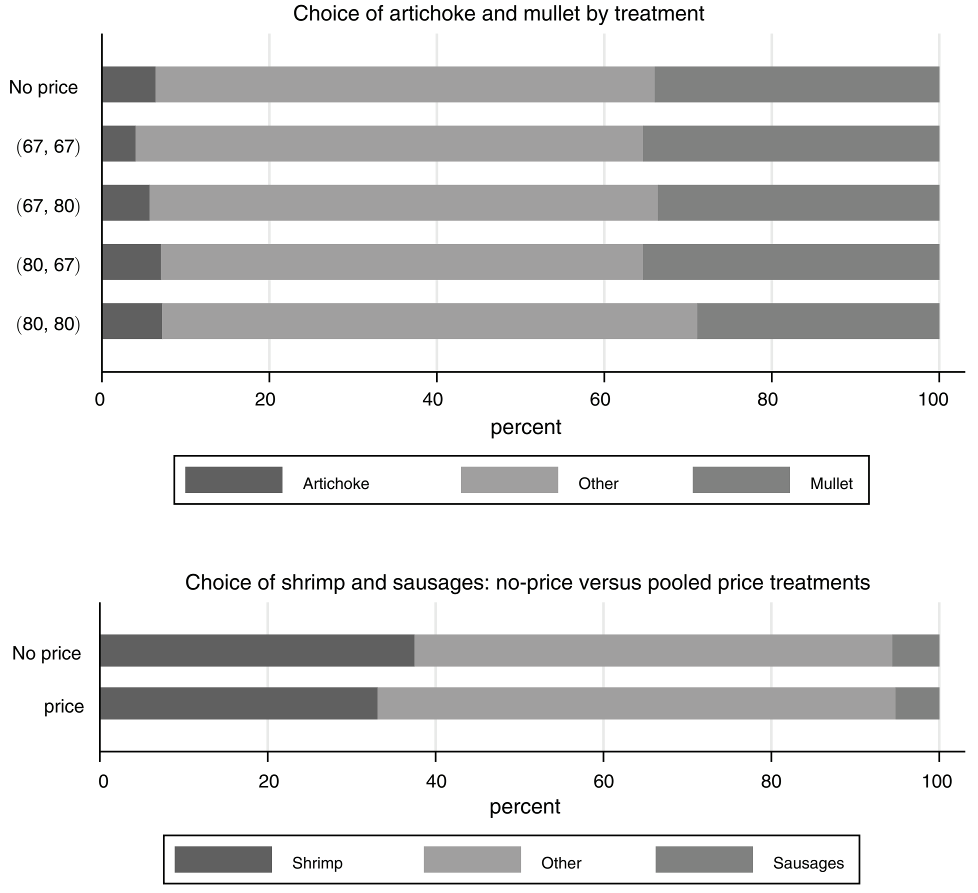

FIGURE 4. SET MENU DINERS' CHOICE BY TREATMENT

Notes: Based on 544 set-menu diners. Treatment (p_1, p_2) denotes the parenthesized prices of (artichoke, mullet) in NIS. "No price" denotes a treatment in which parenthesized prices are absent from the set menu, and "Price" denotes the other four treatments, pooled together.

bottom panel presents multinomial logit estimates. Alternative (not reported) specifications, like seemingly unrelated regressions (SUR) and probit, produce almost identical results. The four treatment variables include: a dummy for price treatments (= 1 if prices were printed on set menu, and 0 otherwise); a dummy for whether the

Table 7—Treatment Effects on Main Course Selection of Set-Menu Diners
(*Dependent variable = categorical variable indicating individual's selection of main course*)

	Artichoke (1)	Mullet (2)	Shrimp (3)	Pork (4)	Sausages (5)	Upgrade (6)
	Linear probability model (six independent regressions)					
Prices indicated on set menu	0.02	0.07	−0.09	−0.01	0.01	0.00
	(0.05)	(0.10)	(0.10)	(0.09)	(0.02)	(0.08)
Artichoke = 80 NIS	0.03	−0.06	0.12	−0.07	−0.02	−0.00
	(0.03)	(0.07)	(0.07)	(0.07)	(0.02)	(0.07)
Mullet = 80 NIS	0.03	−0.05	0.06	−0.09	−0.00	0.04
	(0.03)	(0.08)	(0.07)	(0.07)	(0.03)	(0.06)
(Artichoke, Mullet) = (80, 80) NIS	−0.04	−0.01	−0.14	0.15	0.04	0.00
	(0.04)	(0.12)	(0.10)	(0.10)	(0.05)	(0.08)
Observations	544	544	544	544	544	544
R^2	0.05	0.03	0.04	0.06	0.05	0.14
	Multinominal logistic model (joint regression)					
Prices indicated on set menu	0.39		−0.59	−0.24	−1.27	−0.46
	(0.83)		(0.62)	(0.62)	(1.68)	(0.72)
Artichoke = 80 NIS	0.72		0.70	−0.19	−0.00	0.44
	(0.70)	Base	(0.45)	(0.46)	(1.67)	(0.75)
Mullet = 80 NIS	0.86		0.48	−0.23	0.87	0.68
	(0.74)	Outcome	(0.46)	(0.47)	(1.60)	(0.75)
(Artichoke, mullet) = (80, 80) NIS	−0.86		−0.64	0.73	1.22	0.24
	(1.07)		(0.67)	(0.63)	(2.37)	(1.12)
Observations				544		
Pseudo R^2				0.01		

Notes: All reported regressors are dummies. OLS regressions are reported in the top panel, where each of the six columns represents an independent regression with a dummy dependent variable (indicated in column title). The dummy = 1 if the diner chose the main course in the column title, and 0 otherwise. A multinominal logistic regression is reported in the bottom panel. The categorical dependent variable is constructed similarly to the dependent variables in the top panel, and the base outcome is mullet (the most frequent outcome). All regessions control for waitperson, price of set menu, number of diners per table, hour of the day, day of the week, and a quadratic time trend. Robust standard errors, clustered at the table level (by 198 tables), are in parentheses.

artichoke was priced at 80 NIS; a dummy for whether the mullet was priced at 80 NIS; and an interaction dummy for whether both were priced at 80 NIS (that is, the (80, 80) treatment).[28]

All regressions include the following (not reported) control variables: six waitperson dummies; a control for the price of the set menu (which was 115 NIS before week 35 and 120 NIS after); a control for number of diners at the table; seven dummies for hour of checkout; six dummies for day of the week; and a quadratic time trend (days elapsed since experiment began).

Results in the top and the bottom panel are similar. Remarkably, we cannot detect even a single statistically significant effect of parenthesized prices on main course

[28] Standard errors allow for clustering at the table level, where correlations are most likely to occur. Clustering at the day level, a more general specification, does not change our results (standard errors shrink very slightly). Further, a randomization inference test at the week level—falsely reassigning weeks to treatments (see Table 4), 10,000 times, and estimating the probability to get coefficients at least as large as those reported—yields p-values that are around those implied by the reported standard errors.

selections. Still more telling, as discussed regarding Figure 4, the coefficients are often in the "wrong" direction. These findings are not sensitive to whether or not the above controls are included, either as a group (as in the reported regressions) or one by one. Nor are they sensitive to specification or to the way the standard errors are calculated. In short, we find no evidence of non-BC effects of prices on demand in the restaurant.

V. Conclusion

This paper attempts to disentangle BC from non-BC effects of prices on demand. Whereas the positive effects of prices on consumers' perceptions are well documented, our data suggest that they do not automatically translate into large effects on consumer choices relative to BC effects. Further, the marginally significant non-BC effects we do observe do not appear to be primarily due to incomplete information.

In our field experiment, which uses real-world price variations, we cannot detect any non-BC effects. Our lab experiment suggests a possible positive non-BC effect on demand, with average elasticity around 0.13. When compared with the much larger BC price effect, it seems unlikely to generate upward-sloping segments of the demand curve. Our confidence intervals allow us to reject the hypothesis that the non-BC-to-BC price elasticity ratio is greater than one-quarter pre-tasting to one-third post-tasting (we can hardly reject that the ratio is zero). Nonetheless, failing to control for non-BC effects may bias the estimated BC price elasticity upward from about -1.02 to about -0.91 which, depending on the issue at hand, may or may not be important.[29]

At this point we would like to point out two limitations to the generalizability of our results. First, this paper has only examined demand for two sets of food products in two specific settings. It is quite possible that for other types of commodities, or in other contexts, prices exert non-BC effects on demand that are of considerably different magnitudes. For example, non-BC effects might be larger for goods that are more similar to each other (like different brands of olive oil). Notice, however, that BC effects may also be larger in this case as the goods would be closer substitutes.

Second, we note that disentangling the consumer's BC from sticker prices (in the lab) and from à la carte prices (in the restaurant) may, in itself, make our experimental settings different from other settings (including the equilibrium models mentioned in the introduction). It is conceivable that this very disentangling, which constitutes our identification strategy, influences behavior. In the restaurant, for example, diners may interpret the inclusion of a main course in the set menu as an additional (perhaps negative) signal about its quality. Similarly, subjects' reaction to sticker prices may be attenuated in the lab, where sticker prices are not reflected in the choice set,

[29] To take the simplest example, a standard tax incidence analysis that "naïvely" uses standard estimates of price elasticities can imply that consumers would bear a larger share of the tax than they actually would, since the demand response to a tax hike may be stronger than the (compound) response to other changes. Suppose, just for the sake of the example, that the elasticities obtained in the lab are real-world demand elasticities. Suppose, further, that the market is competitive, that a tax hike has *no* non-BC effects and that the supply elasticity is one. Then consumers pay 49.5 percent of a per-unit tax. However, the "naïve" calculation implies that consumers pay 52.4 percent of the tax.

compared with a standard supermarket setup, where sticker and BC prices are one and the same.

With these caveats, we believe that this paper provides strong and consistent evidence regarding the magnitude of non-BC demand effects, and hope it stimulates further research on a wider range of products.

Appendix A: Sample Set Menu

Special menu

FIRST COURSES

Sea bream carpaccio

Caesar salad

Chigga

Olio calamari & smoked shrimps

MAIN COURSES

Shrimp Gnocchi in tomato & cream sauce (82 NIS)

Pork shank in herbs (78 NIS)

Red mullet fillet in roasted pepper & cream sauce (80 NIS)

House sausages (56 NIS)

Stuffed artichoke in lemon sauce (80 NIS)

DESSERT

Espresso Panacota

Poppy seed chocolate and raisin cake

Wild berries bejet

115 NIS

Note: The set menu reproduced here is almost identical to the original. One word in the description of one of the courses had to be replaced since it compromised the anonymity of the restaurant.

Appendix B

Here, we show that price treatments did not cause an (identifiable) effect on the choice, faced by groups of diners at the table level, whether to order à la carte or from the set menu. Table B1 reports results from table-level regressions, with all 2,078 table-level groups of diners who participated in the experiment. A set-menu dummy is regressed on treatment and control variables. Columns 1 and 2 present, respectively, OLS regression estimates and the marginal effects of probit regression estimates. The four treatment variables are identical to those in Table 7 (and are explained on page 25). All regressions include the following (not reported) control variables: 18 waitperson dummies; a control for the price of the set menu (which was 115 NIS before week 35, and 120 NIS after); a control for number of diners at the table; a control for number of tables placing orders on same day; 7 dummies for hour of checkout; 6 dummies for day of the week; and quadratic trends for day of the month and days since the experiment began.

Results in the two columns closely resemble each other and suggest that treatments did not affect the choice of menu to order from. This finding does not depend on whether or not the above controls are included, either as a group (as in the reported regressions) or one by one.

Table B1—Treatment Effects on Set Menu Choice
(*Dependent variable = dummy indicating whether table chose set menu*)

	Linear probability model (1)	Probit model (marginal effects) (2)
Prices indicated on set menu	−0.01	−0.02
	(0.03)	(0.03)
Artichoke = 80 NIS	−0.03	−0.02
	(0.03)	(0.02)
Mullet = 80 NIS	−0.01	−0.01
	(0.04)	(0.03)
(Artichoke, Mullet) = (80, 80) NIS	0.05	0.04
	(0.05)	(0.05)
Observations	2,078	2,078
R^2	0.06	
Pseudo R^2		0.09

Notes: All reported regressors are dummies, each = 1 if the condition in its title is satisfied, and 0 otherwise, according to treatments. OLS regression estimates are reported in column 1, and the marginal effects of probit regression estimates in column 2. The dependent variable is a dummy, = 1 if the table chose to order from the set menu, and 0 otherwise. All regressions control for waitperson, price of set menu, number of diners per table, hour of the day, day of the week, and quadratic trends for day of the month and days since the experiment began. Robust standard errors are in parentheses.

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