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Magic at the marketplace: Choice blindness for the taste of jam and the smell of tea

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ABSTRACT

We set up a tasting venue at a local supermarket and invited passerby shoppers to sample two different varieties of jam and tea, and to decide which alternative in each pair they preferred the most. Immediately after the participants had made their choice, we asked them to again sample the chosen alternative, and to verbally explain why they chose the way they did. At this point we secretly switched the contents of the sample containers, so that the outcome of the choice became the opposite of what the participants intended. In total, no more than a third of the manipulated trials were detected. Even for remarkably different tastes like Cinnamon-Apple and bitter Grapefruit, or the smell of Mango and Pernod was no more than half of all trials detected, thus demonstrating considerable levels of choice blindness for the taste and smell of two different consumer goods.

1. Introduction

In Johansson, Hall, Sikström, and Olsson (2005) we demonstrated that participants may fail to notice mismatches between intention and outcome in a simple decision task. In the study we showed the participants pairs of pictures of female faces, and gave them the task of choosing which one they found most attractive. Unknown to the participants, on certain trials, we used a card magic trick to covertly exchange one face for the other. On these trials, the outcome of the choice became the opposite of what they intended. We registered whether the participants noticed that anything went wrong with their choices. Across all the conditions of the experiment, no more than 26% of the manipulation trials were detected. We call this effect *choice blindness* (for details, see Johansson et al., 2005).

Processing of faces is of great importance in everyday life (Bruce & Young, 1998; Rhodes, 2006; Schwaninger, Carbon, & Leder, 2003). This suggests to us that choice blindness will generalize widely to other visual stimuli, and even across modalities. But we cannot rule out the possibility that there is something about the hypothesized 'holistic' processing of human faces (e.g. Tanaka & Farah, 1993; Tanaka & Sengco, 1997) that prevented our participants from properly categorizing and verbalizing the mismatch between their original choice and the manipulated outcome. Moreover, while it is clear that lasting judgments of attractiveness for human faces can be made within a split second (Olsson & Marshuetz, 2005; Willis & Todorov, 2006), it is possible that a less constrained procedure would have generated a different result.

For these reasons we were interested in investigating whether the phenomenon of choice blindness would extend to choices made in more naturalistic settings. As we see it, consumer choice is a perfect domain in which to test this paradigm. The modern marketplace is an arena where the tug of explicit and implicit influences on the behavior and opinions of consumers are played out in a particularly fierce manner. Recently, psychologist have weighed in heavily on the side of non-conscious influences on consumer choice, both as a general framework of analysis

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(Chartrand, 2005; Dijksterhuis, Smith, Van Baaren, & Wigboldus, 2005), and with the discovery of various implicit effects, such as those arising from preference fluency (Novemsky, Dhar, Schwarz, & Simonson, 2007), placebo effects of marketing (Irmak, Block, & Fitzsimons, 2005; Shiv, Carmon, & Ariely, 2005), name-letter branding (Brendl, Chattopadhyay, Pelham, & Carvallo, 2005), and from incidental brand exposure in minimal social interactions (Ferraro, Bettman, & Chartrand, 2009). Even the age old claim about subliminal influences on choice behavior has been revitalized in recent developments (Fitzsimons, Chartrand, & Fitzsimons, 2008; Winkielman, Berridge, & Wilbarger, 2005).

At the same time the marketplace is an arena of remarkable vividness and explicitness, where everything is written on the sleeve (or at least in the barcode) of the products on display. In modern societies people not only have a long history of consumption decisions to fall back upon, they also have an enormous repository of symbolic knowledge about the goods available (comparing the average person today to the most knowledgeable 17th century scientist, they probably ought to be considered as *scholars* of consumer brands and products). But not only this, consumers often have firm opinions about marketing and branding of products as such, and they think and reflect about how these factors influences their own decisions. Thus, one cannot deny that there is some validity to traditional forms of consumer surveys based on introspection, and to the methods of multidimensional sensory rating often used by industry researchers (for different perspectives on this debate, see Chartrand, 2005; Dijksterhuis et al., 2005; Schwarz, 2003; Simonson, 2005; Strack, Werth, & Deutsch, 2006; Woodside, 2004).

However, to establish the actual balance between implicit and explicit processes is a truly daunting task. In this context, choice blindness is a particularly interesting method to use, as it pairs explicit choices with implicit changes. As a method of investigation it elevates inert

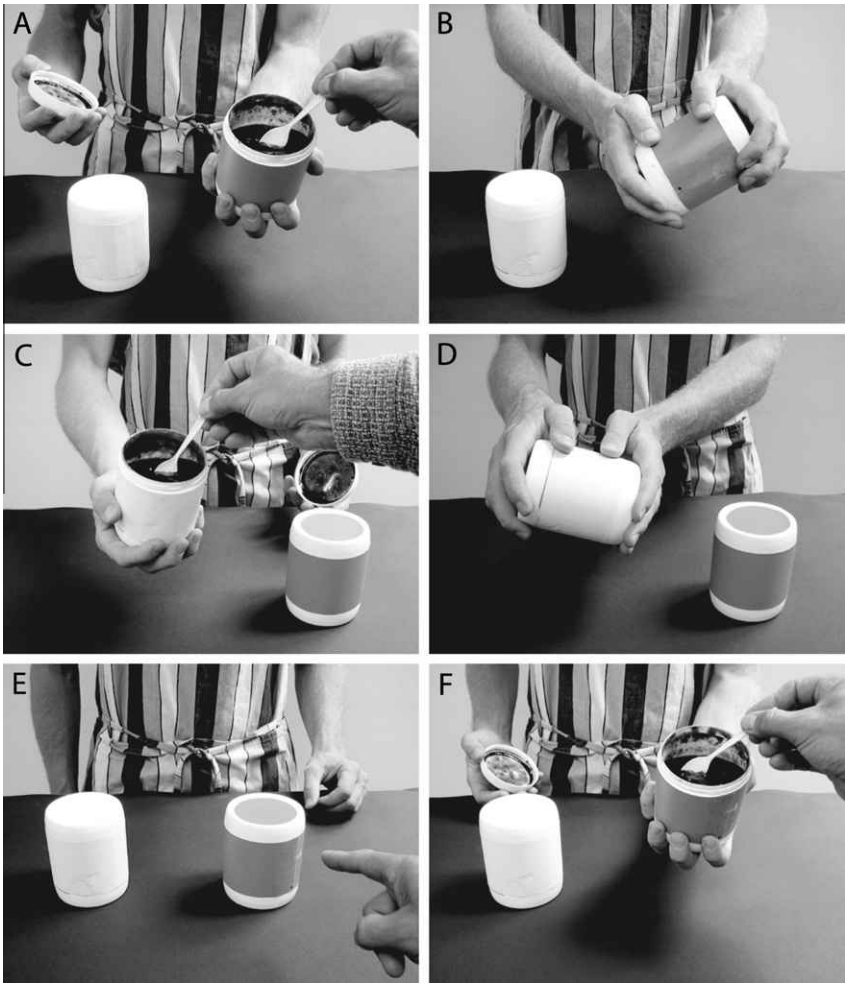

Fig. 1. A step-by-step illustration of a manipulated choice trial in the jam condition. (A) The participants sample the first jam. (B) The experimenter secures the lid back on and flips the jar upside down whilst putting it back on the table. The jar looks normal, but it is lidded at both ends, and with a divider inside, containing one of the included samples at each end. (C) The participants sample the second jam. (D) The experimenter performs the same flipping maneuver for the second 'magical' jar. (E) The participants indicate which jam they prefer. (F) The participants sample the chosen jam a second time, but since the containers have been flipped they now receive the alternative they did not prefer.

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hypothetical statements to powerful covert counterfactuals (i.e. from what do we think would have happened if they had chosen otherwise, to what actually happens when they get what they did not choose). Instead of just focusing on various influences leading up to the point of decision or retrospective judgments of satisfaction, choice blindness concerns the representational details at the moment of deciding, and to what extent we are introspectively aware of these.

To investigate whether choice blindness would extend to modality specific choices between different consumer goods in a naturalistic setting, we set up a sample stand at a local supermarket, where we invited passerby customers to participate in a blind test to compare either the taste of two paired varieties of jam or the fragrance of two paired blends of tea. In a pretest, using a locally available assortment of jam and tea, we composed candidate pairings roughly matched on color and consistency, and allowed an independent group of participants to rate the similarity of the two alternatives in each pair. In the main study we included one pair from the middle of the distribution and the two most dissimilar pairs from the comparison.[1]

In order to create a convincing covert exchange of the chosen samples, we created two sets of 'magical' jars, lidded at both ends, and with a divider inside. These jars thus looked like normal containers, but were designed to hold one variety of jam or tea at each end, and could easily be flipped over to execute a switch (see Fig. 1).

Based on the piloting and our previous studies of choice blindness we expected to find that participants would fail to notice the mismatch in many of the manipulated trials. Given the gap in similarity between the first pair and the other two, we also expected that a higher detection rate would be found for the less similar pairs. As a part of the choice procedure we instructed the participants to rate how much they liked each sampled alternative. We expected to find a relation between the discrepancy of these likeability scores and the level of detection, such that larger rated differences between the two samples would correlate with higher degrees of detection.

Furthermore, we were interested in studying the effect of incentives on the level of choice blindness. To this effect, half of the participants were offered the chosen sample (either a jar of jam or a package of tea) as a gift to bring home after the completion of the study. We expected that the provision of this incentive would motivate the participants further and increase their attention to the decision process (Hertwig & Ortmann, 2001, 2003), which in turn would lead to a higher rate of detection for the manipulated gift trials. In addition, our setup permitted us to investigate possible indirect influences of the manipulated choices on subsequent behavior. After the participants had made their selection we asked them to rate how difficult they felt it was to tell the two samples apart, and how confident they were about the choice they had just made. We reasoned that the second tasting of the manipulated sample might distort the original memory of the discrepancy of the two options, and that the participants would indicate that they found it more difficult to tell the two samples apart in the manipulated trials than in the control trials. Similarly, we hypothesized that if the participants had any lingering doubts from the experience of the manipulation, this ought to reveal itself as a lowered confidence in the choice.

2. Method

2.1. Participants

A total of 180 consumers (118 female) at a supermarket in Lund, Sweden, participated in the study (three participants were removed due to recording problems). The age of the participants ranged from 16 to 80 years (mean = 40.2; std = 20.0). They were recruited as they passed by a tasting venue we had set up in the store. We presented ourselves as being independent consultants contracted to survey the quality of the jam and tea assortment in the shop. The sample stand was located in one of the outer aisles in the beverage section of the supermarket, sufficiently close to the jam and tea shelves to support our role as surveyors, but removed from any potential contamination from exposure to actual product labels, and in a zone with neutral odors, and only moderate noise and flow of traffic. All participants were naïve to the actual purpose of the study. After the study, they gave their written consent to be included in the analysis. The study was approved by the Regional Swedish Ethics Board in Lund.

2.2. Materials

As stimulus material, we used three pairs of jam and three pairs of tea. The pairs were selected from a pretest in which independent participants rated the similarity of eight pairs of jam and seven pairs of tea, for taste and smell respectively. The scale ranged from 1 (very different) to 10 (very similar). To isolate the dimension of interest (taste for jam, and smell for tea) the pairs were roughly matched with regard to color and consistency by the experimenters. The average rated similarity for the included pairs ranged from 4.05 to 6.55 for the jam, and from 3.25 to 6.4 for the tea. As pilot testing indicated very low levels of detection for the more similar pairs for both tea and jam, in the main study we chose to include one pair from the middle of the distribution, and the two most dissimilar pairs from the match up. For jam the chosen pairs were *Black Currant* vs. *Blueberry* (mean = 5.1; std = 2.5), *Ginger vs. Lime* (mean = 4.1; std = 2.2), and *Cinnamon-Apple vs. Grapefruit* (mean = 4.0; std = 2.7). For tea the chosen pairs were *Apple Pie vs. Honey* (mean = 4.7; std = 2.4), *Caramel & Cream vs.*

[1] We strived to create the most dissimilar product pair matching possible based on the local market selection. This relative comparison would necessarily differ across cultural contexts. Wilson and Schooler (1991) investigated the effects of introspecting about reasons for choosing different brands of jam, and their selection of samples was based on an American Consumer Report study comparing no less than 45 different types of strawberry jam alone. Similarly, a study by Iyengar and Lepper (2000) investigated whether the amount of choice alternatives would affect subsequent purchase decisions for jam, and this study was conducted at an upscale Californian supermarket which carried more than 300 varieties of jam.

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Cinnamon (mean = 3.6; std = 1.8), *Pernod (Anise/Liquorice) vs. Mango* (mean = 3.25; std = 2.5).

For the choice manipulation, two small containers were glued together bottom-to-bottom, creating a single jar with two independent sections with separate screw-on lids. A paper wrapping was then applied over the mid-section to complete the illusion of a single unbroken container (color coded in red and blue to make it easier to distinguish among the alternatives). In each trial two of these containers were used, filled with either two different sorts of jam or tea (i.e. each jar was a mirror of the other one, expect for the colored label, and which compartment that was facing upwards at the beginning of the experiment).

2.3. Procedure

The experiment took place at a local supermarket. We recruited the participants by asking them whether they were willing to take part in a 'quality control' test of the jam and tea assortment at the store. At the start of the experiment we informed the participants that the test was to be done with the product labels removed, focusing only on the taste of the jam, and the smell of the tea, and that they should indicate which sample they preferred the most in each pair. In addition, half of the participants were told that they would receive the chosen package of tea or the chosen jar of jam as a gift at the completion of the test. Two experimenters were present during the test. Experimenter 1 asked questions, took notes, and managed the recording device, while Experimenter 2 conducted the preference test. Each participant completed a total of two trials, one for jam and one for tea. For each participant, either the tea or the jam condition was manipulated. The order of presentation, the type of manipulation, and which pairs of jam or tea that was included was randomized for each participant.

In a manipulated trial, the participants were presented with the two prepared jars. After tasting a spoon of jam from the first jar, or taking in the smell of the tea, they were asked to indicate how much they liked the sample on a 10-point scale from 'not at all good' to 'very good'. We made it clear to the participants beforehand that they could revise their first rating after the second sample, and that we would ask them again after the two samplings which alternative they preferred (thus insuring that the task was conceived of as a direct choice, rather than a 'serial rating' procedure with a derived choice). While Experimenter 1 solicited the preference judgment, and interacted with the participants, Experimenter 2 screwed the lid back on the container that was used, and surreptitiously turned it upside down. After the participants had indicated how much they preferred the first option, they were offered the second sample, and once again rated how much they liked it. As with the first sample, Experimenter 2 covertly flipped the jar upside down while returning it to the table. Immediately after the participants completed their second rating, we then asked which alternative they preferred, and asked them to sample it a second time (for those trials in which equal ratings had been given, the participants were encouraged to deliberate again, and pick one alternative), and to verbally motivate why they liked this jam or tea better than the other one. As both jars had been turned upside down during the prior sampling, and the upper compartments thus were reversed, the participants were now given the opposite of what they actually chose. After the participants had finished the third (manipulated) sample, and explained their choice, they were asked to indicate on a 10-point scale how difficult they felt it was to discriminate between the two alternatives (from 'very difficult' to 'very easy'). Finally, they were asked to indicate on a 10-point scale how confident they were in their choice (from 'very unsure' to 'very certain').

The same procedure was used for the non-manipulated (NM) trials, with the only difference that in the NM trials no jars were turned. For each pair of jam or tea tested, 30 M and 30 NM trials were collected.

After the participant had completed both a jam and a tea pairing, we asked them whether they had felt that anything was odd or unusual with the setup of the tasting session, or with the sampled alternatives. This was done to see whether the participants would spontaneously indicate that some form of change or mismatch had taken place. After this, the participants were debriefed about the true nature of the experiment, and they were again given an opportunity to indicate whether they had registered or suspected that we had manipulated the choice alternatives. The experiment lasted between 5 and 10 min. Between each participant there was a natural reset of about 5 min, during which Experimenter 1 moved the participants out of earshot of the stand to conduct the interview and debriefing, and Experimenter 2 processed the notes and prepared the containers and jars. When people approached the stand and asked if they could try the jams and teas when the experiment was not ready, Experimenter 2 just let them taste or smell the two options and asked them which one they preferred, but without doing any manipulations, and without including their results in the test. This guaranteed that potential participants would not be able to observe the experiment beforehand, and thus influence their decision processes.

We used three different criteria of detection for the manipulation trials. A manipulated trial was classified as a *concurrent* detection if the participants voiced any concerns immediately after tasting or smelling the manipulated jam or tea. A manipulation trial was classified as a *retrospective* detection if the participants at the end of the experiment (either before or after the debriefing) claimed to have noticed the manipulation. Finally, as a more implicit form of detection, even if the participants did not consciously report that something went wrong with their choice, we registered whether they for any reason described the taste or the smell of the chosen sample as somehow being different the second time around (i.e. tasting/smelling stronger, weaker, sweeter, etc., as determined by consensus agreement between two experimenters listening to the recorded experiment trials). We call this final category a *sensory-change* detection. When a concurrent detection was made, the participants were offered the correct sample, and the experiment continued, but the subsequent measures (confidence and discrimination) from these participants were not used in the final analysis.

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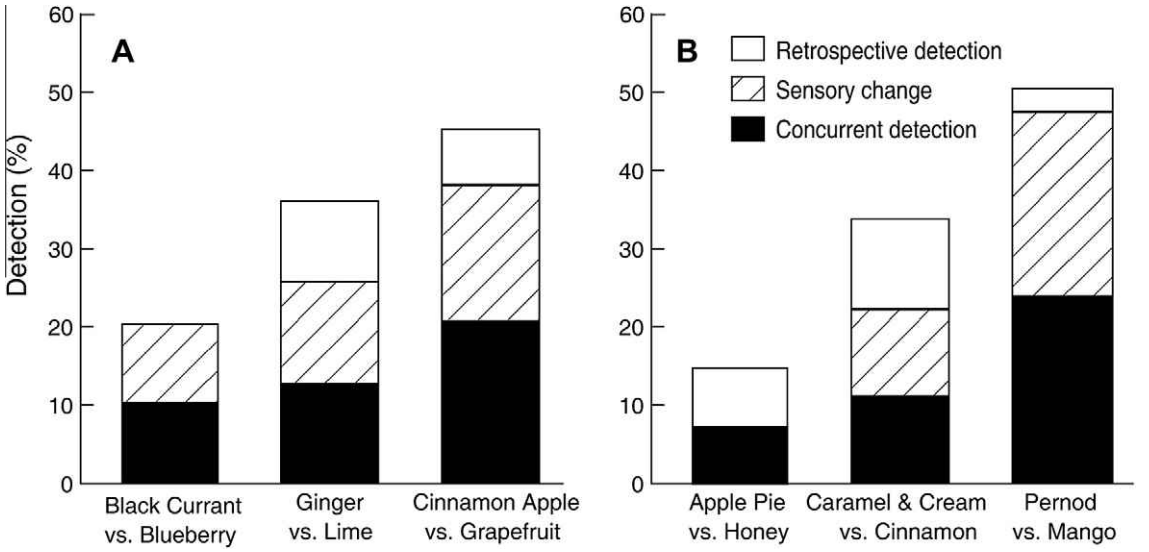

Fig. 2. The data is divided into detection type (retrospective detection, sensory-change, concurrent detection), pair (three stimuli pairs), and modality (A for jam and B for tea).

3. Results

Counting across all pairs, no more than 14.4% of the jam trials and 13.8% of the tea trials were detected concurrently. An additional 6.2% of the jam and 6.9% of the tea trials were detected retrospectively, and 12.4% of the jam and 11.5% of the tea trials were registered as a sensory-change type of detection. In total, 33.3% of the manipulated jam trials, and 32.2% of the manipulated tea trials were detected. We found significant differences in detection rate between the most and least similar jam pairs ($\chi^2_1 = 4.16$, $p < .05$, $w = 0.28$), and between the most and the least similar tea pairs ($\chi^2_1 = 8.85$, $p < 0.005$, $w = 0.45$), but no differences were found between any of the other pairs of jam and tea (see Fig. 2).

We found that the rated discrepancy of preference within a pair was higher for detected manipulated jam trials ($F(1, 84) = 5.08$, $p < .05$, $\eta^2 = 0.057$) compared to the undetected trials; however this was not true for detected tea trials ($F(1, 81) = 0.086$, $p > .7$). Contrary to our prediction, the participants that received the gift incentive had a lower detection rate (19.6%) than participants not receiving a gift (46.3%) in the tea condition ($\chi^2_1 = 7.12$, $p < .01$, $w = 0.38$), but no difference was found for the jam condition ($\chi^2_1 = 0.2$, $p > .6$). There were no differences in the perceived ease of distinguishing between the two samples when comparing the NM trials with the non-detection M trials for neither jam ($F(1, 139) = 2.57$, $p > .1$) nor tea ($F(1, 142) = 3.79$, $p > .05$). Similarly, there were no differences in rated confidence between the NM trials and the non-detected M-trials for neither jam ($F(1, 137) = 0.33$, $p > .5$) nor tea ($F(1, 140) = 1.79$, $p > .1$).The overall rating (NM trials and non-detected M trials, 10-point scale) for the perceived ease of distinguishing between the two samples was notably high for both jam (mean = 7.3; std = 2.4) and tea (8.0; std = 2.5). The overall confidence rating followed a very similar pattern, for both jam (mean = 8.0; std = 2.5) and tea (8.0; std = 2.2). There was no order effects on any of the measures used as a result of the participants choosing the first or second sample when evaluating the products.[2]

4. Discussion

In line with our main hypothesis, the results showed that no more than a third of all manipulation trials were detected by the participants. Thus, in the great majority of trials they were blind to the mismatch between the intended and the actual outcome of their choice, and instead believed that the taste or smell they experienced in their final sample corresponded to their initial choice. Moreover, in two thirds of the trials we classified as detected the participants showed no conscious reaction at the moment they received the manipulated outcome. Instead, they either made the claim at the end of the study that they had felt something was amiss about the situation, or they reported a sensory-change without realizing that the product they were experiencing was not the one they previously preferred. Even for such remarkably different tastes as spicy Cinnamon-Apple and bitter Grapefruit, or for the sweet smell of Mango and the pungent Pernod (that variously evokes associations of liquorice candy, or cough-syrup, or strong aniseed spirits like Absinthe and Ouzo), was no more than a fifth of the manipulation trials detected concurrently, and less than half counting all forms of detection.

But how much trust can we put in this result? Even if the jams and teas we used were classified as clearly dissimilar in a systematic pretest, perhaps some of the participants in the main study failed to notice the change simply

[2] There are three values missing for difference and six values missing for confidence.

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because they could not tell them apart? However, when looking at the post-choice rating of how difficult the participants thought it was to distinguish between the two options, not only do we find no differences between the non-manipulated and the non-detected manipulated trials, the overall rating shows that the participants actually found it very easy to differentiate between the two alternatives (7.3 for jam and 8.0 for tea on a 10-point scale). Indeed, it is precisely this aspect that makes the result so interesting. In conditions that are ecologically relevant for human decision making, despite clearly being able to detect the difference between the two samples, many participants fail to notice that they receive the non-preferred option.

Alternatively, it could perhaps be argued that the participants did notice the manipulation but for some reason refrained from telling us? We can address this issue both by looking at the explicit social and cultural context of the verbal reports, and the various implicit measures included in the study. Firstly, people are well acquainted with the context of product sampling and experiencing a second sample of the same goods as tasting or smelling somewhat different, so no social barriers should exist for reporting an experience of this type (consequently, we also see this reflected in the category of *sensory-change* reports in the study). Secondly, as we revealed the true nature of the experiment to the participants before we explicitly asked them whether they noticed the switch, the most salient pressures of demand would work against the effect, and if anything, generating an over-reporting of the occurrence of detections. More generally, in the current as well as in our previous studies (Johansson, Hall, Sikström, Tärning, & Lind, 2006; Johansson et al., 2005), participants often express strong surprise, even disbelief at times, when being told the true nature of the experiments. We have no reason to believe this reaction is anything but genuine, and it is hard to reconcile with the idea of the participants just pretending that they did not notice the changes made.

If we turn to the implicit measures this position is further strengthened. We found no differences in the rated difficulty to discriminate between the two samples in the non-detected manipulated trials as compared to the non-manipulated trials, and we found no effect of the undetected manipulated trials on the expressed confidence of the participants in their choice. Again, if demand had contributed to the underreporting of detection, then this ought to reflect itself on these non-transparent instruments. The use of confidence measures is exceedingly common in psychological research on decision making, where it is often used as a control or qualifier of the main choice variable (Petrusic & Baranski, 2003; Tunney, 2005), and in our view, it is a noteworthy finding that choice blindness seems to leave so little implicit residue after the mismatched outcome has been presented.

But what does this result mean? Why did the participants fail to notice so many of the mismatches? Do we as consumers not know our preferences better than this? One possible answer is that the participants might have noticed more mismatches if the decision had been of greater importance to them (cf. Moore & Haggard, 2006). This is a reply with intuitive appeal, but it is also severely unconstructive. To make an indictment of the current study based on lack of interest would by necessity cover almost all psychological research on decision making, effectively throwing out all the babies that were not even *in* the bathwater.[3] Obviously, an experimental finding like choice blindness is bound at the limits by choices we know to be of great importance in everyday life. While it lies close at hand to speculate about couples at the altar solemnly affirming their choice of partner, and then (after the minister pulls some unearthly sleight-of-hand!) bringing home a complete stranger, no one would fail to notice such a change (and this, we hope, includes even those involved in the most hasty of Las Vegas marriages). Yet, there is ample territory to explore between the consumer survey of the current study, and the preposterous idea of covert spouse swapping.

Additional evidence on the issue of interest comes from the incentive condition. Incentive manipulations has consistently been shown to increase engagement and attention in research on decision making (Hertwig & Ortmann, 2001, 2003; but see Read, 2005), yet in the current study receiving the preferred product as a gift did not generate an increase in the detection rate, but instead resulted in a lowered level of detection for the tea condition. This indicates that choice blindness can remain robust even in the face of real world consequences.[4]

To fully answer the question of what processes that determine the outcome of mismatch detection in our task we need to look beyond interest. Implicit measures used in experimental psychology typically are not as clean and unambiguous as researchers often like to think they are (De Houwer, 2006; De Houwer & Moors, 2007). Like the parent phenomenon of change blindness, choice blindness is likely to be sensitive to both motivational and attentional factors, to various encoding and retrieval demands, and to the particular nature of the external feedback used (e.g. see Mitroff, Simons, & Franconeri, 2002; Rensink, 2002; Simons & Rensink, 2005). In this study, we found that the degree of choice blindness exhibited by the participants was modulated by the similarity of the choice pairs, with significantly higher rates of detection for the most dissimilar pairs compared to the most similar ones. At present we cannot say whether the increase in detection was driven by enhanced feature matching in the more dissimilar pairs, or perhaps by greater opportunities for labeling and identification for certain varieties of jam and tea. This can be further investigated by including both similarity and ease of labeling in the choice matrix (e.g. *Black Cur-*

[3] In contrast to many experimental paradigms we have tried in the psychological sciences, we find that participants *enthusiastically* partake in the process of evaluating jam and tea, and as is evident from the mean attractiveness scores for the chosen items in the current study (6.8 for jam and 7.1 for tea on a scale from 1 to 10), people really tend to like it (for example, the average European consumer consumes more than 1 k of jam every year, see EU Market Survey: Preserved fruit and vegetables, 2003).

[4] The lower rate of detection in the incentivized tea condition could either stem from participants becoming more 'obedient', less critical, and less willing to report an detection if they receive a gift, or it could evidence the reverse, that some degree of over-reporting was present in the non-incentive conditions. As the critical feature of incentive compatible methods is that they actually encourage participants to be truthful, because doing to maximizes their gains (for example, by receiving the product they prefer, as in our task), we think the latter options is more likely to be true.

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rant vs. Blueberry might be about as similar as *Strawberry vs. Raspberry*, but perhaps not as easy to label and identify).

However, labeling and feature matching cannot represent the full story either. We have recently shown that choice blindness can persist even for choices involving easily identifiable semantic attributes. In this study participants made hypothetical choices between two consumer goods based on lists of positive and negative attributes (e.g. for laptops: low price, short battery-life, etc.), and then we made extensive changes to these attributes which went unnoticed by the participants when they discussed their choice (Johansson et al., in preparation). Similarly, we have demonstrated that choice blindness even can be found for easily quantifiable monetary gambles of the kind typically used in studies of decision making and risk (Kusev et al., in preparation). Feeding the insight from these two studies back into the current setup, our next goal is to try to separate the influence of different factors on choice blindness, by experimentally vary attention (trust), incentives (price), similarity (taste) and attributes (label, package) within the same study, using jam as our model domain.

Several implications follow from the current study. At face value it establishes some very counterintuitive facts about the imprecision of our preferences, and our lack of self-knowledge in consumer decision making (primarily for taste and smell, but with obvious potential for generalization). Previous research has established how expectations can have an impact on taste – i.e. that a cherry flavored beverage is perceived to taste like orange if it is incongruently colored orange (Dubose, Cardello, & Maller, 1980), or that white wine colored pink will taste sweeter (Pangborn & Hansen, 1963; see also Woods, Poliakoff, Lloyd, Dijksterhuis, & Thomas, in press; Zampini, Sanabria, Phillips, & Spence, 2007), but choice blindness goes beyond sensory discrepancies and targets the goals and desires themselves. If participants are willing to accept a reversal of their decision as being what they really wanted, the outcome of a choice cannot be said to simply reveal an underlying preference (cf. Gul & Psendorfer, 2008, this is a point for the economists to ponder in their modeling). The fact that participants often fail to notice mismatches between a taste of Cinnamon-Apple and Grapefruit, or a smell of Mango and Pernod is a result that might cause more than a hiccup in the food industry, which is critically dependent on product discrimination and preference studies to further the trade (Perkins, Forehand, Greenwald, & Maison, 2008; Simonson, Carmon, Dhar, Drolet, & Nowlis, 2001). A single industry commissioned study on consumer perception of something like coloring of apples, or texture of breakfast cereal most likely will involve orders of magnitude more resources than the current experiment, yet the results generated within these ambitious and costly projects are plagued by the same fundamental troubles of introspective report and self-knowledge as simple surveys by high school students are (see Schwarz, 2003).

The interesting thing here is that while the current study helps to demonstrate that there is no Archimedean point from which to observe and measure preferences, it also adds to our arsenal of potential tools to grapple with this problem. Choice blindness is a paradigmatically implicit effect in the sense that participants are unaware of the mismatch (and the manipulation that produced it), yet it must be noted that the implications for the implicit influence camp in consumer research might not be additive, or even supporting. The implicit effects we listed in the introduction have excellent scientific credentials, but they are subtle in expression, and often look more like gentle decisional nudges than salient drivers of consumer choice (see for example Ferraro et al., 2009; Novemsky et al., 2007; Shiv et al., 2005). It is possible to find examples where this indeed is the case, Ariely and Norton (2008) for example, push a model that accounts for how seemingly insignificant, even trivial details of the decision context (such as the use of social security numbers as numerical anchors in a wine auction) sometimes can perpetuate itself across time and situations (what they call "coherent arbitrariness"). Similarly, we have recently shown how choice blindness itself can serve as a powerful feedback mechanism and substantially change the preferences of the participants in a repeated choice paradigm (Hall, Johansson, Tärning, Sikström, & Chater, in preparation, see also Coppin, Delplanque, Cayeux, Porcherot, & Sander, 2010; Egan, Santos, & Bloom, 2007; Sharot, De Martino, & Dolan, 2009 for other recent attempts at disentangling the feedback effects of choice on preferences). But these kinds of demonstrations have been few and far between.

In this context, we are curious whether any researcher would be willing to stand up and predict that the implicit influences we listed in the introduction (priming, fluency, etc.) would generate decisions that offer substantial resistance to choice blindness. If not, this puts them at immediate risk of being swamped or washed out in the competitive consumer landscape.[5] As Simonson (2008) concludes: "while the principles governing context, framing, and task effects may be general, the resulting "preferences" often leave no trace and have little if any effect on subsequent decisions" (p. 157; see also Yoon & Simonson, 2008). In our view, the attempt to trace such longitudinal and cross-contextual effects of various implicit influences, and to decide whether they represents the norm or just curious exceptions, is one of the most urgent and important quests to pursue in consumer research.

In summary, we have demonstrated considerable levels of choice blindness for decisions between samples of jam and tea at a local supermarket. This result extends our previous results (Johansson et al., 2005, 2006) for visual stimuli to the modalities of taste and smell, and establishes the effect in a non-laboratory environment. In our view, this 'wedge' between decision and outcome can be used to estimate the representational detail of the choices people make, and may help to provide a resolution of the current impasse of unconscious or conscious influences in consumer psychology.

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[5] We are grateful to Dan Simons for suggesting this point.

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