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Organic defaults in online-shopping: Immediate effects but no spillover to similar choices

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Abstract

Changing defaults—the preselection that becomes effective without active choice—is becoming a prominent policy tool, after having been proven to be effective in areas as varied as retirement savings, organ donation and product customization. Yet, little is known about how default effects spill over to subsequent similar behaviors. In an online shopping scenario, we found standard default effects on the share of organically produced products in the overall selection of products. These effects did not spill over to subsequent active shopping choices. This was true for defaults that were hard and easy to change (Exp. 1, $N = 90$), for immediate and delayed subsequent choices (Exp. 2, $N = 106$) and for self-selected defaults (Exp. 3, $N = 181$). These findings suggest that the reach and scalability of default manipulations in policy making may be limited, but also speak against the possibility for negative spillover.

1 | INTRODUCTION

In order to address environmental issues, policy makers increasingly rely on behavioral insights to deploy cost-efficient and potentially scalable interventions to encourage pro-environmental behavior (PEB; Abrahamse & Steg, 2013; Barbarossa & De Pelsmacker, 2016; Osbaldiston & Schott, 2012; Prothero et al., 2011). When deciding about interventions, policy makers should ideally know about interventions' immediate and delayed effects on behaviors similar to and different from the target behaviors (Dolan & Galizzi, 2015; Nilsson, Bergquist, & Schultz, 2017; Truelove, Carrico, Weber, Raimi, & Vandenberg, 2014). The most desirable behavior change interventions would be those that trigger the targeted PEB and “spill over” to subsequent behaviors (Stern, 2011; Thøgersen & Crompton, 2009; Whitmarsh & O'Neill, 2010). For example, an intervention that is set up in one supermarket to motivate consumers to buy organic groceries, could spill over and motivate organic choices in a different supermarket or at a later time. Delayed effects, however, have received little scientific attention (Marchiori, Adriaanse, & De Ridder, 2017; Rogers & Frey, 2014). In the current paper, we investigate the impact and

potential spillover of manipulating “defaults” on organic grocery shopping.

Defaults, pre-selected choice options that become effective if the decision maker does not actively specify otherwise, in other words does not “opt out,” have been proven effective and are popular with policy makers (Thaler, 1980; Thaler & Sunstein, 2008). Starting with Samuelson and Zeckhauser's (1988) demonstration of the so-called status-quo bias, default manipulations have been shown to impact major life decisions such as organ donations (Abadie & Gay, 2006; Johnson & Goldstein, 2003), medical decisions (Anshel et al., 2014; Halpern et al., 2013) and pension planning (Choi, Laibson, Madrian, & Metrick, 2002, 2003, 2004; Madrian & Shea, 2001). They have also been shown to influence more mundane consumption decisions such as configurations of computers (Brown & Krishna, 2004), bikes (Herrmann et al., 2011) and cars (Johnson, Hershey, Meszaros, & Kunreuther, 1993; Park, Jun, & MacInnis, 2000). More recently, defaults have been applied to promote PEBs, for example to influence choices for “green” electricity tariffs (Ebeling & Lotz, 2015; Pichert & Katsikopoulos, 2008; Vetter & Kutzner, 2016). In a field study, a vast majority of households that received a green electricity tariff by

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default retained it, even though their previous “grey” tariff was cheaper than the green default option (Pichert & Katsikopoulos, 2008). A meta-analysis (Jachimowicz, Duncan, Weber, & Johnson, 2019) of 58 default studies (pooled $n = 73,675$) speaks for the considerable influence of defaults ($d = 0.68$, 95% confidence interval = 0.53–0.83).

Although growing evidence demonstrates the effectiveness of defaults for encouraging PEBs, these effects have usually been studied for efficiency behaviors, that is infrequent PEBs that are usually costly and have long term consequences, such as choosing a green energy tariff. However, to our knowledge, causal effects of default manipulations have not yet been studied for curtailment behaviors (Karlin et al., 2014), that is low-cost and frequent PEBs such as grocery shopping. Additionally, there is very limited evidence as to whether default effects impact subsequent PEBs.

2 | SPILLOVER EFFECTS

When examining how PEBs spill over, one usually distinguishes between three possible outcomes. Exercising one PEB might increase the probability of performing another PEB (positive spillover; Lanzini & Thøgersen, 2014; Thøgersen, 1999), decrease it (negative spillover, Mazar & Zhong, 2010; Zhong, Strojcek, & Sivanathan, 2010; Geng, Cheng, Tang, Zhou, & Ye, 2016) or leave it unaffected (no spillover, McKenzie-Mohr, Nemiroff, Beers, & Desmarais, 1995; see also Dolan & Galizzi, 2015; Nilsson et al., 2017; Thøgersen & Crompton, 2009; Truelove et al., 2014). Spillover has been investigated from one PEB to similar ones over time and to other PEBs, for example from water to electricity consumption, and across life contexts (Littleford, Ryley, & Firth, 2014; Thøgersen & Ölander, 2003).

For interventions aimed at increasing PEBs, positive as well as some negative spillover effects have been documented (Maki et al., 2019; Truelove et al., 2014). For example, a recent correlational study revealed that consumers who started buying some organic food product(s) tend to subsequently increase the range of organic products they purchase (Juhl, Fenger, & Thøgersen, 2017). Positive spillover to PEBs such as turning off the lights and biking was also induced through verbal feedback and economic incentives to buy organically labeled groceries (Lanzini & Thøgersen, 2014).

Self-perception (Bem, 1972; Freedman & Fraser, 1966) and consistency needs (Festinger, 1957; Festinger & Carlsmith, 1959; Thøgersen, 2004) appear to be partially responsible for positive spillovers. According to self-perception theory, engaging in an initial behavior changes one's self-perception as a certain type of person (e.g., an environmentalist), and this self-perception leads people to act in line with how such a person might behave (Ariely & Norton, 2008). Starting from the seminal demonstration of the foot-in-the-door effect (Burger, 1999; Freedman & Fraser, 1966), empirical research continues to support this explanation. In a study that particularly looked at the effect of self-perception, participants were asked to sign a petition and were subsequently asked to volunteer for a food drive. Individuals, who were labeled as “helpful individuals” by research

confederates after signing the petition, were found to be more likely to volunteer than people in the control group (Burger & Caldwell, 2003). Similarly, positive spillover was found when making participants remember their past PEBs (Conway & Peetz, 2012; Cornelissen, Pandelaere, Warlop, & Dewitte, 2008; Fishbach, Dhar, & Zhang, 2006; Van der Werff, Steg, & Keizer, 2014a, 2014b). The fact that more similar behaviors are more likely to result in positive spillover also supports a consistency needs account (Margetts & Kashima, 2017; Truelove et al., 2014).

It is further evidence for the role of self-perception and consistency needs that pre-existing pro-environmental attitudes and identities seem to moderate spillover effects. For example, it has been found that strong pro-environmental attitudes and identities not only predict PEBs (De Groot & Steg, 2007; Van der Werff et al., 2014a, 2014b; Van der Werff, Steg, & Keizer, 2013a, 2013b; Whitmarsh & O'Neill, 2010), but are also more likely to result in positive spillover following an intervention (Clot, Grolleau, & Ibanez, 2016; Meijers, 2014). Engaging in an initial behavior will make an individual's corresponding attitude about that behavior more salient, especially if the attitude is strong and already chronically activated. The perception of one's attitudes in turn will trigger a motivation to act consistent and lead to an increased likelihood that the individual will engage in other similar behaviors (Cornelissen et al., 2008; Van der Werff et al., 2014a, 2014b). It can thus be assumed that the situational salience as well as the chronic strength of pro-environmental attitudes facilitate positive spillover.

3 | DEFAULT INTERVENTIONS AND SPILLOVER EFFECTS

The self-perception and consistency explanation suggest that attributions of behavior might be a critical moderator of spillover effects (Weiner, 1985). Only to the degree that behaviors are attributed or explained internally, that is being caused by oneself as compared to caused by external demands, should they be relevant to self-perception and consistency. Interventions that are designed to discourage external attributions and encourage internal attributions should therefore be more conducive of positive spillover.

Against the backdrop, instigating positive spillover seems at least possible using default interventions. While the perceptual aspects of defaults are in principle transparent (Bovens, 2009; Hansen & Jespersen, 2013), it has repeatedly been argued that those confronted with them neither routinely perceive their presence nor their effect on behavior (Ivanković & Engelen, 2019; Schmidt, 2017). Additionally, people generally think that they are less influenced by behavior change interventions than others (Junghans, Cheung, & De Ridder, 2015)—a phenomenon called the third person effect (Perloff, 1993)—which has also been shown for defaults (Hummel & Maedche, 2019). For example, a study found that when disclosing a default manipulation for healthier food choices to customers, nearly all customers (87%) responded that it did not affect them (Kroese, Marchiori, & De Ridder, 2015). This suggests that defaulted behaviors

are not attributed externally and might hence lead to positive spillover.

Beyond the default setting itself, additional aspects of the decision context might promote positive spillover. Subtle interventions with minimal or gradual changes to the choice context have been shown to be more conducive of positive spillover. In one demonstration, gradual and weaker changes to thermostat defaults, in steps from 20 to 19°C, showed positive temporal spillover whereas more abrupt and stronger changes, from 20 to 17°C, not only eliminated the effects but even reversed them to yield negative spillover (Brown, Johnstone, Haščič, Vong, & Barascud, 2013). Whereas the subtle change might still have been perceived in line with one's identity and attitudes, the stark change might have led to external attributions and, additionally, to reactance tendencies to counteract the influence attempt (Brehm, 1966). Given the subjective perceptions of defaults discussed above, they should qualify as such; a subtle intervention.

Second, interventions that allow behaviors to be construed more abstractly should be more conducive of positive spillover. More abstract construals elicit a focus on the ultimate purpose of a behavior, whereas concrete construals tend to highlight contextual reasons for it (Liberman, Sagristano, & Trope, 2002; Nussbaum, Trope, & Liberman, 2003). Consequently, it has been argued that abstract construals increase the propensity of a behavior to be attributed internally (Conway & Peetz, 2012). Empirical evidence shows that temporal-distance is a way to increase abstraction and induce positive spillover. In three studies Conway and Peetz (2012) found that recalling one's concrete actions in the recent past leads to compensatory behavior, whereas recalling one's abstract actions in the distant past leads to consistent behavior. Temporal-distance between the manipulated and subsequent behaviors should therefore increase spillover for PEBs.

Third, interventions that can be self-designed seem a promising way to foster internal attributions of the subsequent behaviors and thus promote positive spillover (Hertwig, 2017; Hertwig & Grüne-Yanoff, 2017; Lades, 2014; Reijula & Hertwig, 2020). Giving people the option to reflect on a choice situation increases the salience of one's identity and attitudes (Lades, 2014) and should therefore increase consistency needs and the likelihood of spillover. A qualitative study indicates that people who nudged themselves to receive an organic food box had a stronger pro-environmental self-identity and involvement and bought more organic groceries (Torma, Aschemann-Witzel, & Thøgersen, 2017). Self-selected defaults might thus be a way to induce spillover. In sum, positive spillover seems possible following default interventions, most likely for interventions that "engage" the self, and when they are encountered by people with the existing predisposition to act pro-environmentally.

4 | STUDY OVERVIEW AND HYPOTHESES

In a series of three studies, we investigated default effects and their potential spillover in an online shopping scenario with decisions made between conventionally and organically produced groceries. We

asked participants to compose shopping carts for a specified budget. After the data collection one of the composed shopping carts was randomly selected and awarded to the respective participant as an incentive. To fill the first cart, participants made dichotomous choices with either an organic or a conventional default setting. In order to study possible spillover effects over time, participants filled a second cart based on choices where no default was present, which we will henceforth refer to as "active choice." As additional predictor and potential moderators of spillover effects, we assessed the attitude towards organically produced groceries and its strength.

In Experiment 1, we established the paradigm. We hypothesized that (H1) an organic (conventional) default setting would lead to more organic (conventional) products in the shopping cart composed in the presence of the default. Furthermore, we hypothesized that positive attitudes towards organic products would also manifest in behavior and lead to more selected organic products (H2). Regarding spillover, we assumed that a defaulted behavior would lead to positive spillover in a subsequent choice situation (H3), reflected in a persisting effect of the initial default manipulation for active choices. We also hypothesized that strong pro-environmental attitudes would lead to even more positive spillover following the default manipulation (H4), reflected in an interaction effect between the default and attitudes for the active choices. We also manipulated how blatant the influence attempt was by creating defaults that were either easy or effortful to deselect. Hard to change defaults came with a pop-up window asking for confirmation about the deselection of the default and a short time delay. We expect the blatant version of the default to be perceived as a strong influence attempt, causing external attribution and reduce spillover (H5), reflected in an interaction between default and effort on active choices.

Based on the finding that more abstract and distant past PEBs promote internal attribution and therefore positive spillover, we manipulated the time distance between initial and subsequent choices in Experiment 2. We predicted that increasing the time-distance between composing the initial and subsequent grocery cart would result in more positive spillover (H6).

In Experiment 3, we aimed to directly foster internal attributions of the defaulted behavior by having participants self-select their default. Studying the effects of self-selection, however, requires a strict control for personal preferences as these will be related to the selection itself. We decided to rely on actual behavior, measured in a base-line shop rather than on attitudes for a control. We hypothesized that while controlling for existing preferences, self-selected as compared to other selected defaults would result in more positive spillover (H7).

5 | EXPERIMENT 1

In Experiment 1 we tested the first five hypotheses. For the first set of choices we manipulated the default option (organic vs. conventional) and the effort required (high vs. low) to deselect the default option. Subsequently, we asked participants to make further product

choices in an active choice format. As a manipulation check of the effort manipulation, we included items measuring the perceived threat to freedom in making the choice after each shop.

5.1 | Methods

5.1.1 | Participants

The 90 participants (72 women, 18 men) who took part in the study were between 18 and 32 years of age ($M = 22.4$, $SD = 5.1$), with one outlier aged 62. Nearly all were students. Thirty-five participants were psychology students, the rest were from a variety of other academic backgrounds. Participants took between 8 and 15 min ($M = 11.4$, $SD = 1.6$) to complete the study. The study was run in a laboratory as part of a series of four experiments. The current experiment was the fourth starting approximately 45 min into the session. The study was programmed with SoSci Survey (Leiner, 2014) and conducted via a browser in the laboratory.

5.1.2 | Procedure

Participants were told that they would visit two online shops. They were instructed to compose a shopping cart consisting of 20 products they deemed attractive. They were also informed that they had the chance to win the cart together with the money they did not spend. At the very beginning of the study, we asked participants for their e-mail address and an eight-digit code (consisting of the first two letters/digits of: their mother's name, their father's name, the city they were born in and their date of birth) in order to allow for anonymity while still being able to determine the product choices of the winner of the raffle.

Next, participants received a list of groceries and were asked to select the products they were interested in. In that way we wanted to make product choices more relevant to participants. They had to select 20 products from a wide range of 72 products (e.g., spaghetti, yoghurt, carrots, banana, gherkins, tortilla chips). For easier orientation, items were classified and ordered into six different product categories (cereal products, dairy products and eggs, vegetables, fruits, canned and frozen food and sweets and snacks; see Figure 1). After this initial task, participants were asked to visit two different shops (see Figure 2). In each shops, participant had to decide for each product whether they wanted an organic or a conventional option of that product. The selected products from the list were divided between the two shops.

In the first shop, participants were introduced to defaulted choice sets of half of the products and subsequently to the second shop with active choice sets without a default with the other half. For each shop we created a shop name and a blue and a yellow shop label that was presented at the top, while participants were partaking in the choice task of each shop. Names and labels were counterbalanced across conditions. Details on the choice task will follow below.

After visiting the first shop, participants received feedback via another list about all the products they had chosen in the first shop. Every item was displayed with a small picture and its description, including whether it was organic or not. Furthermore, participants saw the additional amount of money that they would receive (details below). Next, participants were asked to rate their shopping experience. Interspersed with distractor items, we included items measuring participants' perceptions of external influence attempt. The second shop with containing active choice sets was followed by the same procedure: participants received feedback about their chosen products and the additional money, followed by the request to rate the shopping experience. Next, participants were asked to indicate their attitude towards organic groceries and the strength thereof. We placed these questions after the shopping simulation to avoid the non-shopping-related activation of environmental identities. Attitudes measures were unaffected by the experimental manipulations. An analysis of variance (ANOVA) revealed no significant difference between the default conditions $F(1, 86) = 2.76$, $p = .10$, $\eta_p^2 = .03$ or the effort conditions $F(1, 86) = 0.00$, $p = .96$, $\eta_p^2 = .00$. Finally, we collected demographics, thanked, and debriefed participants.

5.1.3 | Purchases of organic products

Ten out of the 20 selected products were presented in each shop. The order of the products was randomized. Products were presented in dichotomous choice sets (see Figure 2). Each choice set consisted of an organic and a conventional option of the same quantity and product type. The positioning of the organic and conventional products (left or right side of the screen) was randomized between, but not within, participants. We used the German organic label that was introduced by the federal ministry of food, agriculture and consumer protection. In the experiment the label was added to every organic unpackaged product. Organic packaged products had the label directly on the packaging. We tried to select the products and their pictures carefully, so they would appear equally attractive. Participants were also presented with the price of the products. Prices were informed by realistic prices from online shops. None of the product prices exceeded four euros. Conventional products were always slightly cheaper than organic products.

By clicking on the "Add to Cart" button, the chosen product was added to a participant's shopping cart, which was then put into the raffle. If participants chose the cheaper conventional product, the price difference was added as a cash payment to participants' potential win. Participants were informed about the process in the instructions. In that way, we wanted to implement the real-life tradeoff between organic products and the savings of a cheaper conventional product.

5.1.4 | Default manipulation

In the first shop (default shop) one option was preselected by default for each product pair. In other words, whenever participants were

FIGURE 1 List of grocery products from which participants could choose. Participants were asked to select at least from three categories and at least two products from chosen categories and a total of 20 products. Products were sorted into six categories. The original material was in German, which was translated for this graphic [Colour figure can be viewed at wileyonlinelibrary.com]

Cereal Products

- crispbread
 - whole wheat toast
 - spaghetti
 - fusilli
 - spaetzle
 - tortelloni
 - wheat flour
 - spelt flour
 - rice for rice pudding
 - long grain rice
 - couscous
 - bulgur
-
- 3

Dairy Products & Eggs

- cream cheese
 - gouda
 - maasdam
 - mozzarella
 - whole milk
 - long-life milk
 - regular quark
 - yoghurt
 - cream
 - buttermilk
 - butter
 - eggs
-
- 4

Vegetables

- cucumber
 - carrot
 - salad
 - cocktail tomatoes
 - avocado
 - red paprika
 - zucchini
 - eggplant
 - mushrooms
 - broccoli
 - onions
 - rocket salad
 - cauliflower
 - potatoes
-
- 5

Fruits

- lemon
 - banana
 - apple
 - kiwi
 - mango
 - orange
 - grapefruit
 - mandarine
 - pear
 - raspberries
 - blueberries
-
- 4

Canned & Frozen Food

- glas gherkins
 - glas beetroot
 - canned corn
 - frozen peas
 - frozen spinach
 - frozen mirepoix
 - frozen raspberries
 - frozen berry mix
 - applesauce
 - canned tomatoes
-
- 0

Sweets & Snacks

- strawberry jam
 - raspberry jam
 - nougat-chocolate spread
 - milk chocolate bar
 - dark chocolate bat
 - tortillas chips
 - seasalt chips
 - peanut flips
 - trail mix
 - pistachios
 - snack-mix
 - chocolate muesli
 - crunchy muesli
-
- 4



confronted with a choice set, one tick box was already selected. Between participants, the default varied to be either organic or conventional. Within participants the default was kept the same for all choice sets.

5.1.5 | Effort manipulation

For half of the participants, we increased the effort to deselect the default, making the default manipulation more blatant. When participants tried to select the non-defaulted product, a pop-up window

would appear, asking participants to confirm the deselection of the product. On confirmation, the pop-up window disappeared only after a 2 s delay. If the participant reconsidered and decided to stay with the default, the pop-up window immediately disappeared. The other half of participants could simply select the non-defaulted product without additional effort, making the default manipulation subtler. In the following we will refer to these conditions as high and low effort condition.

We included a manipulation check for the effort manipulation by measuring participants' perceptions of external influence on their behavior (modeled after Dillard & Shen, 2005, Shen & Dillard, 2005)

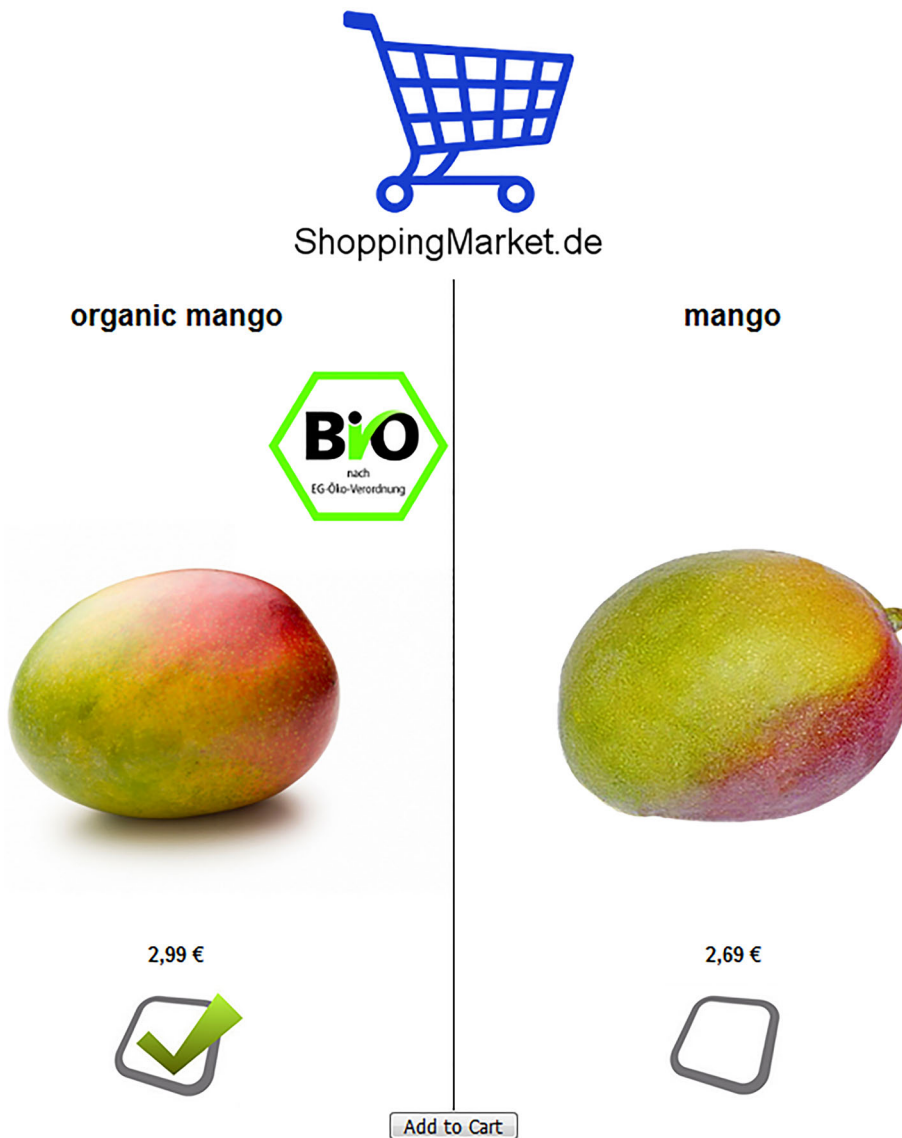


FIGURE 2 Example product presentation: here organic (left) and conventional mango (right). Prices were presented in Euros. The tick mark could be changed by clicking on the tick boxes. Participants could add the product to the shopping cart by clicking on “Add to Cart.” The original material was in German, which was translated for this graphic [Colour figure can be viewed at wileyonlinelibrary.com]

directly after each shop. In 11 items, participants were asked to rate the shopping experience on a 5-point Likert type scale. Four items measured the perception of an external influence attempt: “I was free to decide for myself,” “An attempt was made to make a decision for me,” “Attempts were made to manipulate me,” and “I was put under pressure.” The other seven items were distractor items, referring to the presentation of the products, the user interface or the name of the shop (e.g., “I found the pictures of the products appealing”). Distractor items were not included in the scale and were not analyzed. The scale was highly reliable (first shop: $\alpha = .82$, $M = 2.73$, $SD = 1.02$, $MIN = 1.0$, $MAX = 4.75$ and second shop: $\alpha = .84$, $M = 2.09$, $SD = .92$, $MIN = 1.0$, $MAX = 4.00$).

5.1.6 | Spillover effects of the default manipulation

In the second shop, the remaining 10 products were also presented in choice sets consisting of an organic and conventional option, as in the

first shop, but without preselected ticked boxes. The active choice format was chosen to measure the spillover effect of the default manipulation in the first shop. To measure the effect of the default on choices, we aggregated choices of each shop and used the percentage of organic products people chose in each shop as the main dependent variable.

5.1.7 | Attitude towards organic products

To examine the predisposition to prefer organic products as a possible moderator of the default manipulation, we measured participants' attitudes towards organic products (e.g., Stolz, Stolze, Hamm, Janssen, & Ruto, 2011) and attitudes' strength (e.g., Brannon, Tagler, & Eagly, 2007; Vetter & Kutzner, 2016; Wegener, Downing, Krosnick, & Petty, 1995). The attitude scale consisted of 14 5-point Likert-type scale items. The extremes of the 5-point scale were labeled “I absolutely do not agree” and “I completely agree.” Even though the original

authors proposed the measure to contain five factors, we used it as a one-dimensional measure based on the mean of the 14 items. For easier interpretability, we use a mean score instead of principal component analysis, which proved to be highly reliable ($\alpha = .80$, $M = 3.62$, $SD = .57$, $MIN = 2.29$, $MAX = 4.71$).

We also measured attitude strength (Brannon et al., 2007; Vetter & Kutzner, 2016; Wegener et al., 1995). On a 7-point Likert-type scale participants could indicate the level of *importance* ("How important is the topic of organically produced food?," "How important is the topic to you compared with other issues?"), *certainty* ("How sure are you that your opinion on organically produced food is correct?," "How likely are you to change your opinions about the topic?," "How certain do you feel about your attitude toward the topic?"), *personal relevance* ("How relevant is the topic of organically produced food for you personally?"), *elaboration* ("How much have you thought about the topic before?"), and *subjective knowledge* about the production of organic products ("How well informed are you about the topic?"). Furthermore, *subjective ambivalence* was measured similar to Priester and Petty (1996) with the following three statements: "Concerning the topic of organically produced food, I feel ... no conflict at all/feel no indecision at all/completely one-sided reactions versus ... maximal conflict/maximal indecision/completely mixed reactions." As in previous studies (Brannon et al., 2007; Vetter & Kutzner, 2016), we used a one-dimensional index of attitude strength that we obtained by combining the different self-report measures, performing a principal component analysis, and then combining those items that clearly loaded on the same factor (as indicated by one large eigenvalue and a drop in eigenvalues thereafter, visible in a scree-plot). The attitude strength items measuring different dimensions clearly loaded on one reliable factor ($\alpha = .90$).

5.1.8 | Demographics

Demographic data were measured at the end of the study and included gender, age, first language and occupation. We also assessed participants' food preferences by asking which of the following food types they eat: meat, fish, eggs, milk, grain, vegetables, fruits, canned food, frozen food, sweets, and snacks. Those were just used for exploratory purposes and not analyzed further in this paper.

5.2 | Results

All data analyses were conducted in R (R Core Team, 2020). We analyzed effects with linear mixed-effects models, using the *lmer()* command in the *lme4* package (Bates, Maechler, Bolker, & Walker, 2015). Further analyses were conducted with the *lm()* command. All regression analyses use mean-centered predictors.

5.2.1 | Manipulation checks

At first, we looked at participants' perceived external reasons for their behavior in Shop 1 that contained the default. An independent-

samples *t*-test was conducted to compare perceived external influence attempt in the high-effort and the low-effort conditions. There was a significant difference in the scores for high effort ($M = 3.00$, $SD = 1.04$, $MIN = 1.25$, $MAX = 4.75$) and low effort ($M = 2.48$, $SD = .94$, $MIN = 1.0$, $MAX = 4.5$) conditions; $t(88) = 2.47$, $p = .015$, 95% CI [0.10, .93], Hedges's $g_s = .51$. The results suggest that participants who received a pop-up window whenever they wanted to change the default were more likely to perceive an external reason for their behavior than participants who did not receive a pop-up window.

5.2.2 | Attitude measurement

Attitude and attitude strength were highly correlated, $r = .61$, $p < .001$. We calculated all analyses with both measures and found the same pattern. Since both measures yield similar results, we will just report results with the attitude measure to reduce multicollinearity.

5.2.3 | Default effect

We used a mixed-effects model to predict the percentage of organic products being chosen in each shop. The predictors were the default setting in Shop 1 (organic vs. conventional), the effort manipulation in Shop 1 (low vs. high), the shop (Shop 1: default vs. Shop 2: active choice) and the attitude towards organic products. We modeled random intercepts for every subject and fixed effects for the experimental manipulations.

The analysis revealed a significant effect for the default manipulation, $b = 12.53$, $SE = 4.7$, $t(82) = 2.66$, $p = .009$. Overall, participants who received an organic default chose more organic products than participants who received a conventional default. We also found a significant effect for participants' attitudes, $b = 23.43$, $SE = 4.23$, $t(82) = 5.54$, $p < .001$. Participants with high attitudes towards organic products chose more organic products than participants with low attitudes. There was no interaction between attitudes and the default manipulation, $b = 1.18$, $SE = 8.45$, $t(82) = .14$, $p = .88$. Participants with high attitude scores were equally influenced by the default as were participants with low attitudes.

The analysis revealed no main effect for the effort manipulation, $b = 3.94$, $SE = 4.71$, $t(82) = .84$, $p = .40$ and no interaction between the effort and the default manipulation, $b = -5.65$, $SE = 9.41$, $t(82) = -.60$, $p = .55$. A higher effort to deselect the default did not have an influence on participants' choices. People in the high effort condition did not stay more often with the defaulted option than people in the low effort condition.

Most critical to our study, the analysis revealed a significant interaction between the default and the shop, $b = -18.34$, $SE = 3.79$, $t(82) = -4.84$, $p < .001$. The interaction indicates that the default had a stronger influence in the first shop than (if any) in the second shop with active choice. In other words, participants who received an organic (conventional) default chose more organic (conventional)

products in the first shop, but this effect decreased or even disappeared in the second shop. The mixed-model analysis did not reveal any further significant effects, all t 's < 1.59, particularly, it did not reveal a three-way interaction between the effort manipulation, the default and the shop, $b = 7.61$, $SE = 7.59$, $t(82) = 1.00$, $p = .32$.

To further analyze the interaction between the default and shop, two further linear regressions were calculated corresponding to the plots in Figure 3. The first regression analysis was calculated with percentage of organic products chosen in the first shop as dependent variable, the second regression was run analogously on the second shop. In the first shop, where the default was manipulated, we found an effect of the default, $b = 21.7$, $SE = 4.77$, $t(82) = 4.55$, $p < .001$, and of participants' attitudes, $b = 23.5$, $SE = 4.28$, $t(82) = 5.48$, $p < .001$. Again this analysis confirms for choices in the first shop that participants who were confronted with an organic default, chose more organic products than participants who were confronted with a conventional default. Furthermore, independent of the default the higher a participant's attitude was, the more organic products he or she chose on average. There was no significant interaction between the two, $b = -1.7$, $SE = 8.57$, $t(82) = -0.21$, $p = .837$. The analysis did not reveal any further significant effects, all t 's < 1.05.

In the second shop, again including participants' attitudes, no evidence of an effect of the default was found, $b = 3.35$, $SE = 5.36$, $t(82) = .63$, $p = .53$, while participants' attitudes still affected the percentage of organic products chosen, $b = 23.5$, $SE = 4.28$, $t(82) = 5.48$, $p < .001$. In other words, this analysis in addition to the mixed-effect model clarifies that the effect of the default did not only decrease in the second shop, but disappeared. There was also no significant interaction between the default manipulation and attitudes, $b = 4.14$, $SE = 9.62$, $t(82) = 0.43$, $p = .668$. The analysis did not reveal any further effects, all t 's < 0.89.

5.3 | Discussion

As in previous studies, participants' decisions were influenced by the presence of a default manipulation (Johnson & Goldstein, 2003; Pichert & Katsikopoulos, 2008) confirming H1. Supporting H2, choices were predicted by attitudes. Replicating previous studies, the default manipulation did not interact with attitudes. It influenced participants' choices regardless of whether they held clearly positive attitudes, here, towards organic products, or not (Vetter & Kutzner, 2016). Furthermore, in line with the meta-analysis on defaults (Jachimowicz et al., 2019), higher effort to deselect the default did not strengthen the default effect.

With regards to spillover, as soon as the default was replaced with an active choice format in a subsequent shop, only participants' attitudes predicted their choices. The default manipulation did not spill over (H3). This was true regardless of whether the default was encountered by participants holding positive attitudes towards organic groceries or not (H4). This is noteworthy given the already left-skewed distribution of mainly positive attitudes in the investigated population. Since we did not find evidence for positive spillover, the increased effort to deselect the default, which made the influence attempt more obvious, could not further reduce the spillover effects (H5).

In the next two experiments, we investigated moderators that should increase the chance of finding positive spillover effects. We first focused on temporal distance. Conway and Peetz (2012) showed that recent moral or immoral behavior can lead to compensatory behavior, whereas temporally distant behavior can lead to consistent behavior. The authors argue that time distance will lead to more abstract representations of the former behavior and activate identity concerns that trigger commitment and amplify positive spillover

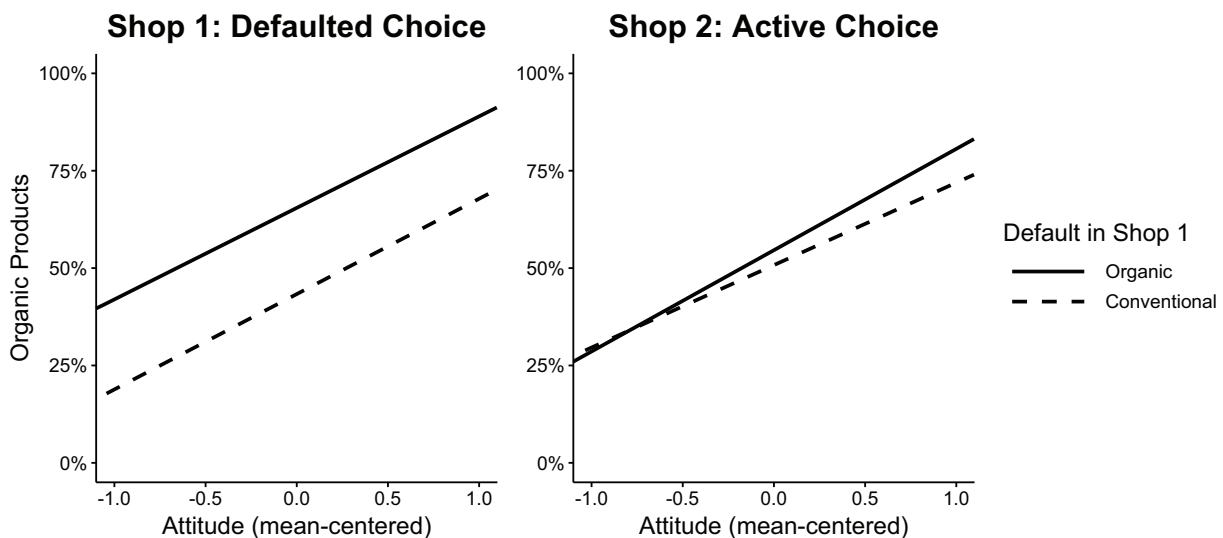


FIGURE 3 Regression lines for the first shop with a default and the second shop with an active-choice format as a function of the default manipulation in the first shop. The regression plots show the relationship between the percentage of organic products participants chose and their attitudes as a function of the default manipulation in the first shop. The lines represent regression lines for each condition

tendencies. In Experiment 2, we therefore introduce a time delay between the first and second shop.

6 | EXPERIMENT 2

In Experiment 2, we again test the hypotheses that an organic (conventional) default setting would lead to more organic (conventional) product choices (H1), that attitudes influence choices (H2), that defaulted choices lead to positive spillover (H3) and that whether pro-environmental attitudes will strengthen positive spillover (H4). Additionally, we now test the hypothesis (H6) that an increased time-distance between an initial behavior that is manipulated by a default and a subsequent behavior strengthens positive spillover. A temporal distant behavior should be processed as an abstract representation and has been shown to foster consistent behavior (Conway & Peetz, 2012). A more abstract representation of the defaulted choices in the first shop should increase spillover to the active choices in the second shop. We test this by manipulating the time delay between the first shop and second shop in addition to manipulating the default (organic vs. conventional).

6.1 | Methods

6.1.1 | Participants

The study was programmed based on the first experiment with SoSci Survey (Leiner, 2014) and conducted via a browser in the laboratory. One hundred and six people (88 women, 18 men) participated in the study. Participants were between 18 and 47 years of age ($M = 23.1$, $SD = 4.4$). Nearly all of them were students, of whom 32 participants had a background in psychology. The study was run in a laboratory as part of a series of four experiments. The current experiment was the second, starting approximately 10 min into the session. Participants took 11 min and 34 s on average ($SD = 3.8$) in the condition with no time delay, and 19 min and 26 s on average ($SD = 10.5$) in the condition with the time delay to complete the whole study.

6.1.2 | Procedure

The procedure was similar to Experiment 1. One difference to the first experiment was that instead of 20 products, participants could now select 40 products (worth approximately 100€), half of which were presented in each shop (compare Figure 1). Doubling the number of products, we aimed at increasing reliability of the choice measure and, by increasing the value of the basket to be won, to strengthen the incentive for participating. After visiting each shop, participants again received feedback about the products they selected. Instead of the pictures of all chosen products, participants saw a pie chart indicating their share of organic and conventional products. The pie chart was presented after decisions had been made in each shop. By presenting

a pie chart between shops instead of a list, we wanted to make choices of the two categories (organic vs. conventional) more salient before participants would enter the second shop. Attitudes measures were again unaffected by the experimental manipulations. An ANOVA revealed no significant difference for attitudes between default conditions $F(1, 102) = 2.12$, $p = .14$, $\eta_p^2 = .02$) and time distance conditions $F(1, 102) = 1.42$, $p = .24$, $\eta_p^2 = .01$.

6.1.3 | Default manipulation

Once again, participants were confronted with a defaulted choice format in the first shop. Half of the participants randomly received an organic default, whereas the other half received a conventional default.

6.1.4 | Time distance between default shop and active choice shop

Before they were asked to visit the active choice shop, half of the participants engaged in an unrelated experiment on motivated reasoning. The experiment lasted approximately 6 min, on average ($M = 6.1$, $SD = 2.4$). The other half of the participants were asked to visit the active choice shop right after they had visited the default shop.

6.1.5 | Spillover effects of the default manipulation

The second shop was equivalent to the second shop in the first experiment. The second half of the products was again presented in an active choice format and participants had to select either the organic or the conventional product.

6.2 | Measures

We measured attitudes towards organic products and attitude strength similar to the first experiment. The attitude score was again constructed as a mean score based on the 14 items by Stolz, Stolze, Hamm, Janssen and Ruto (2011, $\alpha = .77$, $M = 3.55$, $SD = .55$, $MIN = 2.29$, $MAX = 4.79$). The strength score combined by principal component analysis was based on 11 items ($\alpha = .87$; e.g., Brannon et al., 2007; Vetter & Kutzner, 2016). Again, demographics were measured in the end of the study and included gender, age, first language, occupation, and food preferences.

6.3 | Results

All data analysis was conducted in R (R Development Core Team, 2020). We analyzed effects with linear mixed-effects models,

using the *lmer()* command in the *lme4* package (Bates et al., 2015). Further analyses were conducted with the *lm()* command. All regression analyses use mean-centered predictors.

6.3.1 | Attitude measurement

Attitude and attitude strength were highly correlated, $r = .75$, $p < .001$. We again calculated all analyses with both measures, and because both measures yielded similar results we will report only results with the attitude measure to reduce multicollinearity.

6.3.2 | Default effect

Again, we first calculated a mixed-effects model to predict the percentage of organic products chosen in each shop as criterion based on the default (organic vs. conventional) manipulation in the first shop and the time distance (no delay vs. delay) manipulation between the shops, the shops itself (shop 1: default vs. shop 2: active choice) and attitude. We modeled random intercepts for every subject and fixed effect for the experimental manipulations.

The effect for the default manipulation was marginally significant overall, $b = 7.66$, $SE = 4.30$, $t(98) = 1.78$, $p = .08$. There was a tendency for participants who received an organic default to choose more organic products than participants who received a conventional default. Like in the first experiment we found a significant effect for participants' attitudes, $b = 28.55$, $SE = 3.95$, $t(98) = 7.23$, $p < .001$. The more positively participants evaluated organic products the more organic products they chose. There was no interaction between attitudes and the default manipulation, $b = .93$, $SE = 7.9$, $t(98) = .12$, $p = .91$. Participants with positive attitudes were equally influenced

by the default as were participants with less positive attitudes. The analysis revealed no main effect for the distance manipulation, $b = .61$, $SE = 4.30$, $t(98) = -.51$, $p = .61$ and no interaction between the distance and the default manipulation, $b = 8.99$, $SE = 8.61$, $t(98) = 1.05$, $p = .30$, and no three-way interaction between default, shop and distance, $b = -1.22$, $SE = 85.06$, $t(98) = -.24$, $p = .81$. Replicating the results of the first study, the analysis revealed a significant interaction between the default and the shop, $b = -6.22$, $SE = 2.53$, $t(98) = 1.78$, $p = .016$. This indicates that the default had an influence in the first shop, but again might not have had an effect in the second shop with active choice. The mixed-model analysis did not reveal any further significant effects, all $t < 1.61$.

To analyze the interaction between the default and the shop, two further linear regressions were calculated (see Figure 4). In the first shop, the default manipulation, $b = 10.04$, $SE = 4.39$, $t(102) = 2.29$, $p = .02$, and attitudes predicted choices, $b = 27.26$, $SE = 4.03$, $t(102) = 6.76$, $p < .001$. The interaction between the two was not significant, $b = 2.32$, $SE = 8.07$, $t(102) = 0.29$, $p < .774$. In the second shop with active choices, only attitudes, $b = 29.00$, $SE = 4.13$, $t(98) = 7.02$, $p < .001$, but not the default, $b = 4.54$, $SE = 4.49$, $t(98) = 1.01$, $p = .32$, predicted choices. This analysis clarifies that active choices made in the second shop were unaffected by the default. The interaction between the default and attitudes was also not significant, $b = 1.21$, $SE = 8.27$, $t(102) = 0.15$, $p < .884$. There was no further significant effect, all t 's < 0.93 .

6.4 | Discussion

The second experiment replicated the finding that defaults influenced participants' choices and did so across the whole range of attitudes towards organically produced groceries, providing support for H1 and H2. That is, even clear-cut attitudes did not eliminate the default's influence.

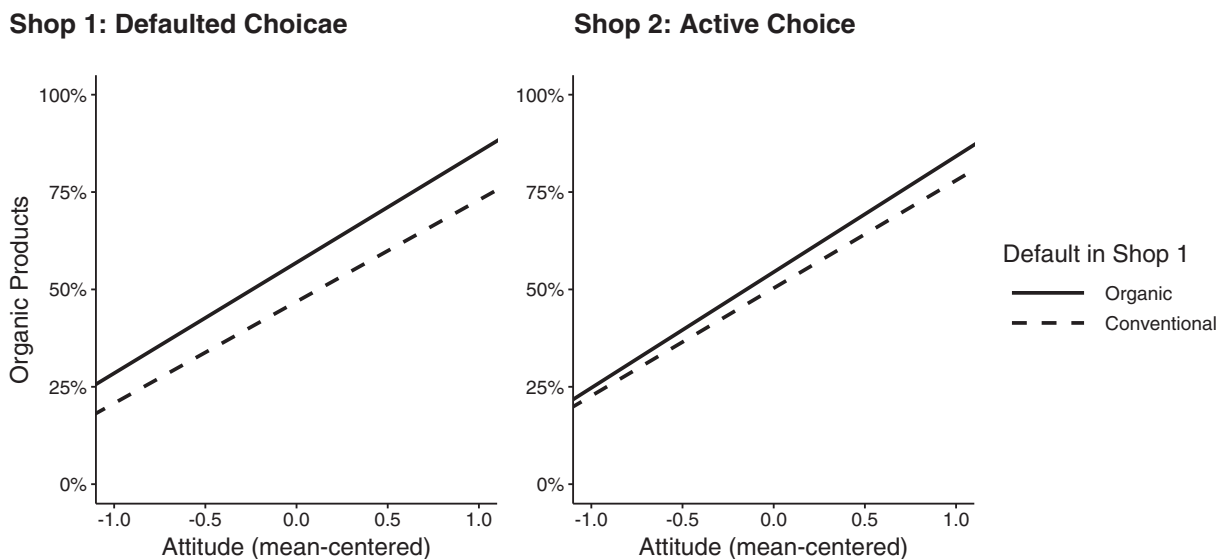


FIGURE 4 Regression lines for the first shop with a default and the second shop with an active-choice format as a function of the default manipulation in the first shop. The regression plots show the relationship between the percentage of organic products participants chose and their attitudes as a function of the default manipulation in the first shop. The lines represent regression lines for each condition

Another finding replicated was the lack of a spillover effect of the default on choices in the second, active choice shop (H3). Only participants' attitudes predicted their active choices, but did not lead to spillover (H4). H6 was neither confirmed, a longer time delay between the defaulted choices and the active choices did not lead to more positive spillover. Independent of the time delay, participants who were defaulted to buy more organic (conventional) products subsequently did not buy more but also not less organic (conventional) products.

In the last experiment, we had participants self-select a default to foster internal attributions of the defaulted behaviors and, thereby, to create positive spillover. We hypothesized that any default, organic or conventional, would create positive spillover when self-selected but not externally provided.

7 | EXPERIMENT 3

In the last experiment we additionally tested for the possibility that self-selected defaults would induce positive spillover (H7). We asked participants to visit three instead of two consecutive shops. In the first shop, participants were confronted with an active choice format. In the second shop, we externally manipulated the default (organic vs. conventional) for half of the participants. The other half of the participants could self-select the default. In the third shop, all participants were confronted with an active choice format again.

As a measure of spillover, we compared choices made in the first and the third shop, thereby controlling for participants' initial preferences. Participants' preferences will partly determine the self-selection of the default. Assessing the spillover effects of this self-selected default, these initial preferences have to be controlled for. We did so by analyzing changes in choices from the first to the third shop. Positive spillover would be evident in participants choosing more organic (conventional) groceries in shop 3 than in shop 1, after having self-selected an organic (conventional) default in shop 2. Such a change can then not be attributed to pre-existing preferences. Thus, including the first shop provided a rigorous, behavior-based control for participants' preferences.

7.1 | Methods

7.1.1 | Participants

The study was conducted via a browser ($N = 103$) in the laboratory and online ($N = 78$) using Prolific (2014). Participants ($N = 181$, 116 female) were between 18 and 66 years of age ($M = 27.22$, $SD = 8.67$), 76% of them were students. Of those, 25 participants were psychology students, the rest were from a variety of other academic fields.

7.1.2 | Procedure

The procedure was similar to the first two experiments. This time, participants had to select 30 products (worth approximately 75€). Next,

participants were asked to visit three consecutive shops. Each shop contained 10 products. For each product participants sequentially had to decide whether they wanted the organic or the conventional product option. The active choice format was implemented in the first and third shop, while the default manipulation was implemented in the second shop. As in the second experiment, participants received feedback on the ratio of selected organic to conventional products after each shop. As in the first experiment we include the measure of perception of external influence attempt after each shop. The rest of the procedure was identical to the previous experiments.

7.1.3 | Default and default source manipulation

The new element in the third experiment was a manipulation of the default source. After participants had visited the first shop, which had an active choice format, half of the participants were asked to state their general preference for organic or conventional products with one question. They were informed that this stated preference would be used as a default to preselect products in the next online shop. In this way participants self-selected the default, which we refer to as the self-selected condition. For the other half of participants in the external default condition, the default was chosen randomly, as was the case in the first two experiments.

7.1.4 | Spillover effects of the default manipulation

By using three shops (first: Active, second: Default, third: Active) we could control for the general preference of participants by calculating difference scores, such that the first shop functioned as a base-line measurement for the subsequent two shops.

7.1.5 | Measures

We measured attitudes towards organic products ($\alpha = .77$, $M = 3.40$, $SD = .58$, $MIN = 1.79$, $MAX = 4.64$) and attitude strength ($\alpha = .87$) with the same instruments as in the previous two experiments. Demographics and food preferences were measured at the end of the study.

7.2 | Results

All data analyses were conducted in R (R Development Core Team, 2020). Analyses of variance were conducted with the *aov()* command.

7.2.1 | Manipulation check

After each shop, we measured participants' perception of being externally influenced. In the first shop with an active choice format, in

which we had not induced any experimental manipulations, the average score was $M = 1.90$ ($SD = .77$). For shop 2, scores were subjected to a two-way ANOVA with two levels of defaults (organic vs. conventional) and two levels of how the default was determined (external vs. self-selected). No effect emerged for the default, organic versus conventional, $F(1, 178) = .021$. However, self-selected defaults were perceived as less of an external influence, $F(1,178) = 46.43$, $p < .001$, $M = 2.23$, $SD = .97$, than externally provided defaults, $M = 3.20$, $SD = .96$. The interaction effect was non-significant, $F(1, 99) = 1.20$, $p = .28$.

7.2.2 | Changes between the shops

For those participants who self-selected the default, the default was confounded with participants' attitudes towards organic products. Attitudes correlated with self-selected default choice, $r(90) = .56$, $p < .01$. Participants with positive attitudes towards organic products were more likely to choose an organic default than participants with less positive attitudes who were more likely to choose a conventional default.

In order to control for individual preferences, we created behavior change scores with positive values indicating an increase in choices for organic products over base-line. The main dependent variables are thus an increase in the percentages of "organic" choices between the defaulted choices and active choices in the first shop, measuring default effects, and an increase in the percentages between the active choice in the last and the first shop, measuring spillover effects. Figure 5 illustrates the two change scores.

Difference scores were subjected to a 2 (default: conventional vs. organic) \times 2 (mode: self-selected vs. external) ANOVA. Results revealed a significant effect of the default manipulation, $F(1, 177) = 12.18$, $p < .001$, $\eta_p^2 = .061$. In the defaulted shop, compared to the first shop, participants with an organic default chose more organic

products than participants with a conventional default. There was an unpredicted effect of the self-selection, $F(1, 177) = 7.75$, $p < .01$, $\eta_p^2 = .042$, in that participants who self-selected the default, either organic or conventional, chose more organic products than participants who received an external default. The interaction between the default and the source of the manipulation was not significant, $F(1, 177) = .22$.

Results for the difference between the third and first shop revealed no effect of the default manipulation, $F(1, 177) = 1.37$, $p = .243$, $\eta_p^2 = .007$, that is, no spillover. We only found the same unpredicted effect of self-selection, $F(1, 177) = 4.59$, $p = .034$, $\eta_p^2 = .025$. Participants who self-selected the default, independent of their selection, chose more organic products in the third shop compared to the first shop than participants who received an external default. The interaction was not significant, $F(1, 177) = .21$.

7.3 | Discussion

As in the previous experiments, we found clear evidence for a default effect (H1). Organic as compared to conventional defaults increased the share of organic products in the shopping cart over the active choice baseline. In line with the previous experiments, we did not find evidence for spillover (H3). In a third active choice shop, the percentage of organic products did not vary as a function of the default manipulation on the previous shop. Contrary to H7, there was no evidence that self-selecting the default did promote spillover.

8 | GENERAL DISCUSSION

The present findings extend evidence for default effects to curtailment behavior, in this case organic grocery shopping in an online

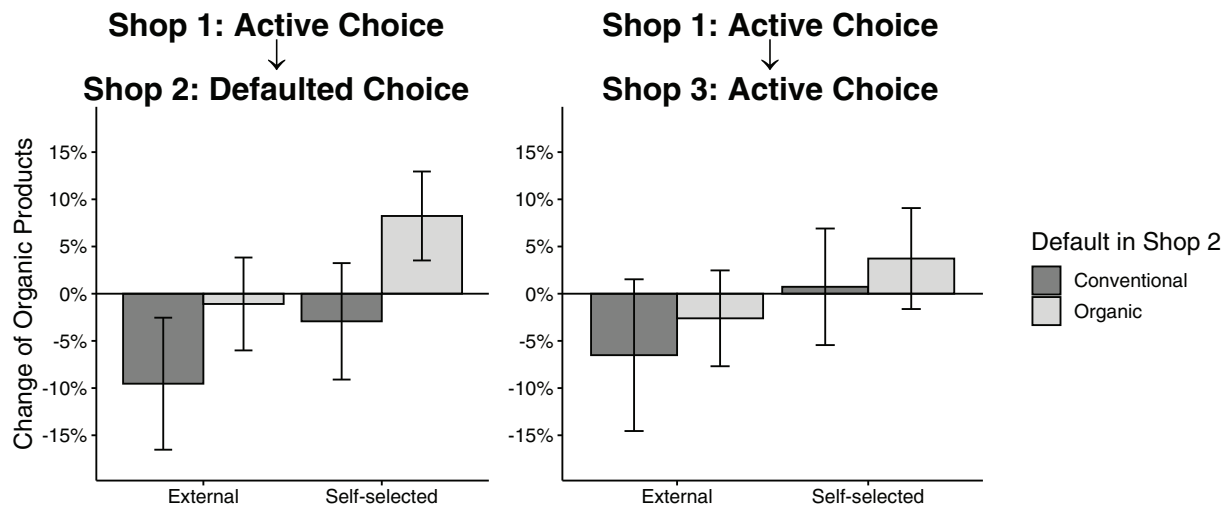


FIGURE 5 Change scores, with positive values indicating an increase in organic choices—defaulted choices in Shop 2 minus baseline active choices in Shop 1 (on the left) and active choices in Shop 3 minus baseline active choices in Shop 1 (on the right)—by the direction of the default and the source of the default. Error bars represent confidence intervals of 95%

shopping scenario. In three studies, shopping-cart composition reflected the default setting to either organically or conventionally produced groceries. We found default effects across all studies (H1) and an effect of attitudes on choices (H2), but no evidence for spillover effects (H3), neither positive nor negative. Active choices that were similar to preceding defaulted ones were unaffected by the default manipulation. This was true for respondents holding positive attitudes towards the choice targets (H4), defaults hard and easy to deselect (H5), immediate and delayed active choices (H6) and for self-selected defaults (H7).

Our findings are in line and extend observational findings of defaults effect for online grocery shopping (Anesbury, Nenycz-Thiel, Dawes, & Kennedy, 2016). The lack of spillover following default interventions converges with recent findings relying on a paradigm from behavioral economics, the dictator game. These studies find default effects on pro-social giving, but effects vanish with the removal of the defaults (D'Adda, Capraro, & Tavoni, 2017; Ghesla, Grieder, & Schmitz, 2019).

For the current studies, a lack of statistical power seems not a viable explanation for not detecting a spillover effect. Based on a meta-analysis of experimental and quasi-experimental studies on spillover (Maki et al., 2019), the effect size to be expected depends on the similarity of the initial and the subsequent behavior. In the meta-analysis behavior pairs such as buying energy efficient light bulbs and buying other energy efficient devices are categorized as highly similar. We assume that our behaviors, shopping for groceries of the same categories, in two similar shops, and from the same shopping list belong to the same category and the choice tasks were highly similar. Expecting an effect size for spillover of $d = .74$ (Maki et al., 2019, p.309) for similar behaviors, the experiments would have detected such a spillover with a probability above 90%, and would have detected effects of $d = .53$ (Exp 1), $d = .49$ (Exp 2) and $d = .37$ (Exp 3) with a probability of 80%.

One explanation for the lack of positive spillover is, however, that the behavior instigated by the default is not internally attributed and hence not perceived as a signal for aspects of the self (Maki et al., 2019). This explanation is compatible with the fact that the effect of the default did not depend on a priori attitudes (for similar findings see Vetter & Kutzner, 2016). It seems as if defaults add a propensity to choose a certain option, independent of the person encountering them. Another reason might be inherent to the two-stage shopping task. Participants might have perceived their tendency to shop organic (or conventional) groceries in the first shop as enough progress towards a sustainability (or savings) goal. In the literature on moral licensing, such a progress focus, as compared to a goal commitment focus, has been shown to compete with positive spillover (Fishbach & Dhar, 2005; Susewind & Hoelzl, 2014). Tasks that activate the overall goal rather than progress towards it should be more conducive of positive spillover.

More research is needed to elaborate on other possible features of a default interventions that might promote positive spillover. For example, it has been argued that the desire to act consistent mainly stems from the motivation to present a coherent image to themselves,

but also to others. When people are inconsistent in their behavior, they risk social sanctions from spectators who see them as hypocritical or two-faced (Abrahamse, Steg, Vlek, & Rothengatter, 2005; Cialdini, 2008; Maslow, 1968; Suh, 2002). In our design participants knew that their choices would be anonymous. Further research could look into, whether disclosing the behavior to others can foster spillover.

The difficulty of the instigated behavior might be another important variable. Looking at the foot-in-the-door effect, where an initial small request is followed by a larger request, the difficulty of the initial behavior in relation to the second behavior might affect consistency needs. Adopting an initially easy PEB may act as a gateway to the adoption of more challenging and potentially impactful behaviors, similar to a "virtuous escalator" (Thøgersen & Crompton, 2009). However, it could also be argued the other way around, that a pro-environmental identity is amplified the more difficult or costly the initial behavior is perceived to be. For example, participants who were forced to donate some of their payment to a charity (costly behavior) were found to be more truthful in a subsequent task than controls (i.e., positive spillover), while those who learned that a similar donation had been given on their behalf, but not deducted from their payment (costless behavior), were less truthful in the subsequent task than controls (i.e., negative spillover, Gneezy, Imas, Brown, Nelson, & Norton, 2012).

Higher perceived distance and prompts to commit to superordinate goals have been shown to promote positive spillover (Fishbach et al., 2006; Fishbach & Dhar, 2005). Yet, in two experiments we did not find evidence for a delay or the self-selection of defaults to promote positive spillover. It seems most plausible that the manipulations did not create the required conditions. The delay of roughly 6 min in the same experimental setting might not have been enough to create the experience of psychological distance. In a real situation, organic shopping unfolds much more gradually, and the "defaults" represented by availability of products in a store usually have prolonged effects on repeated behavior (Juhl et al., 2017). Further studies should investigate other feasible manipulations related to psychological distance, like inducing abstract thinking, which might promote consistency (cf., Conway & Peetz, 2012).

Self-selection of defaults seems like a promising design feature for this purpose. It most likely activated participants' pro-environmental identities by explicitly considering the potential benefits of organic products, and more organic groceries were chosen overall. This effect was independent of the default, organic or conventional, and is in line with the idea that participative choice architecture tools could foster generalizable and lasting behaviors (Hertwig, 2017; Hertwig & Grüne-Yanoff, 2017). However, more research on this matter is needed to understand the boundary conditions and reliability of this effect.

In sum, one might speculate that defaults on their own do not represent an interruption to habits (Axon, 2017; Bamberg, 2006; Verplanken, Walker, Davis, & Jurasek, 2008; Walker, Thomas, & Verplanken, 2015). According to the habit discontinuity hypothesis pro-environmental lifestyle changes are more likely in the context of major habit disruptions, such as those related to lifestyle course changes. A

recent study showed that an intervention promoting sustainable behaviors was more successful for people who recently had moved houses compared to people who had been living at the same place for a while (Verplanken & Roy, 2016). Yet, further research should examine whether defaults might lead to positive spillover in the context of discontinuity.

Thus, defaults might be best suited to influence efficiency behaviors, one-off decisions with lasting impact such as choices of energy tariffs, rather than curtailment behaviors, repeated consumption decisions with immediate impact. These findings however also speak against the possibility of negative spillover for curtailment behaviors following a default intervention. When deciding about the implementation of an intervention this is a valuable insight for policy makers (Dolan & Galizzi, 2015; Nilsson et al., 2017; Truelove et al., 2014). In order to contribute to lasting change in curtailment behaviors, defaults would however need to be in place permanently. PEB that are initially seen as different, and as outside normal behavior, can become mainstream and be accepted as normal through repetition and habit (Dahlstrand & Biel, 1997; Rettie, Burchell, & Barnham, 2014). If that were the case, they might foster the development of pro-environmental social norms or identities in the long-run, which then could lead to positive spillover to other PEBs.

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