CARRYOVER OF DEFAULT EFFECTS: THE INTERPLAY OF NUDGES, PRIOR PREFERENCES, AND EXPERIENCED CHOICE CONSEQUENCES

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ABSTRACT

Evidence for the carryover of default effects is sparse. Most previous studies investigating the downstream effects of defaults have yielded null results or revealed backfire effects that boost subsequent behavior in the opposite direction of earlier defaults. The present work introduces a framework for understanding how the immediate effects of defaults carry over to preference and subsequent choice behavior in alignment with the earlier defaults. Our framework identifies circumstances under which carryover of default effects is likely to occur, proposing that carryover is more likely when (1) defaults are preference-inconsistent, (2) the consequences of nudged choices have been experienced, and (3) those consequences are not aversive. In contrast to prior research, we theorize that, under these enabling conditions, the perception of choice consequences as more favorable than expected increases in incidence and intensity, resulting in amplification of preference updating in favor of the default. Evidence from five experiments (N=7006) in the domain of experiential consumption supports our theorizing. We find that carryover of default effects is attenuated when defaults are preference-consistent and when choice consequences are not experienced through immediate consumption of chosen alternatives. This research helps reconcile past findings and offers a roadmap for choice architects to avoid backfire effects while maximizing the benefits of using default nudges to encourage behavior change.

Keywords:
Choice Architecture, Defaults, Nudges, Default Carryover, Consumer Choice, Preference Updating, Preference Heterogeneity

Marketers and policy makers act as choice architects when they implement default nudges that promote a particular *target* choice or behavior. For example, Walt Disney World has designed defaults to encourage children's healthy eating (Peters et al 2016); defaults are implemented in corporate pension programs to boost retirement savings (Thaler and Benartzi 2004); transportation providers use seat reservation defaults to increase take-up of advanced seat selection (Goldstein et al. 2008); and when a Youtube video ends, another "recommended" video soon begins to play by default, increasing recommendation acceptance. Defaults like these are pervasive in the marketplace. They are powerful nudges that influence immediate choices (Jachimowicz et al. 2019) by assigning one option to automatically apply to a decision maker in the absence of active effort to change it (Johnson et al. 2021). However, the influence of defaults on consumers' target behavior may not be as straightforward as it seems.

Beyond their immediate effects, defaults may have enduring effects that manifest later, when consumers are in different choice environments. For example, will airline passengers who encounter a seat upgrade default be more or less likely to upgrade their seat when they later book another flight with a different airline? Similarly, could being defaulted into watching a fitness video on Youtube affect a viewer's decision about whether or not to watch fitness related videos that later appear in their Facebook feed? Understanding phenomena like these and the downstream effects of defaults more generally, is critical to informing the appropriate use of default choice architecture as a marketing and policy tool. We propose and provide evidence for a framework that explains why and under what conditions default effects are likely to carry over.

Despite a rich body of research concerning defaults and their immediate effects on choice behavior, the downstream effects of defaults have received less attention (Cadario and Chandon 2020; Ghesla, Grieder, and Schmitz 2019; Wisdom, Downs, and Lowenstein 2010). Indeed, the

empirical record is sparse and inconsistent. To date, only limited evidence has emerged that default effects can carry over to influence downstream behavior in a manner aligned with defaults encountered earlier (Putnam-Farr and Riis 2016; Van Rookhuijzen, De Vet, and Adriaanse 2021; Venema, Kroese, and DeRidder 2018). Indeed, the majority of studies yield null results (Ghesla et al. 2019; Kuhn, Ihmels, and Kutzner 2021; Michaelsen et al.2021; Schmidtke et al. 2022; Van Rookhuijzen et al. 2021) or demonstrate backfire effects that boost subsequent choice behavior contrary to the earlier defaults (Donkers et al. 2020; Wisdom et al. 2010). Reconciling these conflicting findings is important to advancing our understanding of how, and under what conditions, default nudges encourage beneficial behavior change that endures.

In this paper, we develop a framework for understanding how the immediate effects of default nudges carry over to produce enduring effects. We adopt the conceptualization of preferences as expectations constructed from prior experiences retrieved from memory (Ariely and Norton 2008; Feldman and Lynch 1988; Johnson, Häubl, and Keinan 2007; Weber and Johnson 2006; Wilson et al. 1989) and we extend this memory-based perspective by drawing on literature concerning the updating of preferences. We highlight the critical role of preference updating following from the experience of the consequences of nudged choices. Perceived incongruity between expectations and favorable consequences that follow from preference-inconsistent choice prompts preference updating in favor of that choice (Festinger 1962; Geers and Lassiter 1999, 2005; Mellers and Ritov 2010; Oliver 1980; Sirgy 1984). We theorize that the immediate effect of preference-inconsistent defaults serves to increase experience of this incongruity, magnifying preference updating in favor of defaulted alternatives.

Following from this reasoning, we propose that carryover of default effects is more likely when (1) defaults are preference-inconsistent, (2) the consequences of nudged choices have been

experienced, and (3) those consequences are not aversive. We theorize that these three enabling conditions together increase the incidence and intensity of the perception of nudged choice consequences as more favorable than expected. Consequently, preference updating in favor of target behavior is amplified. We find support for our framework across five experiments (N = 7,006) showing that carryover of default effects to preference and subsequent choice behavior arises when the proposed enabling conditions hold but is attenuated when defaults are preference-consistent and when the consequences of nudged choices are not experienced.

DOWNSTREAM EFFECTS OF DEFAULT NUDGES

What little is known about how defaults might produce enduring behavior change is not particularly encouraging for choice architects. For one thing, consumers' approval of nudges is inversely related to the effectiveness of the behavioral interventions (Cadario and Chandon 2019), suggesting that consumers might find highly effective defaults very disagreeable. In addition, defaults often increase the effort required to choose a non-default alternative, making them relatively heavy-handed interventions. This feature of a default may make the influence attempt easy for consumers to detect, prompting psychological reactance that shifts preference away from the target behavior (Brehm 1972; Furth-Matzkin and Sunstein 2018; Sunstein 2017). Defaults may also boost licensing (Khan and Dhar 2006), balancing (Dhar and Simonson 1999), and other compensatory effects. Evidence for default inspired licensing is scant in the context of pro-social behaviors (Ghesla et al. 2019; d'Adda, Capraro, and Tavoni 2017), but compensatory effects have been found when defaults are included in sequential product configuration settings (Donkers et al. 2020) and in the context of low-calorie-food defaults (Wisdom et al. 2010).

Finally, as noted above, several studies have revealed no evidence of default carryover (Ghesla et al. 2019; Kuhn et al 2021; Michaelsen et al. 2021; Schmidtke et al. 2022; Van Rookhuijzen et al. 2021). In light of this body of research, choice architects may be justified in thinking that defaults are, at best, only rarely helpful in encouraging enduring behavior change. At worst, choice architects looking at this track record might be concerned that defaults turn consumers' preferences against the target behavior, producing backfire effects downstream.

However, some emergent patterns in the extant findings may reveal opportunities to reconcile inconsistencies and, perhaps, assuage concerns that defaults may be counterproductive in the long run. A close reading of prior work reveals that all studies (as far as we know) in which carryover of default effects has been tested for but has not been observed have one feature in common. That is, participants have not experienced the consequences of their nudged choices before the measurement of downstream effects (d'Adda et al. 2017; Donkers et al. 2020; Ghesla et al. 2019; Kuhn et al. 2021; Michaelsen et al. 2021; Schmidtke et al. 2022; Van Rookhuijzen et al. 2021; Wisdom et al. 2010). By contrast, in all but one of the studies (Donkers et al. 2020, Study 1) that offer evidence for default carryover, participants experienced the consequences of their choices prior to the measurement of downstream effects. In sum, experience of choice consequences distinguishes prior work in which carryover of default effects has been evidenced.

This observation calls for an analysis of how and why the experience of choice consequences might contribute to the process by which default effects carry over. Prior work examining the impact of experience on preference updating emphasizes the role of expectations (Mellers and Ritov 2010; Oliver 1980; Sirgy 1984) and the power of expectation-disconfirmation as a driver of preference updating (Geers and Lassiter 1999, 2005). We build on this work, theorizing that prior preference and the experience of choice consequences interact with defaults

to influence the updating of preferences systematically. In the remainder of this paper, we develop our framework for understanding the dynamic interplay of these factors, present evidence from five experiments, illustrate the value our framework for choice architects, and illuminate conceptual and methodological insights supporting future research into the downstream effects of defaults.

CONCEPTUAL FRAMEWORK

Our framework is grounded in the conceptualization of preferences as memory-based expectations about the consequences of choice (Ariely and Norton 2008; Feldman and Lynch 1988; Weber and Johnson 2006; Wilson et al. 1989) and in the psychology of preference updating (Aronson, 1992; Bem 1967; Donkers et al. 2020; Festinger, 1962; Shlomi and McKenzie 2014; Simonson 2008;). We argue that the downstream effects of defaults are a function of preference updating that arises when consumers make choices and experience the consequences of their choices, theorizing that defaults interact with prior preference to modulate these processes.

Theoretical Concepts and Definitions

Before we present the details of our conceptual framework, it is helpful to define the terminology we use and to distinguish between related concepts. In this paper, we differentiate between immediate effects of defaults and downstream effects of defaults. We define *immediate effects* of defaults as the observed differences in the selection of default options (which we refer to as *targets*), in comparison to the selection of the same options without a default. We define the *downstream effects* of defaults as differences in preference for or choice of target related options

following choices made in default choice architecture, but observed, subsequently, in the absence of default choice architecture. We also differentiate downstream effects of defaults in terms of the directional relationship between the target of a particular default and a particular downstream effect. The terminology we use highlights whether a particular downstream effect is opposed to the earlier target, which we call a *backfire effect,* versus aligned with the target, which we call a *carryover effect.*

We conceptualize *preference* as a function of prior experiences that inform expectations about the consequences of a current choice. In other words, preference is the expression of expectations that represent beliefs, formed based on memory of related experiences, about the subjective utility of consumption objects (Ariely and Norton 2008; Simonson 2008; Weber and Johnson 2006; Wilson et al. 1989). This conceptualization is in line with the treatment of preferences as attitudes toward consumption objects in the psychology literature (Eagly and Chaiken 2007; Fazio 1990). For the present purpose, we define *prior preference* as the relative preference across alternative consumption objects expressed at a single point in time. We define *preference updating* as changes in preference that follow from choice and from the experience of choice consequences. Finally, we differentiate between *downstream effects on preference*, and *downstream effects on choice behavior*, which in economics literature is typically regarded as revealed preference (Beshears et al. 2008). While we examine how downstream effects on preference influence subsequent choice behavior, differentiating between preference and choice is important because choice does not always perfectly reflect preference (Beshears et al. 2008).

Temporal Dynamics of Choice and Experience

It is apparent that, in some contexts, consumers experience the consequences of their choices immediately, or at least without much delay. The consequence of accepting the default

recommended Youtube video, by allowing it to automatically play, is experienced immediately—the video plays. However, default choice architecture is also used to influence behavior in contexts that typically do not lead to the immediate experience of choice consequences. Take for instance, the opt-out default for a pension program encountered by a new employee during the onboarding process. It may be decades before that employee experiences the full financial and lifestyle consequences of their choice. Naturally, the employee makes many more financial decisions in the intervening time. Of course, choice architects also use defaults in contexts that fall somewhere in between the two extremes just illustrated. In such contexts, it is possible, but not necessary, for consumers to make other target related choices between the time of nudged choices and the experience of their consequences.

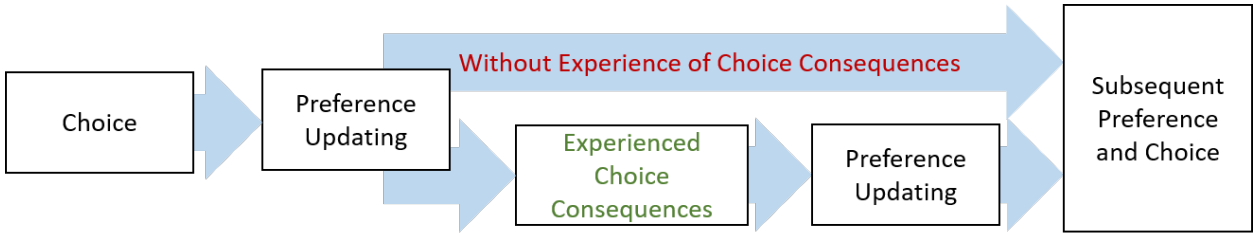
Take, for example, the consumer accepting a default seat upgrade when booking a flight with Airline A. They might book another flight, with Airline B, before or after their flight on Airline A. If the consumer books that second flight before flying in their upgraded seat, a target-related decision—whether or not to choose a seat upgrade on a flight with Airline B—intercedes between the nudged choice and experience of its consequences. Now, suppose the consumer makes this decision after having experienced the extra space and greater comfort afforded by the nudged seat upgrade on Airline A. In that case, experience of those consequences might influence a subsequent seat upgrade decision for the flight on Airline B. If, as we propose, experiencing choice consequences is an enabling condition for default carryover, then it follows that default carryover is more likely in the second case than in the first. It also follows that the defaulted video on Youtube is likely to result in carryover, whereas the typical opt-out default in a pension program is less likely to do so. The temporal relationship between choices, the experience of choice consequences, and downstream effects is central to our framework because, we theorize, the inputs to preference updating that arise following experience of choice

consequences differ from those that arise following choice alone.

The proposal that experiencing choice consequences is an enabling condition for default carryover is built on our conceptualization of preferences as expectations informed by prior experience. This conceptualization implies that preferences are subject to updating whenever new experiences are informative to the formulation of subjective utility beliefs. Figure 1 depicts when preference updating arises in the temporal dynamics of choice and experience. In the next section, we address the mechanisms of preference updating at play when consumers make choices and when they experience the consequences of those choices.

Figure 1

TEMPORAL DYNAMICS OF CHOICE, EXPERIENCED CHOICE CONSEQUENCES, AND PREFERENCE UPDATING



Preference Updating Following Choice Alone

Experience of making a choice can be informative in terms of the subjective component of a utility judgment (i.e., one's attitude toward attributes of an object). Consumers take inferences from their choices to support preference learning through self-perception and self-signaling, whereby inferences about the self are drawn from choice (Amir and Levav 2008; Bem 1967; Bodner and Prelec 2003), and through cognitive dissonance resolution, whereby the shifting of attitudes in favor of a choice reduces discomfort felt when choice is inferred to conflict with prior preference (Festinger 1962). These psychological mechanisms update

preference in favor of the chosen alternative. However, these inference-based preference-updating processes are attenuated when choices are made passively by accepting defaults (Donkers et al 2020). For example, preference updating due to cognitive dissonance is weaker when choices result from omission (vs. commission; Gilovich, Medvec, and Chen 1995), are easily trivialized (Simon, Greenberg, and Brehm 1995), or when denial of responsibility is easily rationalized (Gosling, Denizeau, and Oberlé 2006), all of which are more likely when defaults are accepted. By definition, choices nudged by preference-consistent defaults are not likely to prompt cognitive dissonance in the first place.

Psychological reactance is another mechanism that operates based on inference following choice. When consumers infer an influence attempt from the choice architecture, the perception of a threat to autonomy leads to psychological reactance, which shifts preference to be less favorable toward the nudged alternative (Brehm, 1972). In contrast to dissonance, activation of reactance should not depend on whether defaults are consistent or inconsistent with prior preference. Notably, reactance is more likely to arise in a default choice architecture than in a default-free one, all else being equal, because a default can make the influence attempt obvious.

The operations of four psychological mechanisms support the expectation that preference updating following choice alone results in downstream backfire effects from defaults. Preference updating in favor of nudged alternatives via self-perception, self-signaling, and cognitive dissonance is weakened, compared to when consumers make the same choices in the absence of defaults. Moreover, reactance shifts preference away from default alternatives. However, it is essential to note that such backfire effects are less likely to be observed at the aggregate level when defaults have strong immediate effects. On the one hand, preference updating in favor of a selected target is weaker than if the same alternative is chosen in a default-free architecture. On

the other hand, consumers are more likely to select that alternative when it is the default target, resulting in the aggregation of more instances of (weaker) preference updating in favor of the target. These countervailing forces can mask backfire effects in aggregate. This might help explain some of the null findings in the literature. While our focus in the present research is on conditions conducive to carryover effects, it is important to acknowledge the potential for backfire effects in the absence of experienced choice consequences. We present empirical evidence suggesting that, although not detectable in aggregate, backfire effects following choice alone are detectable when accumulation of preference updating across choices is controlled for.

Preference Updating Following Choice and Experienced Consequences

Psychological research shows that repeated decisions and experiences within a particular choice context can produce relatively stable preference changes (Hoeffler and Ariely 1999). Sociocultural research further demonstrates that preference change occurs through repeated action within contexts that enable the learning and updating of particular tastes. Preferences may change as consumers actively reshape the consumption contexts that structure their tastes (Maciel and Wallendorf 2017), or as external forces create changes to broader cultural or economic contexts (Lieberson 2000; Peterson and Kern 1996). Furthermore, the experience of choice consequences enables preference learning of attribute values (Amir and Levav 2008) in support of the discovery of inherent preferences (Simonson 2008). As such, in addition to informing the subjective component of a utility judgment, experience of choice consequences informs the objective component of a utility judgment through learning about the object of evaluation (i.e., attribute levels). Critically, subjective utility judgments deriving from experience might contribute more to preference updating when they are incongruous with expectations.

The classic consistency theories of self-perception (Bem 1967) and cognitive dissonance

(Festinger1962) offer mechanisms for preference updating that, in addition to being activated following choice, operate in response to experience of choice consequences and are strengthened when experience is perceived as more favorable than expected (Liu et al. 2020). Furthermore, while these inference-based processes are attenuated when choice is made by default acceptance, their operation in response to experienced choice consequences is independent of the choice process leading to the individual experience. These consistency theories converge with principles of reinforcement learning (Vlaev and Dolan 2015), accounts of expectation-disconfirmation (Geers and Lassiter 1999, 2005; Kahneman and Snell 1992: Mellers and Ritov 2010; Oliver 1980; Sirgy 1984), and theories of anticipated emotions and choice (Mellers 2000). Together, these frameworks suggest that perceived incongruity between expectations and favorable consequences that follow from preference-inconsistent choice supports preference updating in favor of that choice.

Action change theory (Vlaev and Dolan 2015) holds that affective responses to appetitive stimuli are integrated with actions to influence future behavior through reinforcement learning. In the context of information technology use, Bhattacherjee and Premkumar (2004) find support for a two-stage model in which attitudes shift as a function of pre-usage beliefs that are disconfirmed by usage stage perceptions. Along similar lines, Shlomi and McKenzie (2006, 2014) proposed a two-stage model of preference updating in which beliefs are updated based on experienced outcomes, which then leads to the updating of preferences based on revised beliefs. When experience is incongruous with expectations, contrast (as opposed to assimilation) processes increase in likelihood with more experience (Geers and Lassiter 2005, 1999; Klaaren, Hodges, and Wilson 1994). Consequently, the increased incidence of experiencing expectation disconfirmation resulting from choices being nudged by preference-inconsistent defaults, should

increase the degree to which the perceived experience is integrated with prior experience to inform the updating of preference.

The role of the immediate effect of defaults. Preference updating may largely be a subtle process, but regardless of the choice architecture in which consumers make choices, their experience of choice consequences should produce some preference updating. Small preference updating effects can accumulate. Thus, differences in choice shares as a function of choice architecture are important because they determine the incidence rate of experience of the target alternative. The immediate effect of defaults on choices should result in increased preference updating in favor of the target as a function of greater experience of the target.

The role of alignment with prior preference. While experience of a non-aversive, preference-consistent target may sometimes be perceived to exceed expectations, such expectation-disconfirmation should be more likely, and should feature greater incongruity between expectation and experience, when the non-aversive target is preference-inconsistent. Furthermore, consumers' tendency to accept a default depends on their prior preference (Johnson and Goldstein 2003), and on the interaction of prior preference with the effort implications of accepting versus rejecting the default (Kaiser et al. 2014). As such, the downstream effects of defaults also depend on the interplay of consumers' prior preference and the choice architecture. Unless preference is extreme, the immediate effect of defaults (especially heavy-handed defaults) tends to be larger when the target is preference-inconsistent. This is not to say that preference updating does not occur in the preference-consistent case, but the relative increase in incidence rate as a function of the default is necessarily lower.

Summary and Hypotheses

To the degree that preference-inconsistent defaults increase the choice of the preference-

inconsistent target, they boost experience of the consequences of those nudged choices. Given that those consequences are favorable, they are likely to produce expectation-disconfirmation that supports the updating of preference in favor of the target. Carryover of default effects to preference results from the increased incidence of this incongruity-based preference updating in favor of the target. In turn, updated preferences influence subsequent choice behavior. When defaults are consistent with prior preference, as opposed to preference-inconsistent, the foregoing dynamics differ in two ways. First, we expect immediate default effects to be smaller, resulting in a lower differential in incidence of experiencing target choice consequences relative to when choices are made in the absence of defaults. Second, perceived incongruity between non-aversive experienced consequences and expectations is less likely, resulting in less preference updating in favor of the target. When choice consequences are not experienced, the mechanisms associated with experienced consequences, and largely responsible for carryover, cannot contribute to the updating of preferences. The flow chart in Figure 2 depicts these proposed relationships.

The foregoing reasoning leads to our proposal that carryover of default effects is more likely when (1) defaults are preference-inconsistent, (2) the consequences of nudged choices have been experienced, and (3) those consequences are not aversive—and that the strength of carryover depends on the magnitude of the immediate effects of the defaults. We derive the following formal hypotheses from our framework, which we tested across five experiments.

Hypothesis 1: Default effects carry over to (a) subsequent choice behavior and (b) preference (b) under specific conditions.

Hypothesis 2: Carryover of default effects is strengthened as the magnitude of the immediate effects of defaults increases.

Hypothesis 3: Carryover of default effects is more likely when the consequences of nudged choices are immediately experienced.

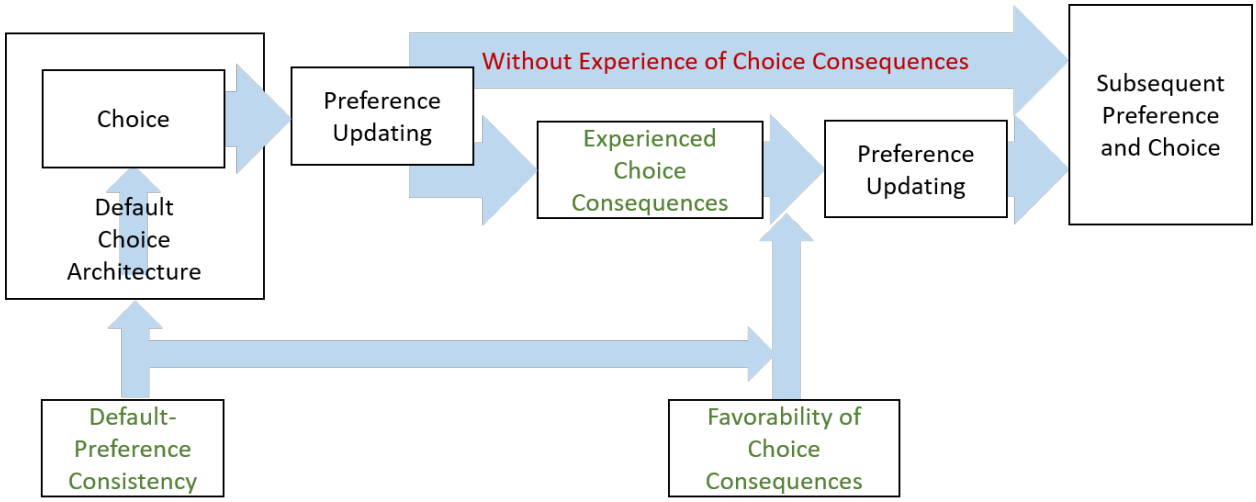
Hypothesis 4: Carryover of default effects is more likely when defaults are inconsistent with prior preference.

Hypothesis 5a: Carryover of default effects to preference is mediated by the experience of target choice consequences.

Hypothesis 5b: Carryover of default effects to subsequent choice behavior is sequentially mediated by the experience of target choice consequences and preference updating.

Figure 2

PROPOSED RELATIONSHIPS BETWEEN DEFAULT CHOICE ARCHITECTURE, DEFAULT-PREFERENCE CONSISTENCY, AND THE FAVORABILITY OF THE CHOICE CONSEQUENCES IN THE TEMPORAL DYNAMICS OF CHOICE, EXPERIENCE, AND PREFERENCE UPDATING



Overview of Experiments

We demonstrate support for our framework in five experiments (N = 7,006) in which participants made decisions with experiential consequences. Experiment 1 illustrates that default effects carry over to subsequent choice behavior when all three enabling conditions hold—specifically, (1) preference-inconsistent defaults (2) followed by experienced outcomes (3) that

are not aversive. Experiment 2 shows that default effects carry over to preference, and offers process evidence linking the immediate default effect and preference updating to effects on subsequent choice behavior. Experiment 3 demonstrates the importance of experiencing the consequences of nudged choices by teasing apart preference updating that occurs when choices are made and their consequences are experienced from preference updating that arises following decision making alone. In Experiment 4, we show that the carryover of default effects to preference that is observed when defaults are preference-inconsistent is attenuated when defaults are consistent with prior preferences. Experiment 5 offers process evidence that alignment of defaults with prior preference moderates the mechanism by which default effects carry over.

EXPERIMENT 1: DEFAULT EFFECTS CARRY OVER TO SUBSEQUENT CHOICE BEHAVIOR

In Experiment 1, we tested for carryover of default effects to subsequent choice behavior in a different, default-free choice environment. Participants chose and watched enjoyable videos and subsequently made a decision about the types of videos to include in a new playlist. All three of the proposed enabling conditions for the carryover of default effects were present in this experiment. Specifically, this experiment featured defaults that encouraged preference-inconsistent choices; those choices led immediately to consumption of the selected alternatives; and those experienced consequences were not aversive (pretests established that the videos were perceived to be enjoyable regardless of prior preference). We manipulated whether a default was absent or present, and if present, the strength of the default. We expected to observe carryover of the default effect to subsequent choice behavior (H1a) and for the strength of the carryover to

depend on the magnitude of the immediate default effect (H2).

Method

We recruited 908 United States residents from Amazon's Mechanical Turk platform who completed the protocol and consented to the use of their data. After consenting to participate, participants reported basic demographic information (age, gender, language of daily use) before proceeding to the preference elicitation stage. In that stage, participants indicated their video genre preferences across three genre pairings, one of which was science videos versus comedy videos, and rated the strength of their preferences on a 7-point scale (anchored by 0 = *slightly prefer* and 6 = *strongly prefer*). Participants were then randomly assigned to one of three choice architecture conditions; default absent, light-handed (LH) default, and heavy-handed (HH) default. In the HH choice architecture the preselected target video was set to automatically play after five seconds unless participants took action to start unlocking the alternative video by clicking on an unlock button. If the unlock button was clicked, the five-second countdown was halted, a play button was displayed for the preselected video, and an unlock code was displayed for the alternate video. If participants entered the unlock code, a play button was displayed for the alternate video. In the LH choice architecture, the preselected target video did not play automatically, and unlocking the alternative video was accomplished by simply clicking on the unlock button, without the need to enter an unlock code (see Web Appendix for a depiction of these conditions).

Participants went through five choice trials, with two video options (one science video and one comedy video) presented in each trial. The preselected video in the default-present choice architectures was always from the video genre inconsistent with participants' prior preference. Participants watched their selected video immediately, before proceeding to the next

choice trial. After five choice trials, we informed participants that a new playlist of videos was to be constructed based on how many comedy and science videos they wanted to include. Participants selected how many of each video genre to include in the new playlist, totaling to five, and this choice served as our dependent variable.

Results and Discussion

To confirm that our choice architecture interventions produced the intended default effects, we compared the choice share for the target (i.e., preference-inconsistent) videos in each of the default-present conditions to their choice share in default-absent condition. To accommodate overdispersion in the data (dispersion = 1.31, Pearson $\chi^2 = 1184.63$), we employed negative binomial regression. Compared to the default-absent condition ($M = 1.25$, $SD = 1.43$), choice share for the preference-inconsistent videos was higher in both the LH ($M = 1.94$, $SD = 1.68$, $b = .43$, $z = 6.61$, $p < .0001$) and HH ($M = 3.08$, $SD = 1.58$, $b = .90$, $z = 14.65$, $p < .0001$) conditions, and was higher in the HH than in the LH condition ($b = .46$, $z = 8.44$, $p < .0001$). See Figure 3 for the nudged and subsequent choice shares.

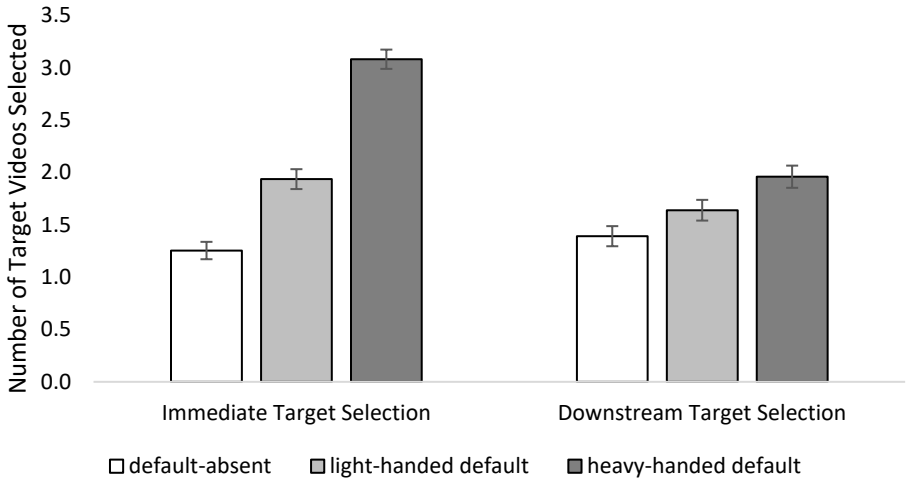
In line with H1a, negative binomial regression (dispersion = 1.87, Pearson $\chi^2 = 1688.27$) revealed that the HH default carried over to later choice behavior, increasing the choice share for preference-inconsistent videos included in a new playlist ($M_{hh} = 1.96$, $SD_{hh} = 1.83$, $M_{absent} = 1.39$, $SD_{absetn} = 1.678$, $b = .34$, $z = 3.45$, $p < .001$). Carryover was smaller and only marginally significant following LH defaults ($M_{lh} = 1.64$, $SD_{lh} = 1.76$, $b = .16$, $z = 1.64$, $p = .100$), and, supporting H2, was marginally lower than in the HH condition ($b = .18$, $z = 1.93$, $p = .053$).

The smaller carryover observed in the LH condition is consistent with our theorizing that default effects carry over by increasing the incidence of experiencing choice consequences that produce preference updating in favor of the target. Given the smaller immediate effect of the LH

default, participants in this condition consumed fewer preference-inconsistent videos and were, therefore, exposed to fewer opportunities to perceive incongruity between their expectations for the target videos and the enjoyable experience of watching them. The results of Experiment 1 demonstrate that, when all three enabling conditions hold, default effects carry over to subsequent choice behavior in a different, default-free choice environment, and this carryover effect is stronger when the immediate effect of the default is larger.

Figure 3

IMMEDIATE AND DOWNSTREAM CHOICE SHARES IN EXPERIMENT 1



Notes: Error bars represent standard errors.

EXPERIMENT 2: PSYCHOLOGICAL MECHANISM AND CARRYOVER TO PREFERENCE

Experiment 1 offered initial evidence that when choice consequences are experienced, defaults that encourage non-aversive, preference-inconsistent behavior shift consumers' subsequent choice behavior to become more favorable toward the target. In Experiment 2, we tested whether default effects carry over to preference (H1b) and examined the links between the

immediate effects of defaults, their influence on preference, and their carryover to subsequent choice behavior (H5a and H5b). Another objective of Experiment 2 was to examine the role of psychological reactance in the downstream effect of defaults. Concerns that reactance to default choice architecture might compromise the benefits of using defaults to encourage behavior change have received some attention (Sunstein 2017) but have not been empirically validated. While we have speculated that reactance is likely to be aroused by defaults, especially when they are embedded in a heavy-handed choice architecture, we think it likely that the impact of reactance on preference updating is diminished when the reactance-inducing situation leads consumers to experience outcomes that positively contradict their expectations. To test these competing conjectures, we included state and trait reactance measures.

Method

We recruited United States residents from Amazon's Mechanical Turk platform, 678 of whom completed the study and consented to the use of their data. The procedure was identical to Experiment 1 up to the end of the choice trials. Following the choice trials, we told participants that they would watch five more videos. They were required to make the consequential choice of how many comedy and science videos, totaling to five, to include in the new playlist. After making this decision, we reminded participants which video genre they had earlier indicated they preferred and asked them how much they prefer that genre over the alternative using the same 7-point preference-strength scale they had used in the preference elicitation stage.

We adapted Dillard and Shen's (2005) procedure to measure state reactance based on the intertwined model of reactance, which holds that reactance consists of the combination of negative affective responses to and anger about a perceived threat to autonomy (Rains 2013). Participants used a 7-point scale (anchored by 0 = *not at all* and 6 = *very much*) to respond to an

anger scale composed of four questions about how they were feeling when choosing from the last pair of videos they had been shown (Did you feel *angry/annoyed/irritated/aggravated* while making the choice?). Next, we asked participants to recall and list the thoughts they had while choosing from the last pair of videos they had been shown. After listing their thoughts (up to 15, with no minimum), participants were shown each of the thoughts they had listed, one at a time, in random order and asked to categorize each of them as being positive, negative, or neutral. Next, participants watched five more comedy and/or science videos in the proportion they had chosen for their new playlist. After watching the videos, participants responded to an 11-item trait reactance scale (Hong and Page 1989; Hong and Faedda 1996; example items: Regulations trigger a sense of resistance in me; Advice and recommendations usually induce me to do just the opposite) using a 7-point scale (anchored by 1 = *strongly disagree* and 7 = *strongly agree*).

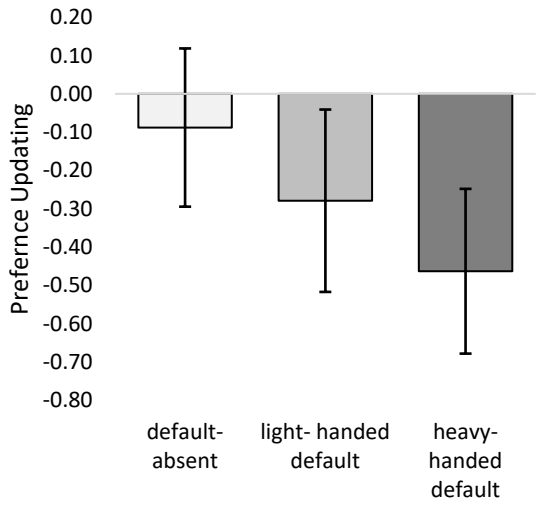
Results and Discussion

Default effects emerged as expected. Poisson regression (dispersion = .97, Pearson χ^2 = 652.82) revealed that, compared to the default-absent condition (M = 1.43, SD = 1.27), choice share for the preference-inconsistent target was higher in the LH (M = 2.11, SD = 1.45, b = .34, z = 4.61, $p < .0001$) and HH (M = 3.13, SD = 1.43, b = .81, z = 12.07, $p < .0001$) conditions.

Carryover to consequential choice. Consistent with H1a, we again observed that default effects carry over to choice behavior. Choice share for the preference-inconsistent target in the new playlist was greater following choices made in the default choice architectures, as compared to when prior choices were made in the default-free architecture (M = 1.29, SD = 1.44). Negative binomial regression (dispersion = 1.65, Pearson χ^2 = 1110.84), revealed significant carryover from both the LH (M = 1.75, SD = 1.65, b = .310, z = 2.86, p = .004) and HH defaults (M = 1.87, SD = 1.82, b = .38, z = 3.57, $p < .001$).

Figure 4

PREFERENCE UPDATING IN EXPERIMENT 2



Notes: Error bars represent standard errors.

Carryover to preference. We computed preference updating as the change in preference strength, taking the difference between the preference strength rating elicited after the choice trials and the rating elicited at the earlier in the experiment (See Figure 4). Thus, positive values for preference updating indicate a backfire effect, whereby preference is shifted in favor of the preferred video genre, and negative values indicate carryover that whereby preference is shifted in favor of the preference-inconsistent target genre. Linear regression showed that, compared to the default-absent condition (M = -0.09, SD = 1.54), preference strength was weakened significantly more in the HH condition (M = -.46, SD = 1.70, b = -.38, t = -2.39, p = .017), but carryover to preference was not significant in the LH condition (M = -.28, SD = 1.79, b = -.19, t = -1.19, p = .236).

Reactance. Responses to the 4-item anger scale (α = .94) were averaged to produce a composite score. We standardized both the anger scores and the number of self-coded, negative

thoughts each participant reported, and summed the standardized anger and negative thought scores to produce the state reactance score. Responses to the 11-item trait reactance scale (α = .88) were averaged to produce a trait reactance score. Consistent with our speculation, state reactance is higher after making choices in the HH default choice architecture (M = .34, SD = 1.81 than it is in either the LH (M = -.22, SD = 1.38, b = .56, t = 3.69, p < .001) or default-absent architecture (M = -.16, SD = 1.40, b = .51, t = 3.49, p < .001). Somewhat surprisingly, we find no evidence that trait reactance interacts with defaults to influence state reactance. As we expected, we find no evidence that state reactance predicts preference updating, nor does it interact with default choice architecture to influence preference updating or subsequent choice behavior.

Process evidence. We conducted a mediation analysis to test our theorizing that carryover to preference is a function of the incidence of experiencing preference-inconsistent, but enjoyable choice consequences, which is boosted by defaults. Our secondary objective was to understand what role reactance plays in preference updating when consumers encounter preference-inconsistent defaults that lead to enjoyable experiences. We tested the mediation model using PROCESS Model 4 (with 5000 bootstrapped samples; PROCESS for R Version 3.5.3; Hayes 2018). We tested for the effect of default condition (with the dummy coding for the multi-categorical variable) on preference updating via two parallel pathways; (1) through the number of target videos selected (and experienced) during the choice trials and (2) through state reactance. Compared to when defaults are absent, selection of the preference-inconsistent video genre was greater under both HH ($a_{1\text{-hh}}$ = 1.79, p < .0001) and LH defaults ($a_{1\text{-lh}}$ = .59, p < .0001), and selection of that target genre weakened prior preference (b = -.335, p < .0001). Bootstrap confidence intervals (5,000 samples) for the indirect effects of HH($ab_{1\text{-hh}}$ = -.49, 95%CI=[-.868, -.311]) and LH defaults ($ab_{1\text{-lh}}$ = -.260, 95%CI=[-.259, -.076]) through

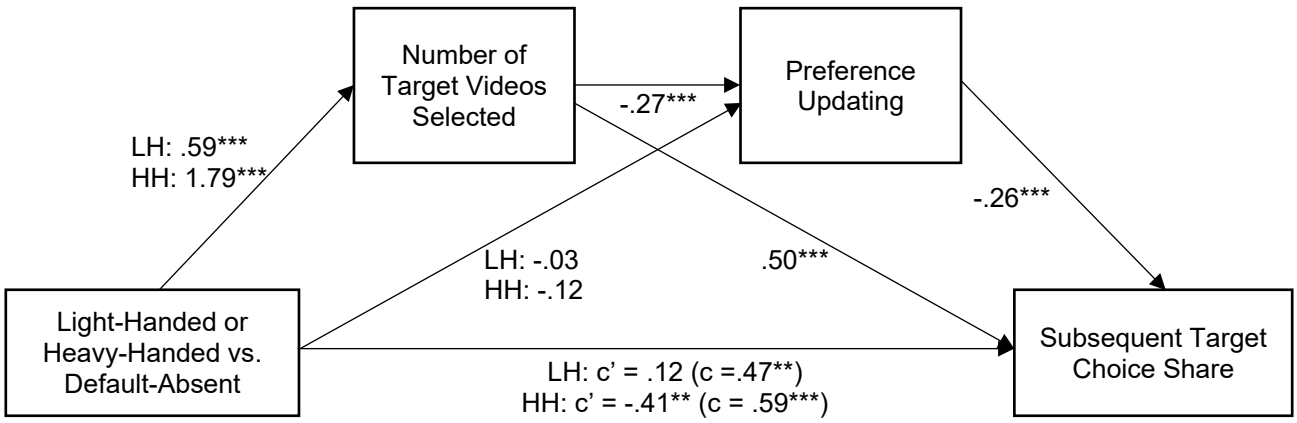
preference-inconsistent video selection do not cross zero, offering support for H5a.

HH ($a_{2\text{-hh}}$ = .501, p < .0001), but not LH defaults ($a_{2\text{-lh}}$ = -.052, p = .729), increased reactance. However, reactance did not significantly affect preference (b = -.007, p = .860), and consequently, the bootstrap confidence intervals for the indirect effects through reactance both cross zero. When reactance and target video selection are controlled for, the total effect of HH default (c = -.375, t = -2.39, p = .017) on preference updating is reduced to a non-significant direct effect (c' = .119, t < 1) and the non-significant total effect of LH default (effect = -.191, t = 1.19, p = .236) is also attenuated (c' = .031, t < 1). It is not altogether surprising to find that increased reactance does not translate into effects on preference. In this particular setting, reactance is prompted in parallel with an increase in exposure to non-aversive, nudged choice consequences that are likely to exceed expectations and, thus, counter the typical effect of reactance on preference. The observed significant mediation of preference updating by target video selections during the choice trials supports our theorizing about the role of the immediate effect of defaults in generating greater potential for preference updating in favor of the default. Of course, we argue that potential is unlikely to be realized if the consequences of nudged choices are not experienced, a hypothesis that we address in Experiment 3.

Having ruled out reactance as a mediator, we proceeded to test for sequential mediation of carryover over to subsequent choice behavior via target video selection and preference updating using PROCESS Model 6 (with 5000 bootstrapped samples; PROCESS for R Version 3.5.3; Hayes 2018). See Figure 5 for the results for each pathway. The first key takeaway is that carryover of HH defaults to subsequent choice behavior is mediated by target video selection (a_1b_1 = .897, 95%CI=[.710, 1.099]) as is carryover of LH defaults (a_1b_1 = .293, 95%CI=[.159, .433]). By contrast, preference updating, on its own, does not mediate the effect (both confidence

intervals cross zero). However, the sequential pathway through preference-inconsistent video selection and preference updating does mediate the carryover of HH ($a_1d_{21}b_2$ = .130, 95%CI=[.076, .191]) and LH defaults ($a_1d_{21}b_2$ = .423, 95%CI=[.020, .072]) on subsequent choice behavior, offer support for H5b.

Figure 5
MODERATED MEDIATION MODEL (EXPERIMENT 2)



**p <.01, ***p < .001
Relative indirect effects:
via Number of Target Videos Selected: LH 95% CI = .159, .433; HH 95% CI = .710, 1.10
via Preference Updating: LH 95% CI = -.077, .086; HH 95% CI = -.125, .059
via sequential pathway: LH 95% CI = .020, .072; HH 95% CI = .076, .199

The effect of preference-inconsistent video selection during the choice trials subsequent on subsequent choice for the new playlist fits with what we know about path-dependencies in repeated consumption of media content (Deng and Mela 2018: Kubey and Csikszentmihalyi 2002). Recent work on the rabbit hole effect (Woolley and Sharif 2022) showed that consecutive media consumption within a category promotes a sense of emersion that encourages more similar choices. While this apparent path dependence is a significant factor in the carryover observed in the present experiment, we note that its effects are likely limited to sequential decisions or to

subsequent choices that are not too far removed from the experienced consequences of nudged choices. We anticipate that the effect that target video selection, encouraged by defaults, through preference updating has a more enduring impact and is the more important mechanism for understanding how the carryover of defaults can encourage enduring behavior change.

EXPERIMENT 3: THE PIVOTAL ROLE OF EXPERIENCED CHOICE CONSEQUENCES

The preceding experiments demonstrated that default effects carry over to preference and to subsequent choice behavior when defaults are preference-inconsistent and lead to experienced outcomes that are not aversive. In Experiment 3, we tested our theorizing that the preference updating that occurs when the consequences of nudged choices are experienced the generation of carryover effects is more likely (H3). In service of this aim, we manipulated whether or not participants watched their selected videos. Manipulating consumption of the selected videos allowed us tease apart preference updating following experience of choice consequences from preference updating that arises following choice alone. We did not expect preference updating following choice alone to contribute substantially to carryover effects. By contrast, our theorizing aligns with prior work showing that inferences taken from the decision making process may produce backfire effects (Donkers et al. 2020). Consequently, we expected to observe attenuation of carryover effects when the videos were not watched.

Method

We recruited United Kingdom residents from the Prolific Academic platform and required them to pass an instructional manipulation check (IMC; Oppenheimer, Meyvis, and Davidenko 2009) before proceeding. Any participant who did not follow the instructions was

prompted to read the instructions again. We prevented a participant from proceeding if they failed to follow the instructions a second time. This procedure served as a screening device to prevent inattentive participants from contributing data, and we used it in all remaining experiments. Of the participants who consented to the use of their data 2117 passed the IMC and completed the study. The preference elicitation stage differed from the prior experiments in two ways. First, participants indicate their preferences across five pairings of video genres, rather than three. Second, instead of rating the strength of their preferences, participants used slider scales (scored from 0 to 99 and anchored by *not at all* and *very much*), all appearing together on a single page, to rate how much they like each of the six video genres that had been featured across the five video genre pairings.

After the preference elicitation stage, participants were randomly assigned to the four cells of a 2 (default: present vs. absent) x 2 (experience: yes vs. no) between-subjects, factorial design. We used the heavy-handed default choice architecture from the prior experiments in the default-present treatments in this and all following experiments. The target video was always from the preference-inconsistent genre. Participants in the experience-yes condition watched their selected video in each of the five choice trials. We instructed those in the experience-no condition that the videos they selected would be added to their playlist for later viewing. After the choice trials, participants used slider scales to evaluate the individual video genres a second time. At the end of this experiment (and those that follow), we asked participants to self-report distractions and technical problems (see Web Appendix for details). We established exclusion criteria in advance. Specifically, we committed to excluding from analysis data from participants reporting more than one listed distraction or technical problem, reporting a single other distraction that caused them to leave their computer during the study, or reporting a single

technical problem that interfered with the experiment.

Pretest data revealed that approximately 12% of participants responded inconsistently across the two tasks in the preference elicitation stage. Their individual rating responses either indicated indifference or a preference reversal compared to their paired preference response. Considering that the individual rating scales appeared together on the same page, facilitating paired comparison, we concluded that preference reversals might be a strong indicator of inattentive responding. The pretest results also raised the concern that our creation of preference-inconsistent (or consistent) defaults might be thwarted if participants were truly indifferent. Therefore, for this and the remaining experiments, we excluded from analysis data from participants who indicated a preference reversal or indifference between comedy and science videos in the preference elicitation stage.

Results and Discussion

Data from 101 participants were excluded based on self-reported distractions or technical problems and 219 were excluded based on preference indifference (84) or preference reversal (135) detected in the preference elicitation stage, leaving data from 1797 participants for analysis. The number of target video selected across the choice trials was subjected to Poisson regression (dispersion = .1.03, Pearson χ^2 = 1841.06), which revealed a significant interaction of default and experience (b = -.043, z = -2.75, p = .013). The default effect was larger in the experience-yes condition ($M_{\text{default-present}}$ = 2.90, $SD_{\text{default-present}}$ = 1.46, $M_{\text{default-absent}}$ = 1.28, $SD_{\text{default-absent}}$ = 1.34, b = .815, z = 16.32, p < .0001) than in the experience-no condition which, nevertheless, featured a large default effect ($M_{\text{default-present}}$ = 2.75, $SD_{\text{default-present}}$ = 1.37, $M_{\text{default-absent}}$ = 1.45, $SD_{\text{default-absent}}$ = 1.36, b = .642, z = 13.25, p < .0001).

We computed preference strength by subtracting each participants' evaluation of their

less preferred video genre from their evaluation of their more preferred genre. Thus, if a participant indicated a preference for science (comedy) videos over comedy (science) videos, their preference strength was computed as their evaluation of science (comedy) minus their evaluation of comedy(science). We computed preference strength for both the initial ratings taken during the preference elicitation stage (t1) and for the subsequent ratings taken after participants had completed five choice trials (t2). We computed preference updating as the change in preference strength, taking the difference between t2 and t1 preference strength. Thus, positive values for preference updating indicate that preferences shifted in favor of the preference-consistent video genre, and negative values indicate updating in favor of the preference-inconsistent target genre.

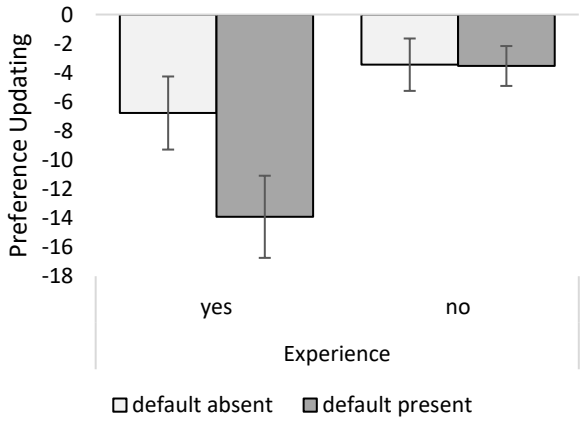
To test for preference updating we computed the mean and 95% confidence interval in each cell of the factorial design. The confidence intervals (Figure 6) do not overlap with zero and all means are negative, indicating that preference updating significantly shifted preference in favor of the target regardless of whether or not choices were made in a default choice architecture, and regardless of whether or not the consequences of those choices were experienced ($M_{\text{absent-yes}}$ = -6.78, $SD_{\text{absent-yes}}$ = 27.91; 95%CI [-9.29, -4.26]; $M_{\text{present-yes}}$ = -13.91, $SD_{\text{present-yes}}$ = 28.89; 95%CI [-16.74, -11.09]; $M_{\text{absent-no}}$ = -3.45, $SD_{\text{absent-no}}$ = 19.08; 95%CI [-5.25, -1.665]; $M_{\text{present-no}}$ = -3.54, $SD_{\text{present-no}}$ = 15.36; 95%CI [-4.91, -2.17]).

Importantly, and in line with H3, the magnitude of preference updating was substantially greater when the participants in the default choice architecture had experienced the consequences of their choices. To test for default carryover, preference updating was subjected to a Type III ANOVA revealing a main effect of default ($F(1, 1793) = 10.70$, $p = .001$, $\eta^2 = .006$) and a main effect of experience ($F(1, 1793) = 38.45$, $p < .0001$, $\eta^2 = .020$), qualified by the expected

interaction ($F(1, 1793) = 10.18$, $p = .001$, $\eta^2 = .006$). Consistent with our framework, when participants watched their chosen videos, experiencing the consequences of their choices, the default effect carried over such that preferences were updated in favor of the target more when the earlier choices were made in a default choice architecture than when they were made in a default-free architecture ($F(1, 1793) = 20.42$, $p < .0001$, $d = -.25$). By contrast, there is no evidence of carryover when choice consequences were not experienced, as reflected in the similar levels of preference-updating across default-present and default-absent conditions ($F(1, 1793) < 1$, $p = .954$).

Figure 6

PREFERENCE UPDATING IN EXPERIMENT 3



Notes: Error bars represent 95% confidence intervals.

As predicted by our framework, these results show that default effects carry over when defaults encourage preference-inconsistent choices that lead to experience of non-aversive choice consequences. In support of H3, we observed default carryover when participants immediately watched their chosen videos, but found no evidence of carryover when choices did not lead to those experiential consequences. We have argued that preference-inconsistent defaults carry over

by increasing the incidence of experiencing choice outcomes that support preference updating. The present results support this line of reasoning. Although some preference updating is present in all conditions, this effect is substantially stronger when the preference-inconsistent target video option is selected and experienced more often due to it having been preselected as the default. It is worth noting that the main effect of experience also points to the importance of the consumption of the chosen alternative to the process of preference updating. Without the experience of watching the chosen videos, preference updating, while significantly different from zero (M = -3.50, SD = 17.21; 95%CI [-4.61, -2.38]), was less pronounced than when consumption was an immediate consequence of choice (M = -10.05, SD = 28.57; 95%CI [-11.94, -8.16]). The simple effect of experience when the default was present ($F(1) = 43.67$, $p < .0001$, d = -.45) reveals that the greater incidence of experience, resulting from the default effect only when consumption followed directly from choice, is substantially responsible for the observed differences in preference updating. Even without a default, the experience of choice consequences resulted in greater preference updating ($F(1) = 4.60$, $p = .032$, d = -.14). Although participants in the default-absent conditions selected relatively few target videos, watching them seems to have had a positive impact on their unfavorable opinions of the target video genre.

Moderated mediation analysis supports this interpretation of the joint effects of default and experience on preference updating. We tested for mediation of default carryover to preference as moderated by experience of choice consequences (with 5000 bootstrapped samples; PROCESS for R Version 3.5.3; Hayes 2018), entering default as the independent variable, the number of target videos selected as the mediator, and experience as the moderator of both the proximal and distal paths. Consistent with the analysis reported above, default and experience interacted (B = .31, t = 2.39, p = .017) such that the significant conditional effect of

default on target selection was larger with experience (a_1 = 1.61, t = 17.29, p < .0001) than without experience (a_2 = 1.30, t = 14.26, p < .0001). In turn, the number of target videos selected interacted with experience (B = -5.15, t = -8.17, p < .0001) to weaken prior preference inconsistent with the target more dramatically with experience (b_1 = -9.68, t = -20.64, p < .0001) than without it (b_2 = -4.53, t = -9.40, p < .0001). The confidence interval surrounding the index of moderated mediation (based on 5000 bootstrapped samples) does not cross zero (index = -9.73, 95%CI [-12.54, -7.03]) indicating that the indirect effect of the default through the number of target videos selected was significantly larger with experience (a_1b_1 = -15.63, 95%CI [-18.10, -13.18]) than without experience (a_2b_2 = -5.90, 95%CI [-7.43, -4.55]).

These results offer additional support for hypotheses 3 and 5a. The remaining direct effect of default on preference updating (c' = 7.12, t = 6.40, p < .0001) is positive, suggesting that the process of choosing in a default choice architecture produced a backfire effect, strengthening preference inconsistent with the target. This backfire effect, apparent only when controlling for the accumulation of preference updating across the number of target videos selected, is predicted by our theorizing and is consistent with past findings. Rejecting the default may serve to reinforce prior preference that is inconsistent with the default. By contrast, accepting the default may be a passive choice that produces less preference updating than making the same selection in the absence of a default (Donkers et al. 2020).

Altogether, the results of Experiment 3 support our contention that experiencing the consequences of one's choices plays a critical role in the dynamics of preference updating involved in the carryover of default effects. While the default effect is itself made stronger by experiencing the consequences of one's choices, the carryover of that effect grows even stronger as more target choice consequences are experienced. Importantly, preference updating in favor of

the target that arises from experience of choice consequences counters preference updating that follows from the decision making process, which appears to reinforce preferences inconsistent with the target.

EXPERIMENT 4: ALIGNMENT OF DEFAULTS WITH PRIOR PREFERENCE

We argue that the alignment of defaults with prior preference plays an important role in modulating the downstream effects of default nudges. Across the prior three experiments we have offered the first demonstrations that preference-inconsistent defaults generate carryover. However, defaults may be less effective at shifting immediate choices when they align with prior preference, simply as a function of ceiling effects deriving from a higher base rate for choice of the target option without a default. Insofar as preference-consistent defaults lead to a smaller increase in the incidence of the experience of target choice consequences (relative to when defaults are absent) as compared to preference-inconsistent defaults, they should produce less carryover. Moreover, given that prior preference is an expression of expectations (Weber and Johnson 2006; Wilson et al. 1989), experiencing favorable choice consequences arising from preference-consistent target choices should be less likely to produce perceptions of incongruity between expectations and experienced consequences.

Together, these two forces can be expected to reduce the incidence and intensity of preference updating in favor of preference-consistent (vs. preference-inconsistent) targets, resulting in attenuated carryover. We tested this reasoning in Experiment 4 by examining carryover of default effects arising from defaults consistent with versus inconsistent with prior preferences when the consequences of choice are experienced. As predicted by H4, we expected

to observe carryover from preference-inconsistent defaults, but not from defaults consistent with prior preference.

Method

We recruited United States residents from Amazon's Mechanical Turk platform, 1091 of whom passed the IMC and consented to the use of their data. Participants responded to the same preference elicitation tasks used in Experiment 3 and were then randomly assigned to one of three choice architecture conditions; default absent, preference-consistent default, and preference-inconsistent default. The preference elicitation stage was identical to Experiment 3. The choice trials used the same interface as the experience-yes condition in Experiment 3, except that in the preference-consistent architecture the target video was from participants' preferred video genre. After five choice trials, in which they watched their selected videos, participants again used the slider scales to evaluate each video genre.

Results and Discussion

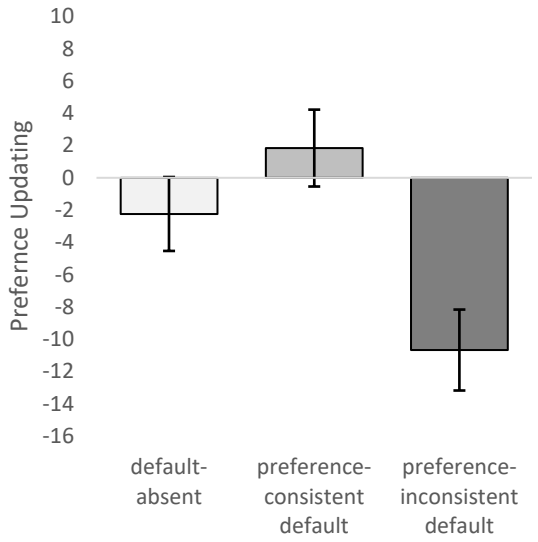
Data from 55 participants were excluded based on self-reported distractions or technical problems and 133 were excluded based on preference indifference (49) or preference reversal (84) detected in the preference elicitation stage, leaving data from 903 participants for the analysis. To confirm the presence of a default effect as a function of default-preference alignment, we computed the choice shares for each participant in the default-absent condition for the video genre consistent with and inconsistent with their reported preference and compared these to choice shares for the target in the preference-consistent and preference-inconsistent default conditions respectively. Separate Poisson regressions showed that the preference-consistent target was selected more often when it was preselected as the default (M = 4.71, SD = .60) than when the default was absent (M = 3.97, SD = 1.26, dispersion = .20, Pearson χ^2 =

92.04, b = .17, z = 3.66, p < .001). The default effect was much larger for the preference-inconsistent target, which was selected more often when it was preselected as the default (M = 3.70, SD = .1.38) than when the default was absent (M = 1.10, SD = 1.23, dispersion = .75, Pearson χ^2 = 336.02, b = 1.21, z = 13.51, p < .0001).

We computed preference updating the same way as in Experiment 3. The confidence intervals (Figure 7) overlap with zero in both the default-absent (M = -2.24, SD = 20.49; 95%CI [-4.54, 0.06]) and preference-consistent default conditions (M = 1.84, SD = 19.95; 95%CI [-0.54, 4.22]), indicating that preference updating was not significant when participants chose and watched videos in a default-free architecture or in a default architecture that nudged choices consistent with prior-preference. By contrast, preference updating was significant and in favor of the preference-inconsistent target when that alternative was preselected as the default (M = -10.67, SD = 22.84; 95%CI [-13.18, -8.16]).

Figure 7

PREFERENCE UPDATING IN EXPERIMENT 4



Notes: Error bars represent 95% confidence intervals.

We followed up a significant one-way ANOVA on preference updating ($F(2, 900) = 27.29$, $p < .0001$, $\eta^2 = .06$) with pairwise comparisons while controlling for alpha inflation using the Bonferroni method to adjust p-values. Comparing preference updating across the two default-present conditions revealed a significant difference ($p < .0001$, $d = .58$), while only a marginal difference in preference updating between preference-consistent and default-absent conditions was detected ($p = .064$, $d = .20$). Preference updating in favor of the preference-inconsistent target following preference-inconsistent defaults was significantly greater in magnitude ($p < .0001$, $d = .39$) than in the default-absent condition.

Participants in Experiment 4 experienced the consequences of their choices (enabling condition 2) and those consequences, watching enjoyable videos, were not aversive (enabling condition 3). As predicted by H4, carryover of the default effect to preference that was observed in the prior experiments replicated with preference-inconsistent defaults (enabling condition 1), but was substantially attenuated when defaults were consistent with prior preference. These results suggest that prior-preference at the individual level, and preference heterogeneity at the population level, should be important considerations for anyone aiming to understanding how and under what conditions, default effects are likely to carry over.

Traditionally, investigation of the downstream effects of defaults has involved measuring subsequent preference or behavior after decisions have been made in default-present versus default-absent conditions, without taking into consideration prior preference or the potential for preference heterogeneity. Given evidence that prior preference may be an important factor in generating carryover of default effects, this approach may result in misleading findings. This is concerning, in part, because choice architects often aim to encourage behavior that consumers may be otherwise disinclined toward, even if it is in consumers' best interests. At other times, choice architects aim to boost behavior that consumers already favor. In either case, there is

rarely uniformity of preference in the population for the target behavior. Heterogeneity of preference may mask whether or not a particular intervention is effective in producing the intended enduring behavior change. We will return to the conceptual, methodological, and practical implications of preference heterogeneity in our general discussion. First, we offer additional evidence supporting our hypothesis that prior preference moderates the mechanism responsible for carryover of default effects.

EXPERIMENT 5: ALIGNMENT WITH PRIOR PREFERENCE MODERATES THE CARRYOVER MECHANISM

So far, we have demonstrated in four experiments that default effects carry over when consumers experience non-aversive consequences of their preference-inconsistent, nudged choices. Experiment 1 demonstrated carryover to choice behavior; Experiment 2 showed that default effect carryover to preference, which then influence subsequent choice behavior; Experiments 3 highlighted that experiencing the consequences of nudged choices is key to enabling this mechanism; and Experiment 4 illustrated that preference-inconsistent, but not preference-consistent, defaults, prompt the preference updating involved in carryover. Experiment 5 extends our findings showing that default-preference inconsistency enables carryover to choice behavior, and offers process evidence that default-preference consistency moderates the mechanism responsible for this carryover. In addition, we tested for robustness of carryover to preference using a more conservative measure and examined the role that expectation-disconfirmation plays in the preference updating process.

If preference updating is a function of incongruity between expectations—which themselves form the basis for prior preference—and experienced outcomes, then preference that

is more strongly inconsistent with the target should potentiate perception of greater incongruity when an enjoyable outcome is experienced from choice of the target. Therefore, we expected to observe greater preference updating in favor of a preference-inconsistent target as the strength of prior preference increases. Given that preference-inconsistent defaults increase the incidence of experiencing this incongruity, we also expected amplification of the relationship between prior preference and preference updating when defaults were present. By contrast, defaults that are consistent with prior preference are less likely to lead to perceptions of incongruity between expectations and experienced outcomes. Therefore, we did not expect prior preference strength to predict preference updating in favor of the preference-consistent target, regardless of whether prior choices were made in a default-present or default-absent choice architecture.

Method

In addition to using an IMC at the beginning of the study, we also screened out inattentive participants by asking three questions of understanding after participants had read instructions. One of these questions was itself a second IMC that instructed participants to respond in a particular manner. Participants who responded incorrectly to any of the questions were prompted to reread the instructions and were presented with the questions again. Participants were prevented from proceeding to the main part of the study if they failed to respond as instructed a second time. We recruited 3641 United States residents from Amazon's Mechanical Turk platform who passed the IMC and questions of understanding, and consented to the use of their data. Participants indicated their preference, as in Experiments 3 and 4, and then evaluated the individual video genres. Unlike the prior experiments, individual genre evaluations were elicited using 11-point scales (anchored by 0 = *do not like them at all* and 10 = *like them very much*) that were presented on a fresh page for each video genre. Participants were then randomly assigned to the four cells of a 2 (default: present vs. absent) x 2 (target: preference-

inconsistent vs. preference-consistent) between-subjects, factorial design. After five choice trials in which they watched their selected videos, participants responded to two dependent measures, the order of which we randomized. Participants used the 11-point liking scale to rate the target genre. To measure subsequent choice behavior, we told participants that we would build a new playlist of five more videos for them and that it was up to them how many would be included from each of three genres, one of which was the target. The other alternatives were gardening videos and life hack videos, both of which are video genres that had not been included in the preference elicitation stage. Participants chose the number of videos from each genre, totaling to five, to include in the new playlist.

Results and Discussion

Data from 169 participants were excluded based on self-reported distractions or technical problems and 745 were excluded based on preference indifference (505)[1] or preference reversal (240) detected in the preference elicitation stage, leaving data from 2727 participants for analysis. We confirmed the expected default effect, subjecting the number of target video selected across the choice trials to Poisson regression (dispersion = .78, Pearson $\chi^2 = 2133.90$), which revealed a significant effect of default ($b = -.28$, $z = -23.14$, $p < .0001$), a significant effect of target ($b = .39$, $z = 32.06$, $p < .0001$), and the expected interaction ($b = .19$, $z = 15.33$, $p < .0001$). The default effect was larger in the preference-inconsistent condition ($M_{\text{default-present}} = 3.07$, $SD_{\text{default-present}} = 1.62$, $M_{\text{default-absent}} = 1.20$, $SD_{\text{default-absent}} = 1.40$, $b = .94$, $z = 23.05$, $p < .0001$) than in the preference-consistent condition ($M_{\text{default-present}} = 4.62$, $SD_{\text{default-present}} = .76$, $M_{\text{default-absent}} = 3.82$, $SD_{\text{default-absent}} = 1.37$, $b = .19$, $z = 7.09$, $p < .0001$).

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[1] This high level of indifference, as compared to the levels observed in Experiments 3 and 4 is likely do to the use of an 11-point scale instead of the 100-point slider scale. We repeated our analyses with the data from these 505 participants included and observed only one difference that would alter the inferences drawn, which we note below.

Carryover to preference. We computed preference updating as the change in evaluation of the target video genre, taking the difference between the evaluation after the choice trials and the evaluation in the initial preference elicitation stage. Thus, positive values for preference updating indicate that preferences shifted in favor of the target video genre. This represents a more conservative measure of preference updating than was used in the prior experiments because it focuses exclusively on changes in preference with respect to the target genre, rather than taking into account changes in preference for both the target and the alternative genres. We subjected preference updating to a Type III ANOVA revealing a main effect of default ($F(1, 2723) = 8.57$, $p = .003$, $\eta^2 = .003$) and a main effect of target ($F(1, 2723) = 62.61$, $p < .0001$, $\eta^2 = .020$), qualified by the expected interaction ($F(1, 2723) = 4.25$, $p = .039$, $\eta^2 = .002$). Consistent with H4 and the results of Experiment 4, the default effect carried over to preference when the target was inconsistent with prior preference ($F(1, 2723) = 12.77$, $p < .001$, $d = .17$), but not when the target was consistent with prior preference ($F(1, 2723) < 1$, $p = .545$, $d = .04$).

Carryover to choice behavior. The preference-dependent carryover of the default effect is also observable in subsequent choice behavior in a default-free setting. Negative binomial regression (dispersion = 1.15, Pearson $\chi^2 = 3126.21$) testing for carryover of the default effect to choices made for a new playlist revealed a significant effect of default ($b = -.047$, $z = -3.33$, $p = .001$), a significant effect of target ($b = .369$, $z = 26.10$, $p < .0001$), and a marginally significant interaction ($b = .027$, $z = 1.89$, $p = .058$). In the preference-inconsistent condition, significantly more target videos were included by participants in the new playlist when they had made their prior choices in the default choice architecture ($M_{\text{default-present}} = 1.51$, $SD_{\text{default-present}} = 1.51$, $M_{\text{default-absent}} = 1.31$, $SD_{\text{default-absent}} = 1.36$, $b = .148$, $z = 2.59$, $p = .010$). The carryover effect was attenuated and not significant in the preference-consistent condition ($M_{\text{default-present}} = 3.00$,

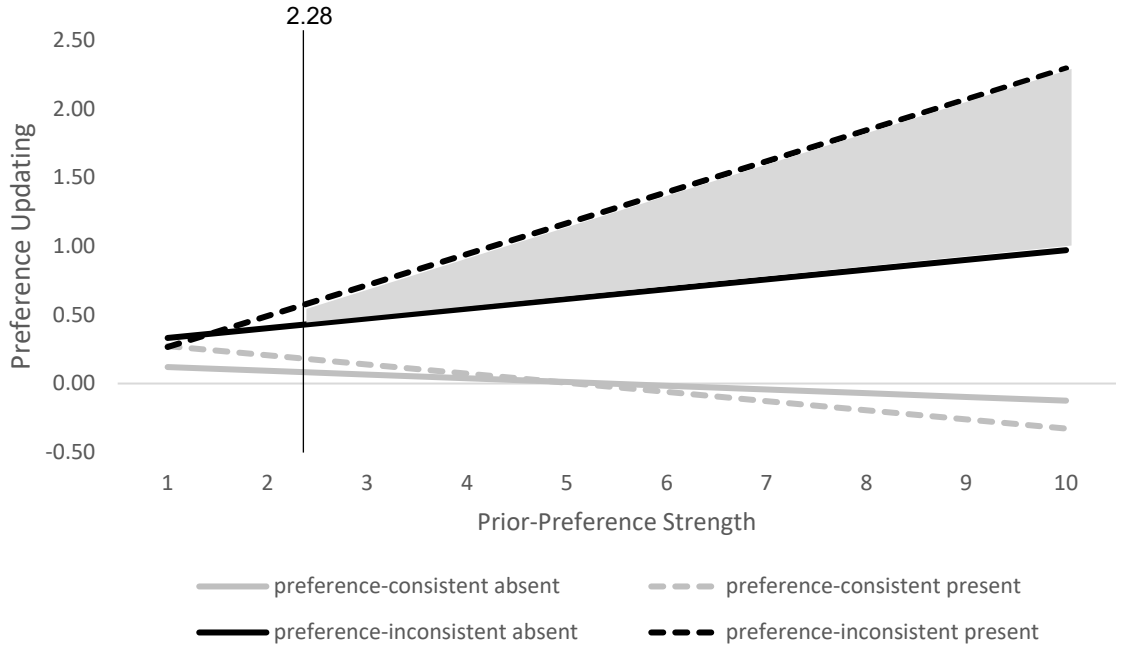
$SD_{\text{default-present}}$ = 1.58, $M_{\text{default-absent}}$ = 2.89, $SD_{\text{default-absent}}$ = 1.51, b = .041, z = 1.27, p = .206). This finding extends the moderating role of prior preference in carryover to preference, as demonstrated in Experiment 4, to carryover of default effects to choice behavior.

Prior preference strength. To examine the hypothesized role of expectation disconfirmation in the preference updating process we regressed preference updating on default, target, and prior preference strength, which was computed by subtracting each participants' initial evaluation of their less preferred video genre from their evaluation of their more preferred genre. Only the two-way interaction of target and preference strength (b = .138, t = 3.27, p = .001) and the three-way interaction (b = .154, t = 2.55, p = .011) emerged as significant (all other ps > .20). Analysis of the simple slopes (see Figure 8), revealed that preference updating became significantly stronger when prior preferences were more strongly inconsistent with the target in the absence of a default (*simple slope* = .111, t = 3.82, p = .0001), and even more so in a default choice architecture (*simple slope* = .225, t = 7.76, p < .0001). We decomposed the interaction of default and prior preference strength when the target is preference-inconsistent using the Johnson-Neyman technique to identify that the effect of the default on preference updating was significant when prior preference strength was greater than 2.281 (B_{JN} = .234, SE = .119, p = .05). We also observed a significant, but small, negative effect of prior preference strength on preference updating when the default was preference-consistent (*simple slope* = -.067, t = -2.06, p = .039)[2], whereas no such backfire effect on evaluation of a preferred target was detectable in the absence of a default (*simple slope* = -.027, t = -.89, p = .375).

[2] When the data from indifferent participants is included in the analysis the simple slope when choosing in a preference-aligned default choice architecture is marginally significant (*simple slope* = -.048, t = -1.78, p = .075).

Figure 8

THE THREE-WAY INTERACTION OF PRIOR-PREFERENCE STRENGTH, DEFAULT, AND DEFAULT-PREFERENCE ALIGNMENT ON PREFERENCE UPDATING IN EXPERIMENT 5



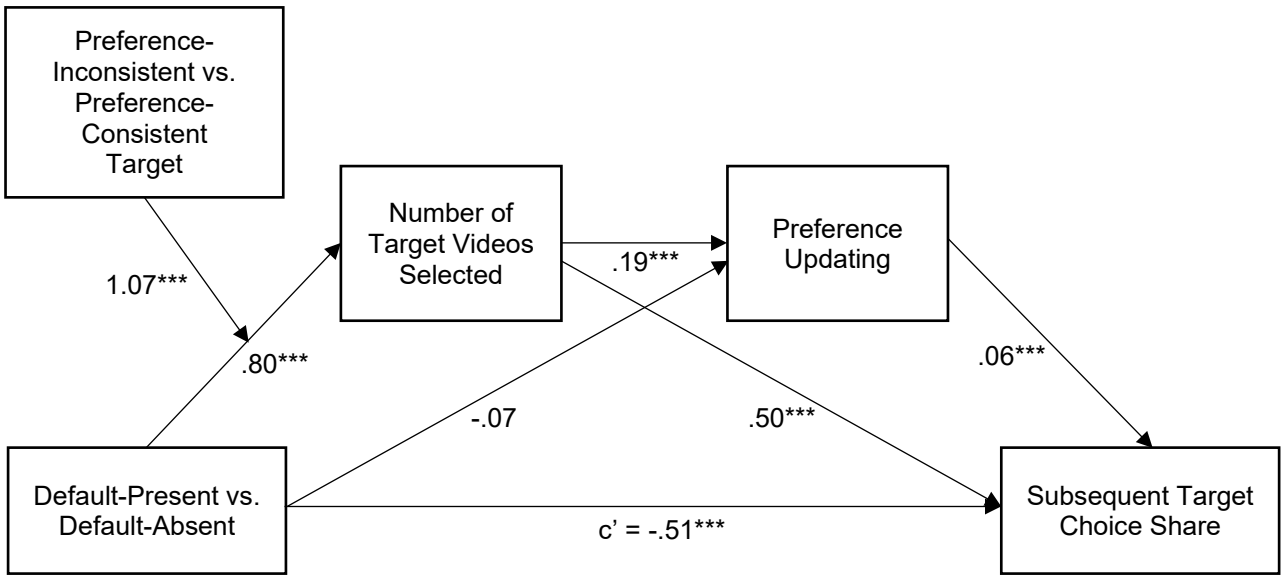
Notes: Johnson-Neyman point and the rejoin of significance are indicated for the simple effect of default when the target is preference-inconsistent.

The observed relationship between the strength of target-inconsistent prior-preference and preference updating is consistent with our argument that prior preference is an indicator of expectations, and that favorable outcomes, when they are experienced, are perceived as incongruous with expectations when the choices leading to those outcomes are inconsistent with prior preference. The positive effect of defaults on this this relationship can be traced to the increased incidence of experienced incongruity that facilitates the updating of preference in favor of the target. The small backfire effect with increasing preference strength that we observe when the target is preference-consistent is not inconsistent with our theorizing. Given that incongruity between expectations for a preferred outcome and the experience of that favorable outcome are less likely than when the outcome is not preferred, we acknowledge that such incongruity is

possible. However, unlike the case of the negative expectations associated with target-inconsistent prior preference, when the target is consistent with prior preference, greater preference strength implies an increased likelihood that an enjoyable outcome should be experienced as less hedonically rewarding than expected.

Process evidence, Moderated mediation analysis supports our theorizing that carryover of the default effects is a function of the incidence of experiencing choice consequences that support preference updating in favor of the default. We used PROCESS Model 83 (with 5000 bootstrapped samples; PROCESS for R Version 3.5.3; Hayes 2018) to test for sequential mediation of default carryover to subsequent choice behavior via target video selection and preference updating, as moderated by preference-alignment of the target (Figure 9). This analysis revealed three key takeaways. First, consistent with the findings from Experiment 2, carryover of heavy-handed defaults to subsequent choice behavior is mediated by target video selection (a_1b_1 = .926, 95%CI=[.833, 1.038]). However, as our framework suggests, this indirect effect is attenuated when the target is consistent with prior preference (a_1b_1 = .397, 95%CI=[.333, .459]; Index of moderated mediation = .529, 95%CI=[.430, .634]). Second, we find that the sequential pathway through target video selection and preference updating mediates carryover when the target is preference-inconsistent ($a_1d_{21}b_2$ = .023, 95%CI=[.012, .035]) and this indirect effect is attenuated when the target is preference-consistent ($a_1d_{21}b_2$ = .010, 95%CI=[.005, .015]; Index of moderated mediation = .013, 95%CI=[.007, .021]). Third, the remaining direct effect of default on preference updating (c' = -.510, t = -8.63, p < .0001) is negative. As was observed in Experiment 3, the process of choosing in a default choice architecture is to strengthen preference inconsistent with the target, but this backfire effect is counteracted by preference updating associated with experiential consequences of consuming the chosen alternatives.

Figure 9
MODERATED MEDIATION MODEL FOR EXPERIMENT 5



***p < .001
Conditional indirect effects
via Number of Target Videos Selected:
Preference-consistent 95% CI = .833, 1.-28; Preference-inconsistent 95% CI = .333, .459
Index of moderated mediation 95% CI = .430, .634
via Preference Updating: Index of moderated mediation 95% CI = -.015, .004
via sequential pathway:
Preference-consistent 95% CI = .012, .035; Preference-inconsistent 95% CI = .005, .015
Index of moderated mediation 95% CI = .007, .021

GENERAL DISCUSSION

This research examined the carryover of default effects to preference and to subsequent choice behavior. Results from five experiments offer evidence that the effects of preference-inconsistent defaults carry over when the non-aversive consequences of nudged choices are experienced. The findings show that carryover is dependent on the experience of consequences of nudged choices and is attenuated when defaults are consistent with prior-preferences. In contrast to past work investigating the downstream effects of defaults, which has focused on

behavioral outcomes (i.e. revealed preference), this work also tested for downstream effects on preference (i.e. attitudes toward consumption objects). Process evidence supports our theorizing that, by increasing incidence of the experience of non-aversive target outcomes, defaults boost preference updating in favor of target alternatives and, in turn, boost subsequent choice of those alternatives.

These findings help to reconcile conflicting results in the extant literature. Prior research examining the downstream effects of defaults has, for the most part, not found that defaults have downstream effects on choice behavior (Ghesla et al. 2019; Kuhn et al. 2021; Michaelsen et al. 2021; Schmidtke et al. 2022; Van Rookhuijzen et al. 2021) and there is limited evidence that defaults can produce backfire effects (Donkers et al. 2020; Wisdom et al. 2010). We observed that all prior studies that did not offer evidence of carryover were conducted using paradigms in which participants do not experience the consequences of their choices. By contrast, there have been a few studies offering evidence of carryover effects (Putnam-Farr and Riis 2016; Van Rookhuijzen et al. 2021; Venema et al. 2018) in which participants did experience choice consequences. We theorized that the conflicting results might be explained, at least in part, by this distinction. Our findings bear out this observation and offer evidence that carryover is more likely when people experience the outcomes they have been nudged to choose.

Whether participants experience the consequences of their choices might seem like a purely methodological distinction. However, it is conceptually and practically important. The importance of this distinction is emphasized by the fact that we also find that the inconsistency of defaults with prior preference is an important factor in the generation of carryover effects. To the extent that defaults shift choice more dramatically when they are inconsistent with prior preference, they potentiate a more dramatic increase in the experience of target choice

consequences. In addition, when the consequences of a preference-inconsistent (vs. preference-consistent) target choice are not aversive, there is greater likelihood that experience is perceived to exceed expectations, facilitating more intense preference updating in favor of the target.

The five experiments described above offer compelling evidence that default-preference consistency and experience of choice consequences moderate the generation of default carryover. We suspect that at least some of the disparities in the extant literature derive from the fact that prior work examining the downstream effects of defaults has not considered these moderators. For choice architects, these moderators represent ecological realities with practical implications for the design, implementation, and assessment of default interventions. In practice, different contexts in which choice architects might be designing defaults vary in the temporal dynamics of choice and experience of choice consequences. In addition, preference-heterogeneity is generally unavoidable in most populations that are exposed to a choice architecture intervention. It is, therefore, important to understand the conceptual implications and practical consequences of ignoring preference heterogeneity and overlooking the temporal dynamics of choice and experienced outcomes when studying the downstream effects of default. To probe the methodological, conceptual, and practical implications of our findings we conducted two simulations using data from our experiments.

Simulations: Methodological, Conceptual, and Practical Implications

The data from Experiment 3 allow for simulation of experiments in which temporal dynamics of choice and experienced outcomes are ignored, and the data from Experiment 4 allow for simulation of experiments in which preference heterogeneity is ignored. Here, we present two simulations utilizing those data to empirically examine how these factors influence our ability to detect and understand carryover of default effects when they are not, or cannot, be

taken into consideration. The simulation results highlight the importance for researchers and choice architects to account for temporal dynamics and preference heterogeneity when investigating the downstream effects of defaults and when designing and testing interventions that use defaults to encourage behavior change.

Preference heterogeneity. Despite the importance of the heterogeneity of consumers' preferences in marketing research and practice (e.g., Grewal, Mehta, and Kardes 2004; Hutchinson, Kamakura, and Lynch 2000; Yang, Allenby, and Fennell 2002) preference-dependent dynamics in the downstream effects of defaults have largely been ignored until now. A critical implication of the moderating role of default-preference alignment in the generation of default carryover is that ignoring preference heterogeneity might compromise the ability of choice architects to understand and measure the enduring impact of defaults nudges. As illustrated in our experiments, defaults may produce downstream effects that diverge across segments. Hence, carryover of default effects may be difficult to detect at the aggregate level if the majority in a population look upon the default option favorably. Of equal concern, the failure to detect carryover may actually mask consequential carryover effects within segments of particular interest to researchers and choice architects.

Choice architects aiming to encourage sustained behavior change may be interested in nudging consumers to adopt a particular behavior they otherwise disfavor. If this targeted segment of the population is in the minority, our findings suggest that choice architects piloting a default intervention may be led to erroneously conclude their efforts are fruitless. By contrast, if the intervention is piloted in a population where this segment is the majority, choice architects might erroneously conclude that the carryover observed in their pilot will generalize to another population in which, perhaps unbeknownst to the choice architect, the target segment is in the

minority. We examine the conditions under which such inferential errors might be likely arise, by simulating experiments across populations that vary in preference heterogeneity.

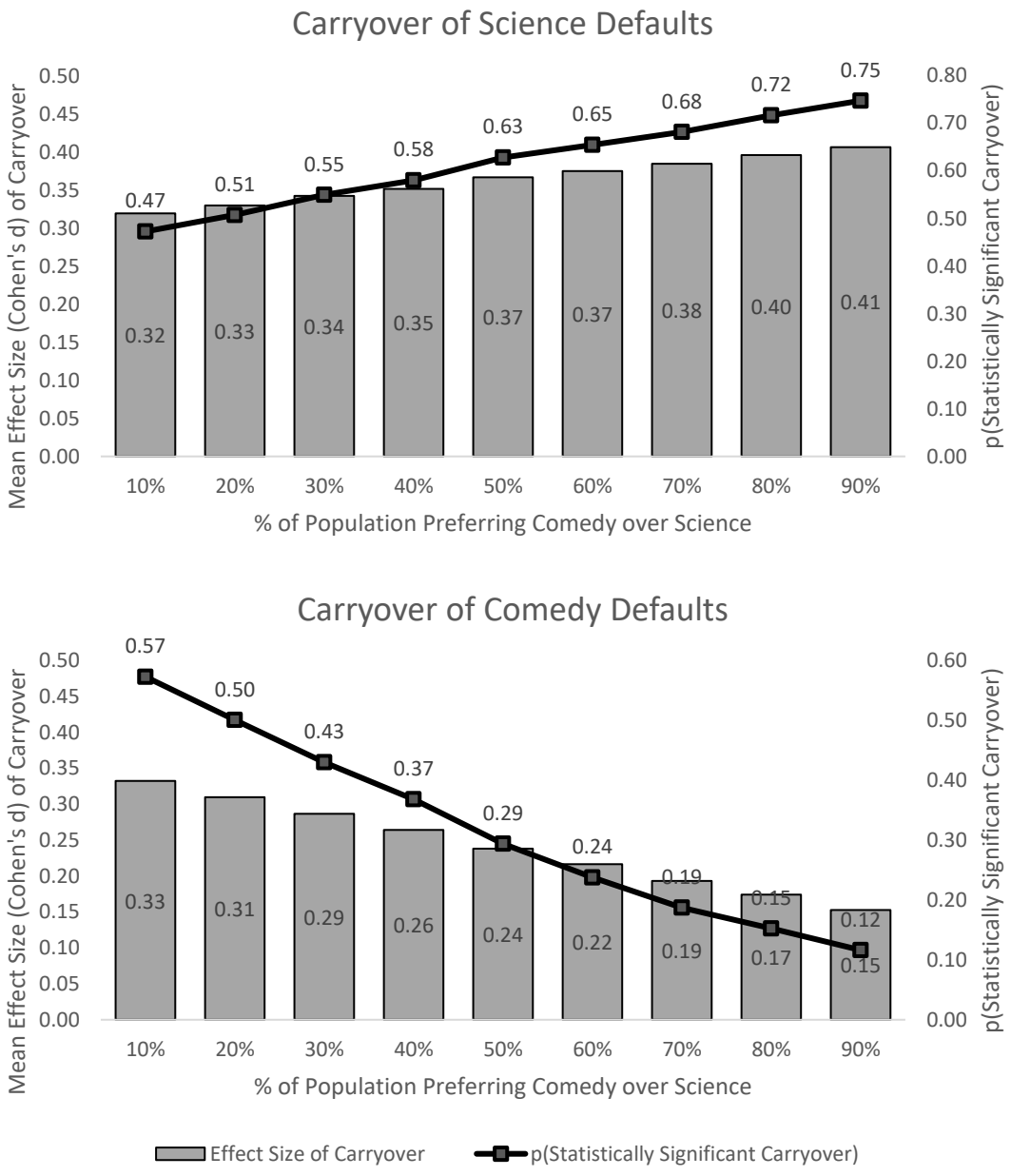
Simulation method. We simulated the carryover of default effects to preference across different levels of preference heterogeneity (η) in a population using the data obtained from Experiment 4. Recall, Experiment 4 featured three conditions: default-absent, preference-consistent default, and preference-inconsistent default. The dependent variable was preference updating, measured as the change in preference strength from before to after the choice trials. For the purpose of this simulation, we recoded the default-present conditions according to the preselected video genre (comedy or science). We crossed the default factor with prior-preference to separate the data into the six cells of a 3 (default: absent, comedy, science) x 2 (prior-preference: comedy, science) design, from which we constructed our simulated populations. We recomputed preference updating based on preference strength for science videos (subtracting each participant's evaluation of science videos from their evaluation of comedy videos).

We simulated the carryover of default effects at nine levels of preference heterogeneity (η) in a population, specified by the proportion of the population who prefers comedy to science (η = .10, .20, .30, .40, .50, .60, .70, .80, .90). For each condition (default-absent, comedy default, science default) at each level of η, we simulated a population of N=100,000 by sampling with replacement from the recoded data obtained from Experiment 4 in the proportion specified by η. For example, to simulate a population for the default-absent condition in which 10% prefer comedy videos we took 10,000 samples with replacement from the default-absent-comedy-preferred cell and 90,000 samples with replacement from the default-absent-science-preferred cell. In each simulated experiment, we took a random sample of 100 from each of the three simulated populations. We compared preference updating in each of the two default-present conditions to the default-absent condition using Dunnett's (1955) test, and computed Cohen's d

for each comparison as a measure of carryover effect size, transformed so that a positive effect indicates directional carryover in favor of the default genre. We replicated the experiment 10,000 times at each level of η, thus conducting 90,000 simulated experiments in total.

Figure 10

RESULTS ACROSS 10,000 SIMULATED EXPERIMENTS AT EACH OF NINE LEVELS OF HETEROGENEITY OF PREFERENCE



Simulation results. To summarize the data from the simulated experiments, we computed the mean effect size and the proportion of significant ($p < .05$) positive effects for comedy defaults and for science defaults at each level of η. As can be seen in Figure 10, as the proportion of the population who prefer comedy videos increases, carryover of comedy defaults decreases while carryover of science defaults increases. The effect is more dramatic for comedy defaults than for science defaults, with the carryover effect shrinking by more than half as the proportion who prefer comedy increases from 10% to 90%. These findings illustrate the importance of taking preference heterogeneity into account when examining the downstream effects of defaults. When preference heterogeneity is ignored, the observed carryover of default effects is a function of the proportion in a population for whom the default alternative is preference-consistent versus inconsistent. Thus, researchers and choice architects risk erroneous conclusions about the efficacy of defaults for encouraging sustained behavior change when they do not consider consumers' prior preference in their analysis. These results also suggest that a highly-powered pilot of a default, producing a statistically significant result with a small effect size (e.g., $d = .15$), might lead to the conclusion that the downstream effect is too small to be efficacious. Of course, if the target segment is the small minority in the population that disfavors the target, this conclusion would be faulty. In fact, the effect size within the target segment is much higher than is revealed by an analysis that ignores preference heterogeneity.

Experience of choice consequences. Some consumption choices invariably lead to immediate consumption of the selected option. For example, choosing a music album to listen to on the streaming app on one's smartphone typically results in the first song on the selected album starting to play as soon as the choice has been made. As we have shown related downstream choices behavior is likely to be influenced by carryover from a default choice architecture used in the app if the defaults are preference-inconsistent and lead to enjoyable outcomes. Other

consumption choices are necessarily separated in time from the experience of their consequences. For example, consumers can still order music albums in the form of compact discs (CDs) from an online retailer (as the first author has recently done on behalf of his internet-averse father), but cannot listen to the albums until they have been delivered. In the meantime, the consumer might make other music choices, such as choosing a radio station to listen to or selecting a CD to play from their music library. Those intervening choices are less likely to be influenced by carryover from a default choice architecture experienced while ordering the CDs.

Many consumptions choices fall somewhere in between the two extremes described above, with some consumers experiencing choice consequences immediately and others experiencing some delay, either by their own choice or not, during which related decision might be made. For instance, one consumer might build a playlist of albums in a music-streaming app and listen to it immediately. Another consumer who builds a playlist in the same app might not listen to it until several days have passed, either because that was their plan all along or because their plan to listen immediately is interrupted by some other life priority. Our findings suggest divergence in the carryover of defaults effect across these two consumers, with carryover more likely to emerge for the music preferences of the consumer who immediately listens to their new playlist. How does the proportion of consumers who experience the consequences of their nudged choices influence carryover of default effects? To help answer this question, we conducted simulated experiments to detect how carryover of default effects manifests across populations that vary in this proportion.

Simulation method. We simulated the carryover of default effects to preference across different levels of heterogeneity (η) in the experience of choice consequences in a population using the data obtained from Experiment 3. Recall that Experiment 3 used a 2 (default: present

vs. absent) x 2 (experience: yes vs. no) between-subjects, factorial design in which we manipulated experience of choice consequences by controlling whether or not participants watched their chosen videos as part of the five choice trials. Carryover, indicated by greater preference updating in favor of the preference-inconsistent video genre in the default-present (vs. absent) condition, was observed only when participants had watched their selected videos. We used the condition assignment and preference updating data from Experiment 3 to simulated populations that vary in the proportion who experience the consequences of their choices.

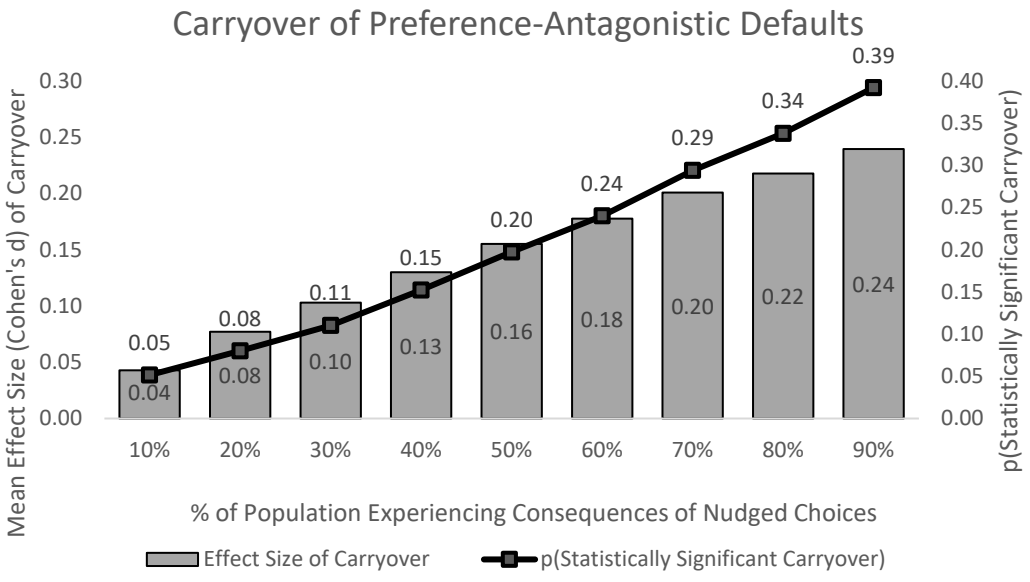
We simulated nine levels of heterogeneity of experience, specified by the population proportion that experiences the consequences of their choices (η = .10, .20, .30, .40, .50, .60, .70, .80, .90). For each condition (default-absent, default-present), at each level of η, we simulated a population of N=100,000 by sampling with replacement from the Experiment 3 data in the proportion specified by η. That is, to simulate a population in which 10% experience the consequences of their choices in a default-absent (present) choice architecture, we took 10,000 samples with replacement from the default-absent-experience-yes (default-present-experience-yes) cell and 90,000 samples with replacement from the default-absent-experience-no (default-present-experience-no) cell. For each simulated experiment, we took a random sample of 100 from each simulated population, performed a two-sided Welch's two-sample t-test, and computed Cohen's d for the carryover effect size. A positive effect indicates carryover, such that observed preference updating in favor of the preference-inconsistent video genre is greater in the default-present condition. We replicated the experiment 10,000 times at each level of η, thus conducting 90,000 simulated experiments in total.

Simulation results. To summarize the data from the simulated experiments we computed the mean effect size and the proportion of significant ($p < .05$) positive effects at each level of η.

As can be seen in Figure 11, carryover of the default effects increases with the proportion of the population who experience choice consequences. For instance, in a population in which 20% experience choice consequences, the effect size (d = .08) is half that observed when 50% of the population experience choice consequences (d = .16), which is itself only two-thirds the effect observed when 90% of the population experience choice consequences (d = .24). Naturally, the probability of observing a statistically significant carryover also increases with the proportion of the population who experience choice consequences.

Figure 11

RESULTS ACROSS 10,000 SIMULATED EXPERIMENTS AT EACH OF NINE LEVELS OF PROPORTION OF THE POPULATION EXPERIENCING THE CONSEQUENCES OF NUDGED CHOICES



Implications. The simulation results point to important implications for choice architects and researchers interested in designing, testing and implementing interventions that encourage lasting behavior change. Where experience of choice consequences is variable, carryover of

default effects depends on the proportion of the population who experience those consequences. It is in that segment that carryover is most likely to emerge. Unaware that experience of choice consequences moderates carryover, a choice architect piloting an intervention in a context in which very few consumers immediately experience choice consequences risks arriving at the erroneous conclusion that their intervention will be ineffective at promoting sustained behavior change. By contrast, a choice architect piloting an intervention in a context in which most consumers immediately experience choice consequences risks concluding that their intervention will be effective in the long term in a context in which fewer consumers immediately experience choice consequences. When evaluating the enduring impacts of defaults, practitioners and researchers should consider the temporal relationship between choices, consumption of the selected products, and related default-free choices that may be made before or after consumption of the of the selected options.

Another important implication of the dynamic role of experienced choice consequences in the carryover of default effects is that choice architects might want to consider using other tools at their disposal to promote the experience of choice consequences before consumers make other choices relevant to the target behavior. For example, in a restaurant setting, choice architects nudging the consumption of healthier meal options might consider removing dessert items from the main menu and offering a separate dessert menu only after consumers have experienced the consequences of their nudged dining choices. This might help boost choice of healthier dessert options, or at least reduce the risk of backfire effects, which might otherwise prompt greater choice of unhealthy desserts.

In practice, choice architects in digital settings often have available to them massive amounts of data about consumers. Appropriate analytic approaches with good predictive validity

for identifying consumer preferences may be employed in the design of smart defaults that account for consumers' prior preference in a particular domain of interest to the choice architect. This could be particularly important in contexts in which choice consequences are not immediately experienced, where it would be desirable to avoid backfire effects among consumers who favor the behavior a choice architect is aiming to encourage. For example, in product configuration platforms, an algorithm might be implemented that institutes default choice architecture only for consumers predicted to disfavor the target behavior, while customizing another persuasion tool, such as an informational message, for consumers who favor the target behavior.

Future Directions

It is important to acknowledge that we held constant the quality of the experienced consequences in our experiments. Specifically, choice consequences were not aversive. Our framework suggest that the downstream effects of defaults for aversive outcomes should mirror those revealed in the present research. Our theorizing about how defaults interact with prior-preference to produce preference updating from experienced outcomes suggests that nudging aversive experiences using preference-consistent defaults should result in backfire effects. Given that consumers tend to expect positive outcomes from their preferred alternatives, experiencing aversive outcomes will tend to produce a negative incongruity. However, backfire effects should be attenuated when preference-inconsistent defaults nudge consumers to experience unfavorable outcomes, because lower expectations allow for less potential negative incongruity when nudged choice consequences are experienced.

Backfire effects have been found in some contexts where choice consequences have not been experienced (Donkers et al. 2020; Wisdom et al. 2010). Our framework goes some way in

explaining these findings and offers one path to reconciling them with the more common null findings in the literature. That is, backfire effects are expected due to the attenuated preference updating that follows choice alone when defaults are present, but are detected in aggregate only when the immediate effects of defaults are small. While we did not observe backfire effects in aggregate in our experiments, results of our mediation analyses suggest that when the immediate effect of defaults is controlled for, backfire effects following choice are present at the individual level. As the immediate effect of defaults increases, so too does the aggregate accumulation of weaker preference updating, which could make it difficult to detect backfire effects. It may be that prior null results are a function of this aggregation.

However, both Wisdom et al. (2010) and the field study in Donkers et al. (2020) revealed downstream backfire effects following relatively strong immediate default effects, suggesting other mechanisms might also be in operation. A feature unique to both of those examples, and not addressed in the present work, is the salience of trade-offs that support preference construction via compensatory path dependencies across choices. Wisdom and colleagues observed a licensing effect, where nudged healthy food choices led to subsequent higher calorie food choices. Donkers and colleagues observed lower-price choices following nudged increases in spending, and higher-priced choices following nudged decreases in spending. Undoubtedly, trade-off considerations play an important role in preference construction across related choices, especially when a limited resource (e.g., money) or a limiting goal (e.g., total calorie consumption) is salient in the trade-off of relevant attributes. More work is needed to tease compensatory effects apart from the dynamic effects of defaults on preference updating.

Up to now, our discussions of backfire and carryover effects have generally been at the level of the individual. However, we suspect that individuals' perceptions of the alignment

versus opposition of defaults and their downstream effects differ primarily in degree and very rarely in direction. That is, whether a particular downstream effect is aligned with or opposed to an earlier default is, we propose, generally agreed upon even though different people may perceive the degree of alignment or opposition to be weaker or stronger. For example, we assume that an increase in preference for coffee will be broadly agreed to be aligned with a default choice architecture in which a large coffee cup size is preselected to encourage more coffee consumption, and is, thus, a carryover effect when it follows from choice made in that default choice architecture. Conversely, a decreased preference for coffee is opposed to the default, and is thus a backfire effect. While, by our assumption, we reject the possibility that the relationship might be reversed, we acknowledge that the perceived strength of the association between the default (i.e. large coffee cup size) and the particular downstream effect (i.e., increased preference for coffee) necessarily varies across consumers. The association between the defaults and the subsequent choices in our experiments was, necessarily, strong. This leaves open an important avenue for future research to extend these findings to more distant associations and avoid assumptions about the degree to which consumers perceive those associations by measuring those perceptions directly.

Another question ripe for investigation is the extent to which the carryover of default effects endures over time. Critically, the present research is the first to demonstrate carryover of default effects to preference and to show that the updating of preference mediates effects on subsequent behavior. We are encouraged by these findings to speculate that further. Although the present research measured carryover to subsequent choice behavior within the same experimental session, the carryover to preference suggest that these effects are likely to endure. Van Rookhuijzen and colleagues (2021) offered some evidence that this is the case. They

conducted two experiments demonstrating what they referred to as *temporal spillover*. They used defaults to nudge participants to voluntarily respond to a longer version of questionnaire. The default effects carried over to choice behavior for another questionnaire one day later. Venema and colleagues (2018) also showed a longer lasting effect in the context of habit formation for standing at sit-stand desks. Future research investigating the persistence of updated preference over time and the role of habit formation following repeated nudged choice is well advised.

Conclusion

If defaults are to prove effective interventions for encouraging enduring beneficial behavior change, choice architects need to better understand the downstream effects they might have (Dolan and Galizzi 2015). Since the publishing of "*Nudge*" Thaler and Sustein's (2008) seminal book on the topic,of choice architecture, scholars of public policy (Congiu and Moscati 2020; Datta and Mullainathan 2014; Dolan et al. 2010; Hansen and Jespersen 2013; Mongin and Cozic 2018), psychology (Dolan et al. 2012; Hollands et al. 2017), marketing (Chance et al. 2014; Johnson et al. 2012), and other allied fields' (Baldwin 2014; Hollands et al. 2013; Münscher, Vetter, and Scheuerle 2016) have produced numerous frameworks supporting practitioners in the design of effective choice architecture interventions. These frameworks offer choice architects theoretically grounded guidance for formulating choice architecture interventions. While these various frameworks differ in numerous ways, they share a common focus on immediate effects—the proximal behavioral response to an intervention—while downstream effects are neglected. This represents a gap that hinders advancement of theory and the application of choice architecture interventions to encourage behavior change that lasts.

The present research contributes to addressing this gap by offering a framework for understanding how and under what conditions default effects carry over to influence preference

and subsequent choice behavior. The carryover of default effects is more likely when defaults are preference-inconsistent, the consequences of nudged choices have been experienced, and those consequences are not aversive. Our framework offers choice architects a roadmap to avoid backfire effects and maximize the benefits from using defaults to encourage beneficial behavior change.

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