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Defaults are extremely effective at covertly guiding choices, which raises concerns about how to employ them ethically and responsibly. Consumer advocates have proposed that disclosing how defaults are intended to influence choices could help protect consumers from being unknowingly manipulated. This research shows that consumers appreciate transparency, but disclosure does not make defaults less influential. Seven experiments demonstrate that disclosure alters how fair consumers perceive defaults to be but does not attenuate default effects because consumers do not understand how to counter the processes by which defaults bias their judgment. Given that defaults lead consumers to focus disproportionately on reasons to choose the default even with disclosure, debiasing default effects requires that consumers engage in a more balanced consideration of the default and its alternative. Encouraging people to articulate their preferences for the default or its alternative, as in a forced choice, shifts the focus away from the default and reduces default effects.

Keywords: default effects, transparency, disclosure, debiasing, consumer welfare

Ethically Deployed Defaults: Transparency and Consumer Protection Through Disclosure and Preference Articulation

Defaults are a powerful tool for influencing consumers' decisions, which raises concerns about how to ensure that they be used ethically and responsibly (Smith, Goldstein, and Johnson 2013). Policy makers and other concerned parties have suggested that disclosure may be advisable or even necessary to protect consumers from being covertly manipulated (e.g., Electronic Privacy Information Center 2011; House of Lords 2011), assuming that ensuring that

people know their options and what will happen if they take no action could help debias their decisions.

Yet many defaults are effective not because people are unaware of what the default is but because being aware of the default makes salient the advantages of the status quo and the disadvantages of switching (Dinner et al. 2011; Johnson, Häubl, and Keinan 2007; Kahneman, Knetsch, and Thaler 1991; Tversky and Kahneman 1991), leads people to assume that the designated option is recommended (Brown and Krishna 2004; McKenzie, Liersch, and Finkelstein 2006), and provides an easy way out of an otherwise difficult decision (Johnson and Goldstein 2003; Samuelson and Zeckhauser 1988). Indeed, pointing out the existence of a default and what decision will go into effect if decision makers do not actively choose something else does not reduce the default's efficacy (Loewenstein et al. 2015). Perhaps the problem is that even if people know what the default is, they may not understand how it biases choices. Would informing consumers about how defaults are intended to affect their choices enable them to scrutinize the persuasive attempt and make an informed and active decision?

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This research examines whether disclosing how defaults are intended to influence choices reduces default effects. We propose that how consumers respond to defaults depends not just on their awareness of how defaults bias choice or their motivation to resist that bias but also their ability to counter the process by which defaults bias choice. We predict that although disclosure may change consumers' perceptions of defaults, disclosure alone will not debias default effects because consumers are unaware of the processes by which defaults affect their behavior and are thus unable to counter them. Rather, we suggest that disclosure must be supplemented with more active interventions that target the processes underlying defaults to offer effective protection.

THE CASE FOR TRANSPARENCY

The case for transparency is based on the premise that people respond differently to persuasion tactics when they know that others are trying to influence them (Friestad and Wright 1994). Part of what makes defaults effective is that consumers are often unwilling or unable to put effort into making an active decision and passively defer to the default option (Johnson and Goldstein 2003; Samuelson and Zeckhauser 1988). Letting people know what defaults are intended to do might prompt them to more carefully consider whether a default is in their best interest. People are more likely to scrutinize persuasive attempts when prompted to interpret an action as a persuasive tactic (Friestad and Wright 1994; Petty and Cacioppo 1986). More generally, encouraging people to slow down and think carefully about their decisions may help them consider decisions from multiple reference points and be less influenced by the way that choices are presented (Smith 1985). Thus, disclosing the intention behind the default could help to protect consumers by reminding them that others are trying to influence their choices and that it may be in their best interest to actively identify whether the default option is best for them.

Defaults are also effective because consumers typically infer that an option is designated as the default because it is regarded as the best option (Brown and Krishna 2004; McKenzie et al. 2006). Thus, transparency may protect consumers from defaults by encouraging them to more skeptically evaluate the potential effect of the default. If the intended effect seems unethical or otherwise self-serving, consumers may engage in counterarguing (Petty and Cacioppo 1986; Petty, Ostrom, and Brock 1981) or even outright resistance (Brehm 1966; Hass and Grady 1975). Disclosing how a default is meant to influence choices could make that default seem less fair or ethical and perhaps weaken its effect when it seems to serve the default-setter's interests.

WHY DISCLOSURE MAY BE INSUFFICIENT

However, prompting consumers to invest more effort into decisions may not make persuasion tactics like defaults less effective if people do not know how to counter a default's influence. Decision makers must have not only the motivation to cope with persuasive attempts but also the ability (Wegener and Petty 1997), cognitive capacity (Campbell and Kirmani 2000), and control over the outcome (Barry and Shapiro 1992) to adjust their judgments. In addition, people must be aware of and able to execute the necessary tactics to

defend themselves against the attempt (Barry and Shapiro 1992; Larrick 2004; Sagarin et al. 2002).

But awareness is of limited help in many judgment contexts. Research on debiasing has shown that warning people about the possibility of bias, informing them about the direction of a bias, and providing feedback about susceptibility to bias do not improve decision making, and training programs yield only modest improvements (Fischhoff 1982). For example, anchoring influences quantitative judgments even when anchors are known to be irrelevant (Strack and Mussweiler 1997; Tversky and Kahneman 1974), unreliable (Loftus 1979), or manipulative (Galinsky and Mussweiler 2001; Hastie, Schkade, and Payne 1999). Regarding disclosure, participants who receive conflict-of-interest disclosures may have no reference for comparison and may not appreciate the significance of the disclosure (Cain, Loewenstein, and Moore 2011).

We argue that people do appreciate default disclosure but that disclosure alone does not adequately enable them to counter the default's effect. A fundamental reason why defaults are effective is that a default serves as a reference point with which other options are compared, and consequently, losses relative to that reference point loom larger than equivalent-sized gains (Kahneman et al. 1991; Tversky and Kahneman 1991). Query theory proposes that this is because people first identify reasons for choosing the reference option and against choosing the alternative, and these first queries have a greater impact on choices than subsequent queries, encouraging maintenance of the default option (Dinner et al. 2011; Johnson et al. 2007). People cannot "think their way out of" framing like this by transforming a problem into a frame-independent representation (LeBoeuf and Shafir 2003); like visual illusions, framing cannot be avoided with extra thought or awareness (Arkes 1991; Thaler 1991). We thus predict that the tendency for people to view defaults as reference points with which other options are compared makes defaults effective even when they and their intended influence are disclosed. Formally:

H₁: Disclosing how defaults are intended to affect choices changes how fair people believe defaults to be but does not reduce default effects, because people do not know how to counter the processes by which defaults bias their judgment.

DEBIASING DEFAULT EFFECTS

The most effective method of reducing bias is to target the processes underlying the bias (Fischhoff 1982; Milkman, Chugh, and Bazerman 2009; Slovic and Fischhoff 1977). If the reference point set by a default leads people to focus on reasons for choosing the default, then encouraging people to simultaneously consider the default and its alternative, and articulate why they prefer one or the other, may help them engage in more balanced reasoning (Sieck and Yates 1997; Tetlock 1992), thereby reducing the default's effect. For instance, encouraging people to "consider the opposite" primes thoughts that normally would not be accessed (Arkes 1991; Lord, Lepper, and Preston 1984) and reduces errors related to many decision biases (Larrick 2004; Milkman et al. 2009; Mussweiler, Strack, and Pfeiffer 2000; Soll, Milkman, and Payne 2014).

Indeed, changing people's thought processes can attenuate default and other framing effects (Dinner et al. 2011; Johnson et al. 2007; Knoll et al. 2015). Asking people to list reasons for choosing the alternative to the default before listing reasons to choose the default attenuates default effects relative to when these queries are reversed. Note that our preference-articulation intervention is distinct from these interventions, which bias people's thoughts in the opposite direction from the natural thought process people engage in when presented with a default, creating a weighted default choice ("Why would you prefer A over B?"). Our task creates a more neutral dichotomous choice ("Why would you prefer A or B?"), which is meant to serve as a more impartial, evenhanded intervention that encourages people to think about the default and its alternative simultaneously, as in a forced choice. This should help people engage in a more balanced consideration of the alternatives to make a less biased choice. Namely, we predict the following:

H₂: Given that setting an option as the default leads people to focus on reasons to choose it even when its intended effects are disclosed, encouraging people to engage in a more balanced consideration of the default and its alternative can help debias default effects.

In support of H₁, we show that defaults are effective even when their intended effects are disclosed (Experiments 1a–c). Disclosure is not without consequences, however; it can influence consumers' perceptions of the default's fairness and ethicality and consumers' willingness to work with the default-setter again in the future (Experiments 2a–b). In support of H₂, an intervention that encourages consumers to simultaneously consider both the default and its alternative can diminish default effects (Experiment 3) by reducing the tendency for consumers to focus on reasons to choose the default and prompting them to engage in a more balanced consideration of the choice options (Experiment 4). We conclude that disclosure increases transparency but cannot ensure consumer protection. To protect consumers, disclosure must be supplemented with an intervention that targets the thought processes underlying default effects.

EXPERIMENTS 1A–C: DO DEFAULTS WORK WHEN DISCLOSED?

In Experiments 1a–c, we explore whether defaults influence consumers' choices even when the consumers are made aware of the defaults' potential influence. We first investigate the effect of disclosure in a domain wherein consumer protection is of great concern: privacy in social networks. Participants imagined joining a social network and choosing how much of their information to share. Participants either shared no information by default and could opt in to sharing more, or shared all information by default and could opt out of sharing some or all of it. Some participants also received a disclosure about the possible influence of the default before they chose. Experiment 1b replicates these results in an incentive-compatible field setting by offering Amazon Mechanical Turk (MTurk) workers bonus payments for participating in optional studies presented in an opt-in or opt-out format and either with or without disclosure of the possible influence of the default. Finally, Experiment 1c involves a one-shot decision in a nutrition-related field setting. Students were offered free hot chocolate, with or without

whipped cream by default, and some students were told the potential influence of the default. In all studies, we predicted that defaults would be impervious to disclosure, such that participants would be more likely to select the default options, regardless of whether they were informed of the defaults' intended effects.

Experiment 1a: Method

Participants. Participants were 363 students at a West Coast university who filled out this survey in exchange for course credit.

Procedure. Participants imagined that they were joining a social network called The Meter and indicated whether they would like to share their contact information, location, posts, and/or photos with search engines, advertisers, other users, friends, and/or friends of friends (4 information items \times 5 potential recipients = 20 choices in total). Participants were randomly assigned to one of four conditions in a 2 (choice format: opt-in or opt-out) \times 2 (disclosure: present or absent) between-subjects design. Participants in the opt-in conditions learned that by default, none of their information would be shared, but they could check a box next to each item they wished to share with each of the possible recipients. In the opt-out conditions, participants learned that all of their information would be automatically shared with everyone, but they could uncheck a box next to each item they preferred not to share with each recipient. Those in the disclosure conditions were told about the default and its intended effect before they made their selections (see Appendix A for this and all subsequent disclosure wordings); those in the nondisclosure conditions received no additional information.

Experiment 1a: Results and Discussion

Defaults influenced choices regardless of disclosure. Participants shared more personal information with more parties in the opt-out conditions ($M = 6.41$, $SD = 4.10$) than in the opt-in conditions ($M = 4.81$, $SD = 2.47$; $F(1, 359) = 19.46$, $p < .001$, $\eta_p^2 = .05$). However, the amount of information participants shared did not vary depending on the presence or absence of disclosure ($F(1, 359) = 1.05$, $p = .31$, $\eta_p^2 = .003$) or the interaction between the default and the disclosure ($F(1, 359) = .005$, $p = .94$, $\eta_p^2 < .001$). In the next experiment, we seek additional support for this claim through an incentive-compatible field experiment with real financial consequences.

Experiment 1b: Method

Participants. We recruited 429 adults through MTurk. Each participant received a minimum of \$.15 in Amazon.com credit.

Procedure. Participants were invited to complete surveys in exchange for payment. Participants were randomly assigned to one of four conditions in a 2 (choice format: opt-in or opt-out) \times 2 (disclosure: present or absent) between-subjects design. Participants in the opt-in conditions were told they would complete one survey and receive \$.15 by default, but they could complete up to 10 additional surveys and receive an additional \$.02 each by checking the boxes next to any optional surveys they wished to add (i.e., measures of mood, need for uniqueness, risk-seeking, optimal stimulation level, need for cognition, self-monitoring, need for closure, well-being,

impulsivity, and price consciousness). Participants in the opt-out conditions would complete 11 surveys and receive \$.35 by default, but they could complete as many as 10 fewer surveys and receive \$.02 less for each if they unchecked the boxes next to surveys they wished to omit. Those in the disclosure present conditions were told about the default and its intended effect before making their selections, whereas those in the disclosure absent conditions were not.

Experiment 1b: Results and Discussion

Even with real financial consequences at stake, disclosure did not reduce the influence of defaults on behavior. Participants completed more surveys (and received more money) when doing so was the default ($M = 9.56$, $SD = 1.71$) than when it was not ($M = 7.03$, $SD = 3.78$; $F(1, 425) = 76.94$, $p < .001$, $\eta_p^2 = .15$). However, the number of surveys participants completed did not vary depending on disclosure ($F(1, 425) = 1.45$, $p = .23$, $\eta_p^2 = .003$) or the interaction between the default and the disclosure ($F(1, 425) = .94$, $p = .33$, $\eta_p^2 = .002$). Next, we examine whether these results replicate in a one-shot decision in a health-related field setting.

Experiment 1c: Method

Participants. We offered 210 adults free hot chocolate in a common area of a Midwestern university over the course of two days. One participant was excluded from analysis because the research assistant serving hot chocolate accidentally asked if she wanted whipped cream in the no-whip default condition.

Procedure. Participants were offered free hot chocolate, with or without whipped cream by default. Participants were assigned to a condition in a 2 (choice format: opt-in or opt-out) \times 2 (disclosure: present or absent) between-subjects design. In the opt-in conditions, a large sign stated that the hot chocolate would be served without whipped cream unless specified otherwise. In the opt-out conditions, the sign stated that the hot chocolate would be served with whipped cream unless specified otherwise. In the disclosure present conditions, another sign informed participants of the default and its intent. The signs were rotated after approximately every 25 participants and were presented in a different order each day to account for possible order effects. Research assistants, blind to the hypothesis, recorded orders and served hot chocolate.

Experiment 1c: Results and Discussion

Participants were more likely to get whipped cream when it was included by default than when it was not, both with disclosure (90% in the whipped cream default condition vs. 2% in the no-whip default condition) and without (100% in the whipped cream default condition vs. 6% in the no-whip default condition). Due to how rare it was for participants to switch from the default, we analyzed the data using a penalized maximum likelihood estimation (Firth 1993). This analysis revealed a main effect of whether whipped cream was included by default (Wald's $\chi^2 = 51.71$, $p < .001$, odds ratio = 35.65) and a marginal main effect of disclosure (Wald's $\chi^2 = 4.02$, $p = .06$, odds ratio = .34) but, importantly, no interaction (Wald's $\chi^2 < .001$, $p = .89$, odds ratio = .01). To be sure that disclosure had no effect on participants' likelihood of choosing the default, we reexamined the effect of disclosure on choice using a Pearson's χ^2 test with a simulated p -value

based on 10,000 replicates. This test also suggested that the effect of disclosure was not reliable ($\chi^2 = .92$, $p = .62$), as did a permutation test ($\chi^2 = .92$, $p = .62$). In summary, participants were more likely to order a less healthy beverage when it was set as the default, regardless of whether they were made aware of the default and its intended effect.

Discussion

Together, these experiments show that people tend to retain the default option regardless of whether the intention behind the default is disclosed, even when there are real consequences.

It is possible, however, that participants in these studies did not notice, understand, or care about the disclosure's message. Our next studies demonstrate that disclosure of a default's intent and effects does matter to participants; it simply does not affect their choices. We examine whether the effect of disclosure depends on whether the default influences behavior in a way that benefits the default-setter or others, or, with the consequences of the default held constant, whether the default-setter's motives are benevolent or self-serving. This approach has multiple benefits: it allows us to demonstrate that disclosure is noticed and comprehended and to test whether lack of consumer motivation or ability explains defaults' robustness against disclosure. If a lack of motivation to scrutinize the choice underlies defaults' imperviousness to disclosure, then alerting consumers that defaults bias choices might reduce the effectiveness of defaults intended to benefit the default-setter. If a lack of ability to reframe the decision is instead the primary mechanism, as we suggest, then disclosure should have little influence on default effects, regardless of the motives of the default-setter or the potential consequences of the default.

However, we do expect disclosure to influence how fair or ethical the default is perceived to be, as well as consumers' interest in working with the default-setter again in the future. We anticipate that people will judge the way a choice is formatted—opt-out or opt-in—to be more fair and ethical, and that they will be more willing to work with the default-setter again, when they are told up front how the choice format is intended to affect their choices. This prediction is consistent with past research that has shown that explicit disclosure improves perceptions of covert marketing tactics (Wei, Fischer, and Main 2008). Moreover, we also expect disclosure to shift perceptions of fairness depending on how the choice format is intended to affect behavior: people will be especially likely to react negatively if they learn that a default is meant to coerce them into choosing an option not in their best interest, perceiving opt-out formatting to be less fair than opt-in formatting when it nudges people toward options that are perceived to primarily benefit the default-setter.

EXPERIMENTS 2A–B: DOES DISCLOSURE AFFECT PERCEPTIONS OF DEFAULTS?

One important question about disclosure is whether it has any effect at all on consumers. Here, we show that although disclosure does not reduce default effects, it does change consumers' perceptions of the defaults. Experiment 2a examines whether the effect of disclosure varies depending on whether people believe the default is meant to nudge

them toward an option that benefits society or the default-setter. We disclosed the intent of the default either before participants decided or not at all. Experiment 2b examines whether the effect of disclosure varies depending on the expressed motives of the default-setter, independent of the consequences of the default. Participants were presented with a choice of energy-saving apartment amenities in either an opt-in or an opt-out format, were told that the landlord wanted them choose those amenities either to help the environment or to help him qualify for a tax break, and received a disclosure regarding how the choice format was intended to affect their choices either before or after they made their decisions. We show that disclosure influences perceptions of fairness, ethicality, and attitudes toward working with the default-setter, but default effects are impervious to disclosure.

Experiment 2a: Method

Participants. We recruited 779 adults through MTurk. Each participant received \$.15 in Amazon.com credit.

Procedure. Participants were randomly assigned to one of eight versions of a scenario, in a 2 (choice format: opt-in or opt-out) \times 2 (disclosure: present or absent) \times 2 (type of upgrades: green or premium) between-subjects design. Participants imagined touring a new apartment complex offering a variety of optional green or premium amenities (see Appendix B for a full list of amenities). Participants in the opt-in conditions were told that none of the upgraded amenities were included in the rent, but if they would like any of them to be installed, the landlord would add a small amount (between \$2 and \$10) to their monthly rent for each upgraded amenity they added. In the opt-out conditions, all of the upgraded amenities were included in the rent, but if the participants would like any of them not to be installed, the landlord would deduct a small amount from their monthly rent for each upgraded amenity they omitted. Those in the disclosure present conditions were told about the default and its intended effect before they chose amenities, and those in the disclosure absent condition were not. After choosing, participants rated the fairness of their assigned default, specifically, “How fair was it to you, as a tenant, for the landlord to make green amenities [premium amenities/standard amenities] the default?” on a scale ranging from 1 = “completely unfair” to 7 = “completely fair.” We also asked participants which amenities were the default, as an attention check.

To verify that participants perceived the green amenities to benefit society more and the premium amenities to benefit the business more, a separate sample of participants rated how beneficial the amenities would be for the business and for society on a scale from 1 = “not at all” to 7 = “very much.” To rule out the possibility that the results were due to differences in how appealing the amenities were, participants also rated how beneficial the amenities would be to themselves and how well they liked the amenities on a scale from 1 = “not at all” to 7 = “very much.” As intended, the green amenities were judged to benefit society more ($M = 5.64$, $SD = 1.54$) than the premium amenities ($M = 2.65$, $SD = 1.59$; $F(1, 189) = 182.27$, $p < .001$, $\eta_p^2 = .49$), and the premium amenities were judged to benefit the business marginally more ($M = 5.77$, $SD = 1.27$) than the green amenities ($M = 5.38$, $SD = 1.56$; $F(1, 189) = 3.32$, $p = .07$, $\eta_p^2 = .02$). There were no differences in how

much the green or premium amenities were judged to benefit the self ($F(1, 189) = .03$, $p = .86$, $\eta_p^2 < .001$) or in how well liked they were ($F(1, 189) = .66$, $p = .42$, $\eta_p^2 = .003$).

Experiment 2a: Results

Attention check. Analysis of the attention check indicated that most participants (95%) correctly identified whether the amenities were standard or upgraded (i.e., premium or green) by default. The overall pattern of results is the same if we exclude participants who failed the attention check.

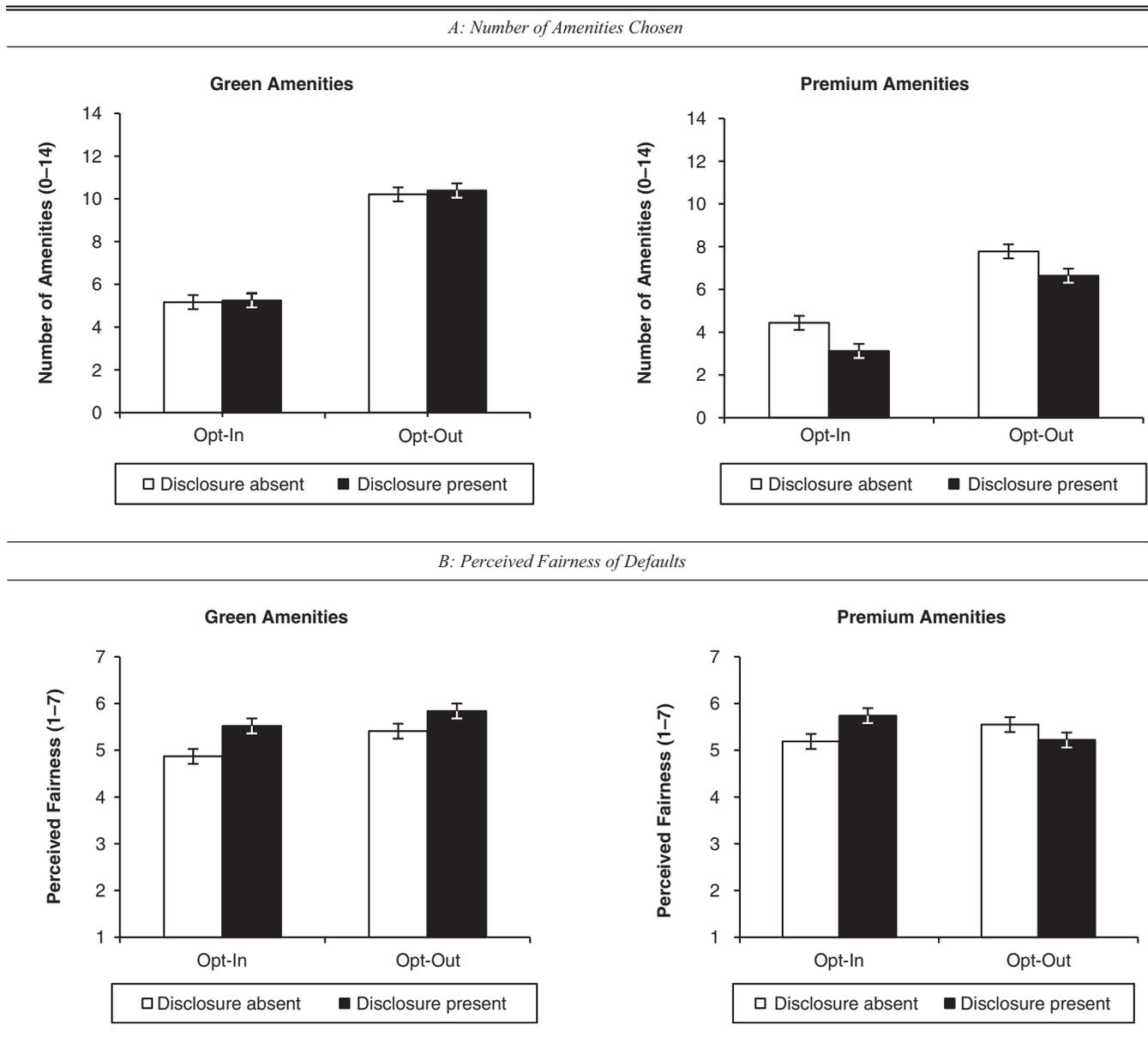
Number of amenities chosen. A main effect of choice format ($F(1, 771) = 334.99$, $p < .001$, $\eta_p^2 = .30$) and a lack of interaction between choice format and disclosure ($F(1, 771) = .08$, $p = .78$, $\eta_p^2 < .001$) show that defaults remained effective despite disclosure. Specifically, participants chose more upgraded amenities when they were included by default ($M = 8.76$, $SD = 3.79$) than when they were not ($M = 4.49$, $SD = 3.14$). The three-way interaction was also not significant ($F(1, 771) = .008$, $p = .93$, $\eta_p^2 < .001$), suggesting that default effects were impervious to disclosure regardless of the type of amenities available.

Although disclosure did not reduce default effects, it did affect choices more generally. A main effect of disclosure indicated that participants chose fewer upgraded amenities with disclosure ($M = 6.36$, $SD = 4.14$) than without ($M = 6.92$, $SD = 4.01$; $F(1, 771) = 5.55$, $p = .02$, $\eta_p^2 = .007$). That is, participants chose fewer upgraded amenities when it was disclosed that the landlord wanted them to choose those upgrades, regardless of whether the choice format required them to opt in or opt out. An interaction between disclosure and the type of amenities ($F(1, 771) = 8.50$, $p = .004$, $\eta_p^2 = .01$) shows that this asymmetry was largely driven by the premium amenities: participants chose fewer premium amenities when disclosure was present ($M = 4.88$, $SD = 3.58$) than absent ($M = 6.12$, $SD = 3.60$; $F(1, 387) = 11.53$, $p = .001$, $\eta_p^2 = .35$) but chose a similar number of green amenities with disclosure and without ($M = 7.85$, $SD = 4.15$ vs. $M = 7.71$, $SD = 4.24$; $F(1, 388) = .10$, $p = .76$, $\eta_p^2 < .001$). Thus, although disclosing how defaults are intended to affect people’s choices does not attenuate default effects, identifying which options a default-setter would like people to choose can make people less inclined to choose those options, especially when the chosen options are likely to benefit the default-setter but not society at large.

Finally, a main effect of the type of amenities indicates that participants chose more upgraded amenities when the amenities were green ($M = 7.78$, $SD = 4.19$) than when they were premium ($M = 5.50$, $SD = 3.64$; $F(1, 771) = 93.92$, $p < .001$, $\eta_p^2 = .11$). An interaction between the choice format and the type of amenities ($F(1, 771) = 12.69$, $p < .001$, $\eta_p^2 = .016$) shows that the boost in green amenities in the opt-out ($M = 10.30$, $SD = 3.33$) versus opt-in conditions ($M = 5.21$, $SD = 3.33$; $F(1, 388) = 228.05$, $p < .001$, $\eta_p^2 = .37$) was greater than the corresponding boost in premium amenities in the opt-out ($M = 7.21$, $SD = 3.36$) versus opt-in conditions ($M = 3.77$, $SD = 2.77$; $F(1, 387) = 111.36$, $p < .001$, $\eta_p^2 = .22$; Figure 1). In other words, making upgraded amenities the default was more effective at nudging people to choose more upgrades when those upgrades were green and perceived to benefit society than when they were premium and perceived to benefit the landlord.

Figure 1

EXPERIMENT 2A: NUMBER OF AMENITIES CHOSEN AND PERCEIVED FAIRNESS OF DEFAULTS ACCORDING TO CHOICE FORMAT, DISCLOSURE, AND TYPE OF UPGRADES



Perceived fairness. Even though disclosure did not attenuate default effects, it did affect how fair the default was perceived to be. A main effect of disclosure reveals that participants thought the way the choice was formatted (opt-out or opt-in) was fairer when disclosure was present ($M = 5.58$, $SD = 1.58$) versus absent ($M = 5.25$, $SD = 1.62$; $F(1, 771) = 8.19$, $p = .004$, $\eta_p^2 = .011$). That is, participants perceived the default to be fairer when the default-setter informed them up front about how the choice format was intended to affect their behavior. There was no overall main effect of the choice format ($F(1, 771) = 2.45$, $p = .12$, $\eta_p^2 = .003$) or the type of amenities ($F(1, 771) = .02$, $p = .90$, $\eta_p^2 < .001$) on perceived fairness, and the three-way interaction was not significant ($F(1, 771) = 1.96$, $p = .16$, $\eta_p^2 = .003$; Figure 1).

All three two-way interactions were significant, however. First, there was an interaction between choice format and disclosure ($F(1, 771) = 5.85$, $p = .02$, $\eta_p^2 = .008$). When the intent behind the default was not disclosed, people believed it was fairer to be allowed to opt out of upgraded amenities that they did not want in exchange for a discount ($M = 5.48$, $SD = 1.58$) than to have to opt in and pay extra for additional upgrades ($M = 5.03$, $SD = 1.63$; $F(1, 385) = 7.76$, $p = .006$, $\eta_p^2 = .02$). However, when people learned that designating an option as the default makes it more likely to be chosen, opt-in and opt-out choice formats seemed equally fair ($M_{\text{opt-in}} = 5.63$, $SD = 1.62$ vs. $M_{\text{opt-out}} = 5.53$, $SD = 1.55$; $F(1, 388) = .38$, $p = .54$, $\eta_p^2 = .001$). We suspect this is because without knowing how the defaults

were intended to affect their behavior, people were more put off by the prospect of having to pay a surcharge to add amenities (when the choice was framed in an opt-in format) than by the prospect of forgoing a discount to keep upgrades (when the choice was framed in an opt-out format). This is consistent with loss aversion: price disparities are more palatable when framed as forgone discounts than surcharges (Kahneman et al. 1991).

Next, an interaction between the choice format and the type of upgrades ($F(1, 771) = 4.97, p = .03, \eta_p^2 = .006$) shows that the type of upgrades influenced whether participants perceived opt-in or opt-out choice formats to be fairer. When the available upgrades were green and perceived to benefit society, participants thought it was fairer to receive green upgrades by default ($M = 5.62, SD = 1.54$) than to have to opt in for them ($M = 5.19, SD = 1.73; F(1, 388) = 6.80, p = .009, \eta_p^2 = .02$). However, when the upgraded amenities were premium and perceived to benefit the landlord, participants thought the choice format was equally fair regardless of whether the upgrades were included by default or not ($M = 5.38, SD = 1.57$ vs. $M = 5.46, SD = 1.56; F(1, 387) = .25, p = .62, \eta_p^2 = .001$).

Finally, there was also a marginal interaction between the type of amenities and disclosure ($F(1, 771) = 3.50, p = .06, \eta_p^2 = .005$). When the upgraded amenities were premium and perceived to benefit the default-setter, disclosure did not matter: participants thought the choice format (regardless of whether it was opt-out or opt-in) was equally fair regardless of disclosure ($M = 5.48, SD = 1.63$ vs. $M = 5.37, SD = 1.50; F(1, 387) = .49, p = .48, \eta_p^2 = .001$). But when the upgraded amenities were green and perceived to benefit society, participants thought the choice format was fairer with disclosure ($M = 5.68, SD = 1.53$) than without ($M = 5.14, SD = 1.73; F(1, 388) = 10.59, p = .001, \eta_p^2 = .027$). We suspect that this is because people perceived the landlord's actions to be fairer when they were explicitly reminded that he wanted them to choose the socially beneficial green amenities, regardless of whether he designed the choice to make them more or less likely to do so.

Notably, although the three-way interaction was not significant, disclosure did influence the degree to which the type of amenities affected how fair participants perceived the choice format to be. When the intent behind the choice format was not disclosed, people believed it was fairer to be allowed to opt out of upgrades in exchange for a discount than to have to opt in and pay a surcharge for upgrades, regardless of whether those upgrades were premium or green ($F(1, 385) = .34, p = .56, \eta_p^2 = .001$). However, when the fact that designating an option as the default makes it more likely to be chosen was disclosed, their perceptions of the fairness of the choice format depended on who they thought would benefit from the upgrades ($F(1, 386) = 6.73, p = .01, \eta_p^2 = .02$), such that people thought it was fairer to make upgrades opt-in than opt-out when they were premium and the business would benefit ($M_{\text{opt-in}} = 5.22, SD = 1.63$ vs. $M_{\text{opt-out}} = 5.84, SD = 1.41; t(194) = -2.21, p = .03, d = .41$) but equally fair to make upgrades opt-in versus opt-out when the upgrades were green and society would benefit ($M_{\text{opt-in}} = 5.52, SD = 1.63$ vs. $M_{\text{opt-out}} = 5.73, SD = 1.61; t(187.08) = 1.44, p = .15, d = .13$; equal variances not assumed). Thus, although without disclosure, people thought that making both premium and green upgrades opt-out rather than opt-in was fairer, with disclosure, they thought it

was fairer to make premium upgrades opt-in rather than opt-out and equally fair to make green upgrades opt-out or opt-in.

Experiment 2a: Discussion

Default effects persisted regardless of disclosure or the type of amenities. However, disclosure did make people less inclined to choose upgrades, regardless of whether they were the default. Disclosure also influenced how fair participants perceived an opt-out versus opt-in choice to be, depending on who could benefit from it. When people were not warned that the default was intended to affect their choices, they thought it was fairer to be allowed to opt out of upgrades that they did not want (and get a discount) than to have to opt in to the upgrades (and pay a surcharge). When people were told that defaults nudge people toward a particular option, people thought it was less fair to make premium, business-benefiting amenities opt-out versus opt-in, but equally fair to make green, society-benefiting amenities opt-out versus opt-in.

One thing to note is that the default-setter's desired outcome and the reason why he/she desires that outcome about may be distinct. For instance, a landlord might encourage a tenant to make an energy-saving choice not because the landlord cares about the environment but because it might save him/her money. In Experiment 2b, we show that even when a default-setter's motives are self-serving, disclosure of the default's intended influence does not diminish its effectiveness. Here, we present all participants with a choice of energy-saving apartment upgrades and directly manipulate the stated motives of the default-setter as either benevolent or self-interested. In all versions of the scenario, the landlord shared his motives up front. In addition, the landlord disclosed how the choice format was intended to influence choices either before or after participants made their decisions. This final manipulation enabled us to explore whether up-front disclosure affects people's choices, their perceptions of the ethicality of the choice format, or their willingness to work with the default-setter again.

Experiment 2b: Method

Participants. We recruited 817 adults through MTurk. Each participant received \$.15 in Amazon.com credit. People who had participated in Experiment 2a were excluded from participating in this experiment.

Procedure. Participants were randomly assigned to one of eight versions of a scenario, in a 2 (choice format: opt-in or opt-out) \times 2 (upgrade motive: society-benefiting or business-benefiting) \times 2 (disclosure: before or after choice) between-subjects design. Participants were offered a choice of energy-efficient apartment upgrades in an opt-in or opt-out format. In all conditions, the landlord explained why he wanted tenants to choose the upgrades before he presented them, such that in the society-benefiting motive conditions, the landlord indicated he would like to save energy and help the environment, and in the business-benefiting motive conditions, the landlord indicated he would like to qualify for a tax break and put some money back in his pockets. Finally, the landlord disclosed how the choice format was intended to affect their choices and reiterated his motives either before or after participants chose their amenities.

After participants chose their desired amenities, they responded to the question “How ethical or unethical do you think it was for this landlord to make green amenities ‘OPT-OUT’ [‘OPT-IN’] with the consequence that those amenities would be MORE [LESS] likely to be chosen (instead of making green amenities ‘opt-in’ [‘opt-out’] so they would be less [more] likely to be chosen)?” on a scale ranging from 1 = “completely unethical” to 7 = “completely ethical.” They also responded to the question “Given how the landlord handled this situation, how much would you like to work with him in the future?” on a scale ranging from 1 = “not at all” to 7 = “very much.” Finally, as an attention check, participants indicated whether the landlord wanted to help the environment or receive a tax break and whether the standard or energy-efficient amenities were the default.

Experiment 2b: Results

Attention check. Most participants correctly identified which amenities were the default (90%) and whether the landlord wanted to help the environment or receive a tax break (87%). The pattern of results is the same if we exclude participants who failed either or both checks.

Number of amenities chosen. There was a main effect of choice format ($F(1, 809) = 585.82, p < .001, \eta_p^2 = .42$) and no interaction between choice format and disclosure ($F(1, 809) = .35, p = .55, \eta_p^2 < .001$), indicating that defaults remained effective despite disclosure. Participants chose more upgraded amenities when they were included by default ($M = 11.22, SD = 3.38$) than when they were not ($M = 5.53, SD = 3.42$). Additionally, a main effect of type of upgrades ($F(1, 809) = 20.67, p < .001, \eta_p^2 = .025$) indicates that participants also chose more upgrades when the landlord wanted to help the environment ($M = 8.91, SD = 4.33$) than when he wanted to qualify for a tax break ($M = 7.85, SD = 4.48$). In other words, participants were more inclined to choose the upgrades that the default-setter wanted them to choose when his motives for offering them were benevolent rather than self-interested, regardless of whether he had set the default to nudge people to choose in his desired manner. However, the lack of a main effect of disclosure reveals that the number of upgrades participants chose did not vary depending on whether the landlord disclosed how the choice format was intended to affect tenants’ choices before ($M = 8.27, SD = 4.34$) or after they chose amenities ($M = 8.48, SD = 4.53; F(1, 809) = 1.48, p = .22, \eta_p^2 = .002$). There were no other significant interactions (all $ps > .34$; Figure 2).

Perceived ethicality. Participants’ perceptions of the ethicality of the default depended on the landlord’s motives and his transparency regarding how the choice format was intended to affect their choices, but their perceptions did not depend on whether the landlord attempted to nudge tenants toward the green amenities by making them the default. A main effect of motive ($F(1, 809) = 26.70, p < .001, \eta_p^2 = .03$) indicates that participants thought the landlord’s actions were more ethical when he wanted to help the environment ($M = 5.00, SD = 1.61$) than when he wanted to qualify for a tax break ($M = 4.40, SD = 1.77$). A main effect of disclosure ($F(1, 809) = 22.70, p < .001, \eta_p^2 = .03$) shows that participants also thought the landlord’s actions were more ethical when he disclosed the influence of the choice format before they made their choices ($M = 4.98, SD = 1.61$) than after ($M = 4.42, SD = 1.78$). But the lack of a main effect of

choice format ($F(1, 809) = 1.96, p = .16, \eta_p^2 = .002$) indicates that perceived ethicality was similar regardless of whether the landlord made standard amenities ($M = 4.77, SD = 1.75$) or green amenities ($M = 4.62, SD = 1.69$) the default. There were no significant interactions (all $ps > .55$; Figure 2).

Desire to work with the default-setter again. Participants’ desire to work with the landlord again depended on his motives and transparency but did not depend on whether he tried to nudge people toward the green amenities by making them the default. A main effect of motive ($F(1, 809) = 48.91, p < .001, \eta_p^2 = .06$) shows that participants were more willing to work with the landlord in the future when he wanted to help the environment ($M = 4.73, SD = 1.66$) than when he wanted to qualify for a tax break ($M = 3.92, SD = 1.77$). A main effect of disclosure ($F(1, 809) = 42.70, p < .001, \eta_p^2 = .05$) indicates that participants were also more willing to work with him again when he disclosed how the choice format was intended to affect their choices before they chose amenities ($M = 4.71, SD = 1.72$) versus after ($M = 3.95, SD = 1.73$). An interaction between disclosure and upgrade motive ($F(1, 809) = 7.89, p = .005, \eta_p^2 = .009$) indicated that disclosure had an especially positive effect on people’s desire to work with the landlord again when disclosure was up front ($M = 5.29, SD = 1.44$) rather than after the fact ($M = 4.20, SD = 1.69$) when the landlord wanted to help the environment ($F(1, 404) = 48.58, p < .001, \eta_p^2 = .11$), whereas disclosure had a more modest effect when it was up front ($M = 4.14, SD = 1.79$) versus after the fact ($M = 3.71, SD = 1.74$) when the landlord wanted a tax break ($F(1, 409) = 6.29, p = .01, \eta_p^2 = .02$). The lack of a main effect of choice format indicates that participants’ desire to work with the landlord again did not vary depending on whether he made the green amenities ($M = 4.60, SD = 1.65$) or the standard amenities ($M = 4.26, SD = 1.75; F(1, 809) = 1.48, p = .22, \eta_p^2 = .002$) the default. No other interactions were significant (all $ps > .19$; Figure 2).

Experiment 2b: Discussion

Experiment 2b shows that disclosing how defaults are intended to affect choices does not attenuate default effects, even when the default-setter’s motives for wanting people to choose a particular option are self-interested. Participants chose more upgrades when they were opt-out versus opt-in and when the landlord wanted to help the environment versus qualify for a tax break, but their choices were unaffected by disclosure. Disclosure did, however, lead people to perceive the choice format to be more ethical, regardless of whether it was opt-out or opt-in. In addition, it made people more inclined to work with the default-setter in the future, especially when he wanted to help society, but even when he wanted to help himself.

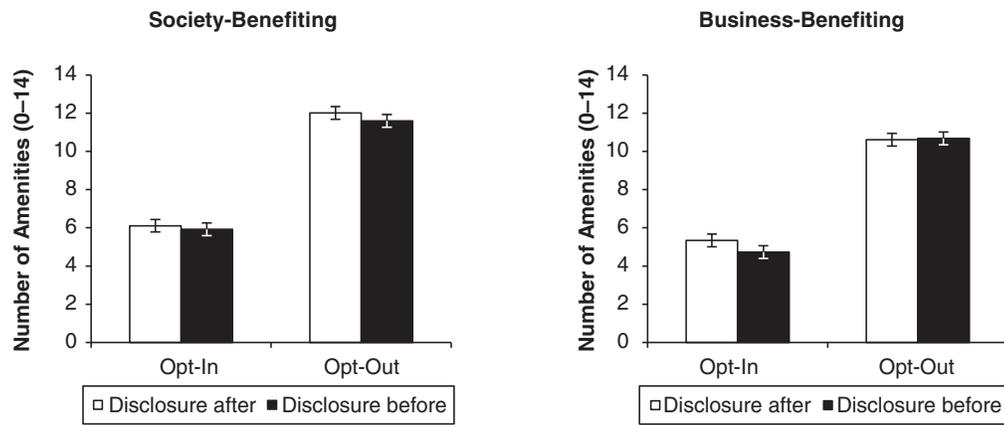
Discussion

Thus far, we have found no evidence that disclosure reduces default effects. A meta-analysis of Experiments 1a–2b (we excluded Experiment 1c because the extremely rare instances of participants switching from the default made analysis of this experiment unreliable) revealed large overall default effects both with and without disclosure (Cohen’s $d = 1.29$ and 1.16 , respectively) and showed no difference in default effectiveness between the disclosure and no-disclosure conditions ($z = .45, p = .65$; Table 1). Disclosure did, in one instance (Experiment 2a), affect people’s choices by making them less

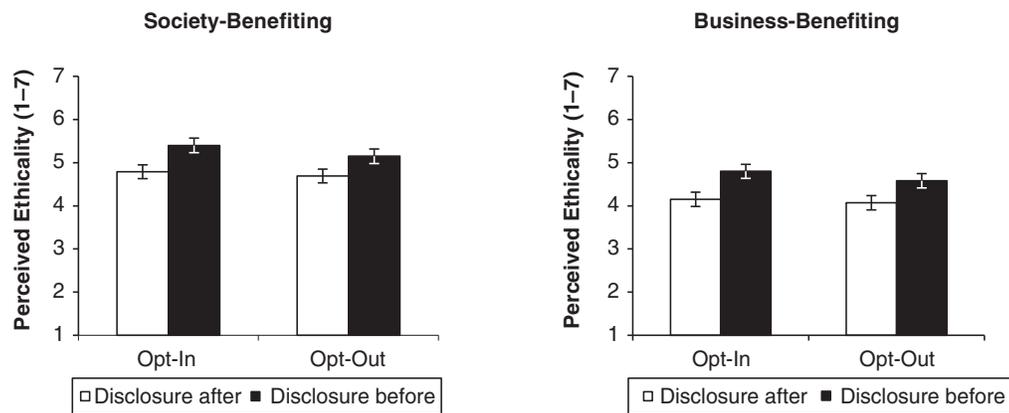
Figure 2

EXPERIMENT 2B: NUMBER OF AMENITIES CHOSEN, PERCEIVED ETHICALITY OF DEFAULTS, AND DESIRE TO WORK WITH DEFAULT-SETTER AGAIN ACCORDING TO CHOICE FORMAT, DISCLOSURE ORDER, AND UPGRADE MOTIVE

A: Number of Amenities Chosen



B: Perceived Ethicality of Defaults



C: Intention to Work with Default-Setter Again

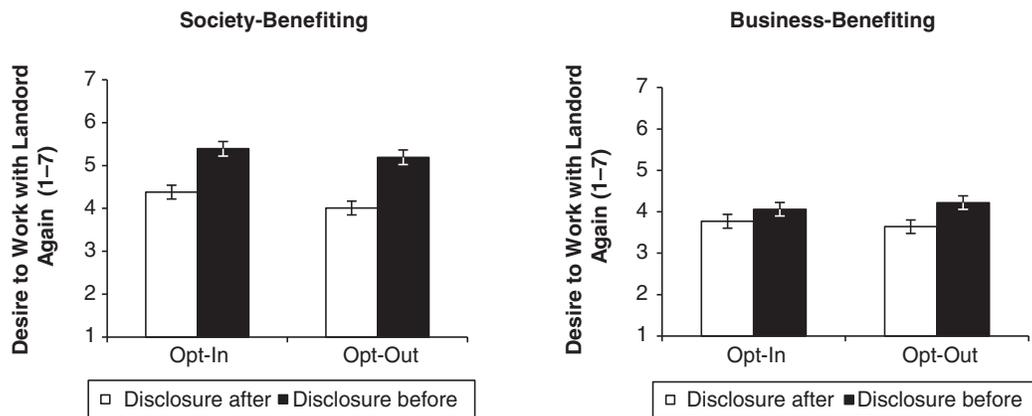


Table 1
EXPERIMENTS 1A–2B: FOREST PLOT AND SUMMARY OF DEFAULT EFFECTS WITH AND WITHOUT DISCLOSURE

Experiment	Transparency	Opt-In	Opt-Out	<i>d</i> and 95% CI
Experiment 1a Amount of personal information shared (0–20)	Disclosure absent	4.62	6.23	
	Disclosure present	5.01	6.57	
Experiment 1b Number of surveys completed (1–11)	Disclosure absent	7.35	9.59	
	Disclosure present	6.73	9.52	
Experiment 1c Percentage who chose hot chocolate with whip	Disclosure absent	6%	100%	
	Disclosure present	2%	90%	
Experiment 2a Green amenities: number of amenities chosen (0–14)	Disclosure absent	5.16	10.21	
	Disclosure present	5.25	10.39	
Experiment 2a Premium amenities: number of amenities chosen (0–14)	Disclosure absent	4.44	7.78	
	Disclosure present	3.12	6.64	
Experiment 2b Society-benefitting motive: number of amenities chosen (0–14)	Disclosure after	6.11	12.01	
	Disclosure before	5.92	11.60	
Experiment 2b Business-benefitting motive: number of amenities chosen (0–14)	Disclosure after	5.34	10.61	
	Disclosure before	4.73	10.68	
Overall	Disclosure absent			
	Disclosure present			

Notes: CI = confidence interval. Means and percentages are shown for opt-in and opt-out conditions for each experiment. Effect sizes (*d*) and 95% CIs are shown for the individual studies (squares) and overall estimates for the disclosure and no-disclosure conditions (diamonds). Experiment 1c was excluded from the meta-analysis because the extremely rare instances of participants switching from the default made analysis of this experiment unreliable. Overall, the size of default effects did not reliably differ depending on the presence or absence of disclosure ($z = .45, p = .65$).

likely to choose upgrades, especially when those upgrades were perceived to benefit the default-setter rather than society, but that effect held regardless of whether the upgrades were set as the default.

However, disclosure did change people's perceptions of the fairness and ethicality of defaults and their interest in working with the default-setter again. In Experiment 2a, without disclosure, people tended to prefer that upgrades be opt-out than opt-in regardless of whom the upgrades would benefit. When people were informed how the default was intended to affect their choices, people thought it was less fair for businesses to use an opt-out format to nudge people toward more expensive, business-benefitting upgrades than it was for them to use an opt-in format, whereas they thought the opt-in and opt-out choice formats were equally fair when upgrades were green and would benefit society. These results are in line with previous research that has found that “decisional enhancement” programs, such as defaults, that target basic, lower-order decision-making processes, are deemed less acceptable than those that target conscious, higher-order processes, in some contexts (e.g., eating, purchasing, exercising, investing decisions), but not in others (e.g., workplace productivity decisions; Felsen, Castelo, and Reiner 2013). Similarly, nudges are seen as less ethical when applied to personally objectionable policies and more ethical when applied to desirable objectives (Tannenbaum, Fox, and Rogers 2016). Moreover, in Experiment 2b, when the choice options were the same and what differed was the default-setter's motives for setting the default the way he did, up-front disclosure led people to perceive the default-setter as more ethical and to express greater interest in working with him again.

These results suggest that defaults function as more than just recommendations that people can choose to follow

or not. If a lack of motivation to consider the decision carefully were the operating mechanism in these studies, we should have seen that disclosure not only affected perceptions of how fair and ethical the defaults were but also reduced the influence of defaults that were intended to benefit a business at the consumer's expense. Instead, the results support the idea that decision makers' lack of ability to make unbiased decisions underlies these default effects, because disclosure had little influence on default effectiveness despite affecting perceived fairness. We suggest that if a lack of ability to consider defaults in an unbiased manner is indeed a prominent mechanism underlying default effects, an effective intervention must not only inform people how defaults are intended to influence their choices but also prompt people to focus less on the default option and to engage in more balanced consideration of the alternatives. In the next experiment, we examine whether supplementing disclosure with a task that encourages consumers to think about the default and its alternative simultaneously and to articulate their preferences for one or the other can help them resist the influence of defaults.

EXPERIMENT 3: CAN DEFAULTS BE DEBIASED?

Even when people are motivated to think carefully about a choice, defaults influence decisions because people do not realize that defaults serve as reference points that lead them to focus on reasons to choose the default. Experiment 3 examines whether prompting people to think about the default and its alternative simultaneously, as in a forced choice, and to articulate their preferences might help them engage in a more balanced consideration of the alternatives and reduce default effects. Participants considered a choice of apartment

amenities and were either only told the intent behind the default or both told the intent and also encouraged to articulate their preference for the standard or upgraded version of each amenity prior to choosing. We predict that preference articulation will reduce defaults relative to disclosure alone.

Method

Participants. We recruited 690 adults through MTurk, each of whom received \$.15 in Amazon credit. Nine participants had already taken the survey and were excluded.

Procedure. Participants were randomly assigned to one of eight versions of a scenario, in a 2 (choice format: opt-in or opt-out) \times 2 (type of upgrade: green or premium) \times 2 (intervention: disclosure or both disclosure and preference-articulation task) between-subjects design. (Because all our previous studies indicate that the disclosure conditions are no different from the no-disclosure conditions, we omitted the no-disclosure conditions in Experiments 3 and 4 to reduce the number of conditions needed.) Participants chose between a variety of upgraded amenities that were either green or premium. Additionally, in the disclosure-only conditions, they were told the intention behind the default as before, whereas in the preference-articulation conditions, they were both told the intention behind the default and encouraged to “jot down a brief note” about why they might prefer to have the standard or premium/green version of each amenity. They received a list of the amenities, each with a short blank in which to enter their thoughts. This intervention was meant to encourage participants to think about the default and its alternative simultaneously, as in a forced choice. After the disclosure or the preference-articulation task, participants indicated which of the amenities from the list that they would like to have added to (opt-in conditions) or omitted from (opt-out conditions) the apartment.

Results

Preference articulation attenuated default effects, as shown by an interaction between choice format and intervention ($F(1, 674) = 16.52, p < .001, \eta_p^2 = .02$). The difference in the number of upgrades chosen in the opt-out and opt-in conditions was smaller when participants articulated their preferences ($M_{\text{opt-out}} = 7.65, SD = 3.95$ vs. $M_{\text{opt-in}} = 5.27, SD = 3.12; F(1, 279) = 31.40, p < .001, \eta_p^2 = .10$) than when they only read the disclosure ($M_{\text{opt-out}} = 8.46, SD = 3.92$ vs. $M_{\text{opt-in}} = 4.07, SD = 2.87; F(1, 399) = 162.92, p < .001, \eta_p^2 = .29$). The three-way interaction was not significant ($F(1, 674) = .42, p = .52, \eta_p^2 = .001$), showing that preference articulation attenuated default effects regardless of the type of upgrades.

In addition, whether participants had to opt in or opt out mattered more when the available upgrades were green than premium, as indicated by an interaction between choice format and type of upgrades ($F(1, 674) = 17.81, p < .001, \eta_p^2 = .03$). Participants chose more amenities when they were opt-out versus opt-in in the green conditions ($M_{\text{opt-out}} = 9.91, SD = 3.52$ vs. $M_{\text{opt-in}} = 5.26, SD = 3.14; F(1, 340) = 166.29, p < .001, \eta_p^2 = .33$) and to a lesser degree in the premium conditions ($M_{\text{opt-out}} = 6.33, SD = 3.53$ vs. $M_{\text{opt-in}} = 3.87, SD = 2.74; F(1, 338) = 51.48, p < .001, \eta_p^2 = .13$).

There was also an interaction between type of upgrades and intervention ($F(1, 674) = 4.10, p = .04, \eta_p^2 = .006$). Whereas

preference articulation did not change participants' likelihood of choosing premium amenities ($M = 4.93, SD = 3.26$) compared with disclosure alone ($M = 5.25, SD = 3.49; F(1, 338) = .73, p = .40, \eta_p^2 = .002$), preference articulation did make participants marginally more likely to choose the society-benefitting green amenities ($M = 8.06, SD = 3.57$) compared with disclosure alone ($M = 7.31, SD = 4.36; F(1, 340) = 2.80, p = .095, \eta_p^2 = .008$).

Overall, participants chose to get more upgraded amenities in the opt-out conditions ($M = 8.13, SD = 3.95$) than in the opt-in conditions ($M = 4.57, SD = 3.03; F(1, 674) = 182.37, p < .001, \eta_p^2 = .21$). They also chose more green amenities ($M = 7.61, SD = 4.07$) than premium amenities ($M = 5.11, SD = 3.39, F(1, 674) = 107.28, p < .001, \eta_p^2 = .14$). Choices did not vary depending on whether participants only read a disclosure ($M = 6.29, SD = 4.08$) or also articulated their preferences ($M = 6.48, SD = 3.75; F(1, 674) = .78, p = .38, \eta_p^2 = .001$; Figure 3).

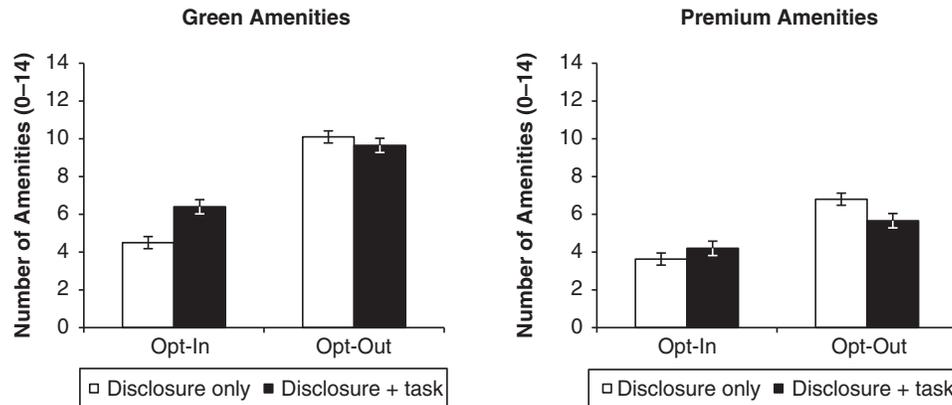
Discussion

Supplementing disclosure with an intervention that encourages consumers to focus less disproportionately on the default by prompting them to articulate their preferences prior to choosing can help consumers defend themselves against the influence of defaults. Overall, participants who not only read a disclosure but also articulated their preferences before choosing were less likely to retain the default option than those who only read a disclosure.

We believe that this intervention deemphasizes the default and encourages consumers to engage in a more balanced consideration of the alternatives. However, it is also possible that preference articulation increases participants' ability to make an unbiased choice simply because it slows them down and forces them to think more carefully. A follow-up study suggests the latter possibility is not the case. Using the choice of premium amenities in the apartment scenario, we forced some participants to slow down and take at least 60 seconds to consider their preferences before choosing (without prompting them to consider the default and its alternative simultaneously or encouraging them to articulate their preferences for one or the other). Contrary to the idea that our preference-articulation manipulation is effective merely because it slows participants down, requiring participants to take extra time conferred no benefits beyond disclosure. Participants did take more time when prompted to do so ($M = 4.09$ min, $SD = 1.50$ vs. $M = 3.58$ min, $SD = 7.94; t(327.31) = 7.97, p < .001$; equal variances not assumed). However, despite the increased deliberation time, disclosure was equally ineffective at reducing the default's effect on choices when participants were prompted to slow down as when they were not. Participants chose more upgraded amenities in the opt-out conditions ($M = 7.42, SD = 4.43$) than in the opt-in conditions ($M = 3.43, SD = 2.35; F(1, 401) = 187.54, p < .001, \eta_p^2 = .32$), but choices did not differ depending on whether participants were prompted to think carefully ($M = 5.57, SD = 3.35$) or not ($M = 5.35, SD = 3.73; F(1, 401) = 1.07, p = .30, \eta_p^2 = .003$) or depending on the interaction between the default and the prompt to slow down ($F(1, 401) = .09, p = .76, \eta_p^2 < .001$). In the next experiment, we examine why preference articulation helps consumers resist default effects by examining how it changes how people think about their options.

Figure 3

EXPERIMENT 3: NUMBER OF AMENITIES CHOSEN ACCORDING TO CHOICE FORMAT, TYPE OF UPGRADES, AND INTERVENTION



EXPERIMENT 4: HOW DOES PREFERENCE ARTICULATION DEBIAS DEFAULTS?

We propose that encouraging people to articulate their preferences for the default or its alternative, as in a forced choice, helps them more effectively cope with the default's influence by prompting them to engage in a more balanced consideration of the options. Experiment 4 examines whether preference articulation prompts people to generate a smaller proportion of reasons in favor of the default and a greater proportion of reasons in favor of the alternative and whether this attenuates the default effect. Some participants listed their reasons for choosing premium versus standard amenities before choosing, and others listed reasons after, enabling us to compare whether the reasons participants generated differed when they articulated their preferences before versus after choosing. After participants made their selections, their reasons were displayed back to them, and they indicated whether each was a reason to choose the premium version, the standard version, or neither (Dinner et al. 2011; Johnson et al. 2007; Knoll et al. 2015). We predict that encouraging participants to articulate their preferences before choosing will lead to more balanced reasons—fewer thoughts favoring the default and more thoughts favoring the alternative—and help participants be less influenced by the choice format.

Method

Participants. We recruited 390 adults through MTurk. Each participant received \$.50 in Amazon.com credit.

Procedure. Participants were randomly assigned to one of four versions of the apartment scenario with only premium amenities, in a 2 (choice format: opt-in or opt-out) \times 2 (preference articulation order: before or after choice) between-subjects design. Participants were shown the same premium amenities used previously, as well as the standard disclosure explaining the intention behind the default. In the articulation-before conditions, participants were asked to "jot down a brief note" about why they might prefer to have the standard or premium version of each amenity prior to making their selections, as they did in

Experiment 3, whereas in the articulation-after conditions, participants completed the same preference-articulation task after making their selections. All participants indicated which amenities they would like to have added to (in the opt-in conditions) or omitted from (in the opt-out conditions) the apartment. After participants made their selections, they reviewed the reasons they had listed and coded each as a reason to choose the premium version of the amenity, the standard version of the amenity, or neither.

Results

Number of amenities chosen. Preference articulation again attenuated the default effects. An interaction between choice format and order of preference articulation ($F(1, 386) = 6.25, p = .013, \eta_p^2 = .02$) indicates that the difference in the number of premium amenities chosen in the opt-out and opt-in conditions was smaller when participants articulated their preferences before choosing ($M_{\text{opt-out}} = 6.38, SD = 3.11$ vs. $M_{\text{opt-in}} = 4.41, SD = 2.53; t(195) = 4.86, p < .001, d = .69$) than when they articulated their preferences after choosing ($M_{\text{opt-out}} = 7.22, SD = 3.81$ vs. $M_{\text{opt-in}} = 3.73, SD = 2.35; t(164.32) = 7.70, p < .001, d = 1.10$). Overall, participants chose more premium amenities in the opt-out conditions than in the opt-in conditions ($M_{\text{opt-out}} = 6.80, SD = 3.50$ vs. $M_{\text{opt-in}} = 4.08, SD = 2.46; F(1, 386) = 79.85, p < .001, \eta_p^2 = .17$). There was no main effect of preference articulation order ($M_{\text{before choosing}} = 5.39, SD = 2.99$ vs. $M_{\text{after choosing}} = 5.52, SD = 3.62; F(1, 386) = .07, p = .79, \eta_p^2 < .001$).

Reasons listed. We next analyzed whether participants generated less biased reasons when they articulated their preferences prior to choosing. Reasons participants categorized as supporting premium amenities were coded as 1, reasons supporting standard amenities as -1 , and reasons categorized as neither as 0. We averaged these codes to create a composite score for each participant. A marginal interaction between choice format and preference articulation ($F(1, 386) = 3.16, p = .076, \eta_p^2 = .008$) indicates that the difference between the number of upgrades chosen in

the opt-out and opt-in conditions was smaller when participants articulated their preferences before choosing ($M_{\text{opt-out}} = -.09$, $SD = .43$ vs. $M_{\text{opt-in}} = -.21$, $SD = .43$; $t(195) = 1.86$, $p = .06$, $d = .28$) than when they articulated their preferences after choosing ($M_{\text{opt-out}} = -.07$, $SD = .54$ vs. $M_{\text{opt-in}} = -.36$, $SD = .45$; $t(188.39) = 3.95$, $p < .001$, $d = .58$). A main effect of the choice format on reasons listed shows that participants also listed more reasons in favor of premium amenities in the opt-out than the opt-in conditions ($M_{\text{opt-out}} = -.08$, $SD = .49$ vs. $M_{\text{opt-in}} = -.28$, $SD = .45$; $F(1, 386) = 17.67$, $p < .001$, $\eta_p^2 = .04$). There was no main effect of preference articulation ($M_{\text{before choosing}} = -.15$, $SD = .43$ vs. $M_{\text{after choosing}} = -.21$, $SD = .52$; $F(1, 386) = 1.97$, $p = .16$, $\eta_p^2 = .005$).

Moderated mediation. We predicted that preference articulation would moderate the effect of the default on the reasons participants listed for choosing premium versus standard amenities, which in turn would affect the number of premium amenities they chose. We found significant moderated mediation (95% confidence interval [CI] = [.02, 1.69]) using PROCESS Model 7 (Hayes 2013) with 1,000 bootstrapped resamples. The indirect path from the default to reasons listed to premium amenities chosen differed depending on preference articulation. This indirect path was significant when participants did not articulate their preferences before choosing (95% CI = [-2.02, -.65]) but nonsignificant when they did (95% CI = [-1.07, .06]; Figure 4).

We next ran mediation analyses separately for the before and after conditions, using PROCESS Model 4 (Hayes 2013) with 1,000 bootstrapped resamples. The default and reasons listed predicted the amenities chosen when participants articulated their preferences after they chose ($\beta_{\text{default}} = -2.29$, $t = -6.41$, $p < .001$; $\beta_{\text{reasons}} = 4.25$, $t = 12.26$, $p < .001$) as well as before they chose ($\beta_{\text{default}} = -1.41$, $t = -5.13$,

$p < .001$; $\beta_{\text{reasons}} = 4.87$, $t = 15.40$, $p < .001$). However, while the default predicts the reasons participants list when they articulate their preferences after choosing ($\beta = -.28$, $t = 3.93$, $p < .001$) and reasons listed mediates the relationship between the default and amenities chosen (95% CI = [-1.95, -.54]), this relationship becomes marginal ($\beta = -.11$, $t = -1.86$, $p = .064$) and reasons listed is not a mediator (95% CI = [-1.19, .05]) in the before conditions, indicating that preference articulation attenuated the default's effect on choice by prompting people to consider the reasons to choose each option in a more balanced way.

Discussion

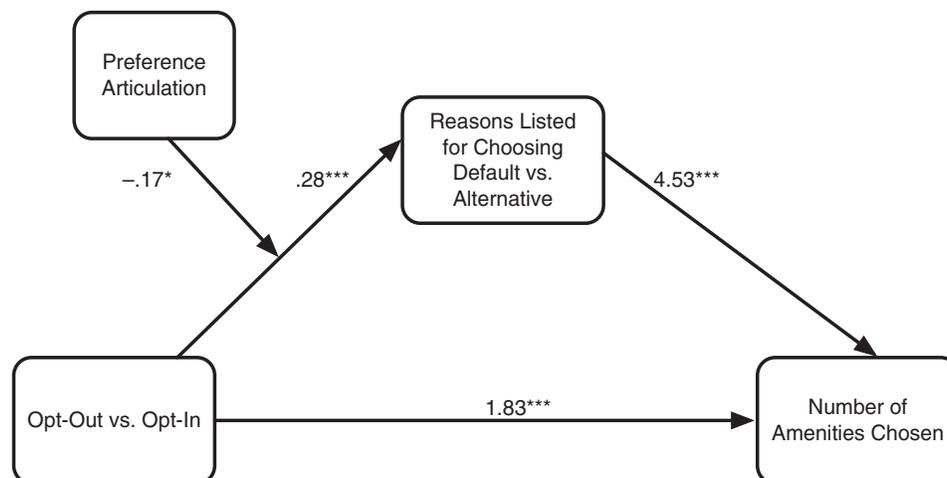
These results illuminate why preference articulation attenuates default effects: prompting people to articulate their preferences for the default or its alternative, as in a forced choice, helps them engage in a more balanced consideration of the options and better cope with the default's influence. Participants who considered why they might prefer the premium or standard amenities before choosing versus after were less likely to focus disproportionately on reasons to choose the default, and consequently, they were less likely to retain the default when making a decision.

GENERAL DISCUSSION

We demonstrate that defaults retain their power to influence choices even when the intention behind them is disclosed to consumers. Disclosure is not completely ineffective: it does change perceptions of how fair the default is, and it improves impressions of the default-setter. But even when defaults are seen as unethical, they influence consumers' choices. To reduce defaults' effects, a more active intervention is necessary: encouraging consumers to

Figure 4

EXPERIMENT 4: MODERATED MEDIATION MODEL OF THE INDIRECT EFFECT OF CHOICE FORMAT ON NUMBER OF AMENITIES CHOSEN THROUGH REASONS LISTED FOR CHOOSING THE DEFAULT OR THE ALTERNATIVE, MODERATED BY PREFERENCE ARTICULATION



* $p < .10$.

*** $p < .001$.

Notes: Index of moderated mediation: 95% CI = (.02, 1.69); indirect path with disclosure only: 95% CI = (-2.02, -.65); indirect path with preference articulation: 95% CI = (-1.07, .06).

consider both the default and the alternative together can help them make less biased decisions.

We contribute to the literature on public policy and marketing by suggesting that choice architects can create transparency by disclosing the nature and intent behind defaults without reducing defaults' effectiveness. Disclosure may even improve default-setters' reputations with consumers. However, disclosure alone does not seem to be an effective means of consumer protection: defaults still guide choices even when they are preceded by disclosure of their effects and the reason why they were instituted. We also contribute to the literatures on persuasion knowledge and defaults by showing that default effects are robust against disclosure of their intent, regardless of whether that intent is selfless or selfish. We further show that many default effects may result not simply from lack of careful scrutiny but from consumers' inability to effectively counter defaults' influence. Mitigating default effects therefore requires an active intervention that enables this ability. Finally, we contribute to the debiasing literature by showing that preference articulation can be used effectively to reframe the reference point of a decision, prompting people to consider their options in a more balanced way and make less biased choices.

One limitation that extends beyond the scope of our research and should be addressed in future studies is the generalizability of these results to other forms of choice architecture. Defaults are common, but different kinds of choice architecture, and even different defaults, may have different mechanisms than those we study here. Choice architecture can rely on order, contrast, or other kinds of framing, among other factors, which require separate consideration with regard to whether disclosure or preference articulation would make them more or less effective.

Another question for future research is whether accountability might be effective as a debiasing technique. It is possible that being accountable for explaining one's reasons for a choice to another party might function similarly to preference articulation and reduce the default effect. Accountability may thus provide a useful tool for consumer protection to which consumers might subscribe autonomously. Alternatively, without an intervention that specifically prompts people to think about their options simultaneously, as in our preference-articulation intervention, it is possible that accountability might simply reinforce people's natural tendency to focus first and foremost on reasons for choosing the default, thus exacerbating default effects.

In conclusion, our research shows that marketers and policy makers can deploy defaults in a transparent manner. Defaults can be equally effective when the intention behind them is disclosed as when it is not, even when consumers are made aware that the default serves the interests of the business instituting it rather than the individual's or society's interests, and even when consumers carefully scrutinize the decision. Disclosure can also enhance consumers' perceptions of ethicality and attitudes toward the default-setter. Second, to protect consumers from defaults that are not in their best interests, policy makers and other consumer advocates can encourage consumers to articulate their preferences regarding the default or its alternative before they choose in situations in which marketers are likely to set defaults to benefit themselves at consumers' expense. Our findings suggest that this type of intervention may diminish

the effect of defaults and give consumers the ability to make decisions in their own best interests.

APPENDIX A: DISCLOSURES

Experiment 1a

Opt-in. As you may or may not know, research suggests that you are LESS likely to agree to a request (like whether to share information) when the request is made in an opt-in format (like this one) rather than an opt-out format. The Meter chose to make the sign-up process an OPT-IN format. This way, you are not "nudged" toward agreeing to share your information with others.

Opt-out. As you may or may not know, research suggests that you are MORE likely to agree to a request (like whether to share information) when the request is made in an opt-out format (like this one) rather than an opt-in format. The Meter chose to make the sign-up process an OPT-OUT format. This way, you are "nudged" toward agreeing to share your information with others.

Experiment 1b

Opt-in. As you may or may not know, research suggests that you are LESS likely to choose optional question sets when the request is made in an opt-in format (like this one) rather than an opt-out format. We chose to phrase the request for optional question sets in an OPT-IN format. This way, you are not "nudged" toward answering more questions which takes more time but also pays more.

Opt-out. As you may or may not know, research suggests that you are MORE likely to choose optional question sets when the request is made in an opt-out format (like this one) rather than an opt-in format. We chose to phrase the request for optional question sets in an OPT-OUT format. This way, you are "nudged" toward answering more questions which takes more time but also pays more.

Experiment 1c

Opt-in. We choose to serve our hot chocolate without whipped cream by default because we want to encourage our customers to choose a more healthy option. Research shows that people are more likely to stick to the default option.

Opt-out. We choose to serve our hot chocolate with whipped cream by default because we want to encourage our customers to choose a more indulgent option. Research shows that people are more likely to stick to the default option.

Experiment 2b

Opt-in. I want to be honest with you, research suggests that you are LESS likely to choose green amenities when the request is made in an opt-in format rather than an opt-out format. I would like to help the environment and save energy. [I would like to qualify for a tax break and put some money back in my pockets.] Nevertheless, I made the request for green amenities in an OPT-IN format, so you are NOT "nudged" toward having the more expensive energy-saving amenities.

Opt-out. I want to be honest with you, research suggests that you are MORE likely to choose green amenities when the request is made in an opt-out format rather than an opt-

in format. I would like to help the environment and save energy. [I would like to qualify for a tax break and put some money back in my pockets.] Therefore, I made this request for green amenities in an OPT-OUT format so you ARE “nudged” toward having the more expensive energy-saving amenities.

Experiments 2a, 3, and 4

Opt-in. As you may or may not know, research suggests that you are LESS likely to choose premium [green] amenities when the request is made in an opt-in format (like the one you are about to see) rather than an opt-out format. The landlord of this apartment complex chose to phrase the request for premium [green] amenities in an OPT-IN format. This way, you are not “nudged” toward having the premium [green] amenities with higher quality finishings [higher energy savings] but also higher rent.

Opt-out. As you may or may not know, research suggests that you are MORE likely to choose premium [green] amenities when the request is made in an opt-out format (like the one you are about to see) rather than an opt-in format. The landlord of this apartment complex chose to phrase the request for premium [green] amenities in an OPT-OUT format. This way, you are “nudged” toward having the premium [green] amenities with higher quality finishings [higher energy savings] but also higher rent.

APPENDIX B: GREEN AND PREMIUM AMENITIES, EXPERIMENTS 2A, 2B, 3, AND 4

Green Amenities

- Energy Star furnace and air conditioner
- Tankless water heater
- Programmable thermostat
- Storm windows and doors
- Airflow-adjusting ceiling fans
- UV-filter film on windows
- Energy-efficient dishwasher and refrigerator
- Compact fluorescent light bulbs
- Energy-efficient washer and dryer
- Dimmer switches for indoor lighting
- Low-flow toilets
- Solar-powered outdoor lighting
- Low-flow faucets and shower heads
- Motion sensors for outdoor lighting

Premium Amenities

- Dehumidifying furnace and air conditioner
- High-capacity water heater
- Programmable thermostat
- Wood-frame windows and doors
- Self-adjusting ceiling fans
- Privacy film on windows
- Brushed-steel dishwasher and refrigerator
- Soft-light light bulbs
- Brushed-steel washer and dryer
- Dimmer switches for indoor lighting
- Comfort-height toilets
- Walkway illuminating outdoor lighting
- Brushed-copper faucets and shower heads
- Programmable timer for outdoor lighting

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