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Do Nudges Reduce Disparities? Choice Architecture Compensates for Low Consumer Knowledge

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Abstract

Choice architecture tools, commonly known as nudges, powerfully impact decisions and can improve welfare. Yet it is unclear *who* is most impacted by nudges. If nudge effects are moderated by socioeconomic status (SES), these differential effects could increase or decrease disparities across consumers. Using field data and several preregistered studies, the authors demonstrate that consumers with lower SES, domain knowledge, and numerical ability are impacted more by a wide variety of nudges. As a result, "good nudges" designed to increase selection of superior options reduced choice disparities, improving choices more among consumers with lower SES, lower financial literacy, and lower numeracy than among those with higher levels of these variables. Compared with "good nudges," "bad nudges" designed to facilitate selection of inferior options exacerbated choice disparities. These results generalized across real retirement decisions, different nudges, and different decision domains. Across studies, the authors tested different explanations of why SES, domain knowledge, and numeracy moderate nudges. The results suggest that nudges are a useful tool for those who wish to reduce disparities. The research concludes with a discussion of implications for marketing firms and segmentation.

Keywords

defaults, financial literacy, nudges, numeracy, socioeconomic status

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Choice architecture can powerfully impact decisions and improve welfare. Firms have adopted choice architecture changes that have increased retirement savings, increased environmentally friendly purchases, increased the number of premium features consumers buy when purchasing an automobile, and influenced other types of consumer decisions (Choi et al. 2004; Goldstein et al. 2008; Johnson et al. 2012; Thaler and Sunstein 2009).

But who does choice architecture influence most? Do changes to the choice environment impact some consumers more than others? We examined two related sources of heterogeneity in nudge effects, testing whether domain-specific skills and knowledge moderate nudge effects and whether socioeconomic status (SES) moderates nudge effects. We hypothesized that choice architecture can reduce choice disparities by having the largest impact on consumers with low SES and low levels of domain knowledge and skill.[1] Though choice architecture is inherent to online retail, many firms might not consider how choice architecture tools impact different consumers to different degrees, potentially reducing or exacerbating inequities between them. Knowledge of factors that make consumers more susceptible to choice architecture effects can allow firms and policy makers to use choice architecture more effectively to achieve the impact they want (Soman and Hossain 2020).

Choice Architecture

Choice architecture describes how a change in the structure of a choice influences behavior without significantly altering economic incentives or what consumers know about each option

[1] By "choice disparities," we mean large gaps in the decisions of people with low compared with high levels of a trait (e.g., SES). For example, low-SES consumers are much less likely to buy in bulk (Orhun and Palazzolo 2019).

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(Johnson et al. 2012; Thaler and Sunstein 2009). Choice architecture can be manipulated to change the decisions that consumers make; these manipulations are often called “nudges” (Loewenstein and Chater 2017; Thaler and Sunstein 2009).

Nudges are inexpensive and cost effective for firms and governments (Benartzi et al. 2017). Perhaps for this reason, they have gained tremendous popularity among policy makers and marketers (Afif et al. 2018; Benartzi et al. 2017). Over 200 “nudge units” currently exist around the world across private and public sectors (Afif et al. 2018). Marketing research has examined how choice architecture tools such as defaults and sorting alter consumer behavior (e.g., Diehl 2005; Goldstein et al. 2008; Johnson et al. 2012). All marketing managers must make decisions about choice architecture. For example, retailers choose which products to display first and whether to use defaults to automatically select a shipping option, insurance, or product add-on (Soman 2015; Thaler and Sunstein 2009). These decisions impact purchases and consumers’ subsequent wealth, health, and well-being.

Choice architecture is often used to facilitate choices that benefit consumers, firms, or both. For example, one auto manufacturer benefited both consumers and itself by changing the default car specifications on their website. Though it had previously defaulted consumers into basic, stripped-down car models, it found that changing defaults to tailor them to different types of customers increased firm profits while also benefiting consumers (Goldstein et al. 2008). Though nudges are frequently designed to help consumers, they can sometimes increase firm profits while decreasing consumer welfare. Nudges that harm consumers have been referred to as “bad nudges,” “dark patterns,” or “evil nudges” (Hansen and Jesperson 2013; Mathur et al. 2019; Soman et al. 2019), in contrast to “good nudges” that benefit consumers. We examine whether “bad nudges” exacerbate choice disparities relative to “good nudges” by impacting low-SES and low-knowledge consumers most.

There are many types of nudges, including defaults, sorting, partitioning, and several nudges that reduce the complexity or number of attributes or options (Cheema and Soman 2008; Chernev, Bockenholt, and Goodman 2015; Dellaert and Haubl 2012; Diehl 2005; Johnson and Goldstein 2003; Johnson et al. 2012; Lynch and Ariely 2000; Sharif and Shu 2017). We examine three types of choice architecture: defaults, sorting, and changes to the number of options. Defaults, a type of nudge that preselects one option but allows consumers to easily opt out of the preselected option, have been called “unquestionably the most prominent ... [nudge], across all domains of application” (Loewenstein and Chater 2017, p. 27). Another nudge, called sorting, organizes options in a systematic way. For example, many products can be sorted by price, consumer rating, total cost, sales volume, or other attributes (Diehl 2005; Lamberton and Diehl 2013; Lynch and Ariely 2000). Another form of choice architecture, which reduces the number of options presented to consumers, can improve decision making, reduce regret, and decrease the likelihood that consumers will defer their decision by choosing nothing (Bhargava, Loewenstein, and Sydnor 2017; Chernev, Bockenholt, and Goodman 2015; Johnson et al. 2012).

Who Gets Nudged?

Previous research on nudges has typically focused on the overall effect of a nudge averaged across all individuals. For example, preselecting cars with premium features as the default increased the automobile purchase price by $1,500 on average (Goldstein et al. 2008) and opting people into retirement contributions resulted in large overall effects on enrollment (Choi et al. 2004). Other investigations have focused on the average cost effectiveness of nudges (Benartzi et al. 2017) or the impact of other nudges (e.g., sorting or changing the number of options) on the average consumer (e.g., Lynch and Ariely 2000; Scheibehenne, Greifeneder, and Todd 2010). Though these nudges have large impacts on average, it is unclear who they benefit most or whether they reduce or exacerbate inequities across consumers.

Yet it is important to consider the heterogeneous impact of nudges rather than only the average effect collapsing across all consumers. Some scholars have suggested that nudges may affect the rich more than the poor (Roberts 2018). Roberts (2018) argues that because structural factors impede the autonomy of vulnerable low-SES consumers, high-SES consumers will change their behavior when nudged, whereas low-SES consumers will be “nudge-proof” due to their lack of autonomy. A different prominent account suggests that scarcity and low income influence decision making by increasing time and attention on a focal task at the expense of tasks and decisions that are secondary or require thinking about the future (Shah, Mullainathan, and Shafir 2012). This might reduce the effect of nudges if heightened time and attention on focal decisions increase motivation and accuracy. In contrast, we predicted that nudges would impact consumers with low SES and less domain-specific knowledge and skills more than those with higher levels of these characteristics for other reasons (detailed subsequently and in Figure 1). Thus, we hypothesized that interventions encouraging the selection of the best option should reduce choice disparities between consumers who differ in SES, domain knowledge, and numeracy. We tested these predictions across a wide variety of contexts and nudges.

We focused on the moderators of SES, numeracy, and domain knowledge for several reasons. We focused on SES partly because it is easy to measure and use for segmentation (Brown-Johnson et al. 2014; firms often have this information about their customers) and partly because SES strongly influences consumer behavior (Cervellon, Poujol, and Tanner 2019; Eisenberg-Guyot et al. 2018; Hill and Sharma 2020). We also focused on SES because previous research on choice architecture has largely neglected how effects of nudges differ across different levels of SES, and because reducing SES inequities is a major goal for many policy makers and firms. Firms and

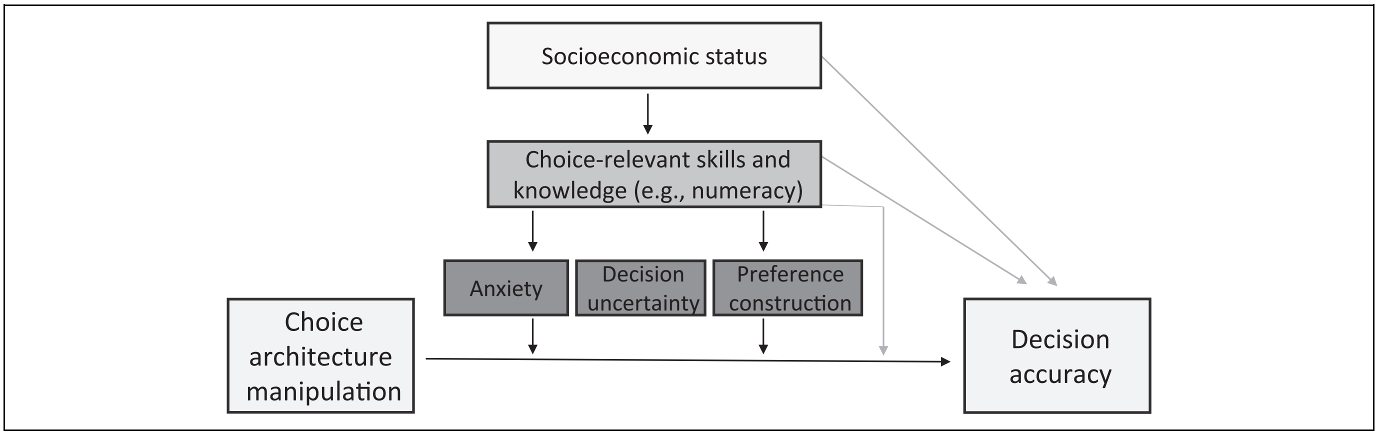

Figure 1. Diagram of our theoretical framework explaining who is more susceptible to choice architecture and why.

Notes: Consumers lower in SES and choice-relevant skills (e.g., numeracy) are impacted more by nudges. The model suggests that the SES moderator is explained by choice-relevant skills and knowledge, which moderates nudge effects partly because of anxiety, preference construction, and decision uncertainty. The relationships depicted by the dark gray arrows were the key relationships in our conceptual framework that we examined in primary analyses, and the light gray arrows were also supported by our data.

policy makers serve individuals with varying levels of SES; our investigation can help them estimate which consumers their nudges will impact most. Furthermore, SES has robust positive associations with numeracy, domain knowledge, and anxiety (Al Bahrani et al. 2019; Lusardi, Michaud, and Mitchell 2013; Skagerlund et al. 2018), which, in our view, shape susceptibility to nudges.

We examined numeracy and domain knowledge as focal moderators because these constructs play major roles in consumer decision making (Graffeo, Polonio, and Bonini 2015; Mitchell, Lennard, and McGoldrick 2003) and are useful for theory building. As we explain in the following section, these variables, along with anxiety, decision uncertainty, and preference construction, determine the extent to which choice architecture influences decisions according to our account.

Understanding heterogeneous effects of nudges could help firms by allowing them to target specific consumer segments, which could make nudges more effective. Furthermore, scholars have suggested that understanding heterogeneity would provide insight into why nudges often have smaller effects when applied at scale (Soman and Hossain 2020).

Theoretical Background

Socioeconomic disparities pervade consumer behavior. SES influences what products and brands consumers buy, how they access credit, and how they are treated in some stores, among other impacts (Cervellon, Poujol, and Tanner 2019; Eisenberg-Guyot et al. 2018; Hill and Sharma 2020). Consumers with lower SES and education (as well as the elderly) are often more vulnerable to marketing scams and manipulations (Hill and Sharma 2020; Langenderfer and Shimp 2001). In addition, there are wide gaps between high-SES and low-SES consumers in terms of how much money they have in stocks, retirement savings, credit card debt, payday loan debt, and other assets or liabilities, which can greatly influence present behavior and future wealth (Bernheim 1998; Eisenberg-Guyot et al. 2018).

Lower SES is associated with lower levels of numeracy and financial literacy (Al Bahrani et al. 2019; Lusardi, Michaud, and Mitchell 2013; Skagerlund et al. 2018). These skills play a role in nearly every type of consumer decision (e.g., Graffeo, Polonio, and Bonini 2015; Skagerlund et al. 2018), and the discrepancy in these skills between low-SES and high-SES consumers can lead to disparate decisions and outcomes. The experience of scarcity that accompanies low SES sometimes narrows attention on a focal decision and influences time allocation, which can impact decisions (Shah, Mullainathan, and Shafir 2012).

Numeracy is the ability to process and use basic numerical concepts; make quantitative estimations; and use probabilities, percentages, and ratios (Peters et al. 2019). In the context of consumer decision making, innumerate people cannot calculate unit prices, use percentages to calculate discounts, compute interest, or even estimate a tip accurately (Graffeo, Polonio, and Bonini 2015; Mitchell, Lennard, and McGoldrick 2003; Santana, Thomas, and Morwitz 2020). Broadly, numerate individuals often make better consumer and health decisions, especially when these decisions involve numbers, calculations, prices, or financial information (Peters et al. 2006). Numeracy refers to the ability to use and process numbers, which is distinct from other traits such as self-efficacy, math emotions (e.g., math anxiety), uncertainty, and subjective numeracy (Peters and Bjalkebring 2015; Peters et al. 2019; Skagerlund et al. 2018).

Financial literacy is the knowledge of basic financial concepts, operations, and facts. It is an important skill used to make financial decisions as well as decisions involving product prices and attributes (Danes, Huddleston-Casas, and Boyce

1999; Hilgert, Hogarth, and Beverly 2003). Financially literate consumers are less likely to overspend and are more likely to save for retirement, invest in stocks, comparison shop, and pay off their full credit card balance (Danes, Huddleston-Casas, and Boyce 1999; Hilgert, Hogarth, and Beverly 2003; Lusardi 2008). Though financial literacy is associated with a wide range of consumer behaviors, financial literacy training only weakly influences financial knowledge, and any effects dissipate quickly (Fernandes, Lynch, and Netemeyer 2014).

Numeracy and financial literacy impact consumer behavior partly because consumers with lower numeracy and financial literacy experience greater anxiety and decision uncertainty when dealing with numbers and financial decisions (Peters et al. 2019; Skagerlund et al. 2018). In addition, rather than retrieve stable preferences from memory, uncertain consumers construct their preferences on the fly more often than do consumers with higher certainty (Hoeffler and Ariely 1999). As a result, their preferences are more labile; they are more reliant on effort-reducing heuristics; and they are more impacted by defaults, the status quo, and changes in the number of options (Chernev, Bockenholt, and Goodman 2015; Huh, Vosgerau, and Morewedge 2014; Hutchinson and Alba 1991; Sengupta and Johar 2001). In other words, consumers with lower numeracy and financial literacy feel more uncertainty and anxiety; thus, we expected that in the context of nudges they would rely on strategies such as choosing the default option or first option presented. Furthermore, we hypothesized that low-SES consumers would be more impacted by nudges because they score lower in relevant skills such as numeracy and because they experience more anxiety when making decisions (Figure 1).

Although the constructs of decision uncertainty, preference construction, anxiety, subjective knowledge, and the three focal moderators (SES, numeracy, and domain knowledge) are all associated with one another, they are distinct (Peters and Bjalkebring 2015; Peters et al. 2019; Skagerlund et al. 2018). Furthermore, they differ from general intelligence and other types of confidence (e.g., general self-efficacy vs. search confidence; Fernandes, Lynch, and Netemeyer 2014; Netemeyer et al. 2018). These constructs have clear discriminant validity; for example, objective numeracy and domain knowledge are types of objective knowledge or skill, which differ from subjective beliefs about ability (e.g., subjective numeracy, confidence, uncertainty; Hadar, Sood, and Fox 2013; Peters et al. 2019). Many people have high confidence in their numeric abilities despite low objective numeracy or vice versa, either of which can lead to harmful financial and health outcomes (Peters et al. 2019). In addition, subjective confidence and anxiety differentially predict memory and evaluations (Peters and Bjalkebring 2015). Decision anxiety can also impact performance independent of objective numeracy. For example, people can be anxious about disconfirming negative stereotypes despite high objective ability. This can create a self-fulfilling prophecy, because the feeling of stigmatization can increase anxiety and negatively impact decisions (Tine and Gotlieb 2013).

Although previous choice architecture research has typically focused on the average effects on consumers, some individual difference moderators of choice architecture effects have been identified. However, nearly all of these moderators have been tested for only a single type of nudge within a single domain. Next, we summarize the research about choice architecture moderators that is most relevant to our hypotheses.

Very little previous research has examined whether the impact of nudges is moderated by SES. For other marketing manipulations such as scams, previous research has suggested that vulnerable consumers (e.g., those who are elderly or less educated) are sometimes targeted and impacted to a greater extent (Hill 1995; Hill and Sharma 2020; Langenderfer and Shimp 2001). Within the context of nudges, recent unpublished papers have found that automatic retirement contributions increased savings more for younger and lower-income individuals than others (Beshears et al. 2016; Choukhmane 2021). This conflicts with other scholars who have made theoretical claims that low-SES individuals are less nudgeable (Roberts 2018). Other work has provided mixed evidence about whether low- or high-income individuals are more impacted by different nudges such as framing (Fishbane, Ouss, and Shah 2020; Hershfield, Shu, and Benartzi 2020; Shah, Shafir, and Mullainathan 2012). Clearly, more research is needed to test these opposing claims across a wide variety of nudges and contexts.

Some theorists have previously suggested that people with more expertise or knowledge might be less impacted by choice architecture. For example, Camerer et al. (2003) claimed that the aim of policy nudges is to create large benefits for those who have lower expertise and make errors, with minimal impact on more rational or expert decision makers. In other words, consumers with more knowledge or expertise may be less impacted by nudges. However, this claim has received very little empirical attention. One investigation found no default effect in the environmental domain among a sample of environmental economists (Löfgren et al. 2012). However, the study did not measure experience, examine moderators, or include a control group of people with low experience, so it is difficult to draw conclusions from it. Another investigation found no effect of experience or education on default effects (Johnson, Bellman, and Lohse 2002). There has been some relevant previous research on numeracy. Peters et al. (2006) found that numerate people are less impacted than innumerate people by manipulations that present numbers as frequencies rather than probabilities, while Chapman and Liu (2009) found the opposite in the context of Bayesian reasoning. Prior research has not examined whether financial literacy and numeracy moderate effects of defaults, sorting, or other choice architecture tools. Clearly, the present studies are needed to clarify these relationships.

Overview of Studies

Across six studies, we tested whether nudges have larger impacts on low-SES consumers and those with lower numeracy and domain knowledge. In Study 1, we demonstrate these effects

Table 1. Questions and Answer Options Used in Study 1.

Questions[a]	Options[b]	Good Default	No Default	Bad Default
Imagine you want a new credit card. Imagine also that you make purchases totaling a few hundred dollars each month and always pay just the minimum payment on your credit card (you will always continue doing this each month in the future). You are pre-approved for these three cards. Given this scenario, choose the best credit card considering monetary costs and benefits.	• *Surge Card (15% APR, no cash back)* • Trek Card (25% APR, 2% cash back) • Journey Card (20% APR, 1% cash back)	72%	66%	52%
Imagine you want a new credit card. Imagine also that you make many purchases each month and always pay off your full balance on the credit card before you accrue interest (you will continue to pay off your full balance each month in the future in this manner). Given this scenario, choose the best card given monetary costs and benefits.	• Ascent Card (15% APR, no cash back) • *Midnight Card (25% APR, 2% back)* • Trust Card (20% APR, 1% back)	66%	64%	50%
Imagine you have debt on two credit cards with the same bank and have money that you would like to use to pay off this debt. Both cards have balances of more than $500. One card has an interest rate that is twice as high as the other. (Assume your choice won't impact your motivation to make future payments.)	• *Pay off $500 on higher interest card* • Pay off $250 on both • Pay off $500 on lower interest card	76%	65%	60%
Imagine your employer matches up to 8% if you contribute from your pay checks to your retirement account. Which of these do you choose? (Assume you have 3 years' worth of your new job's salary saved and plan to retire at 65 and live to 85)	• Contribute nothing • Contribute 2% • *Contribute 6%*	86%	75%	69%
Imagine you have a $425 balance on your credit card, due tomorrow. You have thousands of dollars that you don't need for any other expenses.	• Make min payment • *Pay whole balance* • Pay $100	89%	85%	80%

[a]The questions presented in this table are abbreviated; for exact text, see the Web Appendix.
[b]The options presented in italics are the correct answers.
Notes: The percentages listed are the percentages who chose the correct answer. Overall, across each item, accuracy was significantly higher in the no-default condition compared with the bad-default condition and significantly higher in the good-default condition compared with the no-default condition. APR = annual percentage rate.

in the context of consumer financial decisions such as selecting which credit card to acquire. In Study 2, we show that the findings of Study 1 generalize across different consumer decision contexts (consumer sustainability decisions, consumer financial decisions, and retail product choices) and different types of choice architecture (interventions that sort options, preselect a default, and reduce the number of options, specifically). In Study 3, we used data from individuals whose employers by default automatically enrolled them into a retirement plan, testing whether consumers with lower SES and domain knowledge were more likely to accept the default enrollment according to self-reported decisions in this high-stakes real-life context. In Study 4, we examined whether the effects of domain knowledge and SES generalize to a vastly different domain: consumer health decisions in the context of COVID-19. Finally, in Study 5 and a supplemental study in the Web Appendix, we conceptually replicated Study 1 while addressing alternative explanations and examining proposed mediators.

We preregistered all studies at aspredicted.org, except Study 3, which used an existing data set. To eliminate the file drawer problem for this research, we report all studies that we conducted and all preregistered analyses for each study. Data, preregistrations, and analysis scripts are available at https://osf.io/a7b32/?view_only=f4df788f178844f6b26e5274a9cbdab1, with the exception of Study 3, which was from a syndicated panel that we do not have permission to share. Across studies, we sought converging evidence for our hypotheses.

Study 1: Do Defaults Reduce Disparities?

In Study 1, participants made five consumer financial decisions. For each decision, they were randomly assigned to a good-default, bad-default, or no-default condition. We hypothesized that good defaults would benefit consumers with low SES, low financial literacy, and low numerical ability more than consumers with high SES, financial literacy, and numerical ability. The Study 1 hypotheses, sample size, and analysis plan are available at https://aspredicted.org/blind.php?x=x547ih.

Method

Participants. We requested 450 participants from ROIRocket. Participants (53.1% female; M_{age} = 50.2 years) were given $1 upon completion of the study. ROIRocket provides a population inexperienced with academic surveys (median of two previous academic surveys; see the Web Appendix), and substantially less experienced than participants on MTurk. To increase statistical power to attain SES effects and ensure that we had enough SES variability, we requested that ROIRocket oversample people who did not finish high school as well as people with advanced degrees (in Study 1 only). ROIRocket provided us with far more participants than requested (N = 825). We included in primary analyses all 825 participants who finished the study.[2]

Procedure. After the consent process, participants made five focal decisions. These decisions are displayed in Table 1. For example, one decision asked participants whether they would repay interest on a high-interest credit card or lower-interest card if they had equal debt on both cards (a common task similar to Amar et al. [2011]). Participants were asked to select the option that had the largest total monetary benefits minus costs. These five questions each had a mathematically correct option that would save the most money if it were a real-life decision.

For each question, participants were randomly assigned to one of three default conditions. In the no-default condition, no answer was preselected. In the good-default condition, the correct option (which would save the consumer the most money) was preselected. In the bad-default condition, an incorrect (i.e., more costly) option was preselected. Participants in the good- and bad-default conditions were told, "An option has been preselected for you. You may keep that selection or switch to another option." Because the default condition was randomly determined for each question, participants received different conditions for different questions. We used this design to increase power (McClelland 2000).

After making the five focal decisions, participants completed measures designed to assess their predictions about how much they were influenced by the defaults. Two questions asked them how likely they thought they would be to get a focal consumer financial decision correct if (1) the correct answer was preselected or (2) if an incorrect answer was preselected.

Then, participants completed measures of the factors we predicted would moderate nudge effects—financial literacy, numeracy, and SES. They also completed exploratory measures of agreeableness, need for cognition, self-reported credit score, and self-reported patience (for text of all measures, see the Web Appendix). To assess financial literacy, we used a common scale (Fernandes, Lynch, and Netemeyer 2014) that asked participants multiple choice questions about common financial instruments and techniques such as stocks, 401(k)s, and diversification ($\alpha = .85$). We measured numeracy with 11 questions ($\alpha = .87$) that assessed understanding of probability, frequency, and percentages (Lipkus, Samsa, and Rimer 2001). Following previous research and American Psychological Association recommendations for measuring and conceptualizing SES (Adler et al. 2000; Saegert et al. 2006), the SES measure included three components: education level, occupation status, and income. As in previous SES research, we standardized and averaged the three components for analysis (Adler et al. 2000). The measure had high internal consistency ($\alpha = .78$). Factor analyses indicated that the SES, financial literacy, and numeracy items loaded on three separate factors as expected (Web Appendix Tables A1 and A2; oblimin rotation was used).

We included measures of agreeableness and need for cognition in this study to address alternative explanations that agreeable personalities or desires for elaborative thought (rather than SES and domain knowledge) might explain differences in default effects across people. We also measured the total time participants spent completing the study (log-transformed as preregistered), which served as a proxy for overall survey engagement.

In addition, we included assumption check items to ensure that the correct answers were best for a wide variety of people, including those with low SES and few liquid assets (details in the Web Appendix). After responding to the main measures in Study 1 but before reporting demographics, participants made three consumer decisions with no correct answer, so that we could ensure that the focal moderators generalize beyond the context of questions with a correct answer. The three items asked participants to choose which flight insurance option to buy; which laptop computer to buy based on price, image, and consumer reviews; and which painkiller to purchase based on price and brand. Each item had three options and participants saw each item with one of the three options preselected or with no option preselected. Results from these questions were not included in the primary analyses mentioned in the preregistration, so we label these analyses as exploratory and report them separately from primary analyses.

Participants also reported demographics and how many past studies they had completed. We included an attention check to ensure that effects were robust when accounting for people who rushed through the survey. The attention check asked them to select a particular answer for a fake question added in the middle of the financial literacy scale. Following our preregistration, we included all participants, including those who failed the attention check, in primary analyses (though all effects remained significant when excluding attention check failures).

Analytical approach. In each study, we analyzed results using binomial generalized mixed effects models. We estimated decision accuracy (1 = correct, 0 = incorrect) as the dependent variable and treated participants as random factors to properly model variance across people (Bates et al. 2015). As preregistered, the models in studies with three default conditions included a contrast-coded default condition term (1 = good default, 0 = no default, −1 = bad default) and the orthogonal contrast. All models contained item fixed effects that accounted for variation in difficulty across different questions. We tested hypothesized moderators of default effects and standardized the moderating variables.

[2] We included all participants in primary analyses because we preregistered that we would include all who finished the study. All significant effects remained significant when including only the 450 requested (i.e., the first 450 to finish; see the Web Appendix).

Results

Defaults strongly influenced decisions on average. Participants in the bad-default condition answered 62% of items correctly (choosing the most advantageous option) compared with 71% in the no-default condition and 78% in the good-default condition ($z = 10.73$, Exp(B) $= 1.62$, $p < .001$). Simple effects tests indicated that the difference between the no-default and good-default conditions was sizable ($z = 4.39$, Exp(B) $= 1.62$, $p < .001$), as was the difference between the no-default and bad-default conditions ($z = -4.63$, Exp(B) $= .61$, $p < .001$).

Socioeconomic status. As predicted, there was an SES × default condition interaction, such that default effects were larger among lower-SES consumers than higher-SES consumers ($z = -3.64$, Exp(B) $= .83$, $p < .001$; Figure 2, Panel A). Simple effects tests indicated that default effects were over 2.2 times larger for people in the bottom half of the SES distribution compared with the top half. SES was weakly correlated with survey engagement ($r = .03$), and its interaction with default condition was robust when controlling for engagement ($z = -3.65$, $p < .001$).

Financial literacy. As we predicted, there was a large financial literacy × default condition interaction ($z = -6.32$, Exp(B) $=$ .75, $p < .001$; Figure 2, Panel B). Participants lower in financial literacy were impacted by defaults more than participants higher in financial literacy. This interaction remained significant when controlling for SES, numeracy, and their interactions with default condition ($z = -2.41$, Exp(B) $= .86$, $p = .016$).

Numeracy. There was also a numeracy × default condition interaction ($z = -6.83$, Exp(B) $= .74$, $p < .001$; Figure 2, Panel C), such that those with lower numerical ability were impacted by defaults more than those with higher numerical ability. This interaction remained significant when controlling for SES, financial literacy, and their interactions with default condition ($z = -3.63$, Exp(B) $= .82$, $p < .001$). This implies that the interactions with numeracy and financial literacy were at least partly independent effects.

Our conceptual framework (Figure 1) suggests that financial literacy and numeracy account for the SES × default condition interaction. Consistent with this, the SES × default condition interaction was greatly reduced when we controlled for numeracy, financial literacy, and their interactions with default condition ($z = -.46$, Exp(B) $= .97$, $p = .648$). In the Web Appendix, we show that mediation models were also consistent with this idea that numeracy and financial literacy account for the moderating effects of SES on default effects.

Questions with no correct answer. We also conducted exploratory analyses of three consumer choice questions with no correct answer. We included these items to examine whether the key results (that consumers lower in SES, numeracy, and domain knowledge are more impacted by defaults) generalized beyond the context of questions with a correct answer. Participants with lower SES were more likely to retain default options on average ($z = -3.22$, Exp(B) $= .81$, $p = .001$). In addition, those with lower financial literacy were more likely to retain the default options ($z = -4.36$, Exp(B) $= .79$, $p < .001$), as were those with lower numeracy ($z = -4.85$, Exp(B) $= .76$, $p < .001$). This suggests that participants with low SES, low financial literacy, and low numeracy are more likely to choose default options and that our key findings are not simply the result of participants with low SES, low financial literacy, and low numeracy having less access to correct answers.

Mispredicting default effects. Participants predicted that defaults would have little, if any, impact on their decision accuracy. We asked participants two questions in which they reported how likely they thought it was that they would answer a focal consumer financial decision question correctly (1) if the correct answer was preselected and (2) if an incorrect answer was preselected (in each case, they were asked to assume they were not told whether the default option was correct).

Participants thought their likelihood of answering correctly would be 65% if assigned to a good default and 64% if assigned to a bad default ($t(824) = 1.84$, $p = .066$). Financial literacy, numeracy, and SES were not significantly associated with participants' predictions of how much they would be impacted by defaults (see the Web Appendix; if anything, more numerate consumers thought they would be impacted more by defaults, though they were actually less impacted). Interestingly, participants were not overconfident on average; they were simply miscalibrated about default effects. They greatly underestimated how accurate they would be when assigned to a good default (estimates = 65%, reality = 78%) and were close to reality when regarding bad defaults (estimates = 64%, reality = 62%).

Robustness tests. We preregistered the following three robustness tests. In the first, we wanted to control for how engaged participants were with the study (assessed via the log-transformed time they spent completing it). In the second, we controlled for agreeableness and need for cognition.[3] In the third, we excluded participants who failed the attention check. All three focal moderators remained significant and similar in size across all of these robustness tests (all zs < -3, all ps $< .001$; for further details, see the Web Appendix).

Discussion

As we predicted, consumers who had lower SES, lower financial literacy, and lower numeracy were more impacted by defaults than consumers who had higher SES, higher financial literacy, and higher numeracy. In other words, good defaults were an equalizer that helped reduce the differences in decision quality between consumers with low versus high SES, numeracy, and financial literacy. Interestingly, participants seemed

[3] In these first two robustness tests and all other robustness tests in any experiment that involved adding a covariate, we also controlled for the covariate × nudge condition interaction.

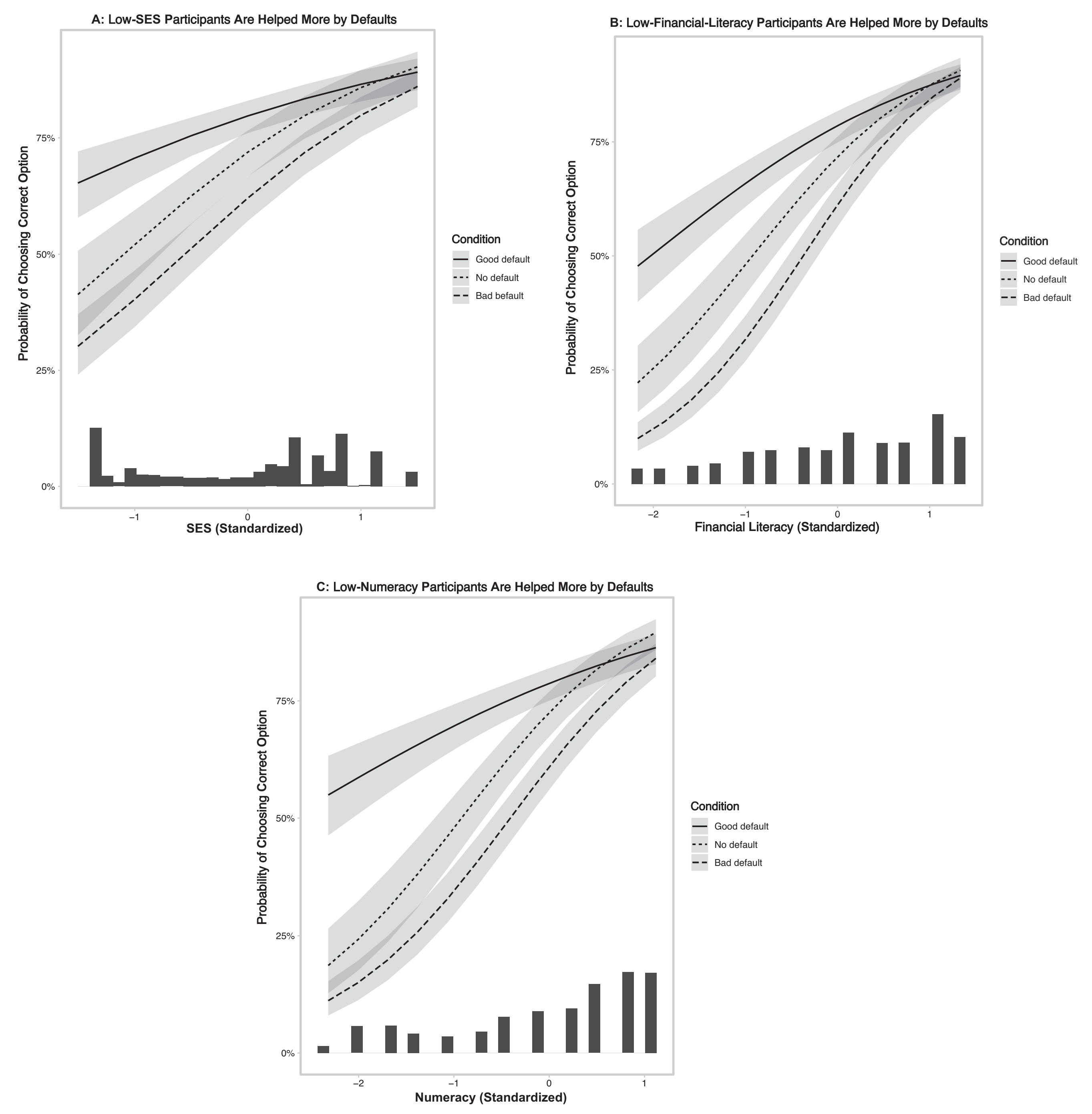

Figure 2. Default effects were larger among consumers lower in SES, lower in financial literacy, and lower in numeracy.
Notes: The bad-default, no-default, and good-default conditions are depicted by dashed, dotted, and solid lines, respectively. Good nudges reduced disparities, as depicted by the small difference between consumers low and high in each variable in the good-default condition (shallow solid line) compared with the no-default and bad-default conditions (steeper dotted and dashed lines). Shaded regions depict ± 1 SE. The histograms along the x-axis depict the distribution of each moderator.

largely unaware of the impact of defaults. They did not anticipate that defaults would influence their behavior, nor did consumers lower in SES, financial literacy, or numeracy predict they would be impacted more. It is worth noting that we oversampled people with very low or very high education in Study 1. Although this increased statistical power, the sample was different from the general population. In subsequent studies, we use more balanced samples (with no oversamples), and in Study 3 we use a more representative stratified random sample of U.S. households.

In a supplemental study, we addressed alternative explanations for the effects found in Study 1, namely that effects of financial literacy and numeracy might be explained by participants who were not understanding the questions, not paying attention, or not conscientious (see Web Appendix). In this supplemental study, we replicated these key interactions from Study 1 and showed that these were robust even when controlling for comprehension of the decision questions and individual differences in conscientiousness. This suggests that people lower in numeracy and domain knowledge are impacted more by defaults, that these effects are replicable, and that they are not attributable to low conscientiousness or poor comprehension.

Study 1 highlights how default effects are moderated by differences in financial literacy, numeracy, and SES. In Study 2, we wanted to examine whether these results generalize across three different types of nudges in three decision-making contexts with incentives for accuracy.

Study 2: Do Nudges Reduce Disparities Across Different Contexts and Types of Nudges?

Study 2 was designed primarily to test whether the results observed in Study 1 generalize across different types of nudges and across different consumer contexts. In addition, we added incentives for half of the decisions to examine whether incentives moderate the effects observed in Study 1. We expected that moderators observed in Study 1 would generalize across the three nudge types, across the three contexts, and across incentivized and nonincentivized decisions. We also included a measure of general fluid intelligence to isolate domain knowledge from general intelligence. We preregistered sample size, predictions, and analyses at https://aspredicted.org/blind.php?x=v3ci5q and report all preregistered analyses.

Method

Participants. ROIRocket respondents (N = 428; 51.6% female; M_{age} = 53.2 years) participated in exchange for a fixed payment of $1 and a $2 bonus if they answered one of the focal consumer financial decisions correctly. In this and all subsequent studies, participants who had completed any of our previous studies were not allowed to participate.[4]

Procedure. The procedure was similar to Study 1, but with three different types of nudges and with decisions that spanned three different contexts. Participants answered six focal questions with mathematically correct answers. The three contexts were retail product choices, consumer financial decisions, and consumer sustainability decisions. The two retail product choices involved choosing a computer with or without insurance, and food with the lowest price per ounce. The consumer financial decisions were slightly altered versions of the debt repayment and retirement questions used in Study 1. The consumer sustainability decisions involved choosing window insulation that would maximize total savings and choosing lightbulbs with the lowest unit price. Participants were asked to choose the item with the lowest average monetary costs and were incentivized to choose these options for half the questions. The Web Appendix provides the full text of each question. The three types of nudges were defaults, sorting, and number of options. The default manipulation was similar to Study 1 but with only the good-nudge and no-nudge conditions (because these often have higher ecological validity),[5] the sorting manipulation varied whether options were ordered from best to worst ("good sort") or randomly ("no sort"), and the number of options manipulation varied whether ten options were presented ("many options") or only two of the best options ("few options"), following Sela, Berger, and Liu (2008). All sorting and default questions had ten options.

The design was thus a 3 (context: retail product choices, consumer financial decisions, consumer sustainability decisions) $\times$ 3 (nudge type: defaults, sorting, number of options) $\times$ 2 (nudge condition: good nudge, no nudge) $\times$ 2 (incentive: $2, $0) experimental design. The questions were organized in three blocks in counterbalanced order corresponding to different contexts and nudge types.[6] The first three questions were incentivized for some participants and the last three questions were incentivized for others.

Following the six focal decisions, participants completed the same measures of financial literacy as in Study 1 and a shortened three-item version of the numeracy measure (Schwartz et al. 1997) to reduce the length of the survey. To isolate domain knowledge effects (financial literacy) from general intelligence, we included a measure of general fluid intelligence called number series (McArdle 2015). The measure asked participants to answer six questions that involved completing a pattern of numbers such as "23, 26, 30, 35, __" (correct answer: 41). Then, participants completed the three-item of measure of SES described in Study 1. Finally, participants reported their credit score range, completed the attention check item, completed a measure of time preferences (see the Web Appendix), and reported their age and gender.

Results

On average, the nudges had their intended effects. We estimated accuracy in binomial mixed-effects models as a function of nudge condition (contrast-coded), with the rest of the model the same as

[4] In each study, ROIRocket provided a larger sample than requested to account for potential dropouts (e.g., 428 rather than 400 in Study 2), though we analyzed all participants who finished the study, as preregistered.

[5] We sampled the good-nudge and control conditions in Study 2 (without bad-nudge conditions), partly because they likely have higher ecological validity (e.g., sorting from worst value to best value in online retail is likely uncommon) and partly because a "bad sort" could make choices easier than a random arrangement of options if consumers notice the pattern.

[6] For example, some participants received two retail purchase questions followed by two consumer financial decisions followed by two consumer sustainability decisions. A Latin square was used to counterbalance the context and nudge type across participants.

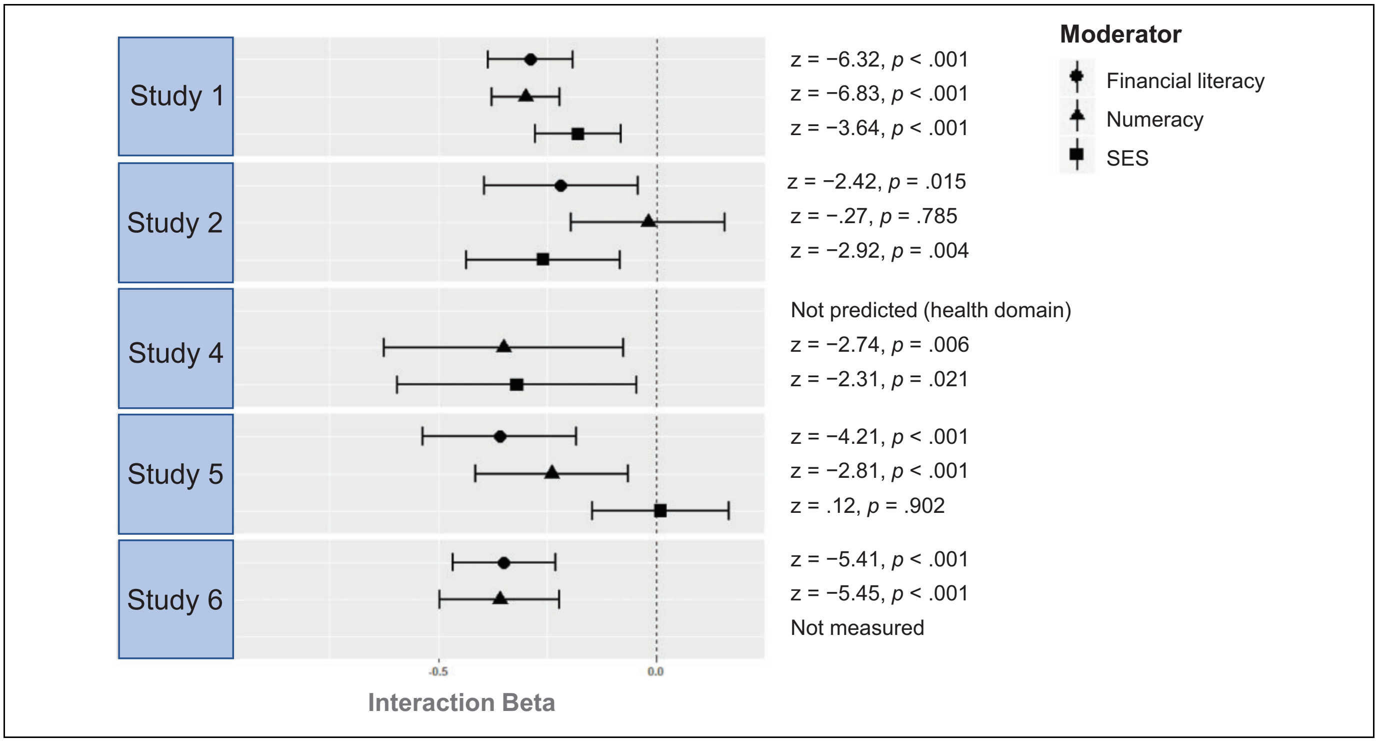

Figure 3. Forest plot conveying the three moderators of nudge effects across studies.
Notesz: The effects were relatively consistent and robust across studies, though numeracy and SES had nonsignificant effects in one study each. Study 3 is omitted because it used a different dependent variable (self-reports of whether participants retained default retirement options).

in Study 1. Accuracy was higher when good nudges were used (M = 56%) compared with no nudge (M = 42%; z = 7.49, Exp(B) = 1.87, $p < .001$). These effects were strong for the default and number of options nudges but nonsignificant for sorting ($M_{good\ default} = 55\%$, $M_{no\ default} = 40\%$; $M_{few\ options} = 68\%$, $M_{many\ options} = 43\%$; $M_{good\ sort} = 46\%$, $M_{no\ sort} = 43\%$).

Socioeconomic status. Nudge effects were moderated by SES such that they impacted low-SES participants more than high-SES participants (z = −2.92, Exp(B) = .77, $p = .004$). That is, nudges designed to facilitate selection of the best option reduced choice disparities by helping low-SES consumers more than high-SES consumers. Consistent with our framework (Figure 1), when we included financial literacy and the financial literacy × nudge condition interaction in the model, the SES × nudge condition interaction was no longer significant (χ^2(2, n = 413) = 1.87, $p = .170$). The SES × nudge condition interaction was not significantly moderated by nudge type (χ^2(2, n = 428) = 2.74, $p = .255$) or decision context (χ^2(2, n = 428) = 4.09, $p = .129$). It was also robust when controlling for survey engagement (z = −2.91, $p = .004$), and SES was very weakly correlated with survey engagement (r = .02).

Financial literacy. As we predicted, nudges had more impact on consumers with lower financial literacy than those with higher financial literacy (z = −2.42, Exp(B) = .80, $p = .015$). Figure 3 shows the robust effects of financial literacy across studies. These financial literacy × nudge condition interactions were not significantly moderated by the type of nudge (χ^2(2, n = 428) = .47, $p = .790$) or by the decision context (χ^2(2, n = 428) = 3.06, $p = .216$).

Numeracy. Unlike in Study 1 and all of our subsequent studies, numeracy did not moderate the impact of nudges (z = −.27, Exp(B) = .97, $p = .785$). In the Web Appendix, we explore different reasons for this difference, concluding that this is partly attributable to low reliability and lower validity on the three-item numeracy scale in Study 2 ($\alpha = .53$) compared with the longer and more sensitive numeracy measure used in Study 1 and subsequent studies (Study 1: $\alpha = .87$). The relationship between numeracy and nudge effects was not moderated by the type of nudge (χ^2(2, n = 428) = .57, $p = .751$) or by the decision context (χ^2(2, n = 428) = 1.57, $p = .457$).

General fluid intelligence. We predicted that general intelligence would also moderate default effects but that it would not fully account for the financial literacy effect. Contrary to our expectations, consumers who scored higher on the measure of general fluid intelligence were not significantly less susceptible to nudges (z = −1.41, Exp(B) = .87, 95% confidence interval [CI] = [.72, 1.05], $p = .157$). The financial literacy × nudge condition and SES × nudge condition interactions remained significant when controlling for fluid intelligence (both *zs* = −2, *ps* < .05). This finding suggests that financial literacy and other forms of domain-specific knowledge likely influence nudge effects more than general fluid intelligence.

Robustness tests. We controlled for survey engagement, which did not appreciably change the interactions of condition with financial literacy or SES (both zs < -2, both ps $< .05$).

Incentives. The incentive manipulation did not significantly influence accuracy ($M_{incentivized} = 51\%$, $M_{nonincentivized} = 48\%$; $z = 1.35$, Exp(B) $= 1.13$, $p = .178$), though it did increase the amount of time participants spent on the questions ($M_{incentivized} =$ 126 seconds, $M_{nonincentivized} = 87$ seconds; $t(2{,}132.01) = 4.23$, $b = .11$, $p < .001$). On average, the nudges increased accuracy about four times more than a $2 incentive. The key interactions were not any smaller for the incentivized questions than the nonincentivized questions (see the Web Appendix).

Discussion

Consistent with Study 1, SES and financial literacy each moderated the effects of nudges in Study 2. These effects were present even though decisions were incentivized. It is not surprising that the effect of financial literacy was not moderated by the decision context, because the decisions we examined in Study 2 all involved numbers, prices, financial information, or calculations. As mentioned previously, financial literacy and numeracy are useful across many contexts of consumer choice because they are used to compare prices and quantities, calculate unit prices, and calculate cost effectiveness and long-term savings (Graffeo, Polonio, and Bonini 2015; Santana et al. 2020). Thus, we did not expect context to moderate effects of financial literacy in Study 2.

Although the results of Studies 1 and 2 demonstrate important and consistent effects, it is not yet clear whether the results generalize to high-stakes, real-life decisions. Therefore, in Study 3, we use data about Americans' self-reported retirement investment choices. We examine whether defaults influence low-SES consumers more than high-SES consumers in this context.

Study 3: Defaults and Retirement Decision Data

In Study 3, we acquired (self-reported) data about Americans' retirement investment decisions. We examined a sample of consumers who work for companies that set defaults by automatically enrolling employees into retirement contributions. Respondents were asked whether they opted out of the default contribution amount and default investment allocation set by their company. We predicted that consumers lower in SES and financial knowledge would be more likely to choose the default options than those with higher SES and financial knowledge.

Method

The secondary data we used consisted of stratified random samples of U.S. households. The panel, Strategic Business Insights (SBI) MacroMonitor, is a syndicated panel that asks respondents questions about their financial decisions and demographics. The panel is conducted with different households every other year. We were given access to four different samples from the panels that were conducted in 2010, 2012, 2014, and 2016, respectively.

Our primary interest was in three questions that asked respondents whether they accepted or rejected their employer's default options in real retirement decisions. Specifically, respondents were asked whether their current employer automatically enrolled them into a retirement plan (753 indicated yes, 3,580 indicated no, and the rest selected "does not apply" because they were retired or unemployed). Following this, respondents who had answered "yes" were asked two questions assessing (1) whether they kept the default contribution percentage and (2) whether they kept the default investment allocation. Of those who reported they were automatically enrolled, 48% indicated they accepted the default investment allocation, whereas 52% opted out and chose a different allocation. For the default contribution amount question, 45% indicated they had accepted the default contribution amount, whereas 55% opted out. We analyzed default selection (1 = chose default option, 0 = opted out of default) in binomial generalized mixed models as a function of the question (allocation or amount) and hypothesized predictors.

We examined measures of SES and financial sophistication. The SES measure followed the preregistered measure used in Study 2 as closely as possible (education, income, and occupation, standardized and combined; for details, see the Web Appendix).

Self-reported financial sophistication was analyzed using the following two measures, consistent with previous research that used the SBI MacroMonitor data (Mrkva et al. 2020). The self-reported financial sophistication item asked participants to rate their agreement with the statement "I consider myself a sophisticated investor" (1 = "mostly disagree," and 4 = "mostly agree"). The financial experience item asked respondents whether they handle their household's financial investments. Other items were assessed in the survey, including gender, age, marital status, number of children, U.S. census region, religion, race, hours worked per week, and risk aversion.

Results

Socioeconomic status. We first tested whether low-SES individuals were more likely to choose the default options. Participants with lower SES were more likely to accept the default options ($z = -5.71$, Exp(B) $= .33$, $p < .001$).

Financial sophistication and experience. We computed a model estimating default choices as a function of self-reported financial sophistication and financial experience. Individuals with lower financial sophistication were more likely to accept the default option ($z = -5.62$, Exp(B) $= .40$, $p < .001$), as were those with lower financial experience ($z = -2.88$, Exp(B) $=$ $.66$, $p = .004$). These effects are broadly consistent with Studies 1 and 2, though SBI used measures of financial sophistication that differed from the financial literacy scale we used in the experiments we designed.

Robustness tests. We also conducted a robustness test in which we controlled for all the covariates listed in the "Method" subsection. This was designed to address alternative explanations that the effects of SES and financial sophistication were actually explained by differences in any of these other variables. When adjusting for these covariates, the effects of SES, financial sophistication, and investment experience remained significant (all zs < -3, ps $< .01$). SES and financial sophistication influenced both default questions individually (Web Appendix).

Discussion

The results of Study 3 demonstrate that consumers with low SES and low financial sophistication are more likely to retain default options, even in self-reports of their high-stakes retirement decisions. This is consistent with working papers that found larger effects of automatic enrollment for younger and low-income individuals compared with older and high-income individuals (Beshears et al. 2016; Choukhmane 2021). It is worth noting that typical default enrollment rates of 3% and 6% are likely insufficient for many people, and it is possible that some respondents who opted out chose higher amounts in Study 3. Therefore, we cannot infer that automatic enrollment improved decisions.

Though Studies 1–3 suggest the results generalize to many important decisions, most of the decisions we examined were consumer decisions with prices or financial elements. In Study 4, we demonstrate generalizability further by examining a dramatically different context of health decisions in the early stages of the COVID-19 pandemic.

Study 4: Do Defaults Reduce Disparities in COVID-19 Consumer Health Decisions?

In Study 4, we aimed to generalize our results from Studies 1–3 to questions about optimal behavior during the COVID-19 pandemic. We hypothesized that participants with lower SES, numeracy, and health literacy would be impacted more by defaults in this context. We also tested whether domain-specific health knowledge moderated nudge effects more than less relevant financial knowledge. Thus, unlike in the previous studies, we did not predict financial literacy would moderate default effects, because it is less relevant for health decisions. Instead, we predicted that health literacy would moderate default effects. We preregistered the sample size, hypotheses, and analyses at https://aspredicted.org/blind.php?x=an4kx6.

Method

Participants. Participants from ROIRocket completed the experiment in exchange for $.50 (N = 305; 50.8% female; $M_{age} = 52.0$ years). This experiment was conducted in April 2020 while much of the United States was under restrictions designed to slow the spread of COVID-19.

Procedure. Participants answered four questions about how they would respond to different scenarios in the context of COVID-19. The four questions, respectively, asked participants whether they would wear a mask in public, how they would disinfect surfaces, what they should do if they have an upset stomach and runny nose, and how long to wait before touching packages delivered to the door (for full text, see the Web Appendix). Participants were told to follow Centers for Disease Control and Prevention guidelines and assume that those guidelines were all correct. Answers were coded for accuracy (1 = correct, 0 = incorrect). For each question, participants were assigned to either the no-default or good-default condition. We did not include a bad-default condition, because it could spread misinformation about COVID-19. The questions assigned to each condition were counterbalanced, and participants received two questions in each condition.

Following these four questions, participants completed measures of numeracy, financial literacy, health literacy, SES, other demographics, and an attention check. We used a longer nine-item numeracy measure in Study 4 (Lipkus, Samsa, and Rimer 2001), because we suspected that the null numeracy result in Study 2 was due to low reliability of the three-item measure. The numeracy measure included two subscales consisting of health numeracy questions (six items) and general numeracy questions about lotteries (three items), respectively. The health literacy measure included items such as interpreting "drug facts" from a medicine label (see Web Appendix). The other measures (financial literacy, SES, and attention check) were the same as in Study 2.

Results

On average, good defaults increased accuracy compared with the no-default condition. Accuracy was significantly lower in the no-default condition (M = 64%) compared with the good-default condition (M = 72%; z = 3.35, Exp(B) = 1.58, $p < .001$).

Socioeconomic status. As we predicted, consumers with lower SES were more impacted by defaults, as indicated by the SES × default condition interaction (z = −2.31, Exp(B) = .73, $p = .021$). The default effect was over four times larger among consumers with below-average SES compared with those with above-average SES. This effect was no longer significant when we added numeracy to the model (z = −1.43, Exp(B) = .81, $p = .151$), consistent with Figure 1.

Health numeracy and general numeracy. Overall, numeracy moderated default effects: less numerate participants were more impacted by defaults than numerate participants (z = −2.57 Exp(B) = .71, $p = .010$). To examine whether domain-specific health numeracy impacted decisions more than general numeracy, we also separately examined subscales that assessed health numeracy and general numeracy, respectively. Health numeracy significantly moderated the default effects, such that those with lower health numeracy exhibited larger default effects ($z = -2.83$, Exp(B) = .79, $p = .004$). In contrast, the general numeracy subscale did not significantly moderate default effects ($z = -1.57$, Exp(B) = .81, $p = .117$).

Health literacy and financial literacy. We predicted that same-domain (health) knowledge would influence default effects more than other-domain knowledge (e.g., financial literacy). Consistent with this, financial literacy did not significantly moderate the default effects ($z = .55$, Exp(B) = 1.02, 95% CI = [.83, 1.40], $p = .582$). Note that one cannot conclude from a nonsignificant result that the moderating effect of financial literacy is zero. However, the 95% CI includes only small positive or negative effects that are smaller than the moderating effects of numeracy and SES (for Bayes factor analyses, see the Web Appendix).

Although we expected health literacy to significantly moderate default effects, this result was only marginal ($z = -1.81$, Exp(B) = .79, 95% CI = [.60, 1.02], $p = .070$). As detailed in the Web Appendix, we suspect that this health literacy result was marginal and smaller than expected because nearly all participants scored very high on the measure (giving us low power due to the low variability). Health literacy was weakly correlated with SES ($r = .13$).

Robustness tests. We preregistered two robustness tests that excluded attention check failures and adjusted for survey engagement, respectively. All significant interactions with default condition remained significant in these robustness tests (all $zs < -2$, $ps < .05$).

Mediation model. We used a bootstrapped mediation model with 5,000 resamples (Preacher and Hayes 2008) to examine whether consumers with low SES are more nudgeable because they are less numerate (see Figure 1). There was a significant indirect effect consistent with the proposed path from lower SES to lower numeracy to larger default effects (indirect effect = −.07, 95% CI = [−.13, −.02]). The effect of SES on the size of default effects was reduced when numeracy was added to the model (from $c = -.19$, 95% CI = [−.31, −.06] to $c^1 = -.11$, 95% CI = [−.25, .03]), consistent with our predictions. An alternative mediation possibility is that SES influences nudges by causing consumers to allocate time differently (Shah, Mullainathan, and Shafir 2012). Contrary to this possibility, SES was not associated with time spent on these questions ($z = -.19$, Exp(B) = .99, $p = .847$), and there was no indirect effect of SES on default effects through decision time in a parallel mediation model ($ab = .00$, 95% CI = [−.01, .01]).

Discussion

The results of Study 4 replicate and extend the results of previous studies to the context of COVID-19 health decisions. Low-SES people benefited disproportionately from nudges even in the context of questions about COVID-19. Low-SES people are disproportionately affected by COVID-19 and thus have the most to gain from interventions that help them. Mediation models were consistent with our framework in which low-SES individuals are more impacted by nudges, not because they allocate time differently but because of differences in domain-specific skills. In Study 5, we test the remainder of our conceptual diagram in sequential mediation models.

Study 5: Why Do Numeracy and Financial Literacy Moderate Nudges?

Study 5 had two purposes. First, we generalized our results across two different samples, including a sample of Master of Business Administration (MBA) students at an elite university. This would ensure that our findings generalized beyond a sample with relatively low financial knowledge. Second, we tested the proposed mediation model displayed in Figure 1 about why financial literacy, numeracy, and SES moderate default effects. Consumers with low numeracy and financial literacy experience greater uncertainty and anxiety when facing consumer decisions involving numbers or math (Skagerlund et al. 2018). In turn, anxiety and uncertainty likely increases susceptibility to default effects (e.g., Huh, Vosgerau, and Morewedge 2014). We tested these proposed paths with mediation models. In addition, we hypothesized that financial literacy, numeracy, and SES would moderate default effects, replicating the results of our previous studies. We preregistered the sample size, hypotheses, and analyses at https://aspredicted.org/blind.php?x=4yz385.

Method

Participants. We requested and preregistered a sample of 200 participants from ROIRocket and an estimated 100 MBA students. All participants received a \$2 bonus if they answered one randomly selected focal financial question correctly. ROIRocket participants also received \$1 fixed payment, whereas MBA students received points for a minor class assignment. The ROIRocket sample was more diverse and older ($n = 212$; 50.9% male; median age = 54 years) than the MBA sample ($n = 75$; 61.3% male; median age = 29 years). The MBA students had higher financial literacy and numeracy compared with ROIRocket participants (financial literacy questions answered correctly: $M_{MBA} = 87\%$, $M_{ROIRocket} = 64\%$; numeracy questions answered correctly: $M_{MBA} = 90\%$, $M_{ROIRocket} = 53\%$; both $ts > 3$, $ps < .001$).

Procedure. The procedure was similar to Study 1, except for the following differences. All five decisions were incentive compatible, and one retail product choice (of laptops with different insurance options) was added (also used in Study 2).[7] We also examined potential mediators by assessing perceived uncertainty, decision anxiety, and preference construction; we suspected that each of these three variables partially accounts for

[7] The questions used the same wordings as in the supplemental study (see Web Appendix section 3 for the wordings), which were slightly different from the question wordings used in Study 1.

the effects of domain-specific skills on default effects (as described previously). The three factors, though correlated, had discriminant validity (see the Web Appendix) and have also been differentiated in previous research (Peters and Bjalkebring 2015; Peters et al. 2019; Skagerlund et al. 2018).

Results

We used the same model structure as in Study 1. Participants were more likely to choose the correct answer in the good-default condition (M = 63%) than in the no-default (M = 60%) and bad-default conditions (M = 56%; $z = 3.23$, $\text{Exp}(B) = 1.33$, $p = .001$).

Socioeconomic status. Unlike in the previous studies, SES did not significantly moderate the effects in Study 5 ($z = .12$, $\text{Exp}(B) = 1.01$, $p = .901$).[8] SES was not correlated with survey engagement either ($r = .00$).

Financial literacy. As we predicted, consumers with lower financial literacy were more impacted by defaults as in the previous studies ($z = -4.21$, $\text{Exp}(B) = .70$, $p < .001$; Figure 3). The default effect was over five times larger among consumers with below-average financial literacy compared with those above-average financial literacy. When we controlled for the numeracy × default condition interaction, the financial literacy interaction remained significant.

Numeracy. Participants low in numeracy were also more impacted by defaults as indicated by the numeracy × default condition interaction ($z = -2.81$, $\text{Exp}(B) = .78$, $p = .005$).

Robustness tests. Financial literacy and numeracy moderated the default effects even when adjusting for survey engagement (and when adjusting for MBA vs. ROIRocket participants; all $zs < -2.5$, $ps < .01$). When we excluded attention check failures, the interaction with financial literacy remained similar in size, though the interaction with numeracy reduced slightly and was marginal (financial literacy: $z < -2$, $p < .01$; numeracy: $z = -1.84$, $\text{Exp}(B) = .84$, $p = .065$).

Mediation model. We conducted mediation models with 5,000 bootstrapped resamples to examine the proposed mediation paths displayed in Figure 1 (Preacher and Hayes 2008). The first models examined the paths from SES to numeracy to the three possible mediators (anxiety, preference construction, and decision uncertainty) to larger default effects. When examining these three mediators in parallel, there was a significant indirect effect through anxiety, consistent with partial mediation through anxiety ($ab = -.01$, 95% CI = [−.027, −.001]). This reflected a positive relationship between SES and numeracy ($b = .39$, 95% CI = [.28, .50]), a negative relationship between numeracy and anxiety ($b = -.29$, 95% CI = [−.42, −.16], and a positive relationship between anxiety and larger default effects ($b = .11$, 95% CI = [.04, .18]). (A second indirect effect through preference construction was significant when examined without the other two mediators, but not in a parallel mediation model with the other two mediators. There was no significant indirect effect through uncertainty, contrary to expectation.) The analogous indirect effects through financial literacy rather than numeracy revealed very similar results (see the Web Appendix). Although there was no direct effect of SES on the size of default effects in Study 5 (unlike the previous studies), we nonetheless found support for the proposed indirect effect through numeracy and anxiety. This is consistent with our conceptual framework, though, like any mediation analysis, it should be interpreted with caution because mediation analyses cannot conclusively determine whether a mediator causes an effect.

Discussion

In Study 5, we examined whether the moderators of default effects observed in Studies 1–4 would generalize to a markedly different sample (MBA students). Consumers with lower financial literacy and numeracy were more impacted by defaults, and the mediation model was consistent with our theoretical explanation (see Figure 1) of these default effect moderators.

General Discussion

Across several studies, nudges not only influenced decision making on average but also influenced choice disparities across consumers. Low-SES consumers were impacted more by nudges, meaning that nudges that facilitated selection of a good option benefited them more than high-SES consumers. Domain knowledge and numeracy also moderated the effects of nudges: Consumers with less domain knowledge and lower numeracy were impacted more by nudges compared with those with more domain knowledge and higher numeracy.

These results generalized across a wide variety of consumption contexts. In addition, the effects were sizable. Across studies, nudges typically had two to five times greater impact among consumers with below-average SES, domain knowledge, and numeracy compared with consumers with above-average SES, domain knowledge, and numeracy. These results remained strong in incentivized decisions and across a series of preregistered robustness tests in which we adjusted for survey engagement, attention check failures, and alternative explanations of our results.

In our studies, we sought to use decisions in which one option was best for essentially all consumers (even those with low SES and few liquid assets). The results of Study 1 were consistent with this assumption. We provided participants with the outcomes of options in Study 1 based on their actual age and liquid assets and asked them which outcome would leave them better off (see the Web Appendix). The vast majority of

[8] We suspect one reason for this is that the Study 5 sample was much less diverse than the sample in previous studies. Having larger numbers of participants at each end of the SES continuum (as we had in previous studies and especially Study 1) provides much more power to detect effects (McClelland 2000).

consumers, including those with low SES and few liquid assets, selected the options facilitated by the good nudges of saving more for retirement and making a full credit card payment as more beneficial than the other options.

Because we tested the moderators of nudges across several contexts, it was possible to examine whether domain-specific skills and knowledge drive these effects. Financial literacy moderated nudge effects in the context of consumer financial decisions but not COVID-19 health decisions. In the context of the COVID-19 health decisions, health numeracy significantly moderated default effects, whereas general numeracy was not a significant moderator. These findings provide evidence that skills and knowledge moderate the effects of nudges primarily in the particular contexts in which those skills and knowledge are relevant.

Implications for Marketing and Policy

Nudges have become pervasive in marketing firms and policy circles because of their low costs and large average impact (Benartzi et al. 2017). Our results demonstrate that, beyond improving decisions on average, good nudges can reduce disparities. Because nearly every standard of ethics endorsed by governments and corporations places value on equality and reducing inequities (e.g., Schwartz 2005), this provides a strong reason to use nudges.

In addition, our findings have implications for nearly any marketing manager or online retailer. Choice architecture is an unavoidable aspect of online retail. For example, retailers must present products in some order, whether they ultimately choose to present products with highest ratings, lowest prices, highest sales volumes, or highest profit margins first (Soman 2015; Thaler and Sunstein 2009). At checkout, retailers can choose to set the default to be the product with no insurance, no add-ons, and the least expensive shipping option, or other options can be preselected that might increase revenue. The results of the present studies suggest that these choice architecture decisions not only impact consumers' choices on average but can help reduce choice disparities. Many marketing managers try to reduce inequity and invest in expensive efforts to do so (Kotler, Hessekiel, and Lee 2012). For example, some marketing firms reduce their prices for the poor or offer financial assistance to expand access to their products and reduce inequities. Because nudges are low-cost interventions and can promote options in the mutual best interest of consumers and firms (Benartzi et al. 2017), the present results suggest that nudges may be an inexpensive alternative way for firms to help the poor.

The present results also suggest that policy makers and firms need to carefully monitor the impact of their choice architecture tools on different segments of the population. Scholars have recently argued that it is important that researchers and policy makers understand heterogeneous effects of nudges across people (Soman and Hossain 2020). This can allow policy makers to design interventions that are effective even if they do not impact all consumer segments or groups of the population. Heterogeneity in nudge effects might also partly explain why some nudges have had smaller effects when applied and implemented at scale by policy makers or firms than when examined by researchers (Al-Ubaydli et al. 2020; DellaVigna and Linos 2020). In addition, our replications of the key results across studies and contexts addresses recent calls for researchers to replicate nudge effects (Al-Ubaydli et al. 2020) and examine effects of the context (Soman and Hossain 2020) to make results more useful to practitioners.

Similarly, understanding heterogeneity can help marketing firms and retailers target consumer segments that would be most impacted by nudges. For example, nudges that present options with lowest unit prices first might increase purchases among low-knowledge consumers who are less familiar with the brand more than high-knowledge consumers. In some cases, managers who ignore heterogeneity in nudge effects might underestimate the effectiveness of nudges if, for example, the low-knowledge consumers most impacted by nudges include many new customers who will continue to purchase the brand in the future. In other cases, managers who are unaware of this heterogeneity might overestimate nudge effects if, for example, nudges influence one-time purchases from low-knowledge consumers rather than high-knowledge repeat customers with greater customer lifetime value. Because low-SES consumers are most impacted by nudges, this may suggest that nudges will be less successful among luxury retailers and anyone with high-SES clientele, compared with retailers catering to low-SES clientele.

When only a one-size-fits-all nudge is available, our results suggest that policy makers should focus on the needs and potential benefits for low-SES and low-knowledge citizens when deciding which option to facilitate with the nudge. Nudges have less influence on high-SES and high-knowledge individuals, and it is reasonable to focus on the policies that will benefit those most impacted. For example, if one health care plan is optimal for low-SES people while another option is better for high-SES people (and only a one-size-fits-all nudge is possible), choice architects should prioritize the needs of low-SES individuals when choosing the default option.

Limitations and Future Research

Although we think choice architecture interventions are important tools that can reduce disparities, they should not be the only tools used to address them. Many disparities are systemic and deeply entrenched for historical, societal, or macroeconomic reasons and require interventions that change laws or elements of the macroeconomy (Feitsma 2018; Loewenstein and Chater 2017). In addition, interventions that use incentives or provide new information can be an effective supplement to nudges (Loewenstein and Chater 2017). Nudges can be part of a solution that reduces disparities, but they are not enough by themselves.

Across our studies, we found that the moderating effect of SES was consistent in a wide variety of contexts including consumer product choices that contained no calculations and no correct answer. Of course, it is possible that these effects do not generalize to decisions in every context. The findings might not generalize to cases in which the nudged behavior is deeply constrained (Roberts 2018). For example, healthy eating nudges might be ineffective if many low-SES consumers live in food deserts, where healthy food is difficult to obtain or expensive. It is also possible that the moderating effects would be smaller or absent for decisions in which knowledge is irrelevant or in which numbers, calculations, and ambiguity are absent. Similarly, if low-SES consumers have strong preferences and more expertise than others within a particular domain, the effect in which nudges impact low-SES consumers most might not generalize to that domain.

Future research should also examine the mechanisms underlying the nudge moderators in more detail. Mediation models were consistent with our framework in which low-SES consumers are more impacted by nudges because they score lower in domain-specific skills such as numeracy (not because they allocate time or attention differently; cf. Shah, Mullainathan, and Shafir 2012). There were also indirect effects through anxiety in Study 5 (but not through uncertainty). Future investigations could expand on this by manipulating psychological processes and by examining different forms of confidence and uncertainty (e.g., Fernandes, Lynch, and Netemeyer 2014). Moreover, future work should examine the extent to which subjective rather than objective knowledge accounts for the effects. It is possible that people with low subjective knowledge would be greatly impacted by nudges even if they have high objective knowledge. Future work could also examine why anxiety plays a role in the differential nudge effects. For example, low-income individuals often feel stigmatized and anxious about confirming a negative ability stereotype (e.g., Tine and Gotlieb 2013), which might account for any effects of anxiety on nudge effects.

Of course, though we manipulated choice architecture, we cannot conclude that SES, financial literacy, or numeracy caused consumers to be less susceptible to nudges, because we did not manipulate these variables. Some researchers have manipulated temporary scarcity or perceived social class (e.g., Shah, Mullainathan, and Shafir 2012). However, we would not expect these manipulations to increase nudge effects because they do not operate through our proposed mechanisms of financial literacy, numeracy, and anxiety.

Conclusion

When signing copies of his book *Nudge*, Richard Thaler often writes “Nudge for good,” encouraging readers to use nudges to benefit people rather than to increase profits at the expense of consumer welfare. The present investigation suggests that “nudging for good” not only helps consumers overall but also reduces inequities. The implications are clear for anyone interested in reducing inequities: nudge for good.

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