J_ID: Customer A_ID: UCAF049 Copyedited by: PR Manuscript Category: Article Cadmus Art: OP-JCRE250050 Da

Author Query Form

Journal: *Journal of Consumer Research*
Article Doi: 10.1093/jcr/ucaf049
Article Title: The Confirmation Nudge: Prompts to Change or Confirm Initial Preferences Steer Consumer Choice
First Author: Kellen Mrkva
Corr. Author: Kellen Mrkva

AUTHOR QUERIES – TO BE ANSWERED BY THE CORRESPONDING AUTHOR

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The Confirmation Nudge: Prompts to Change or Confirm Initial Preferences Steer Consumer Choice

KELLEN MRKVA
SHANNON M. DUNCAN
MARISSA A. SHARIF
AQ1 STANLEY ZUO

Across two field experiments and several pre-registered laboratory experiments, we demonstrate that confirmation nudges, which ask consumers whether they would like to confirm or change their initial choice, impact choice. First, consumers navigating a subscription company's smartphone app were randomized to a control or confirmation nudge condition, which asked them to either confirm their initial choice or switch to an annual subscription. Confirmation nudges increased subscribers' choice of the annual subscription by over 8%age points—an effect size similar to default effects tested by the same company. In experiment 2, conducted by a jewelry retailer, confirmation nudges had countervailing effects, increasing purchases of a nudged service plan add-on, but decreasing originally planned jewelry purchases likely because it added a step and thus frictions to the purchase process. Confirmation nudges had larger effects when nudged options were desirable and among consumers who would benefit from the nudge (experiments 3 and 4). But, they were perceived as more manipulative than comparison conditions (experiment 5). We suggest that confirmation nudges undo tendencies to focus on initially preferred options, shifting attention toward alternatives relative to control conditions. Consistent with this, confirmation nudges were especially effective when the wording of the confirmation prompt focused on the "switch" option.

Keywords: consumer choice, decision making, field experiments, nudges

INTRODUCTION

In modern digital environments, it can be easy and inexpensive to change the design of a consumer choice (Benartzi et al. 2017; Narayanan et al. 2020). Some such changes to choice architecture can have a substantial impact on customer

Kellen Mrkva (kellen_mrkva@baylor.edu) is Assistant Professor of Marketing, Baylor University, Hankamer School of Business, Waco, TX AQ2 76706, USA. Shannon M. Duncan (sduncan5@ualberta.ca) is Assistant Professor of Marketing, University of Alberta, College of Business, 11203 AQ3 Saskatchewan Drive NW, Edmonton, AB T6G 2R6, USA. Marissa A. Sharif (masharif@wharton.upenn.edu) is Associate Professor of Marketing at The AQ4 Wharton School, University of Pennsylvania, Philadelphia, PA 19104, USA. Stanley Zuo (hello@stanleyzuo.com) is Marketing Manager, Web Strategy at Ancestry.com. Please address correspondence to Kellen Mrkva. Supplementary materials are included in the web appendix. The authors thank seminar participants at Theory + Practice in Marketing and Baylor University, as well as Alicia Gan and Eli Holtz for helpful discussions and feedback. The authors thank the Dean's Research Fund at the Wharton AQ5 School, University of Pennsylvania and the Wharton Behavioral Lab as well as Baylor University and the Billington Research Fund, for providing funding for some of the experiments. AQ6

Editors: June Cotte and Oleg Urminsky

Associate Editor: Oleg Urminsky

Advance Access publication August 23, 2025

 • Vol. 00 • 2025
https://doi.org/10.1093/jcr/ucaf049

behavior and revenue (Goldstein et al. 2008). Therefore, it is no surprise that marketers are increasingly using insights from consumer behavior and from their own experiments to nudge online purchases.

We test the effects of one simple choice architecture tool, *confirmation nudges*. A *confirmation nudge*, in short, is a digital choice architecture technique that asks consumers whether they would like to confirm their initially selected product or switch to a different product, without adding new information about the options.

Confirmation nudges have three components. First, they are conditional, meaning that only consumers who initially do not select a particular option see the confirmation prompt. Second, confirmation nudges include a presentation of the identical choice options again without any new information. This screen gives consumers a chance to stay or switch and is intended to nudge consumers to choose and direct more consideration toward the switch option, though the wording of this confirmation screen varies. Third, because confirmation nudges present a second chance to choose, they add one step to the purchase process, usually adding a small amount of time and an extra click to that process. This additional step might add friction to the purchase process and increase purchase abandonment. We refer to these three components as conditional, identical, and entailing an extra step. This definition of confirmation nudges is fairly broad. Confirmation nudges therefore include designs that ask consumers whether they would like to confirm or switch their initial decision, as well as those that explicitly mention only the stay or only the switch option, with the other option implicit (e.g., "Do you want to proceed without upgrading?" and "Do you want to upgrade to the premium plan?"). They also include designs that cause consumers to reconsider the non-chosen option without any explicit prompt (i.e., merely presenting the features of each option again along with a chance to stay or switch, as in experiment 2).

A pure version of a confirmation nudge would simply track an initial choice and then present those who did not choose a particular option with a prompt asking consumers to either confirm or switch their decision. In other words, pure confirmation nudges do not provide new information nor even use different wordings or colors of the choice options. Retailers can also use prompts that combine confirmation nudges with other techniques, such as using higher-contrast button colors or framing benefits differently. We consider these *confirmation plus* designs because they combine a confirmation nudge with other design strategies, any of which could explain their effectiveness. Though confirmation nudges are simple and inexpensive, they have received very little empirical attention. So, it is unclear whether and to what extent they impact behavior.

Choice Architecture

Choice architecture describes changes to the structure of a decision that are designed to encourage a particular choice without significantly changing economic incentives, forbidding options, or providing consumers with new information (Johnson et al. 2012; Lamberton and Diehl 2013; Mertens et al. 2022; Thaler, Sunstein, and Balz 2013). These techniques have become popular not only in academia, but among policymakers and marketing firms (Soman and Yeung 2020; Wendel et al. 2023).

Confirmation nudges differ from other interventions in a few ways (see Table W1 of the web appendix for more details about how confirmation nudges differ from each type of nudge). First, the minimal design of confirmation nudges sets them apart from other nudges that use friction. Unlike interventions such as justification prompts—which require significant effort, such as writing sentences to justify a choice—confirmation nudges entail only one click to confirm a consumer's decision. This low-effort requirement allows them to preserve a fairly seamless user experience.

Second, the mechanisms underlying confirmation nudges do not appear to align with typical theories explaining the effects of nudges. Most nudges leverage ease, attractiveness, social influence, timing, implied endorsement, or other mechanisms to encourage behavior (Goswami and Urminsky 2016; Hallsworth and Kirkman 2020; Huh, Vosgearu, and Morewedge 2014; Johnson 2021). Confirmation nudges, by contrast, introduce a small amount of friction, making it unlikely that ease is the primary mechanism. They also do not rely on social norms or implied endorsement, as they neither signal what others are doing nor do they likely signal what is recommended. Instead, they simply give consumers a chance to either confirm or switch their choice, presenting the same information again without making any option more attractive.

Interestingly, confirmation nudges operate in a context in which their effectiveness might seem counterintuitive. Unlike timely nudges, which are designed to reach individuals when they are most receptive (Hallsworth and Kirkman 2020), confirmation nudges are presented after a decision is made—arguably a suboptimal moment. Self-consistency motives would suggest that consumers are unlikely to change their minds after making a choice (Brehm 1956). Yet, despite these opposing forces, confirmation nudges can be effective, making their mechanism and impact particularly intriguing.

TABLE 1

THE NET EFFECTS OF CONFIRMATION NUDGES ON FIRMS DEPEND ON 4 FACTORS

Shifts attention	Size and location of frictions	Option desirability	Relative gain from shifted choice versus loss from abandoned purchase	Firm outcome
Re-orients attention toward nudged option ("switch focus")	Minimal friction or does not decrease purchases (e.g., post-purchase)	Nudged option is desirable	Shifting choices has a bigger impact on profits than abandoned purchases	Net **beneficial**
Keeps attention on chosen option ("stay focus")	Large friction and pre-purchase	Nudged option is undesirable	Shifting choices has a smaller impact on profits than abandoned purchases	Net **harmful**

Why Would Confirmation Nudges Impact Consumer Choice?

We suggest that confirmation nudges impact choice by undoing people's typical tendencies to focus their attention on an initially preferred option relative to alternatives. In other words, we propose that people who receive a confirmation nudge will devote more relative attention toward a non-selected option compared to people in a control condition who do not receive this nudge and likely focus primarily on the option they initially favor. Prior research has shown that people typically devote most of their attention toward an initial leader and ignore alternatives (Carmon and Ariely 2000; Russo, Meloy and Medvec 1998). A confirmation nudge that asks consumers whether they would like to confirm or switch their choice could cause people to mentally think about *both* options more equally, at least as they are reading the question itself. In other words, many participants will read the confirmation prompt, which will often orient attention toward both options as they are reading it (e.g., "Would you like to confirm or switch your initial choice?").

In cases in which the prompt focuses only on the non-selected option (e.g., "Would you like to switch?"), and does not explicitly mention the selected option, we posit that confirmation nudges will be especially likely to increase how much people attend to and choose the option that was not initially chosen. Confirmation prompts that explicitly mention the "stay" option and only implicitly mention the "switch" option will likely have less impact on attention and choice.

Once an option receives more mental attention, previous research suggests that this increased mental attention should increase subjective value and choice (Jones and Fazio 1992; Mrkva and Van Boven 2017; Mrkva, Westfall, and Van Boven 2019). According to reviews and meta-analyses, visual attention, like mental attention, also increases choice and subjective value (Bhatnagar and Orquin 2022; Krajbich 2019). Theorists have proposed multiple explanations for the causal effects of attention on choice (Jones and Fazio 2008; Loersch and Payne 2011; Mrkva, Ramos, and Van Boven 2020). First, consumers often ask themselves, "Which should I buy?" or "Which is my favorite?" when examining options (Lohmann, Jones, and Albarracín 2019). So, all else equal, if a manipulation increases attention toward one option and thus makes it more mentally accessible during choice, consumers should be more likely to choose that option as the answer to these self-questions (Jones and Fazio 2008). Increased attention can also make evaluations of positive objects more positive and increase their accessibility, which in turn can increase choice (Armel, Beaumel, and Rangel 2008; Downing, Judd, and Brauer 1992).

There are some boundary conditions in which attention has smaller effects on choice. In particular, attention has smaller effects when the attended option is low-value and when the attended option is clearly inferior or dominated by the alternative (Marini, Sapienza, and Paglieri 2023; Smith and Krajbich 2019). More generally, choice architecture tools often have less impact when consumers are more certain about their choice or when one option dominates another (Evangeledis and Levav 2013; Milosavljevic et al. 2012). Therefore, we posit that increased attention would increase choice, unless the option is dominated or very low-value. Past research has suggested that more attention can be ineffective when a message is seen as an attempt to coerce consumers or counteract strong views (Brehm 1966). However, we expected that confirmation nudges would be too subtle to produce these effects.

How Do Confirmation Nudges Impact Firms?

Confirmation nudges add a step to the purchase process and also prompt people to reconsider a non-chosen option. Adding a step could add a small amount of friction to the purchase process, which could lead some consumers to abandon their purchase. In contrast, re-directing attention toward a non-chosen option could prompt people to attend more closely to that alternative and shift their choice, as explained in the previous section. In other words, confirmation nudges might produce two countervailing effects—one that makes consumers more likely to choose the nudged

option (compared to people who are not shown a confirmation nudge) and one that makes them more likely to abandon their purchase completely.

If confirmation nudges cause these countervailing effects, confirmation nudges will sometimes be beneficial and other times harmful for firms. In table 1, we present a 4×2 matrix suggesting that these net consequences for firms largely depend on four factors: (a) how well the confirmation nudge shifts attention toward the nudged option, (b) the size and location of frictions added by the confirmation, (c) whether the nudged option is desirable, and (d) the relative gain from a shifted choice versus the loss from an abandoned purchase in the firm's context.

To illustrate, consider two companies. Company 1 is a subscription company that is testing a confirmation nudge designed to steer consumers toward purchasing an annual subscription rather than a month-by-month subscription. Customers who initially select the monthly subscription receive a confirmation prompt asking whether they would like to switch to the annual subscription. For this firm, imagine that nudging a customer to choose the annual subscription rather than the month-by-month subscription more than doubles the expected lifetime value of that customer. In this case, the nudge would produce a net gain for the company even if it causes one customer to abandon their purchase completely for every one customer it shifts toward the nudged option.[1]

In contrast, company 2 sells expensive jewelry, often priced at $1,000 or more, and is considering using a confirmation nudge to increase purchases of their much less expensive jewelry service plan. For company 2, one abandoned purchase would have a much larger impact on revenue than one additional service plan purchase, because jewelry itself is more expensive. If company 2 uses a confirmation nudge that causes one person to abandon their jewelry purchase completely for every one person it causes to add the nudged service plan to their purchase and produces no other effects, the confirmation nudge would be a net negative for the company.

Firms considering confirmation nudges need to ultimately analyze in their own context the relative importance of abandoned purchases versus shifting people's choices with a confirmation nudge (table 1, fourth column). Table 1 suggests that firms planning to use confirmation nudges should also seek to minimize frictions, maintain a seamless customer experience, and use a confirmation wording that shifts attention toward a desirable nudged option by explicitly mentioning that option.

1 These descriptions of company 1 and company 2 are based on the two companies that tested confirmation nudges in the two field experiments (experiments 1 and 2, respectively). The details about net effects and the context match what the employees shared with us, as we describe in greater detail in experiments 1 and 2. One difference is that, in reality, the confirmation nudge in experiment 1 seemed to nudge far more than one person toward the annual plan for every one person who abandoned their purchase as a result of the nudge, making the company even more confident that it was net beneficial for them. The descriptions here assume that the confirmation nudges produce "no other effects" other than those mentioned, which includes assumptions that future purchases and loyalty are not impacted and that the profit margins of the service plan are not so much higher than profit margins of the jewelry that it offsets the differences in revenue.

Study Overview

We begin with data from two field experiments that test whether confirmation nudges make consumers more likely to choose the nudged option. The first field experiment tests this question in the context of subscription choices, while the second field experiment generalizes this result to jewelry and service plan purchases. The context and large sample size ($n = 407{,}785$) of the second field experiment allow us to detect small effects and examine the potential tradeoffs between adding frictions and steering more consumers toward the nudged option.

In experiment 3, we isolate the confirmation nudge from other elements that were not perfectly controlled in the field experiments, examining whether the confirmation nudge itself or other elements of the designs in the field experiments account for the effects. In experiments 4 and 5, we test the mechanisms underlying confirmation nudges and boundary conditions. We demonstrate that the effects generalize to familiar smartphone choices. We also show that confirmation nudges are less effective when the nudged option is dominated by the other option (experiment 4) and when it is worded in a way that directs more attention to the chosen option rather than the non-chosen (nudged) option (experiment 5).

We report every experiment we conducted (three in the web appendix). All of the laboratory experiments were preregistered via AsPredicted, and the data, analysis scripts, and pre-registrations for all experiments are publicly available at osf.io/zbw9h/.

EXPERIMENT 1: FIELD EXPERIMENT WITH A LEARNING APP SUBSCRIPTION

In experiment 1, confirmation nudges were tested in a field experiment. One of the authors conducted this experiment in the context of a firm that develops and sells educational software and subscriptions for kids' learning programs. Visitors were randomly assigned to either a control condition or one of two confirmation nudge conditions. We analyzed whether confirmation nudges increased the percentage of visitors who chose the annual subscription plan.

Method

Participants and Procedure The experiment was conducted in the context of the firm's iOS app. Consumers who opened the app on iOS and navigated to the

subscription page were randomly assigned to one of three conditions: the control condition, confirmation nudge 1, or confirmation nudge 2 (table 2). The field experiment was conducted on 9,037 users over 10 days. It was designed in hopes of increasing the number of consumers who chose an annual plan relative to the control condition.

All three conditions were identical on the first subscription choice screen. On this screen, consumers could choose whether to select an annual subscription priced at $59.99/year or a monthly subscription at $9.99/month. This screen also noted that the annual subscription ÷ 12 would be $5/month (49% less than the cost of keeping the monthly subscription for 12 months), making it a lower total cost than the monthly plan for consumers who retain the subscription for over 6 months. In all 3 conditions, the "annual" option was pre-selected on the first subscription choice screen, because a previous field experiment by the same company revealed that this increased choice of the annual option.

The key difference between the experimental conditions occurred only among participants who clicked on the "monthly" subscription. In the control condition, no additional screen was presented to participants such that users would proceed to enter their date of birth and payment information. In the two confirmation nudge conditions, after initially indicating they would like a monthly subscription, these participants viewed one confirmation prompt giving them a chance to switch to the annual plan instead.

The confirmation prompt presented an opportunity to switch (and re-stated the prices with a somewhat different wording, format, and more prominent annual button). The two confirmation conditions had somewhat different phrasing from each other (e.g., "Get 6 months free!" vs. "$59.99/yr compared to $119.88/yr") and the confirmation screen had a pink rather than blue annual button in one version (table 2). The experiment was conducted using a popular A/B testing tool, AB Tasty. The collected data by condition included the number of visitors and whether each chose the annual subscription, monthly subscription, or neither.

Analytical Approach In all experiments, we conducted binary logistic regressions with choice as the dependent variable. In each experiment, we compare the percentage of participants in the control condition choosing the annual plan (i.e., the nudged option, which was the annual plan in experiments 1, 3, and 4, and which was the service plan or iPhone 16 Pro in experiments 2 and 5, respectively) to the percentage of participants in the confirmation nudge condition(s) who ended up choosing the annual plan (or service plan or iPhone 16 Pro). That is, the outcome variable always indicated their final choice between the options, whether they made only one choice or whether they viewed a confirmation nudge giving them a second choice between the plans. In experiment 1, condition was contrast-coded to compare the control against the two confirmation conditions ($-2/3$ = control, $+1/3$ = either confirmation condition). The orthogonal contrast code thus compares confirmation nudge 1 against confirmation nudge 2 ($1/2$ = confirmation nudge 1, $-1/2$ = confirmation nudge 2, 0 = control; following Judd, McClelland, and Ryan 2009). We refer to the former contrast as the "confirmation nudge" contrast and the latter as the "confirmation type" contrast. The full primary model was *choice of annual plan* $= \beta_0 + \beta_1 \times$ (confirmation nudge) $+ \beta_2 \times$ (confirmation type) $+ \beta_3 \times$ (chose any subscription) $+ \varepsilon$. We also analyzed pairwise comparisons using dummy codes. To optimize statistical power and account for variance associated with choosing neither plan, these models included a control for whether or not users chose any subscription. When removing this control, the effect of confirmation on choice of the annual plan was still significant ($p < .05$).

Results

Does the Confirmation Nudge Increase Annual Subscriptions? Consumers randomly assigned to a confirmation nudge condition were significantly more likely to choose the annual subscription compared to participants in the control condition, according to the logistic regression ($M_{control} = 8.0\%$ chose annual, $M_{confirmation\ conditions} = 9.4\%$ chose annual), $z = 3.52$, $p < .001$. Equivalently, out of the subscribers who chose one of the two subscriptions, 41.5% chose the annual plan in the control condition compared to 51.2% and 49.7% in confirmation conditions 1 and 2, respectively (table 2). There was no appreciable difference between the two different confirmation nudge conditions, $z = 0.49$, $p = .622$.

Compared to the control condition, pairwise comparisons revealed that consumers were more likely to choose the annual plan in the first confirmation condition ($M_{confirmation\ nudge\ 1} = 9.3\%$, $M_{control} = 8.0\%$, $z = 3.25$, $p = .001$; Bonferroni corrected $p < .01$) and in the second confirmation condition ($M_{confirmation\ nudge\ 2} = 9.5\%$, $z = 2.82$, $p = .005$; Bonferroni $p < .05$).

Did the Additional Screen Make People More Likely to Choose Nothing? One potential concern about adding an additional confirmation screen is that it might cause more consumers to abandon their purchase or choose neither option, because it adds a step to the subscription signup process. The effect of condition on the proportion of users who chose neither subscription was not significant (80.7% in the control chose neither subscription; 81.3% of those in a confirmation condition chose neither), $z = -0.81$, $p = .417$ (table 2). Of course, absence of evidence is not evidence of absence. Indeed, in experiment 2, we examine data from a much larger field experiment (45 times larger sample size) in which the effect size on failing to complete a purchase was slightly smaller than in experiment 1 but was significant in the larger sample.

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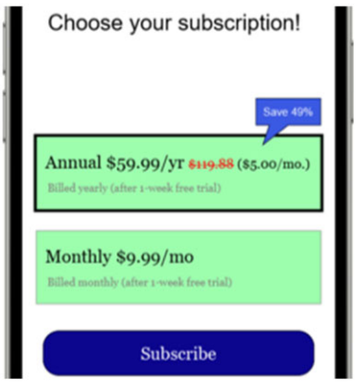
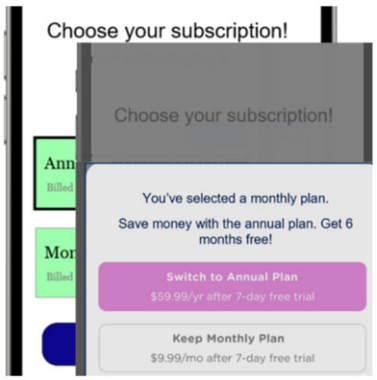
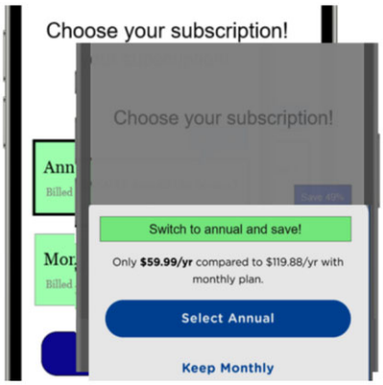
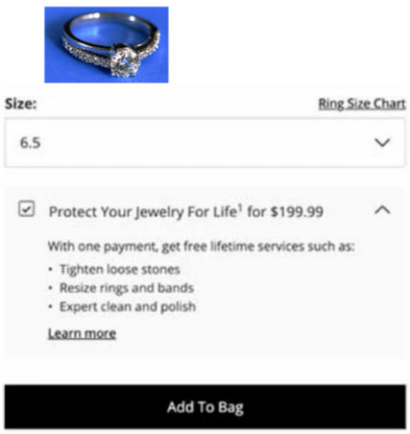
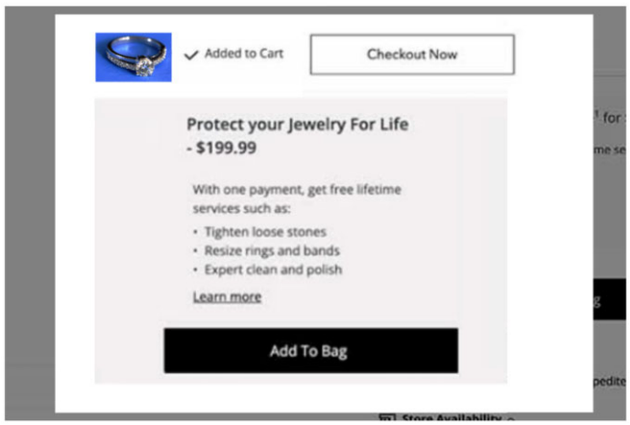
TABLE 2

DESIGN OF CONDITION SCREENS AND KEY RESULTS IN THE FIELD EXPERIMENTS

Exp	*Decision Screens and Percentage Choosing Each Option by Condition*		
1	*Control (and Screen 1 of confirmation nudge)*	*Confirmation nudge 1*	*Confirmation nudge 2*
			
	of subscribers / *of all visitors* 41.5% annual / 8.0% annual 58.5% monthly / 11.3% monthly 80.7% neither	*of subscribers* / *of all visitors* 51.2% annual / 9.3% annual 48.8% monthly / 8.8% monthly 81.9% neither	*of subscribers* / *of all visitors* 49.7% annual / 9.5% annual 50.3% monthly / 9.7% monthly 80.8% neither
2	**Control** (and Screen 1 of confirmation nudge) (Product detail page)	**Confirmation nudge condition: Screen 2** (Confirmation dialog box on top of product detail page)	
			
	Control condition statistics: *of purchases* / *of all visitors* 19.4% service plan / 0.2% service plan 80.6% no service plan / 0.7% no service plan 99.1% no purchase	*Confirmation nudge condition statistics:* *of purchases* / *of all visitors* 31.8% service plan / 0.3% service plan 68.2% no service plan / 0.5% no service plan 99.2% no purchase	

NOTE—Across all experiments, all conditions had the same initial choice screen (shown in column 2). Viewers were randomly assigned to a control condition or a confirmation nudge (which would give them a chance to switch to a nudged option, if they did not initially choose it). Some colors and fonts as well as elements of the jewelry modal in the table differ somewhat from those used by the companies in their field experiments (experiments 1 and 2). The words, button positions, and elements manipulated across conditions are almost identical.

Comparison to a Field Experiment Examining Defaults Before conducting the confirmation nudge field experiment, the same firm conducted a field experiment examining the effect of defaults on subscription choice among 14,903 users of their app. In this experiment, preselecting the annual option as the default increased the percentage of users who chose annual from 3.0% to 4.2%. Equivalently, out of subscribers (who chose either the annual or monthly subscription), 27.0% chose annual in the annual default condition compared to 18.9% in the control condition (a difference of 8.1 percentage points (pp) compared to 8.6 pp difference for the confirmation nudge). Though some caution is warranted before comparing the effect sizes between field experiments run at different times, it appears that confirmation nudges can have effect sizes that are as large as default effects in this context.

Discussion

In a consequential field experiment with users of a learning app, two confirmation nudges each increased the proportion of people who chose an annual subscription. After the experiment, this company implemented a confirmation nudge across their app and website and concluded that it produced positive net benefits for the company. In this firm's context, expected customer lifetime value more than doubled when consumers were nudged to choose the annual plan rather than the monthly plan, according to the company's finance team.[2]

In subsequent experiments, we will test why confirmation nudges influence choice. They may work by shifting attention toward non-chosen options, though some possible alternative explanations exist. For example, giving consumers a second chance to choose the annual plan, conditional on them choosing the monthly plan initially, could cause more annual plan purchases partly because any mistakes, decision noise, or random responding would cause an increase in purchases of the annual plan. Other alternative explanations in experiment 1 are that the confirmation screen included more salient annual buttons and a different description of the deal, which could activate savings goals. In experiment 2, the wording of the confirmation nudge is identical to the control condition wording, and in subsequent experiments we address the noise explanation while also keeping the salience and appearance of the options identical across the initial screen and confirmation screens to rule out these explanations.

2 One of the authors (SZ) corresponded with their finance team, who conveyed that the expected customer lifetime value is at least twice as high when customers are nudged to choose the annual plan than when they choose the monthly plan. The company was not able to share any more detailed data on customer lifetime value with us.

EXPERIMENT 2: CONFIRMATION NUDGES HAVE COUNTERVAILING EFFECTS

In experiment 2, confirmation nudges were tested in a different field experiment conducted by a popular jewelry retailer. In this experiment, the wording of the nudged option is also tightly controlled across conditions. The confirmation nudge was intended to nudge consumers toward adding a jewelry service plan to their purchase. In this experiment, we examine the results at multiple stages of the purchase funnel to better clarify how confirmation nudges influence initially selected options added to cart and completed purchases.

Confirmation nudges may shift attention and choice toward the nudged option. But, they also add one step to the purchase process, which might increase frictions. In experiment 2, the confirmation nudge adds a step between adding the jewelry to cart and completing the purchase, which might cause more consumers to leave the website without making a purchase even if they initially add the jewelry to cart. The different context and higher stakes of these purchases also allow us to test whether results generalize beyond lower-stakes subscriptions.

Method

The experiment was conducted with 407,785 participants. Consumers who navigated to one of many product detail pages on a popular jewelry website were randomly assigned to either the confirmation nudge or control condition (table 2). They viewed the product details of the jewelry item along with options to add the jewelry item to their cart, with or without a service plan. Consumers could add the jewelry item to their cart by clicking "Add to Bag." If they wanted to add the service plan to cart, they would check a small box (by clicking on it) inside the service plan description prior to clicking "Add to Bag". The information provided about the service plan stated, "Protect your jewelry for life!" and included the price of the service plan (which varied depending on the item's price). It also noted the services provided by the service plan, which were to resize rings and bands, tighten loose stones, clean, and repolish.

Then, participants in the confirmation nudge condition who added a jewelry item to cart without a service plan viewed the confirmation nudge. The nudge showed the same information about the service plan once more. They could click "Checkout Now" (to proceed to their shopping cart without a service plan) or click "Add to Bag" in the rectangle describing the service plan to add it to their purchase (there was no small checkbox, unlike on the initial screen). The screen stated, "added to cart" with a thumbnail of the added jewelry, which clarified that they were simply

seeing a second chance to add the service plan (i.e., a confirmation nudge).[3]

Participants in the control condition (and participants in either condition who already added the service plan) skipped the confirmation prompt and could advance directly to their shopping cart screen. Across all conditions, they could then proceed to examine the contents of their cart, enter payment information, and click a button to complete their purchase.

The experiment was conducted using Dynamic Yield A/B testing software, and settings were adopted so that 80% would be randomly assigned to the control condition and 20% to the confirmation nudge (this was chosen over a 50–50 allocation because the company viewed the confirmation nudge as higher risk and atypical compared to the control). The experiment tracked how many people in each condition added the service plan to cart, how many added anything to their cart, how many completed the service plan purchase, and how many completed any purchase. As in experiment 1, results were analyzed with binomial logistic regression. The primary models were formatted as follows: $Y = \beta_0 + \beta_1 \times$ confirmation nudge condition (dummy-coded) $+ \varepsilon$. We computed separate logistic regressions for three binary outcome variables: whether they purchased a service plan, whether they added a service plan to cart, and whether they purchased anything. As in experiment 1, the model estimating service plan purchases statistically adjusted for whether they had purchased anything.

Results

Effects on Choosing and Purchasing the Service Plans The confirmation nudge increased the percentage of carts that included a service plan from 19.4% (control condition) to 31.8% (confirmation nudge condition), $z = 15.07$, $p < .001$. People in the confirmation nudge condition were also more likely to purchase a service plan (16.5% of purchases in the control condition included a service plan vs. 19.8% of purchases in the confirmation nudge condition, though this difference was only weakly significant), $z = 1.99$, $p = .046$.

Did the Additional Screen Cause More People to Choose Nothing? Compared to people in the control condition, those in the confirmation nudge condition were significantly more likely to make no purchase at all, $z = -2.54$, $p = .011$ (99.2% of visitors in the confirmation nudge condition made no purchase compared to 99.1% in the control condition). In other words, the confirmation prompt caused more people to leave the website without making any purchase.

This result may initially appear to conflict with the non-significant effect on likelihood of choosing nothing in experiment 1, but this can be explained by differences in sample size. In fact, the effect size of the confirmation nudge on likelihood of buying nothing in experiment 2 (0.1 pp) was actually smaller than the analogous coefficient for likelihood of subscribing to neither plan in experiment 1 (0.6 pp). The sample size in experiment 2 was substantially larger, which explains why the effect was not significant in experiment 1.

Discussion

In experiment 2, confirmation nudges produced countervailing effects. In other words, confirmation nudges increased the percentage of consumers who bought a service plan but also increased the percentage of customers who abandoned their purchase completely. (There were similar-magnitude but non-significant countervailing effects in experiment 1.)

These countervailing effects beg the question: Was this confirmation nudge net beneficial or net harmful for the company? Unlike the company in experiment 1, the jewelry company did not implement the confirmation nudge after this experiment. The company shared that revenue per visitor in experiment 2 was $0.64 lower in the confirmation nudge condition than in the control condition, suggesting that the net effect of this nudge on revenue was likely negative.[4]

Thus, confirmation nudges can likely backfire, increase purchase abandonment, and produce net negative effects for companies under some circumstances. In this particular context, because jewelry itself is much more expensive than jewelry service plans, the revenue gains from nudging more service plan purchases were smaller than the revenue losses from increasing the likelihood that consumers buy nothing. More generally, this suggests that companies need to examine in their own context whether the gains from shifting choices toward a higher-revenue (nudged) option would outweigh the losses from potentially increasing purchase abandonment because of the added step that a confirmation nudge entails.

EXPERIMENT 3: EFFECTS OF A "PURE" CONFIRMATION NUDGE IN THE LAB

In experiment 3, we sought to test whether confirmation nudges can influence consumer choice in a controlled lab

3 The "added to cart" message helped clarify that the "Add to Bag" option on the confirmation screen was referring to the service plan (not the jewelry). Yet, it is possible that some customers who were rushing and missed that message may have clicked "Add to Bag" on the confirmation screen thinking that they were adding the jewelry. If so, it could have inflated effects on the add-to-cart DV. We thank an anonymous reviewer for mentioning this. The screen also contained a small optional video that interested participants could click on if they wanted to learn more about the service plan.

4 We were not able to calculate a p-value for revenue per visitor or determine whether this $0.64 difference was significant, because the company was not able or willing to share enough data about individual purchases or variance in purchase amounts.

setting even when the confirmation screen looks identical to the previous screen. We also examine whether confirmation nudges have more impact on those who would benefit from the annual plan (saving money) or on those who would not benefit (who would incur higher long-term costs by choosing the nudged annual plan).

We randomly assigned participants to one of three conditions. Two of these conditions were the control condition and "confirmation plus" condition, which were both almost identical to conditions in the first field experiment. The third condition was a more controlled "pure confirmation nudge" condition. Including these three conditions allows us to examine how much of the effect might be driven by the confirmation nudge itself and how much is driven by changes in visual prominence or wording that were present in experiment 1. We predicted that confirmation nudges would work even when using a "pure" confirmation nudge. The experiment was pre-registered: aspredicted.org/7LS_NM9.

Method

Participants As pre-registered, we recruited 600 American adults from Amazon Mechanical Turk (MTurk) via CloudResearch (using only CloudResearch-approved participants). A total of 599 participants reached the end of the study (46% female; $M_{age} = 40.62$, $SD_{age} = 11.91$).

Procedure Participants were told that they would be simulating the experience of installing and using a kids' learning app for the first time (the app used in experiment 1). They were able to click on buttons on the simulated smartphone screens in essentially the same way they would in the firm's app. Participants saw a few onboarding screens introducing them to the app (mirroring the real app design) and were then presented with the subscription plan options. They were asked to choose between two plans and to select the option they would "be most likely to choose in real life". The two plans were a $59.99/yr annual plan and a $9.99/month monthly plan. (Therefore, the monthly plan had a higher monthly cost but payments that only continued until the plan was canceled, rather than requiring payment of all 12 months up-front.)

Participants were randomized into either the control condition, "pure confirmation" condition, or "confirmation plus" condition. Table 3 displays these conditions.

After seeing the same initial choice screen in all three conditions, those in either of the confirmation conditions who chose the monthly plan on the initial screen viewed a confirmation screen. The confirmation screen asked, "Do you want to proceed forward with the monthly plan or switch to an annual plan?" In the pure confirmation condition, the plans were displayed again in exactly the same way as the initial choice, thus equating visual features, salience, and framing between this condition and the control. In the "confirmation plus" condition, participants saw the design from confirmation nudge 2 of the first field experiment, in which the "annual" button was more prominent (table 3, right column). This would allow us to more directly compare results to the field experiment while also examining the effects of a more well-controlled confirmation nudge.

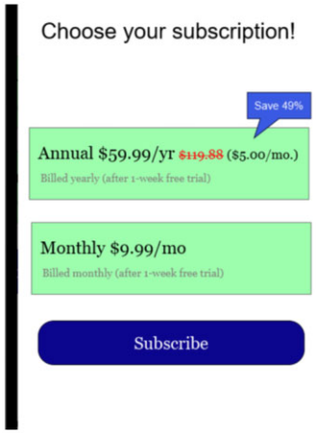
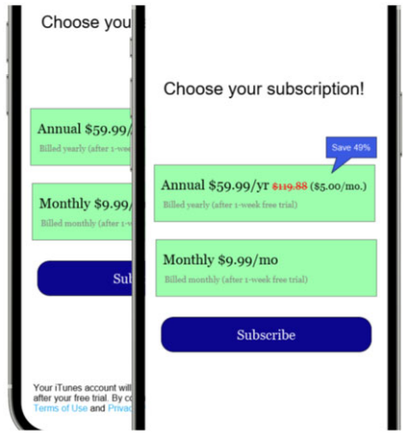
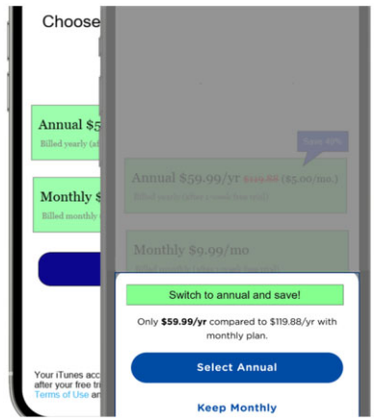
Then, participants answered some follow-up questions assessing potential mediators and alternative explanations. We measured the subjective value of the annual option by asking participants the extent to which they agreed with the statement that the annual plan seemed like a great deal (–3 = *strongly disagree*, 3 = *strongly agree*). Participants were also asked to report the relative objective value of the options by indicating to what extent the annual subscription or monthly subscription would have lower total costs for them overall if they selected it.

They also completed a 3-item measure of implied endorsement, which asked them to rate their extent of agreement with statements that it seemed like the company "recommended the monthly plan rather than the annual plan" (reverse-coded), "thinks that the annual plan is the best option for me", and "thought it was very important for me to choose the annual plan" (adapted from Dinner et al. 2011; –3 = *strongly disagree*, 3 = *strongly agree*). Then, participants reported how likely (from 0% to 100%) they would be to keep their subscription for 6 or more months, for only the free trial, and for longer than the free trial but less than 6 months. This task was included so that we could examine whether those nudged by the confirmation would save money or lose money by switching to the annual plan (those keeping the plan for more than 6 months would save money by choosing the nudged annual plan).

We included items assessing several constructs representing alternative explanations of our results (subjective ease, perceived frictions, visual prominence, decision points, inferred social norms, perceived commitment, and endowment; all except visual prominence and perceived commitment were adapted from Reeck et al. 2023). The ease item assessed participants' agreement with the statement "It was more difficult to choose the annual plan compared to the monthly plan" (–3 = *strongly disagree*, 3 = *strongly agree*). Friction was a 3-item measure assessing feelings that choosing a subscription was "full of obstacles", "smooth" (reverse-coded), and "difficult", endowment assessed whether they felt they would be giving something up by choosing the annual rather than monthly plan, and perceived commitment asked whether "It seemed like choosing the annual plan would be a big commitment" (all three measures: –3 = *strongly disagree*, 3 = *strongly agree*). Visual prominence asked, "Did the app design make one option seem more prominent than the other" (–3 = *the monthly option was much more prominent*, 3 = *the annual option was much more prominent*). Decision points asked, "How many decisions did you make when completing the app simulation? These can be

TABLE 3

DESIGN OF CONDITION SCREENS AND KEY RESULTS IN EXPERIMENTS 3 AND 4

Exp	*Decision Screens and Percentage Choosing Each Option by Condition*					
3	*Control*  *of all* (no neither option) 33.8% annual, 66.2% monthly		*Pure confirmation* Do you want to proceed forward with the monthly plan or switch to the annual plan?  *of all* (no neither option) 43.2% annual, 56.8% monthly		*Confirmation plus* Do you want to proceed forward with the monthly plan or switch to the annual plan?  *of all* (no neither option) 46.5% annual, 53.5% monthly	
4	*Control, non-dominated* Same as experiment 3 Control, except there was an extra "finalize purchase" screen		*Confirmation nudge, non-dominated* Same as experiment 3 "pure confirmation" condition, except there was an extra "finalize purchase" step		*Control, dominated* Same as experiment 4 non-dominated except price of annual plan was $119.88/yr ($9.99/mo)		*Confirmation nudge, dominated* Same as experiment 4 non-dominated, except price of annual was $119.88/yr ($9.99/mo)	
	of plans chosen 33.2% ann 66.8% mon	*of all* 30.9% ann 62.3% mon 6.8% none	*of plans chosen* 37.0% ann 63.0% mon	*of all* 34.4% ann 58.6% mon 7.0% none	*of plans chosen* 12.1% ann 87.9% mon	*of all* 11.1% ann 80.3% mon 8.6% none	*of plans chosen* 12.5% ann 87.5% mon	*of all* 11.7% ann 81.8% mon 6.5% none

NOTE—Across all experiments, all conditions had the same initial choice screen (shown under control). In experiment 4, the "finalize purchase" screen gave participants a chance to finalize their purchase or click an "X" if they wanted neither option.

decisions you made in your head and/or decisions you made by selecting a button."

We also asked participants how likely they would be to use the app in real life and whether they had kids, to assess whether the confirmation nudge was robust among those most likely to use the app and among those whom the app was targeted toward (parents with kids). They also completed demographics (gender, age, income, occupation, and education), an attention check asking them to select a particular option if they are reading the question text, and an item assessing implied profit motives ("I assumed that the company would profit greatly if people chose the annual plan."; –3 = *strongly disagree*, 3 = *strongly agree*) to probe whether they perceived the confirmation nudge as steering people toward profitable options for the firm.

To analyze these data, the primary (pre-registered) model was a logistic regression estimating choice of the annual subscription as a function of condition: choice of annual plan $= \beta_0 + \beta_1 \times$ (confirmation plus dummy) $+ \beta_2 \times$ (pure confirmation dummy) $+ \varepsilon$. Like our field experiments, the model compared choice of the annual subscription in the control condition to choice of the annual subscription in the confirmation conditions during the first or second choice (i.e., the outcome variable always indicated their final choice between the annual and monthly plan, whether they made only one choice or whether they viewed a confirmation nudge giving them a second choice between the plans).

Results

Subscription Choice Overall, 59% of participants chose the monthly plan compared to 41% who chose the annual plan. A logistic regression was computed to estimate subscription choice as a function of condition.

Consistent with experiments 1 and 2, confirmation nudges increased the percentage of people who chose the annual plan. Specifically, 34% of those in the control condition chose the annual plan compared to 43% of those in the pure confirmation condition and 47% of those in the confirmation plus condition. In the primary pre-registered model, planned pairwise comparisons indicated that participants were significantly more likely to choose an annual plan in the confirmation plus condition compared to the control, $z = 2.58$, $p = .010$, and the pairwise difference between the pure confirmation condition and control was near significant, $z = 1.92$, $p = .055$. Though the latter pairwise comparison was only near significant, we find strong and consistent evidence in both of our other studies that tested these same two conditions with larger samples. There was no significant difference between the pure confirmation and confirmation plus conditions, $z = 0.67$, $p = .504$.

Some measures were included to examine who is most impacted by the confirmation nudges (these analyses were not pre-registered). First, to address robustness among those most likely to use the app, we conducted spotlight tests (Spiller et al. 2013). These tests indicated that the confirmation nudges were robust among participants who reported a 100% likelihood of using the app in real life, $z = 2.31$, $p = .021$ (and across any other likelihood between 34% and 100% in a floodlight analysis; Johnson–Neyman point = 33%). The effect was also robust among those who had children (for whom the app is targeted), $z = 2.11$, $p = .035$. The effects of confirmation nudges were not significantly moderated by participants' reported likelihood of using the app in real life, $z = 1.51$, $p = .132$, nor were they moderated by whether or not they had children, $z = 0.42$, $p = .677$.

Do Confirmation Nudges Help Consumers Save Money or Increase Costs? Notably, the effects were larger among people who would save money by choosing the annual plan (i.e., those who reported a higher likelihood of keeping the subscription for 6 or more months) compared to those who would save money by choosing the monthly subscription (interaction: $z = 2.21$, $p = .027$). This is consistent with the idea that the confirmation nudge in this context may be a nudge that helps most save money rather than a harmful tactic that increases most consumers' costs.

Potential Explanations There were no appreciable effects of confirmation nudges on perceived frictions, $t(595) = 0.94$, $p = .349$, decision points, $t(591) = 0.10$, $p = .923$, inferred social norms, $t(596) = 0.24$, $p = .813$, objective value, $t(596) = 1.06$, $p = .289$, implied profit motives, $t(594) = –0.15$, $p = .878$, perceived commitment, $t(595) = −0.15$, $p = .880$, endowment, $t(596) = −0.93$, $p = .351$, nor perceived ease, $t(596) = –1.19$, $p = .235$. (Some participants did not complete all measures, explaining small differences in degrees of freedom across analyses.) The confirmation plus nudge increased visual prominence, $t(596) = 2.32$, $p = .021$ while the pure confirmation nudge did not, $t(596) = 0.27$, $p = .785$. Confirmation nudges did increase the subjective value of the nudged option, $t(596) = 2.25$, $p = .025$, and there was an indirect effect on subscription choice through subjective value (ab = 0.09, 95% CI [0.01 to 0.19]). This single-mediator path through subjective value was not significant in subsequent studies.

The effect of the confirmations on implied endorsement was near significant, $t(594) = 1.87$, $p = .062$, though mediation models suggested there were no indirect effects through implied endorsement (ab = 0.00, 95% CI [–0.01 to 0.02]) nor any of the other alternative mediators (details in web appendix). Of course, a lack of significant effects does not necessarily mean these constructs play no role, so we include many of these measures again in experiment 5 and supplemental experiment 3. In a pre-registered robustness test, the confirmation nudge effect was robust when excluding those who failed the attention check, $z = 2.47$, $p = .013$. Neither were the effects of confirmation nudges

significantly moderated by attention check accuracy, $z = -0.95$, $p = .343$. This may suggest that random responding and carelessness are unlikely to fully account for the effects of confirmation nudges.

Discussion

Confirmation nudges increased choice of the annual plan. This effect was larger among those who would financially benefit from the nudged option than those who would not.

One limitation of experiment 3 is that it did not test effects on purchase abandonment, because there was no option to exit the purchase or choose nothing. Though we add options to exit and choose neither option in experiments 4 and 5, very few participants chose these options (tables 3 and 4), so these experiments do not have enough power to test countervailing effects on purchase abandonment. Another limitation of experiment 3 is that it does not rule out the possibility that the conditional nature of confirmation nudges accounts for the effects. In other words, simply asking a second question conditional on a particular response to the first question could increase choice of alternative options simply because some people have noisy preferences or made mistakes when providing their initial choice. Though the attention check analyses may help address some sources of decision noise, experiments 4 and 5 seek to better address these alternative explanations.

EXPERIMENT 4: MODERATING THE CONFIRMATION NUDGE

In experiment 4, we sought to examine whether confirmation nudges increase relative attention while examining a predicted moderator and boundary condition of the effect. Specifically, we predicted that confirmation nudges would be more effective when there is a tradeoff between two options than when the nudged option is dominated by the alternative (because attention has less impact on choice when attended options are dominated or very low value; e.g., Marini et al. 2023; Smith and Krajbich 2019). If confirmation nudges have little to no effect when the nudged option is dominated, this would also help address alternative explanations that the effect in experiment 3 could be due to random responding, mistakes, or some other sources of decision noise that are corrected when people are asked to choose a second time. The sample size and hypotheses were pre-registered (aspredicted.org/6DN_4T6).

Method

Participants We recruited 2000 American adults from MTurk via CloudResearch as pre-registered (using only CloudResearch-approved participants). A total of 1989 participants reached the end of the study (53% female; $M_{age} = 42.78$, $SD_{age} = 13.38$).

Procedure Participants were randomized in a 2×2 experimental design that manipulated the confirmation nudge (vs. control) and a new manipulation of whether the two options presented a tradeoff (no tradeoff/dominated vs. tradeoff/non-dominated). The app design, initial choice screen, and confirmation prompt in the "tradeoff" condition were the same as in experiment 3. In the tradeoff condition, the pricing of the two plans were also identical to experiment 3: the monthly plan cost was \$9.99/month, while the annual plan cost was \$59.99/year (49% less per month). Thus, the two plans presented a tradeoff between monthly price and commitment length.

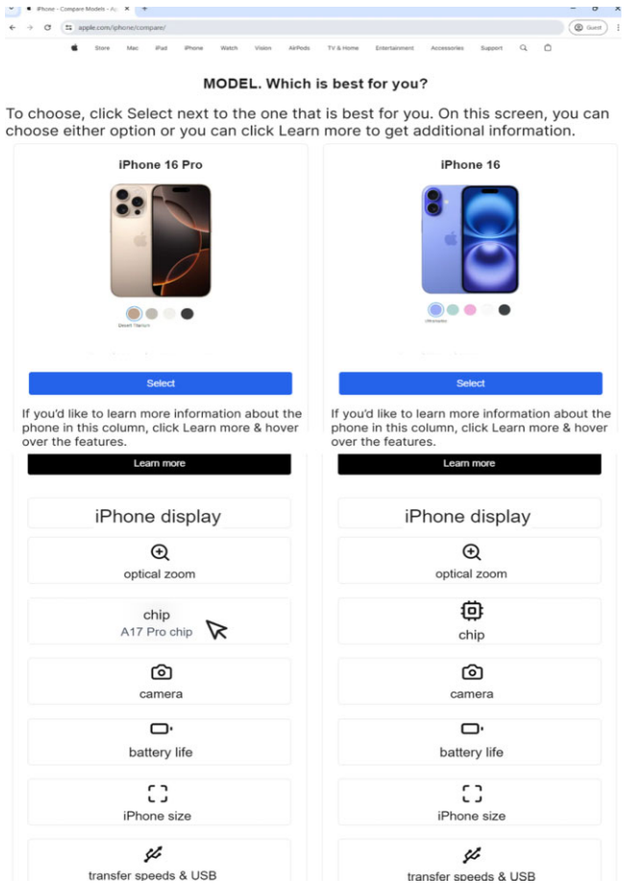
In the "no tradeoff" (dominated) condition, the monthly and annual plans had the same monthly cost: \$9.99/month or \$119.88/year. Thus, the annual option committed users to pay the full \$119.88 up-front rather than having an option to cancel without paying all 12 months. After making their subscription choices, participants in all conditions viewed a "continue and finalize" screen, which gave them a chance to finish the subscription process or tap a large "X" (exit) option that would result in choosing neither plan. The instructions also clarified that clicking the "X" would allow them to exit the app and choose nothing. This was added to make it more similar to the real world, in which consumers could choose to exit the webpage or buy nothing.

After their choice, participants rated the subjective value of the plans in three items, which asked whether it seemed like "the annual plan was a great deal," "the monthly plan was a great deal," and "the annual plan was a much better deal than the monthly plan" (–3 = *strongly disagree*, 3 = *strongly agree*; item 2 was reverse-coded and items were averaged together).

Participants then completed four self-reported mental attention items indicating where they focused relatively more of their thoughts while making their key subscription choice. Specifically, the four items asked participants to indicate "Which of the following thing(s) did you think about as you saw that [last screen]?" for each of the four considerations ("benefits of annual," "costs of annual," "benefits of monthly," and "costs of monthly"; adapted from the 4 categories of query theory measures; Johnson, Häubl, and Keinan 2007). The two items assessing self-reported attention directed toward the monthly plan were subtracted from the two items assessing attention toward the annual plan to form the relative attention measure. This was intended to measure our proposed explanation that confirmation nudges are effective because they increase how much attention people direct toward the annual option that was not initially chosen relative to the monthly option (vs. in the control condition).

TABLE 4

DESIGN OF INITIAL CHOICE AND SECOND CHOICE BY CONDITION IN EXPERIMENT 5 (AND RESULTS)

Initial choice (all 4 conditions)	Second choice in each condition	Results
	**Control condition**: **MODEL. Which is best for you?** We would just like to assess which of the 2 iPhone models you currently think is best for you, whether that answer is the same as on the previous screen or not.	*of phones chosen* 4.2% 16 Pro 95.8% 16 *of full sample* 4.2% 16 Pro 95.2% 16 0.6% neither
	Pre-purchase confirmation (Switch Focus): **Would you like to switch to the iPhone 16 Pro?** On this screen, you can Select either option or you can click Learn more to get additional information. [The rest of the screen was the same as the initial choice.]	*of phones chosen* 7.9% 16 Pro 92.1% 16 *of full sample* 7.8% 16 Pro 90.9% 16 1.3% neither
	Post-purchase confirmation (Switch Focus): ✓ Thank you for your order. View order confirmation **Would you like to switch to the iPhone 16 Pro?** On this screen, you can Select either option or you can click Learn more to get additional information. [The rest of the screen was the same as the initial choice.]	*of phones chosen* 6.6% 16 Pro 93.4% 16 *of full sample* 6.5% 16 Pro 91.9% 16 1.6% neither
	Stay-focus confirmation (post-purchase): ✓ Thank you for your order. View order confirmation **Would you like to proceed forward with the iPhone 16?** On this screen, you can Select either option or you can click Learn more to get additional information. [The rest of the screen was the same as the initial choice.]	*of phones chosen* 2.6% 16 Pro 97.4% 16 *of full sample* 2.6% 16 Pro 94.6% 16 2.8% neither

After this measure, participants completed a measure of consideration order, asking "In what order did you consider these things [as you decided]?" (for all considerations they thought about). This was intended to address alternative explanations of whether confirmation nudges work by influencing the order in which options were considered. Participants also completed the same attention check as in experiment 3, a need for consistency item ("I make an effort to appear consistent to others"; Nichols and Webster 2014) and a perceived demand characteristics item asking participants what they thought the experimenter wanted them to choose.

As pre-registered, we estimated annual subscription choice in a logistic regression as a function of the two manipulations, their interaction, and the initial choice covariate. Formally, the model equation was choice of annual plan $= \beta_0 + \beta_1 \times$ (confirmation nudge) $+ \beta_2 \times$ (tradeoff condition) $+ \beta_3 \times$ (confirmation nudge $\times$ tradeoff interaction) $+ \beta_4 \times$ (initial choice) $+ \varepsilon$.

Results

Subscription Choice As predicted, there was a significant Confirmation $\times$ Tradeoff interaction, $z = 2.09$, $p = .037$, indicating that the confirmation nudge had larger effects in the tradeoff condition compared to the no tradeoff (dominated) condition. Simple-effects tests showed there was a strong effect of the confirmation nudge in the tradeoff condition, $z = 4.17$, $p < .001$ (33% chose annual in the control; 37% chose annual in the confirmation condition), but no significant effect in the no tradeoff condition, $z = 0.88$, $p = .381$ (12% chose annual in the control; 12% chose annual in the confirmation condition). This interaction is consistent with the idea that the effect of the confirmation nudge is significantly reduced when the nudged option is dominated and thus clearly inferior to the other option. There was also a main effect of confirmation nudges on greater likelihood of choosing the annual plan, $z = 3.26$, $p = .001$, and the percentage who chose an annual subscription in the tradeoff conditions was much higher than the percentage who chose annual in the no tradeoff (dominated) conditions (34% vs. 12%).

Self-Reported Attention Confirmation nudges increased self-reported relative attention directed toward the annual compared to the monthly subscription, $z = 2.86$, $p = .004$, consistent with our explanation. In the control condition, participants reported focusing more attention on their chosen option compared to the non-chosen option, $z = 6.45$, $p < .001$, consistent with previous work showing a tendency to focus on owned and selected options while ignoring alternatives. In contrast, participants in the confirmation nudge condition reported focusing more attention on the non-chosen option compared to the chosen option, $z = -3.14$, $p = .002$.

Participants in the confirmation nudge conditions thought more about both the benefits of the annual plan relative to the benefits of the monthly plan and the costs of the annual plan relative to the costs of the monthly plan (both ts > 3.5, ps $< .001$). More relative attention toward the costs of the annual plan (compared to the costs of the monthly plan) was not significantly associated with choice ($|z| < 1$, $p > .30$), whereas more relative attention toward the benefits of the annual plan (compared to the benefits of the monthly plan) had a strong association with choice of the annual plan, $z = 15.60$, $p < .001$.

Mediation Model We conducted a bootstrapped mediation model to examine whether confirmation nudges increase self-reported attention which might account for the effect on choice. Consistent with this explanation, there was an indirect effect through self-reported attention (indirect effect = 0.11, 95% CI [0.04 to 0.19]). This pattern of an indirect effect through attention is replicated in supplemental experiment 1 and across behavioral and self-report attention measures in experiment 5, providing converging support across contexts and measures.

Results of a mediated moderation analysis also suggested that the tradeoff manipulation moderated the confirmation nudge effects by increasing the perceived value of the annual option in the "tradeoff" (non-dominated) condition but not in the "no tradeoff" (dominated) condition (index of mediated moderation = 0.17, 95% CI [0.04 to 0.32]). In other words, confirmation nudges likely fail to increase the perceived value of an option when it is clearly inferior (dominated) or involves no tradeoff (Marini et al. 2023; Milosavljevic et al. 2012).

Discussion

In experiment 4, confirmation nudges had significantly larger effects when the nudged option involved a tradeoff rather than no tradeoff and no significant effect in the no tradeoff condition. This may indicate that there is a boundary condition, in which confirmation nudges are ineffective when used to nudge consumers toward a clearly inferior option. Many other nudges are also more effective when used to nudge people toward more desirable rather than less desirable options (De Ridder, Kroese, and van Gestel 2022). The present results, combined with this past work, suggests that firms should likely use confirmation nudges selectively, avoiding them when the nudged option is undesirable or clearly worse for consumers. Confirmation nudges might be net negative for firms when the nudged option is undesirable, because they can also increase friction and purchase abandonment.

EXPERIMENT 5: HOW DO CONFIRMATION NUDGES INFLUENCE CONSUMERS?

We designed experiment 5 with several purposes in mind. First, whereas the previous experiments used self-report measures of attention, experiment 5 added a direct behavioral measure of attention using mouse-tracking. This would allow for a more direct test of whether confirmation nudges shift relative attention toward the nudged option and whether this shift is associated with the effects on choice. We also manipulated whether the confirmation used a "switch focus" wording or "stay focus" wording. The "switch focus" wording was designed to shift more attention toward the more expensive smartphone (nudged option), while the "stay focus" wording would likely not result in as much of a shift in attention toward the nudged option. If confirmation nudges influence choice by shifting more relative attention toward the nudged option, this would mean that the "switch focus" confirmations should increase relative attention and choice of the expensive smartphone more than the "stay focus" confirmation.

Experiment 5 also more closely examined both the consumer impacts and firm impacts of confirmation nudges. To assess consumer impacts, we added items to assess whether consumers perceive confirmation nudges to be helpful, harmful, manipulative, deceptive, frustrating, and part of a positive or negative customer experience. To assess firm impacts, we assessed whether confirmation nudges induced more negative attitudes toward the company using these nudges and whether they made consumers more reluctant to purchase from the company in the future.

Additionally, we added two experimental conditions in experiment 5 to test two separate questions, namely whether *post-purchase* confirmation nudges can be effective, and whether the effects of confirmation nudges are driven by decision noise. Pre-purchase confirmation nudges can add frictions to the purchase process which can increase the percentage of customers who abandon their purchase (as observed in experiment 2). However, *post-purchase* confirmation nudges offer a promising solution, because they allow customers to switch to a nudged option and update their order after the purchase has already been made so that the confirmation nudge does not interfere with the purchase process. We expected both post-purchase and pre-purchase confirmation nudges to impact consumer choice and not significantly differ from one another.

Though we addressed decision noise in experiment 4, it is still conceivable that effects of confirmation nudges are driven not only by shifts in relative attention but also by noise or mistakes that increase choice of the nudged option because the nudge is presented conditionally. To examine this, we used a control condition in experiment 5 that asks participants for their preference a second time if they initially chose the non-nudged option, meaning that any random responding, mistakes, or other sources of noise should shift choices as much in this control as the confirmation nudge conditions. In other words, this condition was intended to isolate whether the effects of confirmation nudges were due to noise (and the "conditional" component of confirmation nudges) or due to other components of confirmation nudges such as their tendency to shift more relative attention toward non-chosen options. Though this "control" condition had some components in common with confirmation nudges (e.g., the "conditional" component), note that it does not meet the definition of a confirmation nudge, because it simply re-assessed their preferences whether the preferences had changed or not.[5] We pre-registered hypotheses and the interim analysis design (PRIAD; Andre and Reinholtz 2024): aspredicted.org/fzq8-rpdh.pdf. AQ10

Method

Participants We used a PRIAD design, in which we pre-registered a full sample size of 2,000 participants and an interim analysis of the data after 1,000 participants, to potentially save money on this experiment. Some of the main hypotheses were significant after the initial 1,000 participants had completed the study, though at least one main hypothesis was only near significant at that point. So, we recruited another 1,000 participants and report the results using the full sample. In total, 1,977 participants from CloudResearch completed the study. In addition to these 1,977 participants, another group of participants ($n = 1{,}964$) were excluded prior to random assignment and prior to entering the main portion of the study, because they initially chose the nudged option (iPhone 16 Pro) or else exited the survey. These individuals were excluded without being randomly assigned to condition, because in practical applications, confirmation nudges would not be used for these individuals (they did receive a small payment for completing the consent form and initial choice).

Procedure Participants were instructed to imagine they were purchasing a smartphone. They were asked to view a smartphone choice webpage and select the option they would actually select in real life. The webpage in the experiment mirrored an actual Apple iPhone comparison webpage, displaying an iPhone 16 and the iPhone 16 Pro along with information about each option. Participants were instructed to click a "Select" button under whichever

5 The control condition instructions clarified that we were simply interested in re-assessing their preferences whether their preferences had changed or not, which further clarifies that this was not a confirmation nudge. In contrast, "stay focus" confirmation prompts typically meet the definition of a confirmation nudge, because they do not simply re-assess preferences, though they are worded in a way that is focused on the stay option. For example, stay-focus messages like "Do you want to proceed without upgrading?" and "Are you sure you want to quit without saving?" are worded in a way that explicitly mentions the "stay" option but not the "switch" option.

option they chose. All participants first viewed the same initial choice screen (table 4, left column). After making an initial choice, they were randomly assigned to one of the four conditions and viewed a second choice screen that differed depending on their condition. The four conditions are displayed in table 4 (middle column): control, stay-focus confirmation condition, *post-purchase* switch-focus confirmation condition, and *pre-purchase* switch-focus confirmation condition.

In each of the three confirmation conditions, participants were given a second choice between the same two options, and the screen was almost identical to the initial choice, except that a confirmation message was presented. In the control condition, we re-measured participants' preferences while clarifying that they should indicate their current preference, which need not match their choice on the previous screen. Thus, if they made a mistake on the initial screen, changed their preference, or otherwise had noisy preferences, the second choice would measure their current preference and account for these mistakes, changes, and noise. Across all four conditions, this second screen was conditional, meaning that it was only presented to those who initially chose the less expensive smartphone (iPhone 16).

On both the initial and subsequent choice screens, participants could view information about any of the 7 attributes shown in table 4 for either iPhone model (or both) by clicking "Learn more" for that model and then hovering over the desired attribute(s) and option(s). (Clicking "Learn more" caused the attribute boxes with attribute names to appear below the "Learn more" buttons.) Similar to Mouselab (Johnson et al. 1989; Willemsen and Johnson 2019), moving the mouse over an attribute box (e.g., "battery life" box) caused feature information to be revealed (e.g., revealing hours of battery life for that model). On all choice screens, participants could choose neither option and exit the choice by clicking an "X" or any option on the top navigation bars.[6]

Each click and mouseover (as well as the corresponding iPhone model and attribute) was recorded using JavaScript. This allowed us to measure what information participants attended to as well as how much they attended to the iPhone 16 Pro relative to the iPhone 16. A full demo of the initial choice and process tracing is available online (https://iphone-rosy-eight.vercel.app).

In both the switch-focus confirmation nudge conditions (post-purchase and pre-purchase), the confirmation message stated, "Would you like to switch to the iPhone 16 Pro? On this screen you can select either option". In the (switch-focus) post-purchase confirmation nudge condition, the top of the screen had the appearance of an order confirmation or thank you screen, stating "Thank you for your order" with a check mark and "View order confirmation". The "Select" buttons updated their order in the post-purchase condition (rather than updating their cart). The stay-focus confirmation condition was similar to the switch-focus post-purchase confirmation, except that the confirmation wording was intended to orient more attention toward the stay rather than the switch option: "Would you like to proceed forward with the iPhone 16? On this screen you can select either option".

Participants then completed a series of potential mediator measures in random order, namely the predicted mediator (self-reported attention, assessed with the 4-item measure adapted from experiment 4) and measures reflecting five alternative mechanisms (implied endorsement, inferred norms, frictions, decision points, and implied profit motives). To assess each of these five alternative mechanisms, we used the same measures as in experiment 3, with adapted wordings to reflect the iPhone context (see web appendix for exact wordings).

Measures of Consumer Impacts and Firm Impacts Immediately after participants completed the aforementioned measures of potential mediators, we asked them several questions intended to measure consumer perceptions of the confirmation nudge and how these nudges may impact consumers and firms. Specifically, participants indicated the extent to which they felt that the customer experience on the iPhone webpages made them "satisfied" and felt like "a good customer experience" ($0 = $ *don't at all feel this way*; $100 = $ *strongly feel this way*); these two items were averaged for analyses ($r = 0.88$). Using the same scale endpoints, they also reported the extent to which they felt deceived, and also completed a two-item frustration scale assessing whether the iPhone screens made them feel frustrated or angry ($r = 0.76$).

To assess potential company impacts, we measured participants' attitudes toward Apple "right now" ($-50 = $ *very unfavorable attitude*, $50 = $ *very favorable attitude*). We also asked them to indicate what their attitude toward Apple was "just before beginning this survey," which we subtracted from their current attitude to create an attitude change variable. This was done in order to increase power to detect effects on company attitudes (McClelland 2000; Meyvis and Van Osselaer 2018). We assessed future purchase intentions by asking them how likely (from 0% to 100%) they would be to buy from Apple's iPhones

6 In the same way that participants who initially chose the iPhone 16 Pro exited the study, those who clicked an "X" or other "neither" option on the initial screen also exited the study and received a small payment (because confirmation nudges are conditional—used only to influence people who initially choose the non-nudged option—in this case the iPhone 16). Those who clicked any of these buttons on the subsequent screen completed the rest of the study and were coded as choosing neither option. There was also small clarifying text below the "Select" buttons on Screen 2 (Second Choice) clarifying that clicking "Select" for the iPhone 16 Pro on Screen 2 meant switching and updating the purchase (in the stay-focus and switch-focus post-purchase confirmation conditions) or switching and updating the cart (in the pre-purchase confirmation condition) and that choosing the iPhone 16 meant keeping the iPhone 16. The mouse-overs and choices were passed to Qualtrics as query parameters.

webpage in the future (rather than in-store or from a non-Apple retailer) if the purchase screen they viewed was the iPhones webpage.

We also assessed whether participants found the confirmation screens to be helpful, harmful, and manipulative, which were exploratory measures, each assessed with 2-item scales. The perceived helpfulness items asked whether they agree with two statements saying that the screen was "helpful" and "helped me find the option that was best for me" ($-3 = strongly\ disagree$, $3 = strongly\ agree$; $r = 0.77$). To assess perceived harmfulness, they indicated their agreement with two statements saying that the screen "was harmful" and "led me to choose something that was worse for me" ($r = 0.42$).[7] The scale assessing manipulativeness assessed agreement with statements that the confirmation screen "felt manipulative," and "felt like the company was trying to get me to spend more money on a more expensive smartphone" ($r = 0.64$).

In addition to assessing self-reported attention with the 4-item measure, we added a direct behavioral attention measure. Our behavioral measure of relative attention, as described in the pre-registration, was the relative prevalence of mouse-overs on iPhone 16 Pro features minus the prevalence of mouse-overs on iPhone 16 features.

At the end of the study, we also included specific self-report attention items to measure correspondence between behavioral attention (measured via mouseovers) and self-reported attention. We asked participants whether they looked at two attributes that they could have looked at ("battery life" as well as the "transfer speeds & USB" attribute) and one foil attribute that was not actually present ("Face ID"). Finally, participants reported demographics (age and gender), indicated the type of smartphone they own, and answered an attention check item, which was the same as in experiments 3 and 4. There was fairly high correspondence between most of the self-report and behavioral attention items (see web appendix for more details). The correlations between self-report and behavioral measures of attention to both of the specific attributes we asked about were quite high ($r = 0.58$ between self-reported looking at the battery life attribute and an actual mouseover on that attribute; $r = 0.56$ between self-reported looking at the transfer speeds & USB attribute and an actual mouseover on that attribute). For the 4-item self-reported relative attention measure, the correlation with the behavioral measure of relative attention (based on mouse-overs) was somewhat lower ($r = 0.14$, $p = .003$). This lower correlation is still well within the normal range of correlations between a behavioral and self-report measure of the same construct (according to a review by Dang, King, and Inzlicht 2020).

Analytical Approach We analyzed the effects of confirmation nudges on choice using binary logistic regression and on continuous variables using linear regression. The main outcome variable was pre-specified as (binary) choice of the iPhone 16 Pro and there were three primary predictions as well as an ancillary analysis specified in the pre-registration for this main outcome variable. Specifically, the pre-registration mentioned that we predicted that choice of the iPhone 16 Pro would be significantly higher in the two switch-focus confirmation conditions compared to the control and stay-focus confirmation conditions. The model equation for the first aforementioned model is as follows: iPhone 16 Pro choice $= \beta_0 + \beta_1 \times$ (switch-focus confirmation nudge)[8] $+ \varepsilon$. We also pre-registered two other predictions, namely that choice of the iPhone 16 Pro would be significantly higher in the switch-focus confirmation nudge conditions compared to the control condition alone, and that choice of the iPhone 16 Pro would be significantly higher in the switch-focus confirmation nudge conditions compared to the stay-focus condition alone (see table W3 in web appendix for the equations for these models). We also pre-registered an ancillary test comparing the effects of the pre-purchase confirmation nudge to the effects of the post-purchase confirmation nudge, though we noted in the pre-registration that we did not expect a significant difference between the two.

Results

Consumer Choice As predicted, the switch-focus confirmation nudges significantly increased choice of the nudged option (iPhone 16 Pro), $z = 3.70$, $p < .001$. As shown in figure 1, participants in the pre-purchase and post-purchase (switch-focus) confirmation nudge conditions were significantly more likely to choose the iPhone 16 Pro ($M_{pre\text{-}purchase\ confirmation\ nudge} = 7.8\%$; $M_{post\text{-}purchase\ confirmation\ nudge} = 6.5\%$) than participants in the control and stay-focus conditions ($M_{stay\ focus} = 2.6\%$; $M_{control} = 4.2\%$).

The switch-focus confirmation nudges caused significantly more people to switch to the iPhone 16 Pro than the control condition, $z = 2.22$, $p = .027$. This suggests that the effects of confirmation nudges are not fully explained by noisy preferences, mistakes, careless responding, or the

7 These perceived harmfulness items had a weaker correlation than expected. We combined the two items, to be consistent with our intentions for the items and for conceptual reasons. Yet, treating them separately did not substantively alter the results. The effects of confirmation nudges on perceived harmfulness were significant for both items individually and for the 2-item scale (as well as for an alternative 4-item scale that combined perceived helpfulness and harmfulness).

8 This predictor was dummy-coded such that it was coded 1 for participants in either of the two switch-focus confirmation nudge conditions (pre-purchase or post-purchase), else 0. A set of three contrast codes would also be justifiable; all significant effects remained significant when using these alternative contrasts (web appendix). We report the analysis with the single planned comparison (dummy coded) here, both for the sake of parsimony and because we did not consider the alternative specification until well after the data were collected.

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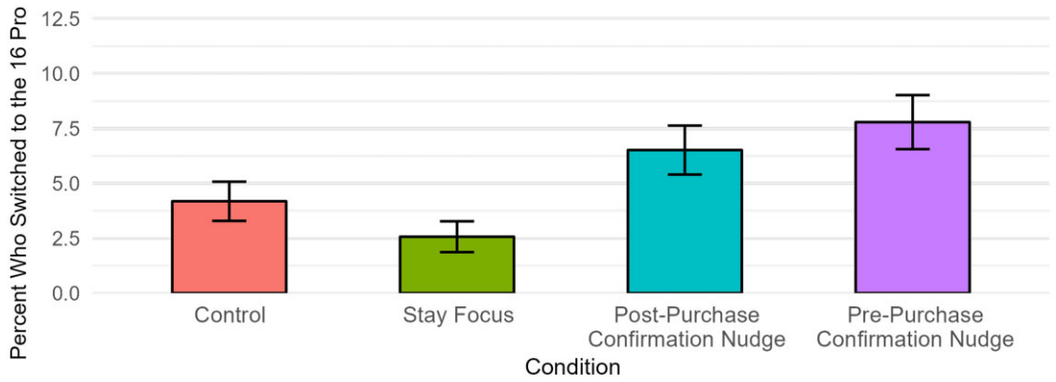
FIGURE 1

PERCENT WHO SWITCHED TO THE IPHONE 16 PRO BY CONDITION



AQ26 NOTE—Error bars depict +/− 1 SE.
AQ28

conditional nature of confirmation nudges, because this control condition gave participants a second choice conditional on their responses to the first choice (just as confirmation nudges do). There was not a significant difference between the control and stay focus conditions, $z = -1.41$, $p = .160$. Yet, it is worth noting that a non-negligible number of participants (4.2%) switched to the iPhone 16 Pro even in the control condition. Though this number who switched in the control was significantly fewer than the number who switched to the iPhone 16 Pro in the (switch-focus) confirmation nudge conditions, it suggests that part of the effect of confirmation nudges is likely driven by noisy preferences and the conditional nature of confirmation nudges.

The switch-focus confirmation nudges also caused significantly more people to switch to the iPhone 16 Pro than in the stay-focus condition, in a planned contrast of the switch-focus vs. stay-focus wording, $z = 3.50$, $p < .001$. This is consistent with the idea that switch-focus confirmation nudges work partly because they shift more attention toward the non-chosen (switch) option relative to the initially chosen option, whereas stay-focus confirmation prompts that keep attention on the stay option are less effective. In the web appendix, we report two other experiments that also indicated that stay-focus confirmation prompts are significantly less effective than switch-focus confirmation prompts.

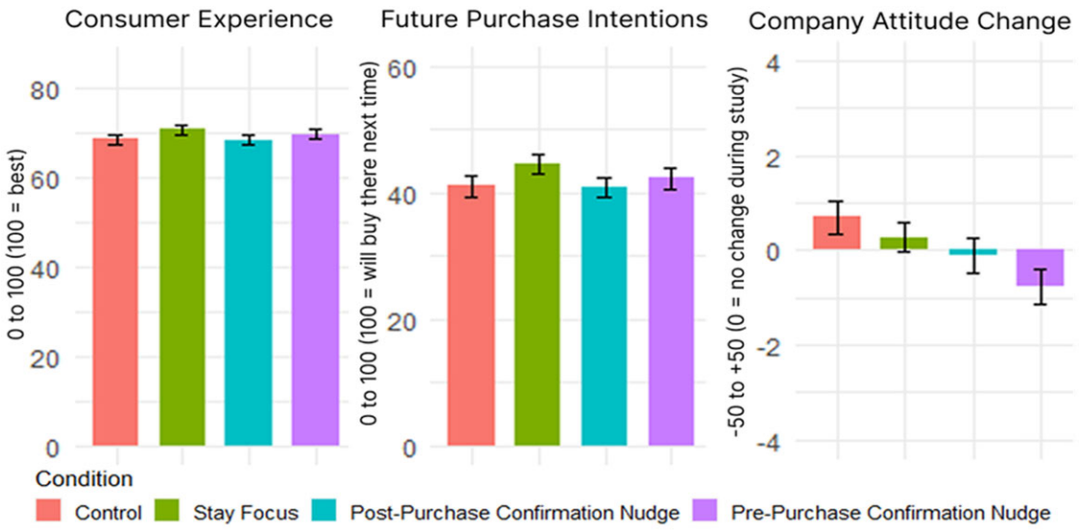
The final (ancillary) pre-registered analysis examined the effects of post-purchase versus pre-purchase (switch focus) confirmation nudges. There was no significant difference between the effect sizes of the pre-purchase compared to the post-purchase confirmation nudges in this analysis, $z = 0.77$, $p = .443$. When analyzing the pre-purchase and post-purchase confirmation nudges separately in post-hoc comparisons, each of these significantly increased choice of the iPhone 16 Pro (respectively, pre-purchase confirmation nudge: $z = 3.61$, $p < .001$; post-purchase confirmation nudge: $z = 2.74$, $p = .006$).

Mediation: Are the Effects Driven by Shifts in Relative Attention? Switch-focus confirmation nudges significantly increased relative attention toward the iPhone 16 Pro compared to the iPhone 16. This effect was significant across both the 4-item self-reported attention measure and the behavioral measure of relative attention (relative prevalence of mouse-overs on iPhone 16 Pro features minus iPhone 16 features), both ts > 3, both ps $< .01$.

In bootstrapped mediation models with 5,000 resamples (Preacher and Hayes 2005), there was a significant indirect AQ11 effect of switch-focus confirmation nudges on choice of the iPhone 16 Pro when examining the behavioral measure of relative attention as the mediator (indirect effect ab = 0.11, 95% CI [0.02 to 0.22]) and also a significant indirect effect through self-reported relative attention in an analogous mediation model with the self-report relative attention measure used in previous studies as mediator (ab = 0.11, 95% CI [0.04 to 0.21]). These indirect effects indicated that the switch-focus confirmation nudges increased how much relative attention was directed toward the iPhone 16 Pro compared to the iPhone 16, which was associated with greater choice of the iPhone 16 Pro. These indirect effects remained significant in parallel multiple mediation models assessing relative attention alongside the five alternative mediators (respectively, ab = 0.09, 95% CI [0.02 to 0.21] with the behavioral relative attention mediator; ab = 0.09, 95% CI [0.03 to 0.19] with the self-report relative attention mediator).

FIGURE 2

AQ27 CONSUMER AND COMPANY IMPACTS OF CONFIRMATION NUDGES WERE MIXED



AQ28 NOTE—Error bars depict +/− 1 SE.

Consumer Impacts and Firm Impacts Figure 2 displays some of the key effects of confirmation nudges on the measures of consumer impacts and firm impacts. We did not predict that confirmation nudges would impact these variables, partly because we thought confirmation nudges would likely be too subtle to influence these measures. Confirmation nudges did not detectably influence participants' reports of whether the purchase process was a good customer experience overall, $t(1939) = -0.37$, $p = .715$ (figure 2). Neither did confirmation nudges significantly impact future purchase intentions, $t(1939) = -0.52$, $p = .607$. There was no detectable effect of confirmation nudges on whether the webpage was perceived as deceptive either, $t(1937) = 1.05$, $p = .295$, nor on the frustration measure, $t(1938) = 0.68$, $p = .498$.

However, confirmation nudges did negatively impact participants' attitudes toward the company using the confirmation nudge, $t(1937) = -2.61$, $p = .009$ (figure 2, right panel). Specifically, on the 101-pt scale, participants in the switch-focus confirmation nudge conditions had slightly more negative attitudes toward the company after viewing the choice screens than at the beginning of the experiment ($M_{change} = -0.43$, SD = 7.92) whereas attitude change was if anything slightly positive in the stay focus condition ($M_{change} = 0.27$, SD = 7.03) and control condition ($M_{change} = 0.70$, SD = 7.77).[9] The switch-focus confirmation nudges also negatively impacted participants' perceptions of how manipulative the screen was, $t(1936) = 13.08$, $p < .001$.

According to the measures of whether the confirmation screens were perceived as helpful or harmful, participants across all conditions agreed much more with statements that the confirmation screens were helpful ($M_{helpful} = 1.07$) than statements that they were harmful ($M_{harmful} = -1.74$). Even in the switch-focus confirmation nudge conditions, participants on average somewhat agreed with statements that the confirmation screen was helpful ($M_{helpful} = 0.91$) and disagreed with statements that it was harmful ($M_{harmful} = -1.63$). Importantly, however, participants in the switch-focus confirmation conditions viewed the confirmation screen as less helpful and more harmful than those in the other conditions (both $|t|$s > 3.5, ps $< .001$).

9 Post-survey attitudes toward the company were still positive across all four conditions (between 7 and 11.5 on the −50 to +50 scale). The analysis of company attitude change had higher statistical power than the other analyses because it accounted for variance associated with participants' pre-survey attitudes by including a measure of these pre-survey attitudes (McClelland 2000; Meyvis and Van Osselaer 2018). Pre-survey attitudes and post-choice attitudes had a very strong correlation ($r = 0.96$) and thus including pre-survey attitudes drastically increased power to detect an effect of condition on the change in company attitudes. This higher power may explain why we observe significant effects on attitude change and not some other variables such as perceived deceptiveness in analyses involving those variables, which were not as highly powered. Indeed, if we analyze post-choice attitudes without accounting for pre-survey attitudes, the effect of condition on attitudes is not significant, $t(1937) = -0.82$, $p = .415$.

Discussion

Confirmation nudges significantly increased choice of the nudged option (iPhone 16 Pro), both relative to a control condition and relative to a confirmation prompt that used stay-focus wording. *Post-purchase* confirmation nudges and *pre-purchase* confirmation nudges each significantly shifted choices toward the nudged option, and there was no detectable evidence that pre-purchase and post-purchase confirmation nudges differentially impacted choice.

Evidence from the stay-focus versus switch-focus wording manipulation, as well as mediation models, was consistent with the idea that confirmation nudges shift choices toward the nudged option partly by shifting attention toward that option. Decision noise and the conditional nature of confirmation nudges also seem to partly account for the effects, though confirmation nudge effects were significant over and above decision noise. This was suggested by the finding that about 4% of those in the control condition changed their preference to the iPhone 16 Pro when we re-measured their preferences, which was significantly less than the percentage who switched in the switch-focus confirmation nudge conditions. Experiment 5 also illustrated several potential consumer and firm impacts of confirmation nudges.

GENERAL DISCUSSION

Across a series of field and lab experiments, confirmation nudges had meaningful impacts on consumer behavior across several different contexts. Importantly, confirmation nudges had significant countervailing effects in experiment 2 and similar magnitude but nonsignificant countervailing effects in experiment 1. In experiment 2, the confirmation nudge prompted more people to buy a nudged service plan but also decreased completed jewelry purchases. Confirmation prompts were more effective when the confirmation screen was worded in a way that focused more on the switch option rather than in a way that emphasized the stay option. This was consistent with the idea that the effects of confirmation nudges are driven partly by their ability to attract more attention toward the switch option. Confirmation nudges were typically perceived as more helpful than harmful, and they impacted people who would benefit financially from nudges more than those who would not. Yet, they negatively influenced how manipulative the screens were perceived to be (experiment 5).

Across experiments, we addressed alternative explanations for these results. Though some confirmation nudges used in the field made the nudged option more visually prominent, the effects of confirmation nudges were robust even when we controlled for visual salience and the wording of each option in the lab experiments. The effects of confirmation nudges were significant over and above decision noise, though decision noise and the conditional nature of confirmation nudges likely partly account for their effectiveness (experiment 5). Inferred endorsement and several other alternative mechanisms did not account for the effects of confirmation nudges on choice (experiments 3 and 5).

The present research has important practical implications. The countervailing effects of confirmation nudges suggest that firms need to determine in their context the relative importance of shifting people toward the nudged option compared to causing another (sometimes smaller) group of consumers to abandon their purchase completely. In some contexts, including the specific subscription company's context in experiment 1, the benefits of nudging people from the monthly plan to the annual plan outweighed the costs of losing potential purchases. Therefore, that company implemented the confirmation nudge on their app and website and continues to use it. They expressed that the projected lifetime revenue was more than twice as high for consumers whom they nudged toward an annual plan compared to those who chose a month-by-month plan. Therefore, if the confirmation nudge shifted one of their visitors from the monthly plan to the annual plan for every one visitor that was shifted away from any purchase, they would still likely benefit overall. However, in other cases, the revenue gained from shifting people toward a nudged option would not offset the negative effects of adding frictions. The context of experiment 2 appears to be one such case: Revenue per visitor was lower in the confirmation nudge condition compared to the control condition, because confirmation nudges shifted some consumers away from expensive jewelry purchases, which had a bigger impact on revenue than the countervailing increase in service plan purchases.

One promising solution to potentially avoid the harmful effects of purchase abandonment is to use post-purchase confirmation nudges. These nudges occur after the purchase is completed, giving consumers a second chance to choose a premium subscription or other nudged option. Thus, the extra step and frictions occur post-purchase. Early indications suggest that these post-purchase nudges (recently pioneered in online retail by a company called Aftersell) can increase revenue without leading to order cancellations, returns, changes in customer satisfaction, or other negative impacts on firms (Gan and Holtz 2024; Rokt 2025). Yet, this hypothesis that post-purchase confirmation nudges would reduce purchase abandonment compared to pre-purchase confirmation nudges requires further testing across contexts before strong conclusions can be made. It is still possible that some post-purchase confirmation nudges would increase purchase cancellations or reduce future purchases, and there was no evidence from experiment 5 that post-purchase confirmations reduced purchase abandonment compared to pre-purchase confirmations (the percentage who chose neither option in that experiment

was very low and not appreciably different by condition). There may also be cases in which post-purchase nudges influence preferential choice less than pre-purchase nudges, but initial evidence from experiment 5 suggests post-purchase confirmations can have sizable impacts on choice, perhaps as large as pre-purchase confirmation nudges.

A practical benefit of both post-purchase and pre-purchase confirmation nudges is that these interventions would often be inexpensive for companies to A/B test and implement, often requiring changes to only a few lines of code (Benartzi et al. 2017; Narayanan et al. 2020). Given that the effects of confirmation nudges can be large but can also vary depending on the context, many firms should A/B test the impact of confirmation nudges in their own context before implementing them (Goswami and Urminsky 2021). When conducting these A/B tests, companies should consider not only standard metrics such as revenue per visitor in each condition, but also the net effects of the confirmation nudge on profits. When possible, these analyses should be supplemented with data examining the long-term effects on future purchases and customer loyalty, to examine whether the confirmation nudge had any detectable impact on reputation and future purchases. For example, the jewelry company in experiment 2 could take the different profit margins of service plans and jewelry into consideration, while also examining whether customers randomly assigned to the confirmation nudge were any less likely to visit and purchase from their website in the future.

Can expert practitioners and academics accurately predict how confirmation nudges will influence consumer choice? To examine their estimates of confirmation nudge effects, we surveyed 44 practitioners and academics. We showed each expert screenshots of the confirmation nudges used in experiments 1, 3, and 4, and asked them to estimate how these nudges would impact subscription choice (see details in web appendix). These experts predicted that 42.0% of subscribers (who choose one of the subscriptions) would select the nudged annual subscription in the control condition compared to 42.3% in the pure confirmation nudge condition from experiment 3 (the actual shift from monthly to annual among subscribers was 9%age points in experiments 3 and 4 pp in experiment 4, compared to this 0.3 pp estimated effect). Respondents did not accurately anticipate how the effects would vary by context in experiment 4 either (i.e., the tradeoff vs. no tradeoff interaction); if anything, they expected the opposite pattern (web appendix). Note, however, that this does not necessarily mean that experts underestimate the net value of confirmation nudges for companies. Respondents only anticipated how much these nudges would shift choices from the monthly plan to the nudged annual plan (among subscribers) and did not estimate the effects on purchase abandonment or the net impact. Thus, experts might more accurately predict net effects even if they underestimate how many subscribers shift toward the nudged plan. In any case, the results of this article will likely be valuable for practitioners, helping them better understand when, how, and for whom confirmation nudges are most likely to impact consumers.

The present experiments also provide insight into the consumer impacts of confirmation nudges. Confirmation nudges had significantly larger effects on those who would benefit financially by choosing the nudged option compared to those who would not benefit financially (experiment 3). Confirmation nudges also had smaller effects when the nudged option was clearly undesirable (experiment 4, no tradeoff condition) than when it had a higher value. This does not mean that consumers are never harmed by confirmation nudges. Yet, it suggests that these nudges are more effective when the nudged option is at least somewhat beneficial. We believe that companies should therefore avoid nudging consumers toward undesirable options and look for win–win situations in which confirmation nudges benefit both consumers and firms.

The extent to which confirmation nudges help consumers would likely also depend on how much attention and care consumers initially direct toward the decision, and whether or not the nudged option is beneficial for most customers. If the nudged option is best for most consumers, confirmation nudges would likely be helpful for consumers overall. These positive effects might also be larger if consumers make inattentive or mistake-prone initial choices and pay closer attention during the confirmation screen.

In most contexts, confirmation nudges are legal and unlikely to result in fines or other regulatory actions. One exception is that confirmation nudges, in some countries, would violate laws if used to lengthen a subscription cancellation process to the point where it would require more steps to cancel than sign up for the subscription (DMCCA 2024). In the context of cookie consent, a court ruling has suggested that confirmation nudges might also be unlawful under GDPR principles if they make rejecting cookies require an extra step relative to accepting cookies (GDPR Hub 2025). Firms also need to consider whether confirmation nudges would cause backlash or negative changes in company attitudes. Experiment 5 suggested that confirmation nudges can negatively influence attitudes toward the company using them, though there was no significant effect on future purchase intentions. Though these effects on attitudes in that experiment were small, they might be larger if companies use confirmation nudges repeatedly or heavy-handedly, or if they try to nudge consumers toward less desirable options.

Our research has implications for theories of self-consistency and choice architecture. It suggests that tendencies to favor initially favored or chosen options can be mitigated with a light-touch nudge. Additionally, the finding that confirmation nudges had similar effect sizes in the lab and field has implications for prominent recent theories, which have suggested that nudges often do not scale to the field without large predictable drops in effect size

(DellaVigna and Linos 2022; List 2022). Confirmation nudges had effect sizes that were almost as large as effects in the lab when using the same design (experiment 3, confirmation plus condition). This seems to suggest that some nudges may generalize from the lab to the field without much reduction in effect size. However, some caution is warranted in comparing our field and lab experiments; though the design of the nudges in some conditions of experiments 1 and 3 were the same, the samples were different, as were the incentives (simulated vs. real choice).

When used wisely, confirmation nudges can help both consumers and firms. But confirmation nudges can sometimes be harmful for firms. And like many choice architecture tools, confirmation nudges can also be used to increase short-term revenue at the expense of consumer welfare. Firms should therefore nudge wisely and nudge for good.

DATA COLLECTION STATEMENT

Experiment 1 data were collected by Stanley Zuo (the 4th author) via AB Tasty (an A/B testing tool) from April 6, 2022 to April 15, 2022 as part of the A/B testing efforts of the firm, which sells subscriptions for kids' learning programs. Participants were users of the company's smartphone app. Employees analyzed the data initially, SZ sent them to KM, and KM turned them into a CSV and R script that could be added to OSF. SZ and KM analyzed the data and MS and SD each examined the results. Experiment 2 data were collected via Dynamic Yield (an A/B testing tool) by Jared the Galleria of Jewelry and shared with Kellen Mrkva on January 5, 2024 (the A/B test was conducted from October 19, 2021 to November 3, 2021). KM turned them into a CSV and R script that could be added to OSF. KM analyzed the data and MS and SD each examined the R notebook showing code and results. Experiment 3 data were collected by Kellen Mrkva, Marissa Sharif, and Shannon Duncan in February 2023 via CloudResearch (MTurk Toolkit). KM analyzed the data and MS and SD each examined the R notebook showing code and results. Experiment 4 data were collected by Kellen Mrkva, Marissa Sharif, and Shannon Duncan in March 2023 via CloudResearch (MTurk toolkit). KM analyzed the data and MS and SD each examined the R notebook showing code and results. Experiment 5 data were collected by Kellen Mrkva, Marissa Sharif, and Shannon Duncan in February 2025 via CloudResearch Connect. KM analyzed the data and MS and SD each examined the R notebook showing code and results. Data for Supplemental Experiments 1–3 (Web Appendix) were collected by Kellen Mrkva, Marissa Sharif, and Shannon Duncan in May 2023 via Cloudresearch (Supplemental Experiment 1), March 2023 (Supplemental Experiment 2), and February 2024 (Supplemental Experiment 3). In each, KM analyzed the data and MS and SD each examined the R notebook showing code and results. Data for all experiments are stored on the Open Science Framework and publicly available at https://osf.io/zbw9h.

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