Nudging App Adoption:

Choice Architecture Facilitates Consumer Uptake of Mobile Apps

Crystal Reeck

Nathaniel A. Posner

Kellen Mrkva

Eric J. Johnson

Crystal Reeck is Assistant Professor, Fox School of Business, Temple University, crystal.reeck@temple.edu, orcid: 0000-0002-1540-5321. Nathaniel A. Posner is PhD student in marketing at Columbia Business School, Columbia University, NPosner26@gsb.columbia.edu, orcid: 0000-0003-1616-2306. Kellen Mrkva is Assistant Professor, Hankamer School of Business, Baylor University, kellen_mrkva@baylor.edu, orcid: 0000-0002-6316-5502. Eric J. Johnson is Norman Eig Professor of Business, Columbia Business School, Columbia University, ejj3@columbia.edu, orcid: 0000-0001-7797-8347.

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Correspondence concerning this article should be addressed to Crystal Reeck, Department of Marketing, Fox School of Business, Temple University, Philadelphia, PA 19122. crystal.reeck@temple.edu.

Abstract:

How can firms encourage consumers to adopt smartphone apps? The authors show that several inexpensive choice architecture techniques can make users more likely to enable important app features and complete app onboarding. Across six pre-registered experiments (n=5,968) and a field experiment (n=594,997), choice architecture interventions manipulating choice sequence, color, and wording of app adoption decisions dramatically increased app adoption. Across experiments, integrating multiple feature decisions into a single choice increased adoption. This integration effect emerges because it decreases decision noise and reduces the prominence of individual features, consistent with support theory. Changing colors to match habitual patterns commonly found in current digital interfaces appears to increase adoption by accelerating consumers' decisions. Finally, wording options *as if* enabling the app was the default response (even without changing the actual default) also increases adoption. These defaultless defaults may be particularly relevant in heavily regulated consumer domains, such as finance or healthcare. The effects generalized across different types of apps and were robust across subsamples varying in demographics, attitudes towards the apps, and political affiliation. These results suggest simple tools that marketing managers and app developers can use to increase app adoption.

Keywords: mobile apps; decision making; choice architecture.

Over 3.5 billion people worldwide use smartphones and over 200 billion apps are downloaded each year (Rosenfelder 2020). Increasing the number of users who install smartphone apps can greatly influence purchases, customer engagement, and brand attitudes (Bellman et al. 2011; Gill, Sridhar and Grewal 2017; Narang and Shankar 2019; van Heerde et al. 2019). It is therefore not surprising that over 75 billion dollars annually is spent on advertisements designed to increase app downloads (Rosenfelder 2020). But advertising is not the only way to increase app adoption. In addition to using advertising, managers can use promotions or incentives to motivate consumers to install and use their apps (Gokgoz, Ataman and van Bruggen 2021; Munzert et al. 2021). But managers and app developers can also use choice architecture as a complementary technique to influence consumer app adoption.

How can managers use choice architecture to facilitate adoption of their apps? We focus on simple changes to the color, wording, and choice sequence during app adoption that can drastically increase the proportion of consumers who install and fully enable smartphone apps. We posit that presenting preferred choice options in colors usually associated with accepting a choice (often blue in digital environments), integrating app feature choices together, and altering option wording can each facilitate app adoption. We advance a theoretical account of these findings based on research on "habit cues" (Anderson and Wood 2021; Carden and Wood 2018) and suggest that presenting preferred options in blue increases adoption partly because it accelerates consumer decisions and appeals to the fact that people habitually select these buttons. We test these nudges in multiple contexts, including experiments that focus on digital contact tracing apps (which were developed and widely deployed during the COVID-19 pandemic in 2020) with a smartphone interface mimicking actual presentations, other experiments using health and fitness apps, and a field experiment looking at clickthrough rate in an employment context. Managers and app developers often want to make the funnel from downloading an app to fully enabling the app smoother; we examine three techniques that could help accomplish this goal and elucidate the mechanisms underlying their influence on digital consumer behavior.

Mobile App Adoption

Increasing app adoption is valuable. Researchers have investigated how mobile app launches and adoption influence revenue and consumer behavior. Most of this research suggests that app adoption is associated with an increase in purchases, purchase intentions, loyalty points accrued, customer engagement, and brand attitudes (Bellman et al. 2011; Gill et al. 2017; Kim et al. 2015; Narang and Shankar 2019; van Heerde et al. 2019). Retailers who launch a mobile app can experience an increase in consumer spending at both their online and offline channels (Narang and Shankar 2019). Though mobile app adoption can increase product returns, this negative outcome does not fully offset the increase in spending. Further, this relationship between app adoption and purchases is likely causal: Exogenous variation in app failures reduces the frequency, quantity, and monetary amount of purchases (Narang et al. 2021).

App adoption can influence other outcomes as well, depending on the purpose of the app. For example, the launch of a news app can increase visits to the corresponding news website (Xu et al. 2014) and results of at least one RCT suggest that adoption of a physical activity app can cause greater levels of moderate to vigorous physical activity (Maher et al. 2015). Consumers can also benefit from other adoption decisions: public health apps that track users' locations and alert users about their exposure to COVID-19 can reduce COVID-19 cases, fatalities, and economic losses (Abueg et al. 2020; Kendall et al. 2020; Wymant et al., 2021; Xiao 2020).

Firms also benefit when users enable app features. For example, features like in-app purchases and premium upgrades provide direct revenue for firms, while firms that acquire email addresses, location tracking, or other user data can make money with this information (Staiano et al. 2014). In some cases, enabling app features is also necessary for an app to function properly. For example, digital contact tracing apps often entail three key features (e.g., exposure notification, exposure logging, and Bluetooth) and these apps fail to function properly when users fail to enable any one of these features. But how can managers encourage more consumers to fully adopt an app, including its key features?

The Mobile Marketing Funnel

Some practitioners and researchers have described a mobile app marketing funnel (or "conversion funnel", Figure 1), consisting of a series of steps from downloading the app and installing it to completing onboarding screens and choices about privacy, notifications, and other app features (Bagherjeiran, Hatch and Ratnaparkhi 2010; Chong, Yang and Chen 2020). There has been relatively little academic literature on interventions designed to increase the percentage who complete this funnel. Digital and social media marketers are especially interested in conversion funnels, and interventions may help facilitate consumer action. For example, one paper examined effects of social norms and self versus other-orientation interventions on clicking a link to download a COVID-19 digital contact tracing app (Sharif et al. 2021). However, this prior work did not examine choice architecture interventions and stopped short of examining whether key app features were enabled to allow the app to function. Another paper examined the traditional economic tools of incentives and informational interventions on adoption of these apps, finding that informational videos did not increase adoption whereas incentives did have a small effect on adoption (Munzert et al. 2021). Nudges can have sizable effects and they are usually much less expensive than incentives to implement, meaning that they can be cost-effective tools for firms and governments (Benartzi et al. 2017).

Other research in the context of website interfaces has also examined how website loading failures and slowdowns influence various outcomes. For example, “slowdown experiments” have been conducted by firms and academics to examine how reductions in loading speed influence user behavior (Kohave, Tang and Xu 2020). Additionally, there is at least one published case study suggesting that removing a burdensome registration step at checkout can have large benefits, increasing revenue by $300 million for one large retailer (Spool 2009). There is less research on how the number of clicks or screens influences behavior, though some practitioners have proposed a “three click rule” (Iglesias et al. 2018), suggesting that everything on a website should be accessible within three clicks (although many critics have disputed this rule; Porter 2021). More generally, a review concluded that small changes in user interface can have a big impact on consumer purchases and retailer profits (Kohavi et al. 2014).

Theoretical Framework

The choice architecture of smartphone apps involves several design decisions, including choices about the color and wording of choices and whether decisions to enable features should be described separately or integrated into fewer screens and choices. We selected the choice architecture interventions examined in the present work based on observed variability in COVID-19 digital contact tracing apps (Tables S1-S3) and theoretical understanding of the psychological processes likely to underpin their effects. We did not test any additional interventions besides those documented in the present article.

Habit cues & color. We hypothesized that the color and wording of choice options would influence consumers’ decisions to enable the app features. Specifically, we posit that presenting one option in blue would increase the proportion of consumers who choose that option, because it would mimic the color of typically-chosen “continue” and “enable” options frequently encountered in smartphone and online environments. In other words, we posited that the color of these options acts as a habit cue that remind people of contexts in which they habitually choose a particular type of option (Anderson and Wood 2020; Carden and Wood 2018; Schneider and Shiffrin 1977). This idea extends to many contexts: just as people may have habitual tendencies to open notifications whenever a red “1” appears (regardless of whether the notification is important; Wood, Tam, and Witt 2005), we propose most people habitually select blue options like “continue” to enable all features on smartphone apps and habitually enable cookies on all websites. They likely develop mental scripts (Abelson 1981) of the app installation process and the blue “continue” or “enable” buttons that they routinely select during this process.

Research in computer science has documented that the color and outlines around options increase the speed with which users select options (Lucaites et al. 2017). In addition to influencing how quickly people make choices, color may also serve as a learned cue that influences what consumers choose, such that consumers should be more likely to choose a blue choice option or an option that says “continue” if they choose the blue “continue” buttons every time they install apps in everyday life.

Support theory & choice integration. Several smartphone apps describe privacy elements and app features as a series of separate screens, each with a separate decision required to enable the app. If a user fails to enable any one of the key features, the app may be less beneficial to the firm or, in the case of digital contact tracing, ineffective. Similarly, apps launched by retailers often present choices to consumers that they could integrate or separate, such as choices to enable in-app purchases, location tracking, badges, and alerts. We hypothesized that integrating multiple features into a single decision (without changing the information itself) could increase the number of users enabling the app.

This prediction builds on a theory of probability judgments called support theory, in which unpacking a single larger category of risk (e.g., dying of natural causes) into separate smaller components (e.g., dying of heart disease, cancer, or other natural causes) makes the risk seem more prominent and probable because the individual components are more readily accessible (Johnson et al. 1993; Tversky and Koehler 1994). We extended support theory from probability estimates to choices, and pre-registered the prediction that separating features would make people more likely to disable feature(s). In contrast, an alternative account would suggest that unpacking app features into separate shorter components would make the information easier to parse and process (Wong and Wong 2014), which might reduce perceived risk or facilitate decisions to enable the app (Song and Schwarz 2009).

Defaultless defaults. We also hypothesized that we could facilitate adoption by formatting adoption *as if* it was the default response, without actually changing what happens if no choice is made. At least two contemporaneous papers have examined nudges that manipulate perceived ownership (of government benefits and vaccine doses, respectively) without changing the de facto default, showing that these nudges can influence decisions (de la Rosa et al. 2021; Milkman et al. 2021). Our "defaultless defaults" manipulation was distinct from standard defaults and from these perceived ownership manipulations. Specifically, we used wording, color, and position to make one option seem like the default, similar to the typical "continue" buttons on smartphone app interfaces (which are often blue and positioned on the top of the screen; if people made no selection the technology would always be disabled, meaning that the de facto default is to disable the features). Unlike the previous research, we also identify separable components of the effect and show that three mechanisms which explain standard default effects (implied endorsement, ease, and endowment; Dinner et al. 2011; Jachimowicz et al. 2019) do not fully explain our results.

Relationship to Previous Choice Architecture Research

Choice architecture tools are changes to the structure of a choice that alter behavior without significantly changing economic incentives or the information that people have about each option (Johnson, 2021; Munscher et al. 2016; Thaler and Sunstein 2008).

There are many types of choice architecture tools, including defaults, sorting, option partitioning, and tools that attempt to simplify the way attributes are presented (Fishbane, Ouss and Shah 2020; Johnson et al. 2012; Munscher et al. 2016). Recent analyses have considered whether choice architecture interventions have sizable effects (Mertens et al. 2022) or negligible effects (Maier et al. 2022). However, a clearer picture emerges when considering that choice architecture is not a unitary construct and has heterogeneous effects depending on the type of nudge, population examined, and context (Bryan et al. 2021; Last et al. 2021; Mrkva et al. 2021; Soman and Hossein 2021; Soman and Yeung 2020). On average, nudges can have significant impact even when examining interventions conducted by organizations that examine real world outcomes and make all results public (i.e., with no publication bias; Linos and DellaVigna 2022).

Furthermore, some nudges have much larger effects than others (e.g., Cadario and Chandon 2020; Last et al. 2021). Prior research suggests that defaults have the largest effect sizes of any choice architecture tool, according to recent systematic reviews (Beshears and Kosowsky 2020; Jachimowicz et al. 2019: Last et al. 2021) and the processes underlying their effectiveness are well-documented. Specifically, defaults influence decision making through ease, implied endorsement, and endowment—that is, by making a particular option seem easier to choose, making it seem like the option that others prefer, and making people focus first and give more weight to features that would reduce willingness to give it up (Dinner et al. 2011; Jachimowicz et al. 2019; McKenzie, Liersch, and Finkelstein 2006). Previous research has also examined how choice architecture that makes products or information more salient influences decisions (Milosavljevic et al. 2012). We show in Experiments 4-6 that the defaultless defaults, integration, and color effects have distinct mechanisms from traditionally-examined choice architecture interventions.

Overview of Experiments

We demonstrate in six pre-registered lab experiments and a field experiment that changing the color, wording, and number of decisions made during app adoption influences choices to enable smartphone apps. In Experiment 1, we designed an ecologically valid exposure notification app interface to test the defaultless default and integration effects. In the Supplemental Experiment (see Web Appendix), we added incentives and demonstrated the robustness of the effects across several diverse subsamples varying in health, age, income, and COVID-19 risk factors. Experiments 2 and 3 added to the external validity of our results. Experiment 2 replicated the defaultless defaults and integration effects on potential users of exposure notification apps in Pennsylvania, New York, New Jersey, and California during the rollout of real exposure notification apps. Experiment 3 examined the effect of color in an A/B test in the field involving real consumers. Experiment 2 and Experiments 4-6 clarified the mechanisms underlying these effects. Experiment 2 examined the contribution of wording to the defaultless default effect, while Experiment 4 examined the contribution of button color and button position to the effect. Experiment 5 independently varied the color and salience of the buttons to further examine the color effect, revealing that habits and not visual salience mediate the effect. Finally, Experiment 6 examined several mechanisms underlying the defaultless default effect and the integration effect, providing evidence that both decision noise and unpacking the feature information, which increased consumers' awareness of individual features, contribute to the integration effect.

We designed our experiments to have ecological value and practical significance (Van Heerde et al. 2021). We designed stimuli resembling real smartphone apps and participants completed each study on their smartphones. The field experiment (Experiment 3) also enabled us to examine the effects of choice architecture using real consumers and outcomes. In Experiment 2, we also collected data from participants who were eligible to install the app and who would benefit from its use. In the taxonomy defined by Harrison and List (2004), our general approach was to adopt framed field experiments, which they argue have high external validity. Note that the privacy protections in place on exposure notification apps did not allow for the tracking of whether individuals properly download and install the apps, making a true randomized control trial impossible using digital contact tracing apps. Moreover, such an experiment would not have been ethically appropriate during the pandemic, as one group would be expected to experience lower uptake of the app and therefore less protection compared to another group. All data, analysis scripts, pre-registrations, and exact copies of the experiment materials are publicly available at https://osf.io/6qyk5/?view_only = 0734057fae4544118cc61f3b1cf242a6.

Experiment 1

In Experiments 1, 2, and 4, we tested whether integrating app features into a single decision and using defaultless defaults would increase adoption of important app features. Our initial experimental context was modeled on a COVID-19 exposure notification app. These apps were designed in 2020 during the COVID pandemic to increase detection and reduce spread of COVID-19, a contagious viral disease that killed millions and disrupted daily life worldwide.

These apps alert users about contacts with people who had positive virus tests by temporarily storing a record of close daily contacts on a user's phone. When users receive a positive virus test, these contacts are anonymously notified that they may have been exposed. We chose this experimental context for three reasons. First, the app is relevant for everyone and virtually everyone would benefit from its installation, unlike other apps where personal taste or needs would determine for whom the app was appropriate. Second, with the COVID-19 pandemic causing millions of deaths worldwide, the use of technology to curb its spread was timely and relevant. Finally, digital contact tracing is an area where nudges would benefit not only individuals but also societal welfare. In subsequent experiments, we generalize and extend our findings to other mobile app contexts, including health, fitness, and employment search. We chose these contexts because they are important and could also benefit multiple parties such as individual users, society, and firms.

Method

Participants. We requested 500 participants from ROIRocket, an online panel provider, and received 509 American participants (20% male, M_{age} = 48). This sample size was chosen prior to data collection and pre-registered along with hypotheses and primary analyses (https://aspredicted.org/w3za8.pdf). We selected this sample because participants from ROIRocket's panel are usually more diverse and less likely to have participated in hundreds of academic experiments compared to participants on Mechanical Turk (Mrkva et al. 2021).

Procedure. Participants were told to imagine they had just downloaded a smartphone app in order to help prevent the spread of COVID-19. They were then presented with a simulation of what the app's interface might look like (similar to Figure 2). This interface was designed to mimic common COVID-19 contact tracing apps, including the beta version of the Google Apple

Exposure Notification interface. We varied elements of the design between conditions. Participants interacted with the interface as if using a real app by tapping the buttons on the phone's screen, and were instructed to make decisions as if they were making them in real life.

Participants were prompted on each screen to enable one or more features required for an exposure notification app to function (the features were "exposure notification," "exposure logging," and "Bluetooth"). We varied the options' presentation using a 2 (presentation: opt-in vs. opt-out format) × 2 (integration: integrated vs. separated choices) between-subjects experimental design.

In the "opt-out presentation", options that involved enabling each feature were worded *as if* they were the default options (Figure 2). Critically, however, the default itself (which was not to enable the app) did not change. The option presented as if it were a default was also colored blue or was bolded (depending on the screen of the app) and listed either on the top or right, mirroring how suggested options are typically displayed on mainstream smartphone operating systems. In the "opt-in presentation," the options involved in enabling each feature were worded and presented as if they were *not* the default.

In the "separated" condition, the choices that enabled features were spread across three screens. This mirrors the design of several exposure notification apps including Minnesota's, which includes three separate privacy decisions, all of which are required for the app to communicate exposure notifications. Participants who chose not to enable a feature did not see the subsequent decisions, because they had already disabled a feature needed for the app to function. In the integrated condition, the same information and decisions were aggregated into one screen and one choice, which was similar to Canada's exposure notification app, as well as the UK NHS app, which integrated two features (exposure notifications and logging) onto a single screen with a single choice to enable or disable both. After making these decisions, participants were reminded of their choices and reported why they chose the options they did (open-ended). They completed several demographic questions (including race, income, and overall health). All experiments were approved by the IRB.

Results

Overall, slightly more than half of participants (54%) decided to enable the app.

Defaultless Defaults. The opt-out presentation more than doubled uptake compared to the opt-in presentation. About 28% of participants enabled the app in the opt-in presentation, whereas 79% enabled the app in the opt-out presentation, $z = 10.86$, Exp(B) = 9.74, $p < .001$. Despite not actually changing the default, presenting an option *as if it were the default* had a large effect, much larger than interventions in previous research that used incentives or an informational video to increase contact tracing app uptake (Munzert et al. 2021).

Integration effect. Integrating the disclosures and feature decisions into a single choice also increased adoption. Specifically, 59% of participants fully enabled the app in the integrated condition, compared to 49% in the condition with separated disclosures and privacy decisions, $z = 2.21$, Exp(B) = 1.59, $p = .027$ (Figure 3).

We also conducted pre-registered robustness tests to address the possibility that these effects were driven exclusively by respondents who were unengaged in the experiment or not paying attention. The effects of integration and defaultless defaults were virtually identical when we excluded participants who failed the attention check (Default Presentation: $p < .001$, Integration: $p = .035$). The results were also robust when controlling for time spent taking the survey and its interactions with the manipulations as well as average time spent on each app screen and its interactions with the manipulations (all ps < .05).

Discussion

In Experiment 1, both manipulations markedly increased the number of people deciding to enable exposure notification apps. The effect of the defaultless defaults manipulation was particularly large, even though the manipulation did not change the de facto default. It is well-known that defaults can have large effects (Jachimowicz et al. 2019; Last et al. 2021); indeed, when Apple recently switched to an opt-in policy that required apps to ask permission before tracking or sharing activity across other apps, the percentage of users allowing tracking plummeted (Sharma 2021). However, many firms are averse to changing the de facto default, which can prompt backlash, especially when privacy, health, or other protected values are at stake (Chapman et al. 2016; Lehmann et al. 2016). So, the observation that defaultless defaults can have a similarly large effect without changing the actual default is an important insight.

One limitation of Experiment 1 is that the decisions had minimal consequences. In a Supplemental Experiment (see Web Appendix), we therefore added incentives and examined additional alternative explanations for the observed effects. The Supplemental Experiment replicated the findings from Experiment 1 and demonstrated that variability in comprehension did not fully account for the observed effects. Additionally, the Supplemental Experiment demonstrated that the effects were robust across a wide variety of people varying in political party, age, race, and COVID-19 risk factors. In Experiments 2-4 we therefore sought to isolate the causes of these observed effects of choice architecture on mobile app feature adoption.

Experiment 2: Disentangling Defaultless Defaults

In Experiment 2, we separated two components of the presentation manipulation. We predicted that the opt-out presentation made people more likely to enable the app both because of the wording itself and because the position and blue color align with options that are habitually chosen in smartphone environments. This experiment partitions this effect into its two underlying components.

To increase external validity of our framed field experiment, we sampled people who lived in states that had recently launched an exposure notification app: California, Pennsylvania, New York and New Jersey. The app interface in the experiment mimicked the interface of the COVID Alert app, which was available in Pennsylvania, New York, and New Jersey.

Method

Participants. A total of 498 (53% male, $M_{\text{age}} = 35$ years) participants from Amazon Mechanical Turk completed the survey. Thirty percent came from CA, 30% from NY, 28% from PA, and 13% from NJ. The hypotheses and analyses were pre-registered at https://aspredicted.org/3b45b.pdf.

Procedure. The procedure was similar to Experiment 1, but we revised the app design to increase external validity, added another condition, and added another question. We designed the app simulation so that it was almost identical to the COVID Alert PA app (Figure 2). We added one intermediate default presentation condition to isolate whether the effects of defaultless defaults were driven by the opt-out wording or by the appearance (color and position, which mimicked the appearance of typical “continue” options on smartphones). This condition had opt-out wording but the (grey) color and (bottom/left) position of the opt-in presentation condition (Figure S2; Web Appendix). These grey buttons are called “ghost buttons” in computer science (Lucaites et al. 2017). To examine whether the manipulations affected those who would be most likely to download the app, we added a question asking whether participants would download an exposure notification app immediately if given the chance.

After enabling the app (or not), participants completed the demographics in Experiment 1 as well as questions asking if they would download the app and whether they want to follow a link to the App Store or Google Play to download the app.

Results

Overall, 69% of participants enabled the app.

Defaultless Defaults. There was a large effect of defaultless defaults, consistent with Experiment 1 and the Supplemental Experiment, $z = 5.14$, Exp(B) = 1.99, $p < .001$.

Furthermore, the condition that isolated opt-out wording indicated that this wording alone increased the percentage who enabled the app, compared to the opt-in wording (from 56% to 69%, $z = 2.59$, Exp(B) = 1.34, $p = .010$). The percentage who enabled the app was even higher in the opt-out presentation condition that also used the typical color and position of standard "continue" buttons (82% vs. 69%; i.e. top option in blue or right option in bold), $z = 2.94$, Exp(B) = 1.48, $p < .01$, consistent with the idea that opt-out wording and the appearance of the buttons (which may activate habitual tendencies to choose blue options) both increase adoption.

Integration effect. Integrating the three privacy choices into a single screen and decision again increased the percentage who enabled the app ($M_{integrated}$ = 74%; $M_{\text{separated}}$ = 63%, Exp(B) = 1.92, $z = 3.22$ $p < .002$), replicating Experiment 1 and the Supplemental Experiment.

Robustness. We report a series of pre-registered robustness tests in the Web Appendix. The effects were robust among people who were willing to download the app and across subsamples varying in health and several demographics.

Discussion

The effect of defaultless defaults is partly attributable to the opt-out wording and partly attributable to the color and position of the opt-out option. In the real world, these two factors are often combined (as in Experiment 1 and the Supplemental Experiment). Experiment 2 indicates that both elements of this combination influence adoption. In Experiments 3-4, we isolated the color of app buttons from position and examined the effects of color in both a real world field experiment (Experiment 3) and an ecologically valid laboratory experiment (Experiment 4).

Experiment 3: Field Experiment of Button Color

We next sought to examine the effects of color further. In Experiment 3, we examined the impact of color using an A/B test at a large entertainment industry firm. Unlike the previous experiments, this was conducted in a real-world context, which could help determine whether results generalize even among real engaged users, outside of the COVID-19 exposure notification context, and when users are unaware that they are being studied (addressing experimenter demand and other alternative explanations).

Method

The A/B test was designed using a tool called AB Tasty. Upon navigating to the page, users were randomly assigned to the grey button or blue button condition as depicted in Figure 4. The two conditions contained the same job descriptions, information, font size, and identical words, with the color of the key button manipulated (blue vs. grey button that stated "view all details"). The A/B test tool reduced the likelihood that the same person would be counted twice; anyone who returned to the website from the same browser and device would not be counted in the A/B test after the first time.[1] We acquired results from the firm's conversion rate optimization manager, Stanley Zuo, including the number of views in each condition as well as the number of clicks on the "view all details" button by condition.

[1] As in all client-side A/B tests, it is conceivable that the same person could have been counted twice or seen both conditions, if they used a different device or browser. However, given the size of the difference in clickthrough rate (over 9000 more clicks on the blue button), it seems very unlikely that repeats would have produced the result.

Results

Overall, 594,997 users visited the page during the A/B test and 65,512 clicked on the key button “view all details” (clickthrough rate = 11%). Clickthrough rate was much higher in the blue condition than the grey condition, $z = 40.43$, Exp(B) = 1.40, $p < .001$. Specifically, 12.7% of users who saw the screen with a blue button clicked on it, whereas 9.4% of users who saw the screen with a grey button clicked on it.

Discussion

In the context of an important behavior of searching for entertainment career opportunities in the real world, a manipulation that used a blue button rather than grey button greatly increased clickthrough rate. Note that colors might have different associations in different cultures or among people who usually encounter “continue” buttons that are not blue.

Experiment 4: Decomposing Color & Position

In Experiment 4, we manipulated the color and position of buttons that would enable app features (Figure S22). This would further isolate whether (top/right) position or (blue) color (or both) increases adoption. We hypothesized that habit cues associated with certain design features (blue buttons located either on the top or the right) would lead to higher rates of app adoption. We predicted that both color and position would influence adoption. We also measured perceived salience and decision time to determine whether salience or habits (the latter reflected in decision time) account for effect(s) of the manipulations (Wood and Runger, 2015).

Method

Participants. We recruited 1042 participants from Amazon Mechanical Turk in Experiment 4 (51% male; $M_{age} = 36$). We conducted an a priori power analysis (details in Web

Appendix) and recruited only participants who had not participated in the prior experiments. The recruitment, hypotheses and analyses were pre-registered at https://aspredicted.org/s8ee8.pdf.

Procedure. Participants completed Experiment 4 on their smartphones, viewing an exposure notification app interface, as in Experiment 1. We separated the effects of color and position using a 2 (color: blue vs. grey) x 2 (position: right vs. left) between-subjects experimental design. Unlike the previous experiments, participants only viewed one feature choice and we did not vary wording.

Following their choice, participants rated how visually salient each of the two buttons was (1 = *not at all*, 5 = *extremely*; adapted from Mrkva and Van Boven 2020) and reported their habitual button-tapping behaviors. This habits measure, adapted from Verplanken and Orbell (2003), asked participants four items assessing habitual tendencies for four types of buttons ("I tap the [blue/grey] buttons [on the right/on the left]…"). For each, participants reported whether they tap the button frequently, automatically, without thinking, or never when installing an app in everyday life (–2 = *strongly disagree*, 2 = *strongly agree*). We also measured the time they spent on the app enable decision as an unobtrusive measure of how quickly and automatically they were able to choose.

We asked participants to answer questions assessing three factors that drive traditional default effects—"ease," "implied endorsement," and "endowment" (Dinner et al. 2011; Jachimowicz et al. 2019). These items, adapted from Dinner et al. (2011), assessed whether they made their choice because the chosen option was easier to choose, because the app designer appeared to want them to choose that adoption, or because they focused first on what they would give up by choosing the other option (Carmon and Ariely 2000; Johnson et al. 2007). Finally, an attention check asked respondents to select “somewhat disagree” if they were reading the instructions and they reported demographics (gender, age, income, political affiliation, and state).

Results

Overall, 85% of participants enabled the app.

Color effect. Presenting the enable button in blue rather than grey increased the percentage of participants who enabled the app features (M_{blue} = 88% enabled; M_{grey} = 83% enabled, $z = 2.44$, Exp(B) = 1.56, $p = .015$, Figure 5). There was also a main effect of color on self-reported habits. In other words, participants had stronger tendencies to habitually choose blue buttons when installing apps in everyday life ($M = 2.71$, $SD = .86$) compared to habitually choosing grey buttons ($M = 2.53$, $SD = .80$), $t(485) = 4.41$, $b = .18$, $p < .001$. This was consistent with our expectation that blue buttons mimic how habitually-chosen “enable” and “continue” buttons typically appear in many app and online interfaces.

Position effect. There was not a significant effect of position, contrary to our hypothesis (M_{right} = 87% enabled; M_{left} = 84%, $z = 1.57$, Exp(B) = 1.32, $p = .116$). We conducted a post-hoc analysis to examine whether the effect of position was moderated by self-reported habits, given that a large number of participants reported no habitual tendency to choose options on the right rather than the left in everyday life (Web Appendix). There was a Habits × Position interaction, $z = 4.64$, Exp(B) = 2.52, $p < .001$, indicating that right position increased adoption significantly more among people who reported habitually choosing options on the right in everyday life.

What explains the color effect? We considered two possible explanations of why the color manipulation increased the likelihood of enabling the app. First, typically-chosen options to “continue” during app installation and “enable” apps are usually presented in blue on smartphone interfaces, so people could develop habitual tendencies to select blue buttons. Following previous research (Fazio et al. 1986; Lucaites et al. 2017; Wood and Runger, 2015), we suggest that habitual tendencies like this could be unobtrusively captured via decision time—if users take less time to enable an app when the button is blue (versus grey), it likely suggests that the blue button serves as a "habit cue" that activates a tendency to quickly choose the habitual option. Second, increasing the vividness or salience of an option can increase attention towards an option (Borji et al. 2013) and increase choice (Milosavljevic et al. 2012).

The blue color manipulation increased perceived salience of the enable button ($M_{blue\text{-}enable}$ = 4.28, SD = .82) compared to the grey condition ($M_{grey\text{-}enable}$ = 3.75, SD = 1.02), $t(1039) = 9.31$, $b = .56$, $p < .001$, and also reduced decision time (M_{blue} = 12.08, SD = 15.78; M_{grey} = 15.15, SD = 21.82)[2], $t(1040) = –2.58$, $b = –.16$, $p = .010$. Faster decision time was associated with greater likelihood of enabling the app, $z = –2.83$, Exp(B) = .78, $p = .005$, consistent with the "habit cues" explanation. In contrast, decisions of whether to enable the app were not associated with either perceived salience of the blue button nor with the difference between the perceived salience of blue and grey buttons (respectively, $z = –1.00$, Exp(B) = .85, $p = .317$; $z = .87$, Exp(B) = 1.06, $p = .383$).

We tested these two potential mediators (decision time and salience) with a parallel multiple mediation model with 5,000 bootstrapped resamples (Preacher and Hayes 2008). This analysis indicated there was a significant indirect effect of blue color on increased likelihood of enabling the app through reduced decision time (ab = .037, 95% CI [.005, .087]), but no indirect effect of color through perceived salience (ab = .046, 95% CI [–.151, .246]). This is consistent with the idea that habitual tendencies to choose blue options might partially mediate the color effect. It is worth noting that the measures of habits and salience were behavioral and self-report

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[2] Means are raw seconds for ease of interpretation. The analysis used log-transformed decision time, and the effect of color on decision time remained significant when using raw decision time.

measures respectively, so it is possible that method variance or some other difference between the measures rather than the constructs themselves could have accounted for the weaker, non-significant salience effect. Because of this and other limitations of mediation analysis (Fieder, Schott and Meiser 2011), we emphasize our manipulations of potential mechanisms across experiments as the strongest source of mediation evidence.

Addressing alternative explanations: Ease, endorsement, and endowment. We also examined whether the choice architecture in Experiment 4 influenced endorsement, endowment, and ease—three mechanisms which have been shown to account for standard default manipulations that pre-select an option (Dinner et al. 2011; Jachimowicz et al. 2019).

In Experiment 4, the color manipulation did not significantly influence self-reported endorsement, $t(1039) = 1.26$, $b = .14$, $p = .207$, nor did it influence self-reported ease, $t(1039) = 1.05$, $b = .13$, $p = .295$, or endowment, $t(1039) = -1.25$, $b = -.15$, $p = .210$. Similarly, position did not influence perceived endorsement, ease, or endowment (all ps > .50; see Web Appendix). This suggests that the effects of color are not attributable to subjective changes in three processes which largely account for standard default effects. We report mediation models in the Web Appendix, showing no indirect effect through self-reported ease, endorsement, or endowment.

Discussion

In Experiment 4, we manipulated the color and position of app options, which revealed that simply changing the color of the enable button from grey to blue can increase the percentage who enable the app, consistent with Experiment 3. In addition, we found that putting the enable button on the right instead of the left can increase adoption for those who report habitually choosing buttons on the right in everyday life.

Experiment 5: Habits Underlie the Color Effect

In Experiment 5, we manipulated button color and salience orthogonally to experimentally disentangle the relative influence of salience and habit cues on choices to enable app features. Salience, in the literature on perception, is defined as contrast relative to the surrounding field and is defined by well-established algorithms (Itti, Koch and Niebur 1998; Walther and Koch 2006). Using an influential salience algorithm, we manipulated both the color of the button that would enable app features and the app background to isolate blue color from salience (Figure S27). The prior experiments demonstrated that blue buttons facilitate adoption when presented against a plain background, but the color and salience of the button were confounded with one another. In this experiment, manipulating the color of the button and the background independently allowed us to compare button color in two conditions in which the enable buttons were equal and high in salience and two conditions in which the enable buttons were equal and low in salience. We predicted that there would be an effect of color (with blue facilitating adoption) even when it was not salient. That is, we anticipated a main effect of button color (with greater adoption for blue compared to grey buttons) that would not depend on salience.

Method

Participants. We pre-registered a sample of 2000 participants who had not participated in our prior app adoption studies from Amazon Mechanical Turk, acquiring a final sample of 2002 participants (43% male, M_{age} = 39). This sample size was based on an a priori power analysis using the effect size of the color manipulation in the prior experiment. We pre-registered the hypotheses and analyses at https://aspredicted.org/3vu2k.pdf.

Procedure. Participants completed Experiment 5 on their smartphones. Participants imagined they had downloaded a health and fitness app to their phones and that the screens they would view appeared upon opening the app. Participants then made decisions on three screens about enabling features in the app interface, including enabling push notifications, sharing motion activity and location tracking, and being contacted via email and text. Participants' decisions on each of the three screens were recorded. We manipulated button color and salience using a 2 (enable button color: blue vs. grey) x 2 (background color: blue vs. plain) between-subjects design. We used MATLAB's Saliency Toolbox (Walther and Koch 2006), a program that calculates the visual salience of different parts of an image, to ensure that salience was balanced between conditions. The salience algorithm confirmed that the two conditions designed to have a salient enable button (blue against a plain background and grey against a blue background) had buttons with high and similar salience, whereas the two remaining conditions had salience scores that were low and similar to one another (see Figure S27 for screenshots of the conditions and computed salience by condition).

As in Experiment 4, after completing the app installation task, participants then rated the salience of the top and bottom buttons shown as well as the extent to which they habitually select blue (Cronbach's a = .78) or grey (Cronbach's a = .69) buttons during app installation. Participants then rated how often apps typically feature a blue or grey enable button ("When you typically use your phone, if you were to use an app like the one on the previous page, the "enable/continue" button is usually blue [grey]," 1 = *strongly disagree*, 5 = *strongly agree*). This question confirmed that blue is more commonly used for enable/continue buttons than grey ($t(1945) = 40.09$, $p < .001$). To assess whether the ease of tapping specific buttons drove the effects observed here, participants were shown a sample screenshot they viewed during the app installation task and asked to rate how easy it was to select each button ("How easy to tap is the top [bottom] button in the image above?" 1 = *not at all easy*, 5 = *extremely easy*). Finally, as in

Experiment 4, we also measured the time participants spent on the app installation decisions as an unobtrusive measure of how quickly and automatically participants made their selections.

Results

We used a mixed effects model to examine individual decisions of whether to enable a feature with two key fixed effects: button color (tracking whether the button to enable features was blue or grey) and salience (tracking whether the button to enable features was distinct from the background color). The interaction between button color and salience was also included in the model along with the random effect of participant.

As predicted, there was a significant effect of button color on feature adoption. Participants were more likely to enable features when the button was blue than when it was grey (M_{blue} = 74.9% enabled; M_{grey} = 71.3% enabled, $z = 2.81$, $p = .005$). There was also a significant effect of salience, as feature adoption was more likely when the button to enable features contrasted more relative to the background color ($M_{salient}$ = 74.5% enabled; $M_{not\ salient}$ = 71.5% enabled, $z = 2.15$, $p = .031$). The interaction between color and salience was not significant ($z = -.69$, $p = .492$), indicating that the background color (blue or plain) did not influence adoption. Consistent with the results of Experiment 4, a mediation analysis revealed a significant indirect effect of blue color through faster decision time on higher likelihood of enabling app features (Web Appendix). Self-reported ease of tapping the buttons did not mediate the color effect (Web Appendix).

The effects remained similar in size and were significant when only those who passed attention checks were included in analyses (Web Appendix). Additionally, there was an effect of blue button color and no significant effect of the salience manipulation when only participants who reported regularly using a health app on their own phone were included (Web Appendix), suggesting that the blue color effect generalizes even to those who are most likely to use an app like the one used in the experiment in real life.

Habits moderate the color effect. Prior research has demonstrated that habits can shape consumer behavior. For example, consumers who habitually eat popcorn while watching movies ate more popcorn while viewing films compared to other consumers, even when the popcorn was stale (Wood and Neal, 2009). We therefore anticipated that the color effect would be more pronounced for consumers who reported they habitually click on blue buttons. We repeated the analysis examining decisions to enable each feature including participants' habitual clicking on blue buttons as a moderator. As with the original analysis, this model yielded a significant effect of enable button color ($z = 2.84$, $p = .005$). Critically, there was a significant interaction between habit scores and button color ($z = 3.27$, $p = .001$), indicating that the effect of enable button color on app feature adoption was more pronounced for consumers who reported more habitually selecting blue buttons in everyday life (Figure S23). Similar moderation effects emerged when the difference between participants' habits for blue and grey buttons was examined as the moderator rather than habits toward selecting blue buttons (Web Appendix). Overall, these findings demonstrate that people with stronger habitual tendencies to select blue buttons were more influenced by button color, consistent with our habit cues explanation.

Discussion

Experiment 5 manipulated both the color of the button to enable features as well as whether it matched the background color. We demonstrate that blue enable buttons facilitate adoption, even when equating objective visual salience using a salience algorithm (though objective salience may not perfectly align with human ratings of salience). The button salience manipulation also increased adoption of app features, even though the salience manipulation was orthogonal to the button color manipulation, suggesting separate effects of blue color and salience. Consistent with Experiment 4, the color effect was mediated by faster decision times. Moreover, the color effect was especially pronounced for those who habitually select blue buttons, further underscoring a role for habit in producing this effect. Though decision times were faster when the “continue” button was blue, it is unclear whether this is driven by a tendency for blue to cue fast responses, for grey to cue slow responses, or whether both of these partly explain the results. Future research could disentangle these two components or examine colors other than blue and grey. Additionally, note that some potential mediators were measured with self-reports whereas decision time was a behavioral measure. It is therefore possible that the mediation results might be explained by different types of measures (methods variance) rather than the underlying habit construct.

Experiment 6: Why Does Integration Increase Adoption?

In Experiment 6, we experimentally manipulated different components of the integration effect to examine the mechanisms underlying this effect. We considered four potential mechanisms that might explain why participants were more likely to enable app features in the integrated condition compared to the separated condition. First, we hypothesized that simply unpacking privacy features onto separate screens would make people reluctant to enable the features, because unpacking privacy risks could make the individual components seem more prominent or severe, consistent with support theory (Kruger and Evans 2004; Van Boven and Epley 2003). Additionally, it is possible that the separated condition influenced decisions by giving participants additional decision points–extra opportunities to consider what they should choose (Cheema and Soman 2008). In other words, some environments could prompt people to stop and consider a choice in their head at various points in time, even if each person makes only one explicit choice to enable or disable features. Another possibility is that decision noise could account for the integration effect: Random responding or other sources of noise could produce an integration effect because people answering randomly would be more likely to disable a feature when given multiple choices rather than one. Finally, it is possible that integrating several features onto a single screen simply reduces the friction or difficulty of enabling features which might increase adoption. Note that these processes are not mutually exclusive, and our approach enabled us to examine their separate contributions to the integration effect.

Method

Participants. We requested 1220 participants from ROIRocket and 1419 completed the study (48% male, $M_{age} = 49$; ROIRocket often oversamples to account for potential failed attention checks or low-quality respondents). A priori power analyses guided the sample size, and we followed the data collection procedure specified in the pre-registration (https://aspredicted.org/iw89p.pdf).

Procedure. Participants completed the procedure on their smartphones and viewed a health and fitness app similar to Experiment 5. Unlike previous experiments, we sought to demonstrate the mechanisms of the integration effect by adding conditions to isolate different potential components of the effect. These conditions and the factors they decompose are illustrated in Figure 6. The app contained five privacy features that participants could choose to enable (allowing location- and motion-tracking, tracking across multiple apps, running in the background, marketing messages, and posts to social media).

We included the separated (separate screens and choices for each feature) and integrated (1 screen and all-or-nothing choice) conditions similar to previous experiments. To test the idea that unpacking feature information onto separate screens (without changing the choice itself) might reduce willingness to enable these app features, we added an “unpacked 1 decision point” condition which described the five features on separate screens but presented one all-or-nothing choice similar to the “integrated” condition. To address the possibility that separating choices influenced decisions by providing additional decision points, we added a “5 decision points” condition to compare to the aforementioned “unpacked 1 decision point” condition. Participants in this “5 decision points” condition viewed the same feature information screens as in the “unpacked 1 decision point” condition but were encouraged on the information screens to consider what they would decide if asked whether to enable each feature. Like the “integrated” and “unpacked 1 decision point” conditions, they made one choice that was recorded (all-or-nothing decision to enable or disable all features). Finally, if decision noise or random responding fully accounted for the integration effect, the key factor driving decisions to enable features would be whether there was one all-or-nothing choice or 5 separate choices. So, a comparison between the separated (5 choices) condition and the “5 decision points” condition (which involved the same information but one versus five recorded choices) would determine whether decision noise accounted for the integration effect.

In addition to these key experimental conditions, there was a separated opt-in condition to replicate the defaultless defaults effect and examine alternative explanations for that effect. This “opt-in presentation” condition was identical to the separated condition, except that the “continue without enabling” button was presented on top in blue with the “enable” button presented in grey on the bottom (similar to Experiment 1). The experiment thus had five conditions.

After participants chose whether to enable the app features, they answered questions about their experience using the app. First, we measured app feature awareness by asking participants whether the app mentioned each of its five features (they could select “yes,” “no,” or “unsure”). We combined these to form a score from -5 to 5 (+1 point for a correct answer, -1 point for an incorrect answer, 0 points for “unsure”). Next, participants answered five questions about their perceived risk of enabling each feature the app mentioned (1 = *not at all concerned* to 5 = *extremely concerned*), which we summed to create an overall “perceived risk” score (α = .83). Participants also reported how many decision points they remembered having while installing the app (10 options ranging from “1” to “10+”). Finally, they answered one question each about the ease of enabling options and perceived endowment, as well as two questions about implied endorsement (one about endorsement by the app designer, and one about endorsement by others, Cronbach’s a = .65).

Results

Integration effect. Before examining mechanisms of the integration effect, we tested whether there was an integration effect in Experiment 6. There was an integration effect such that 85% in the integration condition enabled the five features compared to 52% in the separated condition, $z = 8.19$, $p < .001$.

Two mechanisms: Unpacking and decision noise account for the integration effect. We examined pairwise comparisons to test the three potential mechanisms underlying the integration effect (Figure 6). There was a significant difference between the integrated and unpacked conditions, indicating that unpacking the information (without adding additional decisions) was sufficient to reduce app feature adoption (85% in the integrated condition vs. 71% in the unpacked condition), $z = -4.01$, $p < .001$. There was not a significant difference between the “unpacked 1 decision point” and “unpacked 5 decision points” conditions, (71% vs. 68%), $z = -.72$, $p = .473$, indicating that the number of decision points did not contribute to the integration effect. Finally, there was a significant difference between the unpacked 5 decision points condition and the separated condition (68% vs. 52%), $z = -3.77$, $p < .001$, consistent with the idea that adding additional explicit decisions partly accounts for the integration effect, perhaps through decision noise. Unpacking information about the features onto separate screens without changing anything about the choice itself decreased adoption (consistent with support theory) and separating the choices themselves rather than presenting one all-or-nothing choice also decreased adoption.

Why does unpacking influence adoption? Our results suggest that unpacking is one mechanism accounting for the effect of integration app feature adoption. Though not the primary purpose of this experiment, it is possible to consider another level deeper, namely what accounts for this unpacking effect on adoption. We considered two possibilities based on previous research: unpacking might increase how concerned people are about risks associated with the app features or it might heighten prominence or people's awareness of individual app features.

To examine these two possibilities, we conducted a mediation model examining two potential mediators in parallel (awareness of the five features and concern about the five features). This was conducted using bootstrapping with 5,000 resamples (Preacher and Hayes 2008). There was a large indirect effect of unpacking on app adoption through higher awareness of app features (ab = -.17, 95% CI [-.29, -.08]), whereas the indirect effect of unpacking through concern about the risks was smaller and non-significant (ab = -.09, 95% CI [-.22, .03]). The significant indirect effect through awareness reflected an effect in which unpacking increased awareness of the features, ($t = 8.64$, $p < .001$) and higher awareness of the features was associated with lower likelihood of enabling all features ($z = -3.63$, $p < .001$).

We also considered alternative explanations, namely whether unpacking influenced perceptions of friction, decision ease, endowment, implied endorsement, or the number of decision points. Unpacking did not appear to influence four of these five mechanisms and there was no support for mediation through any of the five. Specifically, there was not a significant effect of unpacking on perceived friction, $t(1414) = -.49$, $p = .626$, nor on decision ease, $t(1414) = -1.17$, $p = .241$, implied endorsement $t(1414) = .80$, $p = .423$, or endowment $t(1414) = -.71$, $p = .477$. There was an effect of unpacking on perceived number of decision points, $t(1414) = 5.50$, $p < .001$, however a mediation model suggested there was not a significant indirect effect of unpacking through decision points on app adoption (ab = -.02, 95% CI [-.07, .03]).

Defaultless defaults, ease, endorsement, and endowment. There was an "opt-in presentation" and "opt-out presentation" condition, allowing us to replicate that effect and examine three alternative explanations for that effect, namely whether subjective ease, endowment, or implied endorsement might account for that effect. We replicated the defaultless defaultss effect ($z = 8.28$, $p < .001$). The defaultless defaults manipulation did not increase self-reported ease or endorsement, and there was no support for the possibility that ease, endorsement, or endowment accounted for the defaultless defaults effect (Web Appendix).

Discussion

In Experiment 6, we examined mechanisms that could account for the integration effect, in which people are more likely to enable several features when they are presented on one screen with one all-or-nothing choice. Experiment 6 indicates that there are at least two mechanisms through which integration influences app feature adoption. First, simply unpacking information about the app features onto separate screens rather than one screen reduces adoption likely by increasing prominence and awareness of the individual features. Second, giving people several separate explicit choices to enable features (rather than one all-or-nothing choice) also reduces adoption, suggesting that decision noise partly (but not fully) explains the integration effect.

Thus, integration of multiple decisions into one should be done with caution – though it increases uptake of apps, consumers may not realize how many features they are enabling.

General Discussion

Increasing app adoption can have numerous benefits, so it is not surprising that firms spend billions each year seeking this end. We demonstrate that improving the choice architecture of the signup process can reduce barriers to enabling apps, ensuring their appropriate functioning and helping secure benefits for firms. These effects were remarkably robust across experiments, app contexts, and diverse subsamples, including groups who were users of similar apps and across subgroups who varied in age and political ideology. Additionally, in the context of health and fitness apps, their adoption can have benefits for both the individual and society, helping curb the spread of disease in one's community and promote public health.

This research has large practical implications and should be used to inform updates to app design. Our discussions with app developers, user experience teams, and other industry experts indicated that failure to enable app features is a major problem and that the vast majority of these experts' organizations had not used the techniques examined in the present research (Tables S8-S9, Web Appendix). Importantly, the principles that make defaultless defaults, color effects, and integrated choices effective are applicable in a broad range of contexts, with implications not only for benefits to firms but also consumer welfare. Countless apps have critical features such as location tracking and other privacy features that users must enable by making several choices. Other consumer behaviors, such as accepting cookies on websites, initiating subscriptions, or signing up for vaccinations, also involve many steps and choices which could be integrated to increase uptake. The effects of these small changes can influence many types of decisions, with implications for consumer privacy and customer relationship management, as well as regulation.

One important aspect of the manipulations in our experiments is that they did not change the actual default, nor did they change the information provided to decision-makers. All decisions to enable features required an active choice and the features would not be enabled for people who did not select any option. The interventions used integrated choices and defaultless defaults to make enabling the app more likely. The latter intervention was designed to take advantage of the automatic responses people have to cues on their smartphones (Anderson and Wood 2020; Carden and Wood 2018), such as habitual tendencies to select the blue "continue" button whenever it is present. Our findings indicate that presenting enable options in colors typically used for continuing in operating systems (e.g., blue) make people more likely to adopt those features by speeding their decisions. Interestingly, across multiple experiments we document that this color effect is mediated by reductions in decision time but not perceptions of response ease. These habit cues likely operate automatically and speed up responses without conscious awareness that decisions have gotten faster or easier (Wood and Neal, 2009; Wood and Runger, 2015). Habits can increase the fluency with which an action is performed without necessarily altering perceptions of fluency (Wood and Neal, 2009). Moreover, habits can accelerate decisions by decreasing information search and reducing consideration of alternatives (Wood and Neal, 2009), which may not be subjectively experienced as an increase in ease. For these reasons, habits may decrease decision time without altering subjective experiences of ease and indeed, response time is often considered the key measure of habitual responding (Wood and Runger, 2015). Thus, the observed dissociation between perceptions of ease and speeding of responding is consistent with prior research on consumer habits.

The choice architecture interventions examined here likely complement rather than replace advertising and promotions. These different marketing tools can target different stages in the consumer journey (Strong, 1925), with advertising and promotions enhancing awareness, interest, or desire, whereas the choice architecture interventions that we examine focus on influencing action and choices to enable features after downloading the app. Whereas advertising and promotions can alter consumer awareness, knowledge, attitudes, and feelings (Lavidge and Steiner, 1961), choice architecture interventions may be best suited to guiding behavior once consumers are already inclined to take action. Advertising and promotions may increase the number of consumers who enter the conversion funnel while the choice architecture interventions that we examine can help usher them through the funnel successfully. Parsing the stages in the customer journey where consumers are lost can help marketing managers identify the appropriate tools to deploy, and most firms will likely benefit from a mix of advertising, promotions, and choice architecture.

The present findings also contribute to a growing literature examining factors that shape adoption of digital contact tracing apps. Existing work has examined how different digital contact tracing apps balance privacy with effectiveness (e.g., Seto, Challa, and Ware 2021), the relative influence of technical features and individual differences to attitudes about adoption of such technologies (e.g., Li et al. 2021), motivations for adopting such technologies (e.g., Shoji et al. 2021), and barriers to adoption (e.g., Panchal et al. 2021). Here, we demonstrate how an understudied aspect of such apps–their user design–can leverage choice architecture to increase installation and appropriate adoption of key features. However, it is important to note that we document our effects in digital contexts beyond exposure notification apps, and we anticipate that the present findings will generalize to a broad array of digital contexts.

Though our results were robust, there are likely some boundary conditions. These findings might not generalize to samples of people with strong, firmly-rooted preferences

(Lichtenstein and Slovic 2006). Additionally, it is possible that people with very high domain knowledge (about privacy or COVID-19) might be less impacted by choice architecture, which recent research has indicated (Mrkva et al. 2021).

The present results suggest an alternative to de facto defaults, which often elicit resistance for sensitive health and privacy decisions (Chapman et al. 2016; Lehmann et al. 2016). In contrast, more subtle interventions that manipulate color, phrasing, and choice integration would be both effective and easier to implement. However, organizations must be careful when implementing these interventions. For example, Experiment 6 suggests that integration may make consumers less aware of the number of features they enable. These findings also come at a time when practices that shape digital consumer behavior are garnering more interest, particularly from regulatory bodies. For example, the Federal Trade Commission has issued new guidance that subscription sign-ups must be transparent and easy to cancel to address complaints of "sludge." Government agencies in both the United States (Federal Trade Commission 2021) and Europe (Competition and Markets Authority 2022) are documenting cases where digital practices encourage behavior that may harm consumers or make it much more difficult to make one choice than another (e.g., enabling versus disabling cookies). Empirical research has demonstrated that even the worst forms of "dark patterns" may be more harmful and widespread than previously acknowledged (Mathur et al. 2019; Di Geronimo et al. 2020). The present research documents the effectiveness of different choice architecture interventions at guiding digital consumer behavior and clarifies the psychological mechanisms underlying that efficacy. In identifying such interventions, we hope to arm marketers and policymakers with tools for promoting consumer and societal welfare.

Figures

Figure 1. Diagram of a typical smartphone app conversion funnel, showing the total percentage going to each page. Data are from Experiment 1 (using data from the "separated" condition, combining participants in the "opt-in presentation" and "opt-out presentation" conditions).

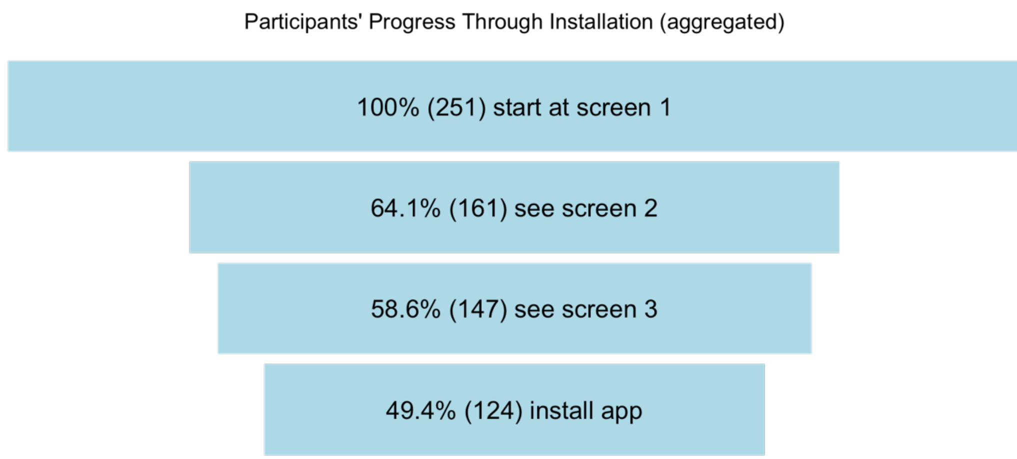

Figure 2. Diagram of the app simulation. The arrows depict the order of screens in the app simulation. The app displayed is very similar to the COVID Alert PA app and was used in Experiment 2 (with a slightly different design in Experiment 1 and the Supplemental Experiment which aligned more closely with a beta version of the Google Apple Exposure Notification interface). The defaultless defaults manipulation is depicted by blue arrows (opt-out) versus yellow arrows (opt-in) and the integration manipulation is depicted by the top row (integrated condition) versus bottom row (separated condition). Some text has been enlarged on this diagram to increase readability.

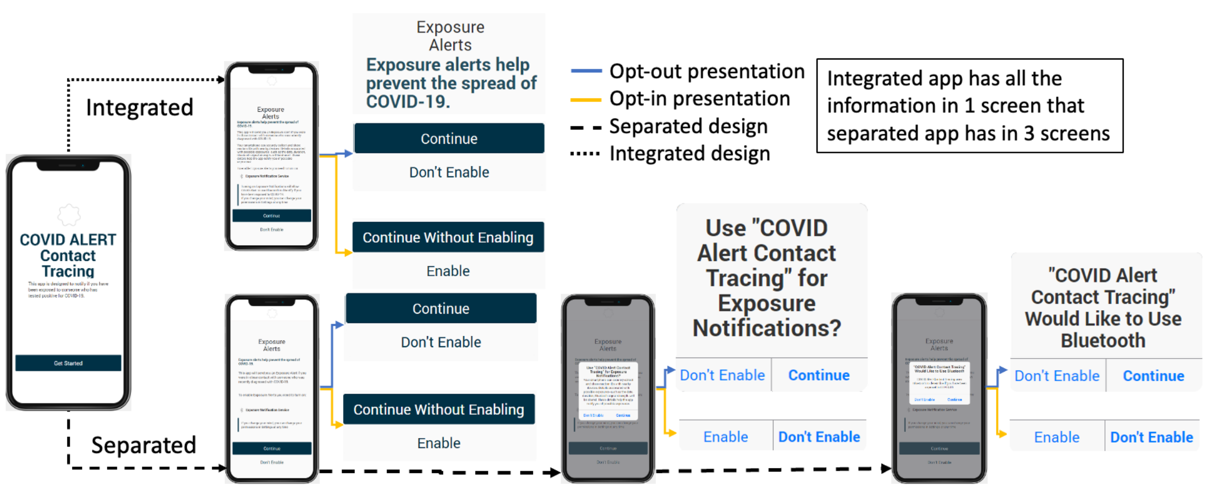

Figure 3. The effects of defaultless defaults (top) and integration (bottom) across all four experiments in which they were tested. Participants were more likely to enable the app (activating all three required features) in the opt-out presentation (top) and when the three decisions were integrated into one (bottom). Error bars depict +/- 1 SE. All results are significant at $p < .05$.

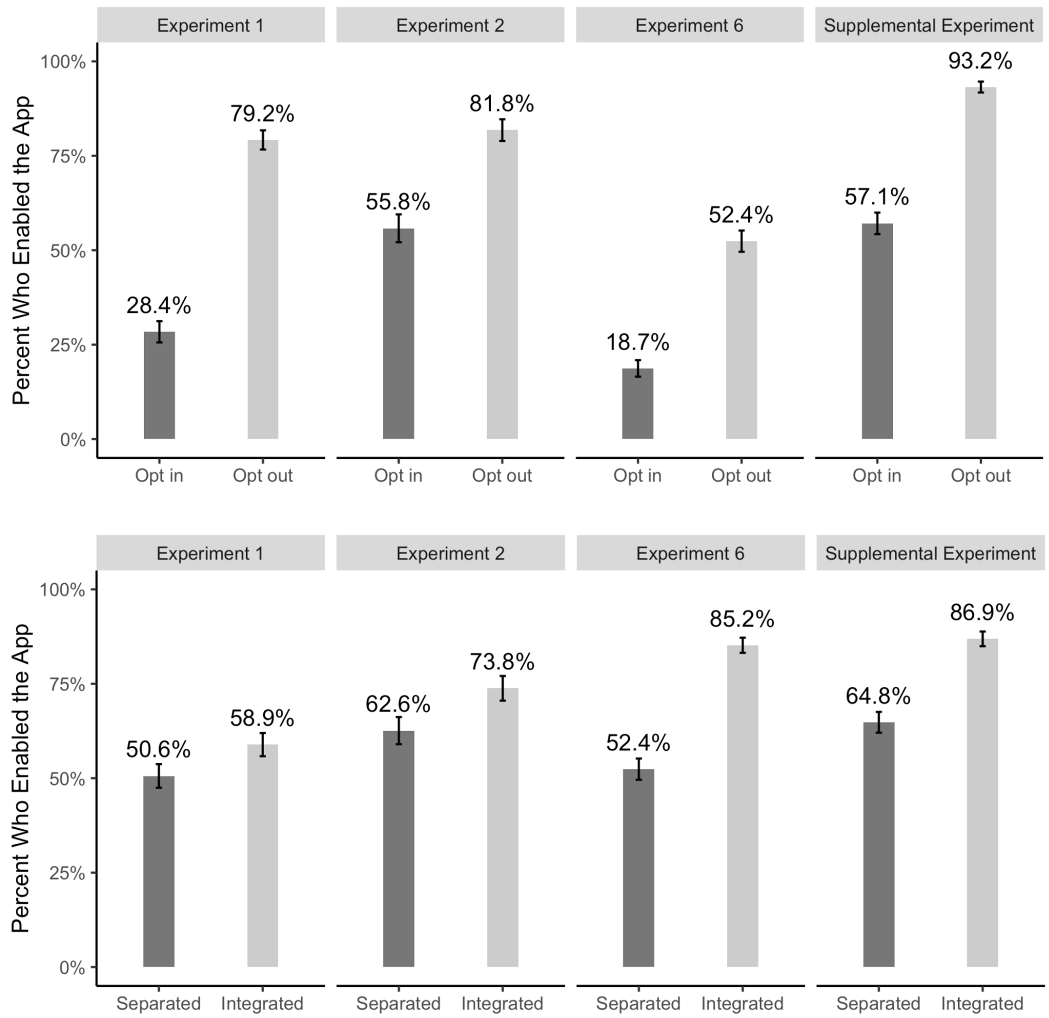

Figure 4. A/B test (field experiment) manipulating the color (grey vs. blue) of a button. Users were randomly assigned to the grey button (left panel) or blue button (right panel) condition.

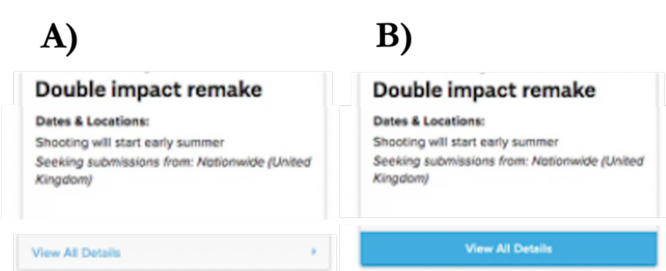

Figure 5. The effect of changing the color of the "continue" button from grey to blue on app installation in Experiment 4. "Button Design" shows the buttons' appearance in each condition.

Condition	Button Design		% Choosing Continue
Continue Option Grey	Don't Enable	Continue	82.9%
Continue Option Blue	Don't Enable	Continue	88.3%
Difference			5.4 pp (6.5%); *p* = .015

Figure 6. Diagram of the conditions designed to test potential mechanisms of the integration effect. Results and the mechanisms each comparison was set up to test are provided in the bottom three rows. The separated condition also contained information about each feature on the corresponding screen similar to the unpacked conditions.

Condition	Integrated condition	Unpacked 1 decision point	Unpacked 5 decision points	Separated opt-out presentation	Separated opt-in presentation
Summary of display	Info about all 5 features Enable features 1-5?	Info about feature 1 Info about feature 2 Info about feature 3 Info about feature 4 Info about feature 5 Enable features 1-5?	On each screen, consider what you would decide if asked whether to enable this feature. (Other than this, format was the same as unpacked 1 decision point.)	Enable feature 1? Enable feature 2? Enable feature 3? Enable feature 4? Enable feature 5?	Enable feature 1? Enable feature 2? Enable feature 3? Enable feature 4? Enable feature 5?
% enabling all 5 features	85%	71%	68%	52%	19%
Mechanism tested		Support theory predicts difference	Decision points predicts difference	Decision noise predicts difference	
Result		M_{diff} = 14%, *p* < .001	M_{diff} = 3%, *p* = .473	M_{diff} = 16%, *p* < .001	

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