The Impact of the Environment on Behavior

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Abstract

This chapter analyzes how the environment affects human behavior by presenting a model that delineates four important pathways addressed through diverse research on inner speech, priming, persuasion, behavioral economics, and public policy. According to this model, the environment can shape human behavior in subtle or forceful ways by (a) framing a decision through the activation of inner questions that produce individualized responses, (b) priming a directional concept that activates goals, (c) introducing propositional information in support of behavioral recommendations (e.g., persuasive communications), and (d) directly changing the environment through policies that mandate or facilitate behavior. We review meta-analytic and experimental research on these psychological influences, address the robustness of two phenomena that had been the subject of contemporary controversies (i.e., priming and default effects), and make recommendations for future research.

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Understanding how the environment affects human behavior has been a goal of psychology since the inception of the discipline. It has also been the focus of several areas of social psychology, from the influence of social norms (Albarracín, 2021; Bicchieri, 2005; Cialdini & Goldstein, 2004; Cialdini & Trost, 1998) to the impact of incidental stimuli on behavior (Bargh, 1997, 2003; Weingarten et al., 2016). Despite this interest, past research has not articulated a general framework of environmental impacts on behavior. Such a framework should incorporate the influence of persuasive communications and interventions to change behavior, while also considering other areas, such as how the environment shapes behavior by evoking inner representations in a more subtle manner. It should also consider advances in behavioral economics, which have generally remained disconnected from the social psychological literature, as well as our knowledge of the influence of public policies on human behavior.

From natural settings (e.g., a prairie) to uniquely human environments that hold government policies and media messages, our surroundings can influence our behavior in subtle or forceful ways. Environmental agents can tell people what to do and physical and symbolic stimuli can both bring to mind a question or response leading to a behavior and constrain behavior directly. At the most subtle level, environmental stimuli may frame questions whose effect on behavior depends on using internal representations to provide answers (Dolcos & Albarracín, 2014; Lohmann, Jones, & Albarracín, 2019) as well as simply prime attitudes, goals, and intentions to execute a behavior (Bargh, 1997, 2000; Dai et al., 2023). For example, walking by a gym may lead people to ponder whether they should exercise more, a question that may in turn activate a commitment to exercise in the future (Senay et al., 2010). Much more forceful influences involve the introduction of propositions in support of a behavioral recommendations

and ultimately policies that channel behaviors (Johnson & Goldstein, 2003;Thaler & Sunstein, 2008) that individuals do not fully control.

This chapter is organized according to four processes that start with an environmental cue evoking a question and end with external mandates that constrain intentions and behaviors. The first part of the chapter introduces these processes. That section is followed by sections on framing of questions, priming of inner concepts, direct behavioral recommendation, and constraining behavior by designing environments that ease or mandate behaviors. The research described in each section has been produced in the last five years and represents advances that link basic processes studied in social psychology with field interventions and policies that can improve with social psychological approaches. The work has been conducted with the utmost adherence to open science practices while also retaining an open mind for social psychological processes criticized both within and outside our field. We believe this integration is unique and look forward to inspiring future work that continues to build this framework.

One complication of our analysis is that research on cognitive and motivational processes often relies on effects of environmental variables to diagnose mental processes. This point has been aptly argued by de Houwer (2011) who has pointed out that observing an effect on behavior does not constitute proof that the mental process one assumes is present. However, as he also pointed out, research on cognitive processes tends to ignore external variables that are essential to any complete psychological understanding. Thus, we rely on process measures as much as possible but are interested in the impact of environmental factors even if not all processes can be isolated within a particular paper.

1. The Environment and Influences on Behavior

This chapter describes recent scholarly advances that gauge and explain the disparate influences of the environment on behavior. We define the *environment* as an individual's surroundings that include physical stimuli, symbols, formal and informal communications, behavioral recommendations, as well as administrative procedures commonly known as *policies*. Physical stimuli and symbols include spatial features, national emblems, and language, each of which may activate inner questions or representations even when they are presented incidentally, in the absence of intent to communicate. Communications include persuasive and didactic messages providing direct recommendations to execute a behavior, and policies include procedures to facilitate or promote behaviors in obvious and direct ways.

Figure 1 depicts the pathways by which the environment may exert an influence on behavior. The pathways, which are not mutually exclusive, go from subtle impacts that merely open the door to a behavior, to insinuating behavior more clearly, to recommending it, to mandating it. Path A illustrates the most subtle environmental influences that affect behavior by framing self-questions, without suggesting that one type of behavior is better than another (Albarracín, 2021; Dolcos & Albarracín, 2014; Lohmann, Jones, & Albarracín, 2019). We present our research investigating how environmental stimuli such as a desert tray at a restaurant may prompt automatic questions such as "Which desert should I choose?", in turn obtaining a habitual behavioral response (Dolcos & Albarracín, 2014; Lohmann, Jones, & Albarracín, 2019). Even though simple stimuli, communications, and policies could in principle activate these inner questions, our research has focused on how simple contextual attributes elicit these inner questions.

Path B concerns directional priming effects on behavior. We define *priming* as the automatic activation of behavior through spontaneous activation of concepts, goals, or motor

representations, often evaluated experimentally through the introduction of a stimulus that acts as a *prime* to behaviors measured subsequently. Whereas Path A simply evokes a question in the observer of a stimulus, priming involves a directional effect because the prime directly activates attitudes and goals in support or opposition of the behavior. We define attitudes as evaluations of an object or a behavior and goals as a desired situation that can mobilize intentions, which comprise a willingness to execute a behavior, and actual behaviors. We review our two recent meta-analyses of the strength and mechanisms of priming, which may be set in motion by environmental stimuli, symbols, persuasive communications, or policies, even though the research on priming has primarily relied on presentation of simple stimuli such as a word or national symbol.

Path C describes environmental inputs that introduce either information in support of behavioral recommendations or direct behavioral recommendations. These communications can influence attitudes and intentions and ultimately shape behavior when recipients encode actionable communication contents. We discuss the impact of information and persuasive communications, focusing on the extent to which explicit behavioral references and multiple-behavioral recommendations increase behavioral impact (Albarracín et al., 2018). We also describe the effect of policies such as government recommendations as well as the impact of policies that meta-communicate support for a behavior. Most of these influences involve the formation of attitudes, goals, and norms on the way to behavior.

Path D describes the process of the environment directly channeling behaviors by reducing the difficulty of executing the behavior or constraining it directly. We review the literature on the default effect within choice architectures, focusing on our work showing that the default effects cease to operate when people have time to deliberate and reach an independent

decision (White et al., 2021). We also discuss the impact of mandates on behavioral intentions and when these influences are direct versus mediated by attitudes and norms.

1.1. The Path of Framing Inner Questions

One of the effects of environments is bringing specific questions to mind without endorsing a particular response. In general, asking ourselves a question has been associated with a higher probability of enacting a behavior than rehearsing an affirmative statement (Godin et al., 2012). Also, questions (vs. statements) presented through external messages as well as self-posed questions have been shown to increase behavioral performance (Müller et al., 2016; Senay et al., 2010; Suri et al., 2014). As an illustration, in a series of experiments we conducted (Senay et al., 2010), participants were instructed to prepare to solve anagrams by spending one minute thinking either *whether* they would work on anagrams (question condition) or simply *that* they would work on anagrams (assertion condition). Immediately following this thinking task, participants worked to solve anagrams. As predicted, the number of solved anagrams was greater for questions than statements. Moreover, according to mediational analyses, questions in these conditions received an affirmative answer because participants had a positive attitude concerning the task. This attitude then influenced a positive intention to work on the task.

One important implication of the process of inner questions is that the environment can trigger these questions, but the person supplies the response. Repeated and relatively mindless behaviors have been estimated to comprise between 30 and 50 percent of our daily behavior. For instance, 48 percent of the annual expenditures of the average U.S. household (e.g., breakfast cereal) involve repeating choices of brand, flavor, and size over a one-year period (Hansen & Singh, 2015; Lohmann, Jones, & Albarracín, 2019; Wood et al., 2002). Although the environment can prompt behaviors automatically, the processes leading to repeating a behavior can be more

complex (Lohmann, Jones, & Albarracín, 2019). For example, while walking through the aisles of the supermarket, the environment can prompt the question, “What should I buy?”, a question that can trigger a covert, mental response that ultimately shapes behavior. In this area, we proposed that instead of automatically reaching for a product because the environment cues the habit directly, repeated choice can emerge from the quick internal conversations we hold with ourselves (Lohmann, Jones, & Albarracín, 2019). This process can still result in choosing the same product again and again, but the repetition is due to our inner question and answer rather than the environment cuing behavior without thought.

It appears that posing similar yet more efficient questions can prompt the formation of intentions in a relatively spontaneous manner. Figure 2 illustrates a spectrum ranging from a self-directed approach that is predominantly top-down (Zell et al., 2012) to fully automatic, stimulus-response processes. Our experiments on inner questions (Lohmann, Jones, & Albarracín, 2019) introduced a default perspective to describe what occurs in the middle of the spectrum from automatic to elaborative processing. According to this viewpoint, when a default option is available, it serves as the most easily accessible response, influencing behavior by providing a ready answer to self-posed questions. When faced with a choice such as what to purchase, a question like "Which one should I choose?" is likely to be answered with a previous product selection that is now highly accessible and leads to repeating the same choice. Nevertheless, this pattern should not be observed when the question is negative or irrelevant. In the instance of asking "Which one should I not choose?" the default response will automatically be "no," reducing rather than increasing the likelihood of choice repetition.

Table 1 presents the results of the experiments introducing questions when participants made free decisions after being trained to choose favorite objects. Unlike Senay et al.’s (2010)

research, whereas we instructed participants to talk to themselves in the form of questions, in Lohmann et al., we studied the processes by which the environment can prompt questions by externally associating a question to a task and using that manipulation as evidence of self-posed questions as a mental process (for the limitations of this approach, see De Houwer, 2011). Specifically, participants first practiced choosing one of several options and then had the opportunity to either repeat these practiced choices or change them when faced with different questions. If, as hypothesized, affirmatively phrased questions can indeed drive behavior repetition, presenting a question like *Which one should I choose?* could facilitate repetition of past behaviors (for how repeated behavior involves self-instructions, see Geeves et al., 2014; Jenkins, 2007; Toner & Moran, 2014). If past repetition has established a response tendency, the prior choice will be likely to come to mind in response to the question *Which one should I choose?* In contrast, a negatively phrased question like *Which one should I not choose?* might lead actors to consider other options and prevent repetition.

Our experiments allowed us to also test if the effects of questions replicate across different stimuli, different questions, and different instructions, as well as whether questions are indeed better than statements. Also, if responses to self-talk questions have automatic features, the effects of the questions should be efficient and observable even under cognitive load. Finally, we studied whether the questions selectively affect stimuli that have been frequently chosen in the past and thus constitute a default response. Through this work, we analyzed self-posed questions as a potential mechanism that drives behavior repetition in situations that are located between the extreme endpoints of the habit-intentionality spectrum (e.g., Lally et al., 2010), where response tendencies exist but are not fully controlled by the environment.

In our first experiment ($N = 43$), we compared the impact of an affirmative question, which was assumed to be naturally self-posed in a choice situation, with a corresponding negative question and with a no-question condition. We developed a four-step choice task, which is shown in Figure 3, designed to train repeated choices. First, as shown on the left, participants saw pictures of three identical doors that were labeled 'left', 'center', and 'right' out of four possible sets of doors distinguished by background color (yellow [shown], blue, orange, and purple). Pressing the 'l', 'c', or 'r' keys selected each door, which, once selected, revealed the picture of an animal, as seen on the right of the figure. In the first phase, participants saw each of the four set of doors 10 times in random order and each door was associated with a particular animal. For example, as shown, the doors with the yellow background had a picture of a dog behind the left door, a picture of a cat in the middle, and a picture of a hamster on the right. One of these pictures was presented for 2000 ms after the corresponding door was selected.

In the second phase of the study, participants indicated which of the three animals they liked best by pressing the appropriate key once for each set of doors. For example, a participant might select the door with the hamster picture behind the right door. In the third phase, which involved training, for each set of doors, participants were instructed to correctly choose the location of their favorite picture over and over again. For instance, whenever participants saw the orange set of doors, the study would not continue until they had chosen the right-hand door leading to the hamster. Each set of doors appeared 30 times, resulting in 120 trials.

In the fourth phase, which involved testing, participants learned that they were free to choose any picture they wanted. However, each set of doors was preceded by a screen, which was presented for 2000 ms and contained (a) a blank image that served as a no-question baseline, (b) the affirmative question "Which one <u>should I</u> choose?", or (c) the negative question "Which

one should I not choose?" (The underlying was present in the original). Participants were first asked to incorporate the questions in their decision making, although this instruction was removed in later experiments because it provides reassurance about the mental process but introduces experimental demand concerns.

Our experimental paradigm was successful in training responses, which were much faster in the last 10 trials than in the first 10 trials of the training phase. Also, introducing questions expedited responses regardless of what question was asked, which is consistent with the findings of Senay and colleagues (2010). However, our main hypothesis concerned choice repetition as a function of the question we presented.

Overall, forty-nine percent of the participants repeated the trained choices in the no-question condition (see Table 1). This rate is above the chance mark of 33 percent but well below 100 percent, thus suggesting a response tendency that is pronounced enough to guide choices while remaining flexible. Moreover, participants repeated their choices more under the affirmative question "Which one should I choose?" than under the no-question baseline (see Table 1).

In Experiment 2, we were interested in replicating the effect obtained in Experiment 1 and ruling out that the effects of questions may appear in response to *any* question, even an irrelevant one. The irrelevant questions resembled the other questions in modality (verbal) and visually (sentence on a screen) but differed in content. The newly introduced irrelevant question was "What should I have for dinner?". Participants ($N = 55$) thus saw the earlier screens plus this irrelevant question screen interspersed before each set of doors presented during the testing phase. As in Experiment 1, participants repeated their choices more often under the affirmative question than under the no-question baseline, $OR = 1.64$ (see Table 1). Moreover, participant

repeated their choices less often in the negative question conditions than in the no-question condition, *OR* = 0.25, and also less frequently in the irrelevant question condition than the no question condition, *OR* = 0.62 (see Table 1). However, as shown in Table 1, repetition in response to the irrelevant question was higher than repetition in response to the negative question.

Experiments 1 and 2 suggested that participants used the questions as a basis for their responses, but they had been asked to rely on the questions for their responses. Thus, even though *Which one should I choose*? is far from being a direct instruction to repeat a choice, it was important to replicate our findings after excluding these directions. Accordingly, in Experiment 3, we explained that the questions were unrelated to the study and were not to be used as directions. We also used pictures of deserts instead of animals, making the stimuli more applicable to important self-control decisions. Second, we changed from using the word "should," which could have denoted obligation or experimental demand, to "Which one do I [not] like?". The irrelevant question used in the study was also different, "What will I do tomorrow?"

Participants (N = 60) in this experiment behaved like the prior ones (see Table 1). Participants were more likely to repeat their choices under the affirmative question than under the no-question baseline, *OR* = 1.27. Also as predicted, participants repeated their choices less after seeing a negative question than after seeing no question, *OR* = 0.47, and also less after seeing an irrelevant question. However, the irrelevant and negative questions were significantly different from which other.

Over the next few experiments, we examined whether questions were superior to statements. During the testing phase of Experiment 4, participants (N = 121) saw either a control

slide with a fixation circle in the middle, an affirmative phrase (“Which one should I choose?” or “Choose this one,” depending on phrase style condition), or a negative phrase (“Which one should I not choose?” or “Don’t choose this one:”). Replicating our previous results, participants repeated their choices more often under the affirmative phrase than under the no-phrase baseline (see Table 1). Although this pattern was present both for questions (“Which one should I choose?”), *OR* = 2.25, and for statements (“Choose this one:”), *OR* = 1.46, it was significantly stronger for questions than statements. Furthermore, participants repeated their choices less often under the negative question (“Which one should I not choose?”) than under the no-phrase baseline, *OR* = 0.44. The same effect was observed for negative statements (“Don’t choose this one:”), *OR* = 0.73, but once again the effect was stronger for questions than for statements, *OR* = 1.68.

Experiment 5 manipulated distraction by having participants listen to a BBC fiction podcast. The rest of the paradigm was identical to Experiment 3, with participants (*N* = 111) seeing pictures of deserts (rather than animals) and the questions "Which one do I like?", "Which one do I not like", "What will I do tomorrow?", or no question at all. As in the prior experiments, the affirmative question was associated with more repetition, *OR* = 1.41, whereas the negative question was associated with less repetition, *OR* = 0.55. The effects, however, were independent of distraction (see Table 1).

Our final study (Experiment 6) was designed to verify that the reason the questions prompt repetition is that they promote activation of a previously trained response. Participants (*N* = 65) were introduced to a desert ordering task like prior tasks. For 40 percent of the trials, participants saw a set of three doors with a colorful background and chose one to order. For the remaining 60 percent of the trials, participants saw a picture of only one desert against the

appropriate background color and had to decide whether to order it. Because the primed questions in the prior experiments alluded to a choice among different deserts rather than a binary, "Yes/No" choice, we hypothesized that, in separate trials, both questions would disrupt repetition because they did not resemble the process that had been rehearsed. In contrast, the joint presentation was identical to the prior conditions and should again show differential effects of affirmative and negative questions. As before, for joint presentation trials, the affirmative question increased choice repetition, albeit marginally, $OR = 1.31$, whereas the negative question decreased choice repetition, albeit marginally (see Table 1). For separate presentation trials, only the negative question decreased choice repetition, $OR = 0.48$. In addition, the separate presentation trials, which allowed us to measure latencies in response to different questions, showed that the positive question made responses faster than the negative one. Thus, although the manipulation of question was external, we were able to verify the speed of retrieval of the response to the question.

All in all, our studies showed that positive self-talk questions led to higher chance of repetition of previously rehearsed behaviors, compared to affirmatively presented statements, negative self-talk questions, or irrelevant questions. Such effects of self-talk questions replicated across different stimuli (e.g., animals vs. deserts), different questions (e.g., *like* vs. *should*), different instructions (e.g., telling participants that the questions are relevant vs. irrelevant), and remained even under distraction. Future research should take the approach of investigating what spontaneous questions emerge from different stimuli, thus gaining a deeper cognitive understanding of this dynamic.

Nevertheless, non-directional self-talk questions are only powerful enough to elicit habitual behaviors and do not have means to activate new behavioral patterns. To introduce or

even recommend a new behavior, the environment needs to activate concepts or provide information that contains directional suggestions on which behavior is superior or should be pursued. For example, the word *morality* may remind people that moral behaviors are superior to immoral behaviors. Similarly, seeing a picture of an Olympic champion may remind people of the idea of achievement and success, leading to intentions to work harder on a task. In the next section, we consider how the environment can prime inner concepts that can in turn activate behavioral goals and promote goal-relevant behaviors.

1.2.The Path of Priming Inner Concepts

Environmental cuing of inner questions is a form of priming that elicits a question and a response. However, priming a concept that suggests a behavior, rather than a question about a behavior, could cue the behavior more directly. Generally, the effects of incidental stimuli on behavior have been established by briefly presenting a stimulus, which operates as an antecedent to a behavioral measure obtained shortly after the stimulus (i.e., the prime) is presented. We conducted two meta-analyses that synthesized the impact of primes on behavioral outcomes, and both found robust behavioral effects in each case. Back in 2016, amidst a period of skepticism about priming due to several failed replications (Doyen et al., 2012; Harris et al., 2013), we conducted our first meta-analysis (Weingarten et al., 2016) of the behavioral outcomes of incidental stimuli and found robust effects of primes on behavior. We (Weingarten et al., 2016) synthesized 352 effect sizes corresponding to the impact of words denoting behaviors (e.g., *run, make, success, achieve*). The words were incidentally presented to participants before measuring a behavioral dependent variable (e.g., anagrams, reaction times, food consumption, and product choices), which was compared with a non-opposite control group such as a word unrelated to the behavior introduced in the experimental group. Effect sizes were obtained from a variety of

reported statistics presented in the original reports, including means, standard deviations, mean confidence intervals, log-odds and log-odds ratios, *t* tests, and *F*-ratios (Decoster & Hall, 2004; Hedges & Olkin, 1985), and each effect size was first coded in terms of Cohen's *g* and later transformed into Hedges' *d* by correcting for small-sample size bias (Hedges, 1981; Hedges & Olkin, 1985).

This first meta-analysis revealed a small effect of introducing a prime on behavior (d = 0.352), which was robust across different methodological procedures (e.g., liminality of the prime, dosage of the prime, and type of control group), different primes (e.g., oriented prime, social prime, and health prime), and different dependent variables (e.g., performance, consumption, and flexibility). Publication analyses, such as the trim-and-fill analysis (Duval & Tweedie, 2000), Egger's regression test (Duval & Tweedie, 2000), and *p*-curve analyses (Simonsohn et al., 2014), revealed some evidence of publication bias but all adjusted estimates remained significant, providing confidence in the impact of the primes on behavior.

In a more recent meta-analysis (Dai et al., 2023), we went beyond those initial findings and compared how different primes affect behavior, disentangling some of the mechanisms of influence based on the conditions under which effects were obtained (using a functional approach, see De Houwer, 2011). We defined *behavioral concepts* as stimuli that denote a behavior or a goal and *nonbehavioral concepts* as stimuli not closely connected to a behavior. For example, Albarracín et al. (2008) introduced the concept *action* using words like *doing, engage,* and *go,* and the concept *inaction* using words like *freeze, pause,* and *stop.* In contrast, Shariff and Norenzayan (2007) evoked *God* with words like *divine* and *spirit,* and Vohs et al. (2006) evoked *money* with words like *dollar* and *salary.* In these examples, whereas *action* and *inaction* have direct behavioral relevance, *God* or *money* do not.

Our recent meta-analysis (Dai et al., 2023) synthesized 351 experiments that also included a prime (a directional prime and a neutral control) and a measure of effects on overt behavior. We found that the effect of the primes on behavior was $d = 0.37$). This result included all replications meeting inclusion criteria and remained after applying the most cutting-edge statistical tools to detect and correct for publication bias (Mathur & VanderWeele, 2020;Stanley & Doucouliagos, 2014; Vevea & Woods, 2005). Furthermore, the robustness of the effect in meta-analyses beyond ours is also high and consistent with our own findings. For example, the effects of money concepts were estimated at $ds = 0.47$ (Dai et al., 2023) and $d = 0.31$ (Lodder et al., 2019a); the effects of religious concepts at $ds = 0.38$ (Dai et al., 2023) and 0.40 (Shariff et al., 2016); and the effects of achievement concepts at $ds = 0.30$ (Dai et al., 2023) and 0.44 (Chen et al., 2020a),

A key question addressed in our most recent meta-analysis (Dai et al., 2023) concerned the extent to which behavioral and nonbehavioral concepts affect behavior and different hypothetical processes of influence. To begin, even though behavioral concepts could be a particularly efficient way of activating behavior, the impact of behavioral ($d = 0.34$) or nonbehavioral ($d = 0.39$) concepts was similar. However, we also had elaborate hypotheses about the possible processes underlying the effects of incidental concept activation in each case.

Two possible psychological processes can explain the link between an incidentally presented stimulus and behavior. The first pathway resembles James' *ideomotor action*, in which an outside stimulus can explicitly make you think of performing a behavior and promote the behavior through those thoughts. However, when the activation of the behavioral concept is automatic, the same notion can be extended to *the direct perception-behavior hypothesis* (Bargh et al., 1996; Chartrand & Bargh, 1999; Mussweiler & Förster, 2000). One good example of the

perception-behavior link is unintentionally mimicking the behaviors of other people (Chartrand & Bargh, 1999). For example, observing others touching their faces can lead to the observers touching their faces as well (Chartrand & Bargh, 1999). However, as proposed by Albarracín (2021), this direct-perception effect should occur primarily with behaviors that were never intentional to begin. For instance, as posture is generally unintentional, stimuli that influence posture could operate without much intentionality. In contrast, stimuli that influence intentional behaviors should continue to activate intentions in more efficient ways, without intentionality ceasing to exist.

Besides mimicry, evoking a concept may also automatically elicit behaviors that were previously associated with that concept. For example, activation of one's sex appears to promote aggressive behaviors for men because masculinity is stereotypically linked to aggression (Mussweiler & Förster, 2000). Likewise, *inaction* through words like *still* and *rest* can trigger inhibitory control (Hepler & Albarracín, 2013). Overall, this type of effect can occur for behaviors that are not completely under intentional control but have evolutionary benefits. Accordingly, mimicry can facilitate smooth interactions (Chartrand & Bargh, 1999); aggression can secure resources (Daly et al., 2001); and inhibitory control helps humans avoid risky behaviors (Luijten et al., 2014). Thus, behaviors of this kind may be unintentionally activated in response to a prime.

When behaviors are intentional, however, a behavioral concept should activate a goal and intention to execute the behavior. For example, two experiments conducted by Bargh et al. (2001) included subliminal primes to activate different goals, namely performance goals and cooperation goals. Suggesting a performance goal led participants to perform better on a

subsequent intelligence test, whereas suggesting a cooperation goal led participants to replenish supplies shared with others who were also completing the task.

Several theories suggest that if a prime activates a goal, it should do so by affecting the attitude toward the behavior in question (Fishbein & Icek, 2011). For example, according to expectancy-value models (Fishbein & Ajzen, 1975; Rosenberg, 1960), attributes or outcomes that are highly valued promote intentions and ultimately behaviors as well. In addition, according to goal theory (Locke & Latham, 1990), goals that have higher value or that are evaluated more positively should instill greater behavioral pursuit than goals without value (Förster et al., 2005;Locke & Latham, 2002).

Even though valuing a goal should lead people to act along its lines, this effect should be present particularly for behaviors that are pursued intentionally. For example, Förster et al. (2005) conducted an experiment in which participants received either $1 or $0.05 to find a target in a search task. The study showed that while the goal was active, concepts related to the goal were more accessible in the higher value condition than the lower value one. In contrast, when behaviors are automatic and when the primes are not behavioral, we expected goal value to matter less. The concept *dollar* or *salary* may suggest an umbrella of different concepts that drive behavior independently of value and goal activation. It might also increase confidence or activate suspicion of others without activating the goal to be selfish or independent. As these alternate effects are not likely to be goal mediated, manipulating the value of the behavior should be inconsequential.

Figure 1 shows how concepts can affect behavior by activating attitudes, goals, and intentions, which should apply to cases in which behaviors are initiated intentionally. Our meta-analysis classified studies that introduced behavioral concepts if (a) they directly referred to a

behavior or a goal (e.g., to run, to win, and to be fast), or (b) they referred to a trait or a value that provided directional guidance for the following task (e.g., evoking equality and measuring inequality of profits during a game, Ganegoda et al., 2016). In contrast to studies with behavioral concepts, other studies introduced concepts that were nonbehavioral when (a) they represented people or stereotypes of groups of people (e.g., athletes, professors, and elderly people), (b) they alluded to a trait or a value that did not provide directional guidance for the following task (e.g., evoking cuteness and measuring indulgence choice, Scott & Nenkov, 2016), (c) they represented an institution, object or entity connected to a value without clear directional implications for the following task (e.g., God and the national flag), or (d) they involved a common object without directional implications for the following task (e.g., money, food, and cigarettes). These nonbehavioral concepts may have incidental effects on behaviors by evoking confidence or other feelings. However, we did not expect nonbehavioral concepts to implicate attitudes or goals, which, as explained were indirectly inferred (see De Houwer, 2011).

In addition to coding the behavioral and nonbehavioral content of the primes, we also separated goal value into three categories, (a) no manipulation, (b) higher goal value (e.g., offering a greater monetary reward, or preselecting participants who value the goal), or (c) lower goal value (e.g., preselecting participants who do not value the goal). For instance, in Hart and Albarracín's (2009) studies, the low-achievement motivation group was coded as lower in goal value because it had lower achievement motivation. In turn, the high-achievement motivation group was coded as higher in goal value because it had higher achievement motivation. Another example is Seitchik and Harkins' (2014) research, in which goal value was heightened by telling participants that their performance would be evaluated by others (vs. no manipulation).

As in the first meta-analysis, we calculated effect sizes by estimating a *d* for the effects of concepts on behavioral measures such as task performance or amount of money donated, instead of intentions or other self-report measures. Additionally, outcome measures could not be measures of the accessibility of the prime (e.g., an IAT) even though such measures are performance-based. For example, the flag studies reported in (Klein et al., 2014) were excluded because their dependent variable was a political attitude rather than a behavior. When it was unclear whether a measure represented accessibility of a concept or enacted behavior, the research team discussed it to reach consensus. The effect size *d* was calculated by comparing the behavioral measure following the prime with the behavioral measure in a neutral control condition.

We then analyzed if behavioral concepts produced effects suggesting greater goal activation, whereas nonbehavioral concepts produce effects that did not suggest the involvement of value. Table 2 shows the weighted average *d* for the effects of the primes on behavior. We found an interaction between prime type and goal value indicating that goal value moderated the effect of behavioral concepts more than the effect of nonbehavioral concepts. As shown in Table 2, the effect of behavioral primes was stronger when value was higher or when it was lower or not manipulated. In contrast, when the primes were nonbehavioral, goal value had no significant effect. These effects persisted after controlling for pre-registration status, inclusion of covariates, and exclusion of participants during data analyses, all of which has been identified as *p*-hacking practices (Wicherts et al., 2016).

Another factor that affects behavioral pursuits is goal expectancy, defined as the perceived difficulty of achieving a goal (Förster et al., 2005; Locke & Latham, 2002). The central question related to goal expectancy is whether more difficult goals increase or decrease goal

striving. On the one hand, difficult tasks may strengthen goal pursuit motivation (Heath et al., 1999; Locke & Latham, 1990; Stajkovic et al., 2006). On the other hand, both expectancy value models (Fishbein & Ajzen, 1975; Rosenberg, 1960) and goal theory (Locke & Latham, 1990) propose that higher expectancy, which is the perceived ease of a goal, enhances goal pursuit (Förster et al., 2005; Locke & Latham, 2002; Wood et al., 1987). Thus, we also coded goal expectancy into three categories, (a) higher goal expectancy (i.e., lowering the difficulty of the task), (b) no manipulation, or (c) lower goal expectancy (i.e., increasing the difficulty of the task). The manipulation of goal expectancy often involves changes to either the objective difficulty of the task or participants' subjective perceptions of task difficulty. Stajkovic et al. (2006) manipulated the objective difficulty of the task by asking participants to list 4 or 12 uses of a wire coat hanger. Also, goal expectancy can be manipulated as a subjective perception, like self-efficacy (Bandura, 1986). Capa et al. (2011) manipulated participants' subjective perceptions of goal attainability by evoking the goal of studying with the presence or absence of positive words. However, neither of our meta-analyses of priming (Dai et al., 2023; Weingarten et al., 2016) found an effect goal expectancy on behavior. Instead, the effect on behavior was similar effects regardless of whether the behaviors were easier and harder for participants.

Lastly, goal theories also suggest that if a concept activates a goal, an opportunity to satisfy that goal will lower goal pursuit and thus reduce the effect of introducing a prime (Bargh et al., 2001; Cesario et al., 2006; Förster et al., 2005). For example, Albarracín et al. (2008) presented participants with action or inaction concepts and then measured the number of thoughts they generated about a text. In between, participants were randomly assigend to complete an active task (i.e., doodling) or an inactive task (i.e., resting). The study showed that participants presented with an action goal were more active in the thought-generation task, but

only when they lacked an opportunity to satisfy their action goal. Similarly, participants presented with an inaction goal were more inactive in the thought-generation task, but only when they had no prior opportunity to satisfy the inaction goal. This goal satisfaction hypothesis was examined in our meta-analyses.

In the two meta-analyses we conducted (Dai et al., 2023; Weingarten et al., 2016), we coded opportunities for goal satisfaction into (a) no delay (i.e., no filler task between the prime and the behavioral measurement), (b) delay without satisfaction opportunity (i.e., inclusion of a filler task that was not relevant to the goal associated with the prime), or (c) delay with satisfaction opportunity (i.e., inclusion of filler task that was relevant to the goal associated with the prime). For example, the research by Van Tongeren et al. (2018) was coded as involving a delay without satisfaction opportunity because it introduced *superhero* and then included an irrelevant personality scale as the filler task between the prime and the measure of helping behavior. The research by Lowery et al., (2007) was coded as involving a delay with satisfaction because it introduced the concept of intelligence and then gave participants a practice exam before an actual statistics exam that constituted the dependent measure. The first meta-analysis showed an effect of this variable such that the effect was smaller when there was a delay and an opportunity for goal satisfaction between the prime and the dependent measure, compared to when there was no delay or when the filler task did not provide opportunity for goal satisfaction. However, in our second meta-analysis, this effect was not present for either behavioral or nonbehavioral primes.

In summary, our two meta-analyses (Dai et al., 2023;Weingarten et al., 2016b), along with several other meta-analyses in the field (Chen et al., 2020b; DeCoster & Claypool, 2004; Lodder et al., 2019b; Shariff & Norenzayan, 2007b; Van den Bussche et al., 2009), found robust behavioral

effects across different methodological settings, different types of primes, and different behavioral outcomes. Publication bias, albeit present, is insufficient to explain the entire effect of presenting a prime. Moreover, our work also showed that primes about behaviors activated behaviors that were valuable, implying that the behavioral concept elicited a goal. However, what happens for nonbehavioral concepts is less clear and may involve activation of fully automatic behaviors and eliciting affective reactions that in turn affect behavior. Unfortunately, given the nature of our meta-analysis, we were unable to fully disaggregate the heterogeneous effects of concepts such as flag, divine, or salary. Understanding these effects remains a clear research priority for the next decade, as does accumulation of studies in which the activation of mental constructs is directly verified when possible.

Up to this point, we have considered relatively subtle influences of the environment on behavior. In one case, the environment evokes a question, thus framing a problem that the individual solves in an idiosyncratic way. In the second case, the environment makes a directional suggestion, which is still subtle but more directly connected to a particular behavior. In addition to these processes, however, environmental influences can be much more obvious influences, as is the case of behavioral recommendations, many of them introduced through public and interpersonal communication. In the next section, we discuss how these behavioral recommendations may shape our behaviors, mostly through changing attitudes, increasing intentions, forming norms, and activating goals.

1.3. The Path of Behavioral Recommendations

Many communications presented in our environment contain behavioral recommendations. Some communications present facts, designed to explain a particular situation such as the number of cases of a new infectious disease and its symptoms. Others are persuasive,

but the goals of persuasive communications also vary. Religious ministers might wish to strengthen their audiences' belief in the afterlife, and a politician might falsely claim that an election was stolen to increase their own popularity. However, other communications and all behavioral interventions aim to change behavior by introducing information to change attitudes or perceptions of normative support and making appeals for an audience to execute specific recommendations. Our work in the last decade has specifically analyzed the impact of such recommendations. Before going into that, we discuss the impact of communications to change behaviors on beliefs about the outcomes and antecedents of a behavior.

1.3.1. Behavioral Recommendations

The processes that underlie the impact of behavior-relevant communications were studied by Albarracín and Wyer (Albarracín, 2002; Albarracín, 2021; Albarracín & Wyer Jr, 2001), who investigated the behavioral impact of messages advocating support for comprehensive exams in an upcoming university referendum. When participants were not distracted by external noise, they first formed beliefs in and evaluations of the outcomes of the voting in support of the policy. They then integrated the implications of these outcome beliefs and evaluations into their attitudes toward the behavior. When participants were distracted, however, they formed attitudes that were also based on the mood they experienced at the time and then rationalized these attitudes by adjusting their outcome beliefs and evaluations.

The actual impact of behavioral recommendations was only investigated more recently. To begin, informational messages are more effective when they describe behavioral outcomes. In a systematic review of the effects of interventions on behavior, we analyzed the impact of interventions with and without specific behavioral recommendations (Albarracín, Fayaz-Farkhad, and Granados-Samayoa, 2023). Interventions targeting a broad belief such as a mindset

were 9 percent more likely to change behavior than their control counterparts. In contrast, interventions recommending a specific behavior were on average 33 percent more successful than control conditions. Further, interventions teaching specific skills to achieve that behavior were 242 percent more likely to promote the recommended behavior, highlighting the impact of explicit behavioral recommendations.

Whether merely discussing a behavior affects the impact of a message on behavior and the processes that might underly such an effect has been investigated recently. Persuasive messages often contain statements that imply a positive evaluation of an object. However, many contain allusions to behavior, by either recommending a behavior or describing a behavior performed by the communicator. As an illustration, a message that states *I think COVID-19 vaccine is effective* is different from a message that states *I think everyone should get the COVID-19 vaccine*. The behavioral allusions may impact behavioral intentions directly or may persuade message recipients that the behavior is worth performing or common among their peers. In a series of preregistered experiments where participants were exposed to either attitudinal or behavioral messages that either advocated in favor of or in opposition of a product (Zhou et al., 2023), we found that the behavioral messages were more persuasive. For example, messages such as “I will not take this ‘bivalent’ vaccine” were more likely to induce stronger intentions to self-test for and vaccinate against COVID-19 than messages such as “Time to move forward and forget about COVID-19.” Further analyses indicated that behavioral messages were more successful at changing attitudes, norms, and intentions because recipients developed a clear mental representation of the behavior described in the message.

Government behavioral recommendations can have similar effects on behavioral intentions and ultimately behavior, by, for example, communicating that the behavior has social

approval (Sunstein, 1996). For instance, the repeal of vaccination exemptions and recommendations of vaccines by government officials can strengthen attitudes, norms, and intentions in support of vaccination (Fayaz-Farkhad et al., 2023).

In a series of experiments, we (Fayaz-Farkhad et al., 2023) studied associations between vaccine policies and pro-vaccination norms and whether policies have a causal impact on attitudes and norms. In a probability phone/Internet panel survey, we correlated the norms reported by respondents to the policies of their state of residence. We recorded state vaccination funding and availability of vaccination exemptions in each state with the hypothesis that more funding and fewer exemptions would be perceived as a positive vaccination norm. We found that higher levels of state funding allocated to the immunization program and the repeal of vaccination exemptions for non-medical reasons correlated positively with social norms, as expected.

In a set of subsequent experiments, we established causality by presenting information about pro-vaccination policies and measuring norms, intentions, and perceived benefits of vaccination. In Study 2, participants imagined moving to a new city whose government recommended healthy children to vaccinate against COVID-19 or recommended *against* healthy children vaccinating against the disease. Studies 3, 4, and 5 included a situation of moving to a new city in which either the city increased or decreased the funding allocated to its immunization program. Studies 4 and 5 also included a control condition in which the city's level of funding for the immunization program is stable.

Study 2 assessed the impact of recommending or not recommending COVID-19 vaccination for healthy children on perceived social norms and vaccination intentions. Participants considered the situation of moving to a city that has implemented a policy that either

recommends healthy children to vaccinate or not vaccinate against COVID-19. Participants who considered a city recommending vaccination reported stronger norms and intentions to vaccinate than participants in the anti-vaccination policy condition. In addition, we had a set of control measures about policies concerning using plastic bags, which should not be affected by the experimental manipulation. Accordingly, reported norms and intentions concerning plastic bags were unrelated to the manipulation.

1.3.2. Multiple Behavioral Recommendations

The research we reviewed up to this point involves single behavioral recommendations. However, in many cases, a single behavioral recommendation is insufficient. Human behaviors are often interrelated, forming clusters of behavioral risks (Borrell-Carrió et al., 2004). For example, unsafe sexual practices and substance use affect each other and both co-contribute to higher HIV transmission rates (Kalichman et al., 2007; Robinson et al., 2016). Consequently, cutting-edge intervention approaches often target multiple behaviors by including messages that make multiple behavioral recommendations to recipients. Our research has also examined the impact of such joint recommendations on intentions, behaviors, and clinical outcomes.

In two laboratory experiments (McDonald, McDonald, Hughes, Albarracín, & Albarracín, 2017), we studied the effects of the number of health recommendations (e.g., quit smoking; relax for a day) contained in a health-promotion message on recommendation recall and intentions to follow the recommendations. We predicted that if recommendations are stored in memory as individual bits of information, a higher number of presented recommendations should increase the number of recalled recommendations. Beyond a point, however, recipients should synthesize a group of recommendations into a single, more general recommendation (i.e., a theme or header), in turn decreasing the proportion of recalled recommendations. We found that, as the

number of recommendations rose, the number of recalled recommendations increased but the proportion of recalled recommendations decreased.

In Experiment 2 of the same series, we studied the effects of the number of recommendations on intended behaviors. We found that the number of intended behaviors increased up to a point, thus showing an increase in number of intended behaviors with a corresponding decrease in the proportion of intended behaviors. Thus, when behaviors are interchangeable, such as when the same benefit can be derived from any of ten types of exercise, recommending all ten is likely to maximize the benefit of the recommendations. In contrast, when behaviors form a set that operates jointly, a lower number of behaviors should maximize performing the most complete possible set. For example, some medications are prescribed as a combination such as taking a drug that depletes a vitamin along with a vitamin supplement. Hence, in these situations, a lower number of intervention recommendations would be most beneficial.

A series of meta-analyses from our lab has tested the effects of multiple-behavioral recommendations by examining the efficacy of multi-behavior interventions. These meta-analyses selected randomized controlled trials with multiple recommendations, such as both quitting smoking and increasing consumption of fruits and vegetables, stopping consumption of both marihuana and cocaine, or both testing for HIV and initiating PrEP (Pre-Exposure Prophylaxis). We counted the number of recommendations received by participants in the trials and obtained a *d* reflecting improvements in behavior and/or clinical outcomes across different conditions. The results from these meta-analysis (Dai et al., 2019; Sunderrajan et al., 2021; Wilson et al., 2015; Zhang et al., 2023) appear in Table 3, which presents either the amount of change observed in different conditions (i.e., multi-behavior vs. single-behavior interventions),

or the linear term which represents the effect of number of recommendations on behavioral changes. As shown, interventions that contained multiple behavioral recommendations (vs. single behavioral recommendation) elicited larger behavioral and clinical changes in each of the domains we examined. Also, there was always a positive linear relation between the number of recommendations and behavioral and clinical changes obtained by an intervention.

In addition to the patterns explored in the experiments (McDonald, McDonald, Hughes, Albarracín, & Albarracín, 2017) and the meta-analyses in Table 3, we have investigated the extent to which the execution of one recommendation facilitates other behaviors. This was the research question in a recent meta-analysis of 803 independent samples from 364 research reports examining the association between the number of behavioral recommendations in multiple-behavior interventions and behavioral and clinical change across eight domains (i.e., diet, smoking, exercise, HIV prevention, HIV testing, HIV treatment, alcohol use, and substance use; Zhang et al., 2023). As shown in Table 3, the number of behavioral recommendations produced a positive, linear effect on behavioral and clinical change. This means that, within the range of recommendations typically included, recommending more behaviors makes each behavior change more likely than recommending fewer behaviors.

In this comprehensive meta-analysis (Zhang et al., 2023), however, we also studied the possibility that behavioral cuing might facilitate the impact of behavioral recommendations after these recommendations are introduced. According to classical conditioning (Pavlov, 1927) and priming (Bargh et al., 1996; Dijksterhuis et al., 2000), a stimulus can provide automatic cues for behavior. Also, habitual behaviors, defined as behaviors that occur with a high level of automaticity are tied to consistent environmental cues (Verplanken & Orbell, 2003;Wood & Neal, 2007). After all, individuals report going to the bathroom to brush their teeth with no thought

once they start their bedtime routine (McCloskey & Johnson, 2019). In the case of behavioral cuing, we proposed that one behavior may activate others through similar automatic processes. For example, if one is asked to both increase vegetable consumption and exercise, performing the two behaviors after the recommendations may increase the probability of one behavior being activated when the other is. Such a mechanism would potentiate the changes produced by multiple interventions.

We (Zhang et al., 2023) conducted analyses to determine if behavioral cuing explained the positive effects of making more behavioral recommendations. Specifically, we gauged if changes in smoking were associated with changes in diet and exercise, for example. We found that changes in HIV behaviors, as well as changes in alcohol and other substance use, were associated with changes in other behaviors, whereas changes in smoking and exercise were not. Presumably, HIV, alcohol use, and substance use behaviors co-occur in ways that ensure co-habituation of these behaviors and potentiation of changes when one behavior changes.

In sum, behavioral recommendations and policies can promote performance of a behavior by influencing attitudes, norms, intentions, and ultimately behavior. However, when multiple behaviors are at stake, it appears that more automatic mechanisms supplement the effects of single behavioral recommendations on outcomes. If a behavior is executed following a recommendation, other behaviors are also executed. These behaviors are then mutually reinforcing and produce average increases in the impact of the intervention. In the future, this area of research will be best served by a more direct observation of processes that remain difficult to be observed directly, particularly in the context of intervention work.

Besides subtle mechanisms such as framing a question, evoking a concept, or making a recommendation, governments and institutions often implement stronger interventions. These

include policies that preselect a behavioral option (i.e., default), reduce the difficulty of a behavior (e.g., provide more opportunities for vaccine), or directly mandate a behavior (e.g., requiring taking COVID-19 vaccines). In our last section, we address this strongest pathway of environmental influence on behavior.

1.4. The Path of Direct Behavioral Shaping

In addition to framing questions, priming behaviors in a more directional fashion, and making behavioral recommendations, environments can be designed to channel behaviors and behavior may be mandated by governments and institutions. Some of these policies operate by shaping norms and attitudes, as is the case with the government recommendations reviewed before in this chapter. For example, in some of our recent experiments on the effects of the normative effects of policies (Fayaz-Farkhad et al., 2023), we investigated the impact of increasing funding to support a behavior. In this series, Study 3 included a manipulation of a different policy, namely increasing or decreasing the percentage of city funding allocated to the immunization program. As with the recommendation of vaccines for children, participants in the increased funding condition had stronger intentions and norms to receive booster shots than participants in the decreased funding condition. In addition, Study 4 replicated these findings but also included a condition with no change in funding and Study 5 measured perceived benefits of vaccination and perceived obligation to vaccinate. In both cases, information about increased funding led to stronger intentions to vaccinate and stronger norms, and norms mediated the effects of the policy on intentions to receive COVID-19 boosters. Furthermore, Study 5 also showed that perceived benefits and perceived obligation mediated the effects of the policies on boosting intentions. Thus, increased access to behavioral opportunities such as more vaccination

sites can certainly increase behavioral performance not only by changing attitudes and norms but also more directly, by making the behavior more feasible.

In this section of the chapter, we discuss two approaches to changing behavior by changing the environment. The first one is default architectures that preselect decisions to promote a behavior. For example, donating organs may be the default, requiring individuals to change from donating to not donating if they are to avoid the default. The second one concerns direct mandates in which the behavior is required even if actors see little benefit in performing it. These cases are discussed in the coming sections as environmental triggers that can guide behavior directly, even though, as discussed before, the presence of the policy can also act as an implicit behavioral recommendation and influence attitudes, norms, and intentions through that mechanism.

1.4.1. Channeling Behavior

Making a desired behavior the default option is an easy way to "channel" decision makers (Thaler & Sunstein, 2008) to do the right thing in domains from organ donation (Johnson & Goldstein, 2003), to pension-plan choices (Madrian & Shea, 2001), to car insurance (Johnson et al., 1993), and to taxi tips (Haggag & Paci, 2014). In the seminal research, participants were asked whether they would be organ donors if they were to move to a new state (Johnson & Goldstein, 2003). The response formats had prechecked either donating or not donating, which were compared with a neutral format that required an active choice with both the donating and not donating options unchecked (Johnson & Goldstein, 2003). In this study, 82 percent of the sample chose to donate when the default was to donate, and 79 percent chose to donate in the neutral condition of active choice. However, only 42 percent chose to donate when the default was set to not donate. In another context, 86 percent of new employees signed up for a retirement plan that

was set as the default, whereas only 50 percent selected the plan when it was not preselected (Madrian & Shea, 2001). Based on these findings, choice architectures have been touted as a promising way of using the environment to shape behavior.

Despite promising seminal findings, a meta-analysis by Jachimowicz et al. (2019) yielded an overall positive effect for defaults ($d = 0.68$) but also negative, null, and positive effects ranging from –0.5 to 2 across studies consistent with sizable unexplained heterogeneity (over 90 percent after accounting for all moderators). Similarly, in recent policy studies, donation defaults have failed to increase donation (Parsons & Moorlock, 2020), and HPV (Human Papilloma Virus) and vaccination defaults have either had no effect or increased reluctance to vaccinate (Reiter et al., 2012).

We have recently proposed that action defaults, such as the default to donate or provide a tip, are successful primarily in contexts that prevent deliberation (for theories about the effects of such, see (Petty & Cacioppo, 1986), such as when people have limited time to make a decision and the default supplies an alternative. Even though default effects had been conceptualized as expediting decision making (e.g., ease or effort Johnson & Goldstein, 2003; Dinner et al., 2011), surprisingly little prior research had investigated the effect of decision time on the impact of defaults. Whereas leaving a cab in a rush should reduce passengers' ability to override the default, deciding whether to vaccinate a child is likely to recruit more deliberation about the benefits of vaccination rather than defaulting to vaccination.

The hypothesis that decision time is important to the decision-making process is common across multiple research areas. For instance, people who encounter decisions with too many options experience decision overload when they have limited time to decide (Chernev et al., 2012; McShane & Böckenholt, 2018). Similarly, people who have limited time to examine

information and nonetheless decide, are more likely to use heuristics (Dhar & Nowlis, 1999; Weenig & Maarleveld, 2002; Wright, 1974; for a review, see Ariely & Zakay, 2001). For example, people who lack decision ability frequently rely on such heuristics as using price as indicator of product quality (Suri & Monroe, 2003) or basing a judgment on the mood they experience for unrelated reasons (Schwarz & Clore, 1983, 2004). Considering these findings, the decision time allowed by the situation should be important for default options as well, particularly because the default provides a decision when people fail to make up their mind.

Indeed, past scholarship on defaults has proposed that decision time is likely to moderate the effect of choice architectures (Johnson & Goldstein, 2003). Yet, Jachimowicz et al. (2019), who meta-analytically examined whether the difficulty of making a decision increases default effects, found no effect ($b = –0.05$, $SE = 0.15$, $p = .75$). Moreover, to the best of our knowledge, the only two studies that had directly examined the impact of time on the influence of defaults found null effects. First, in a study of the use of defaults in the selection of light bulbs, participants who took less time were as affected by defaults as those who took more time (Experiment 2; Dinner et al., 2011). Second, in a study of the use of defaults in the selection of hotel amenities, an experimental manipulation of decision time had no effect on default endorsement (Steffel et al., 2016).

As we embarked on our research, however, we thought that the null effects in the Dinner et al., 2011) and Steffel et al.'s (2016) experiments could have been due to improper calibration of the time required to make decisions. In the Dinner et al.'s study, participants' decisions were slow and quite variable, with fast decision makers taking approximately 20 seconds and slow ones taking approximately one minute. In the Steffel et al. (2016)'s study, participants either did or did not receive a recommendation to take at least one minute, but participants in both

conditions took close to 5 minutes to make their decision (i.e., 4.5 minutes vs. 5 minutes). Thus, the studies either did not manipulate actual decision time (for an analysis of problems with naturally occurring decision times, see Krajbich et al., 2015) or include a condition with a sufficiently short time to allow for a test of the impact of time constraints.

We began our work by inspecting the results from other default experiments to gauge possible variability across studies. These studies, which appear in Table 4, showed that decisions typically requiring little time, such as street petitions and taxi tips (e.g., Haggag & Paci, 2014;Johnson & Goldstein, 2003), showed action default advantages, meaning that the default to sign the petition or tip were upheld. In contrast, studies with decisions typically requiring more time (Brown & Krishna, 2004; Di Guida et al., 2012; Keller et al., 2011; Shepherd & O'Carroll, 2013) showed either null effects or more frequent choices to act in opposition to the action default (see Table 4). For example, defaults to increase uptake of the HPV (Human Papilloma Virus) vaccine resulted in 75 percent agreement for the no-vaccination default and 52 percent for the vaccination one (Reiter et al., 2012).

Following this review of the literature, we conducted experiments to estimate the impact of decision time on default effects, both naturalistically (Experiment 1) and experimentally (Experiments 2-4). We hypothesized that donation defaults would have stronger influences when people have less (vs. more) time to make a decision. We conducted four experiments to estimate the effects of decision times on donation to a charitable or service organization using an action (to donate) default or a no-action (to not donate) default. The default choice formats appear in Figure 4.

Experiment 1 included a naturalistic measure of decision time and estimated associations with donation choices for different defaults. Experiments 2, 3, and 4 experimentally manipulated

choice time to rule out the possibility of reverse causality and confounds related to naturally varying decision times (e.g., educational level). The manipulations of Experiments 2 and 3 combined actual and perceived time, such that when participants had only 2 seconds to decide, they knew that they only had 2 seconds. However, Experiment 3 assessed if *perceived* decision time was responsible for the effect of our earlier time manipulation, and Experiment 4 experimentally separated actual and perceived time. If a shorter decision time simply limits people's ability to make decisions, time limits should increase the success of action defaults because the format alone channels their decision by reducing their ability to consider the alternative or cannot swap their choice. In contrast, people may be aware that their time is limited and choose the default because they are motivated to reach closure more quickly. In this case, perceiving a time limit should reduce the action default advantage.

In Experiment 1, participants ($N = 120$) had the option of donating money to a charity. After receiving extra money for completing an unrelated prior task (i.e., sorting household goods into self-determined groups), participants were asked if they wanted to donate their earnings to a charity ($0.10). They were told either that they would donate unless they indicated that they did not want to (action-default condition), or that they would not donate unless they indicated that they did want to (no-action-default condition; see Figure 4). In addition to manipulating the defaults, we measured the time participants spent making their decision to analyze interactions with our choice-format manipulation.

We hypothesized that participants who made faster choices would be more likely to donate with the action default than with the no-action default. A logistic regression comparing action defaults (50% donation rate) to no-action defaults (48% donation rate) showed no evidence that the default manipulation alone impacted donation rates. However, when participants spent less

than 1.60 seconds making the decision, the action default led to more donations. In contrast, when participants spent more than 3.90 seconds, they donated less with the action-default format than with no-action-default format. The results from this experiment are depicted in Figure 5.

In Study 2, instead of measuring decision times, we manipulated them. We used the same paradigm used in Study 1, except that half of the participants had either 2 seconds (Shorter Time Condition) or unlimited time (Longer Time Condition) to make their decision. Participants were informed of how much time they would have ahead of time. The results from this experiment appear in the top panel of Figure 6. As shown, when decision time was only 2 seconds, participants donated more with the action-default format (73%) than the no-action-default format (31%). In contrast, when decision time was unlimited, the action-default format (50%) had no significant advantage over the no-action-default format (57%).

In Study 3 ($N = 222$), we used the same procedures from Study 2. As shown in the bottom panel of Figure 6, when decision time was 2 seconds, participants donated more with the action default format (86%) than the no-action-default choice format (32%). In contrast, when the decision time was unlimited, the difference between the action and no-action defaults was smaller (74% vs. 57%) and nonsignificant. In addition, this study examined perceptions of time to determine if our effects were due to actual or perceived time. However, perceived time did not vary across conditions, suggesting that actual rather than perceived time and being motivated to finish earlier were likely at play.

Experiment 4 further examined if time constraints alone increase reliance on defaults independently of our time perception. Participants ($N = 637$) underwent the same procedures as in the prior experiments**.** The manipulation of actual decision time again involved participants being allocated to either two seconds or unlimited time to make their decision. In addition, we

manipulated perceived time limits by either telling or not telling participants about the time they had to make their decision. Thus, whereas the two conditions that made participants aware of time replicated Experiments 2 and 3, the new conditions in which participants were unaware of time allowed us to isolate the effects of actual decision time.

Figure 7 presents the results from Experiment 4. As shown, participants were more likely to donate with an action default than a no-action default, but this effect was only present when the actual decision time was shorter. In contrast, the manipulation of perceived time did not have this effect. Instead, when participants who had less time thought they had less time, they donated less in response to the action default than the no-action default. Apparently, calling attention to the time limit motivated participants to correct for the manipulation as a way of avoiding an unwanted influence of the response format (Schwarz & Clore, 1983; Wegener & Petty, 1997) .

In addition to these experiments, we conducted an individual data meta-analysis (Mcshane & Böckenholt, 2017) using all the experimental data we collected, including several pilots and related experiments, and the previously presented experiment. Our meta-analysis contained data from different experiments containing effect sizes reflecting the format and decision time interaction (total $N = 2359$; 795 Short-time decisions, 1564 longer-time decisions). Choice format was manipulated in each study, but studies differed in both choice format and decision time. The meta-analysis, whose results appear in Table 5, showed that participants donated more with action than no-action defaults (78% vs 61%), but these effects were stronger when time was shorter. Donation rates for action and no-action defaults were 78 and 33 percent respectively when times were shorter but 61 and 51 percent when times were longer.

In sum, we examined and found that the amount of time to decide is a critical factor driving default effects, with reliance on defaults when people need to make decisions quickly.

We found evidence supporting this effect across four experiments in which participants had different default choices more when decision time was shorter (vs longer). These findings dovetail well with prior work suggesting that certain contexts can enhance or undermine defaults (Ariely et al., 2009; Jachimowicz et al., 2019). As a result, defaults are not universally or consistently effective but rather successful in specific scenarios (e.g., perhaps taxi tips and emergency room protocols) that should be carefully identified when considering implementation.

One critical question, however, is whether defaults shaped behavior directly or had effects that were mediated by attitudes and norms. Accordingly, we measured effects on attitudes and norms but found that none of the effects we found implicated these variables. Apparently, the impact was quite direct and not reflected in any of the cognitions we measured. In the future, researchers should investigate whether the accessibility of a decision alternative, which is the presumed mediating mechanism, is more impactful when individuals have limited time. Although such a measure would be quite close to a behavior, obtaining it would increase the ability of this research to elucidate how the environment channels decisions.

1.4.2. Mandating Behavior

Another direct environmental pathway to behavior is to mandate the behavior in question. For example, governments and organizations can require that employees vaccinate to be able to travel or work. Governments and businesses can also ban smoking or vaping in public contexts. An important consideration with these policies is that different streams of research make contrasting assumptions about the impact of requiring people to perform a behavior, such as vaccination (Albarracín et al., 2021a). On the one hand, in policy and law scholarship, requirements are expected to guide behavior (Durkheim, 1982; Giubilini & Savulescu, 2019; Jacobson & Jokela, 2021). People may simply conform to what is required because they fear social sanctions

or view themselves as ethical individuals who follow norms (Durkheim, 1982; Prislin & Wood, 2005). They may also view the requirement as beneficial for them and society at large (Attwell et al., 2018; de Figueiredo et al., 2021; Jones & Buttenheim, 2014; Omer et al., 2006; Pitts et al., 2014; Schumacher et al., 2020; Thompson et al., 2013).

On the other hand, research in psychology and other behavioral sciences often view requirements with skepticism (Betsch & Böhm, 2016). As proposed by the theory of psychological reactance (Brehm, 1972; Brehm, 1966; Hornsey et al., 2018; Sprengholz & Betsch, 2020), people may oppose a requirement they perceive as curtailing their freedom (Giubilini & Savulescu, 2019). In a review of the literature on COVID-19 vaccination hesitancy, for example, Lin et al. (2020) warned against mandates as possibly eliciting reluctance to immunize "even among originally receptive groups" (Lin et al., 2020). In the same vein, other researchers have cautioned that "considerable part of the target group could be annoyed when the act comes into force, consequently showing reactance and opting out of other vaccines that are still voluntary," concluding that well-meant mandates could do more harm than good (Sprengholz & Betsch, 2020).

Our own research has led to the determination that vaccine mandates are beneficial, even in situations in which they directly shape behavior without converting the unconverted. In a set of studies (Albarracín et al., 2021b), we investigated whether intentions to vaccinate varied as a function of introducing a requirement and allowing participants freedom of choice to vaccinate. A survey of White American, Black American, and Hispanic American respondents assessed the degree to which requiring the COVID-19 vaccine to work, travel, or attend school increased or decreased intentions to vaccinate. This study was followed by three experiments, the final one preregistered, comparing various scenarios about a vaccination decision relevant to the workplace. Respondents considered a situation in which either a new employer (Studies 2 and 3)

or their current employer (Study 4) indicated that they (a) "require that you get vaccinated against a new disease" (requirement condition), (b) "prefer that you get vaccinated against a new disease but that vaccination is up to you" (freedom of choice condition), and (c) indicated that "getting vaccinated against a new disease would give you more freedom and flexibility." The last condition, which was present in Studies 2 and 3, emphasized freedom but controlled for freedom of choice per se.

In each study, we gauged the impact of the mandate manipulation on vaccination intentions. In addition, in Study 4, we estimated the impact of the manipulation on perceived vaccination benefits (i.e., positive outcomes of receiving the vaccine), perceived fairness (i.e., perception that receiving the vaccine is fair to and respectful of individuals), perceived obligation (i.e., perception that vaccinating is moral and law–abiding), and perceived vaccination norms (i.e., perception that vaccination is popular and prevalent). These measures allowed us to distinguish when the impact of the mandate is direct, as opposed to when the mandate itself is used to infer positive benefits or social norms.

Study 1 consisted of a survey of 299 participants who were selected to include Non-Hispanic White ($N = 108$), Black ($N = 105$), or Hispanic ($N = 96$) respondents living in the US recruited from the platform Prolific (www.prolific.com) in January 2021. Participants were asked, "Will you get the COVID-19 vaccine if it is required to work, travel, or go to school?" (Requirement question) and "If you could get the COVID-19 vaccine for free today, would you want to be vaccinated today?" (Control question), both answered as either "Yes" or "No."

Analyses of these questions, which appear in Table 6, showed that, across the board, participants were more willing to vaccinate in response to the required question than the control question. Considering different racial/ethnic group and excluding a small number of multi-racial

participants, eighty six percent of white respondents intended to vaccinate in response to the required vaccination question but only 69 percent of them reported intending to vaccinate in response to the control vaccination question. Ninety four percent of Hispanic respondents intended to vaccinate in response to the required vaccination question but only 80 percent of them intended to vaccinate in response to the control vaccination question. Eighty percent of Black respondents intended to vaccinate in response to the required vaccination question but only 56 percent of them intended to vaccinate in response to the control vaccination question. In summary, mandating vaccination appeared to increase intentions to vaccinate in different populations.

Study 2 was conducted in March 2021 with 359 participants in a 3-cell, within-subjects experiment. Participants considered a situation in which they have a new job and the company tells them that (a) "they require that you get vaccinated against a new disease" (requirement condition), (b) "they prefer that you get vaccinated against a new disease but that vaccination is up to you" (freedom of choice condition), and (c) "getting vaccinated against a new disease would give you more freedom and flexibility" (condition that emphasizes freedom as a vaccination outcome and thus controls for freedom of choice). Results showed that intentions were significantly stronger in the required vaccination condition than either the freedom condition, $d = .14$, or the control for choice freedom condition, $d = .10$ (see Table 6).

(Hong & Faedda, 1996)Study 4 was a preregistered experiment administered to a nationally representative sample of United States adults in April 2021. This sample, which was comparable to the US Census, involved 606 participants from Qualtrics panels recruited with quotas for gender, education, and race/ethnicity, who provided complete data and passed the quality checks instituted by Qualtrics. As in the previous studies, participants considered the requirement and

freedom of choice scenarios but were only assigned to one of the two conditions. We measured vaccination intentions as well as a set of possible mediators including (a) perceived benefits of vaccination, (b) perceived norms for vaccination, (b) perceived fairness of vaccination, and (d) perceived obligation to vaccinate. Additionally, we measured psychological reactance to again explore whether our proposed effects were moderated by this individual difference. The results on intentions, which appear in Table 6, showed that describing vaccination as a requirement led to stronger intentions to vaccinate than describing vaccination as a free choice for participants to make, $d = .10$. This effect was present regardless of participants' level of psychological reactance.

In Study 4, we also analyzed the effects of the experimental manipulation on perceived benefits, perceived norms, perceived fairness, and perceived obligation. We found that the requirement led to greater perception of benefits when participants were low in psychological reactance but had a nonsignificant reverse effect when they were high. In contrast, the requirement led to lesser perceived obligation when participants were high in psychological reactance but had no effect when they were low. In sum, low-reactance individuals saw the requirement as an indication that there was a benefit. High reactance ones felt unobligated by the mandate, but their intentions were still more positive because the behavior was mandated.

Our research consistently suggested that making a behavior a requirement for work, attending school, or travelling is successful in promoting behaviors, more so than either giving people freedom to choose or reminding them of the freedom the behavior might confer. Also, the studies provided no evidence that requirements elicited reactance in a way that undermined the likely efficacy of a vaccine mandate. Rather, the behavioral intentions of participants who scored higher in psychological reactance were either more positively influenced by the mandate (see

Study 3) or as influenced as their lower reactance counterparts (see Study 4). Study 4 also showed psychological reactance might lead people to feel less morally obligated to follow a mandate but still be motivated to behave in accordance with it. All in all, our results supported a positive impact of mandates on behavior, which were small but still effective for the highly difficult task of increasing vaccination intentions.

1.4.3. Summary

In summary, our work showed that the effect of a behavioral default may only operate when people lack time to elaborate, and such effect is quite direct and does not seem to involve attitudes, intentions, or goals. On the other hand, mandating behaviors provide a way to elicit a certain behavior, although many of the effect sizes are small, at least in the case of vaccination. Even if mandates lead to psychological reactance, such reactance may only decrease people's moral obligation to follow the mandate but still increase the intention to perform the behavior.

2. Closing Remarks

The environmental impact on human behavior has been and will continue to be the focus of various areas of social psychological research, as well as other disciplines, such as business, marketing, and neuroscience. This chapter described four pathways through which the environment influences behaviors, namely (a) activating inner questions that lead to repetition of past behaviors, (b) priming directional concepts that activate behavioral goals, (c) introducing behavioral recommendations via communications or policies, and (d) directly channeling or mandating the behavior via policies. Specifically, our research work has shown that (a) affirmative self-questions can lead to repetition of habitual behaviors, (b) behavioral priming is robust and using behavioral concepts (e.g., *run* and *make*) is more effective in activating higher value goals, (c) behavioral recommendations are powerful and can facilitate execution of other

recommendations, thus making multi-behavior recommendations particularly effective, and (d) default effects cease to operate when people have time to elaborate but mandating behaviors through policies is a reliable way to encourage behavior.

Despite our advances, our current understanding of each of these pathways is far from sufficient. Although our work found robust effect of self-talk questions on behaviors, it is still unclear how different features of the environment influence the inner speech. Under what circumstances are we more likely to frame questions instead of affirmative statements? What types of environments activate inner questions that promote positive behaviors? Similarly, although the effect of introducing behavioral concepts clearly operates through goal activation and depends on the goal value, the mechanisms of influence of nonbehavioral concepts remain unclear. Does evoking nonbehavioral concepts involve activation of fully automatic behaviors, framing of inner questions, or elicitation of affective reactions? Moreover, as we noted at the beginning of this chapter, these four pathways are not mutually exclusive. For example, behavioral recommendations or policies may also elicit certain self-talk questions (e.g., “Should I follow the mandate?”) and policies can also serve as implicit behavioral recommendations and therefore change attitudes and norms. The integration and understanding of the interplay among these pathways should therefore be the focus of future research.

Another way in which the pathways in Figure 1 are not mutually exclusive is that framing questions and priming responses to the questions should interact. For example, Loersch and Payne (2011) have shown that the same stimulus can produce different effects depending on the type of questions implicit in an experimental task. Accordingly, the environment should guide behavior most effectively when it can promote both a question and the preferred response. Furthermore, priming concepts involves syntactic processes that parse structures that are larger

than single words (Albarracín et al., 2011; Albarracín, Hart, et al., 2006; Albarracín, Noguchi, et al., 2006). As a result, a concept is processed differently when accompanying words vary in order and depending on the environmental situation individuals experience (Albarracín et al., 2011). The complex dynamics of framing questions, directionally priming behaviors, instructing individuals, and shaping behavior are only beginning to be understood and should be part of our future research agenda.

Lastly, the four pathways described in this chapter were characterized from our own recent research and our model does not exhaust all forms of environmental influences on behaviors. For example, besides behavioral recommendations and policies, other people's behaviors themselves may passively exert social influence on our attitudes and behaviors (Cialdini & Goldstein, 2004; Cialdini & Trost, 1998). Another type of environmental influence may come from the social feedback we receive (e.g., evaluations, grades). As human beings, we are social creatures who care about other people's evaluations and thus feedback from others has critical impacts on behavior (Burgers et al., 2015; Park & Crocker, 2008). We hope that our model and our theoretical integration can be the starting point for a more complex model that integrates these and other influences as well.

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Table 1. Effects of Questions on Mean Proportion of Choice Repetition (Adapted from Lohmann, Jones, & Albarracín, 2019)

Question	Experiment 1	Experiment 2	Experiment 3	Experiment 4		Experiment 5	
				Questions	Statements	Control	Distraction
None	.49	.57	.47	.41	.43	.52	.53
Affirmative	.62	.68	.52	.61	.51	.61	.58
Irrelevant	–	.49	.39	–	–	.49	.51
Negative	.19	.31	.32	.25	.36	.39	.42
N	43	47	60	65	56	107	107

Note. The metric here is proportion of repeated choices. In all cases, participants had 3 choices, so the chance of repeating any one of them at random is .33.

Table 2. Effects of Goal Value for Behavioral and Nonbehavioral Concepts (Adapted from Dai et al., 2023)

	Higher value	Value not manipulated	Lower value
Behavioral concept	0.39	0.38	–0.47
Nonbehavioral concept	0.18	0.38	0.47

Note. Mean Estimates were obtained from no-intercept regression models. This no-intercept model simultaneously entered the interaction term between goal value and prime type, as well as mean-centered prime modality, publication status, year of report, country, proportion of prime, content of prime, liminality, social desirability of outcome, type of dependent variable, type of control, presence of funneled debriefing, presence of task prior to the prime introduction, pre-registration status, inclusion of covariates, and exclusion of participants as covariates. The coefficients should be interpreted as an estimated mean effect size for that combination level when all other covariates were at their mean level. $k = 856$, $I^2 = 58.1$, $\tau^2 < .001$

Table 3. Summary of Meta-Analyses on the Efficacy of Multi-Behavior Intervention Programs

Reference and domain	Type of Intervention	*Statistics*	*k*
Wilson et al. (2015), Smoking, physical activity, and diet	Multi-behavior conditions	*d* = *0*.17***	216
	Single-behavior conditions	*d* = *0*.07***	15
Dai et al. (2019), substance use	Linear effect of number of recommendations	*B* = 0.073***	233
Sunderrajan et al. (2021), HIV	Multi-behavior conditions	*d* = *0*.44***	205
	Single-behavior conditions	*d* = *0*.21	38
Zhang, A. et al., (2023), all health-related behaviors	Linear effect of number of recommendations	*B* = 0.03**	216

Note. *d* = Weighted-mean Cohen's *d*; CI = Confidence interval; *B* = Unstandardized coefficients; SE = Standard Error; *k* = Number of Effect Sizes; *** $p < .001$

Table 4. Time and the Default Effect in the Previous Literature (Adapted from White et al. 2019)

#	Short reference	Description of participants and decision	Likely decision time	Observed effect
1	Johnson et al. (1993)	Drivers deciding whether to acquire the right to sue when purchasing insurance	Short	Default effect
2	Johnson & Goldstein (2003)	Driver license applicants deciding whether to become organ donors	Short	Default effect
3	Madrian & Shea (2001)	Employees deciding whether to enroll in a retirement plan	Long	Default effect
4	Araña & León (2013)	Individuals deciding whether to pay additional taxes on vacation expenditures to help prevent global warming	Long	Default effect
5	Haggag & Paci (2014)	Taxi passengers deciding whether to tip the default percentage on a taxi ride	Short	Default effect
6	Reiter et al. (2012)	Parents deciding whether to have their sons receive the vaccine against the Human Papillomavirus	Long	Reverse effect
7	Di Guida et al. (2012)	Experimental participants deciding whether to switch to a new task during the experimental session	Long	Null effect
8	Keller et al. (2011)	Participants deciding whether to receive a reminder to be vaccinated against the flu	Long	Reverse effect
9	Shepherd & O'Carroll (2013)	Participants deciding whether to be organ donors	Long	Null effect
10	Brown & Krishna (2004)	Consumers deciding whether to accept the default settings for specific products (e.g., keyboards, computers, and vacation packages)	Long	Reverse effect when people were skeptical

Note. Positive effects indicate an action default advantage, whereas negative effects indicate an action default disadvantage.

Table 5. Results from Meta-analysis (Adapted from White et al. 2019)

Donation Behavior			
	B	*SE*	*p*
Experiment (Random Effect)	0.300	0.545	
Experiment × Condition (Random Effect)	0.134	0.366	
Constant	-1.058	0.209	.001
Choice Format	2.102	0.221	<.001
Decision time	0.951	0.148	<.001
Choice Format × Decision time	-1.647	0.212	<.001

Note. Multilevel Logistic Regression predicting Donation from choice format (no-action-default vs. action default), Decision time condition (Longer or Two-seconds), and their interaction. Studies assumed to have random intercepts and fixed slopes given similar samples. *B* is the estimated logit coefficient. *SE* is the standard error of the coefficient. $k = 8$ independent studies. Choice format coded as (0 = no-action-default; 1 = action default) and Decision time coded as (0 = Longer Time; 1 = Shorter Time)

Table 6. Summary of Four Studies from Albarracín et al. (2021)

Study number	*Platform*	*Measured Outcome*	*Vaccine Required?*	*Means (SEs) or proportions*	*Statistics*	*N*
Study 1	Prolific	Behavioral intention	Yes	86%	*χ2 (1)* = 27.61*** *d* = 0.64	299
			No	68%		
Study 2	Mechanical Turk	Behavioral intention	Yes	3.90	*t (716) = 3.63*** *d = 0*.14	359
			No	3.72		
Study 3	Mechanical Turk	Behavioral intention	Yes	3.96	*t (712) = 3.31*** *d = 0*.12	357
			No	3.85		
Study 4	Qualtrics Panel	Behavioral intention	Yes	3.59	*F (1, 593)* = 6.24* *d = 0*.10	606
			No	3.36		

Note. SE = Standard Error; d = Cohen's *d*; N: Sample size; *** $p < .001$ ** $p < .01$ * $p < .05$

Figure 1. Processes of Influence of the Environment on Behavior

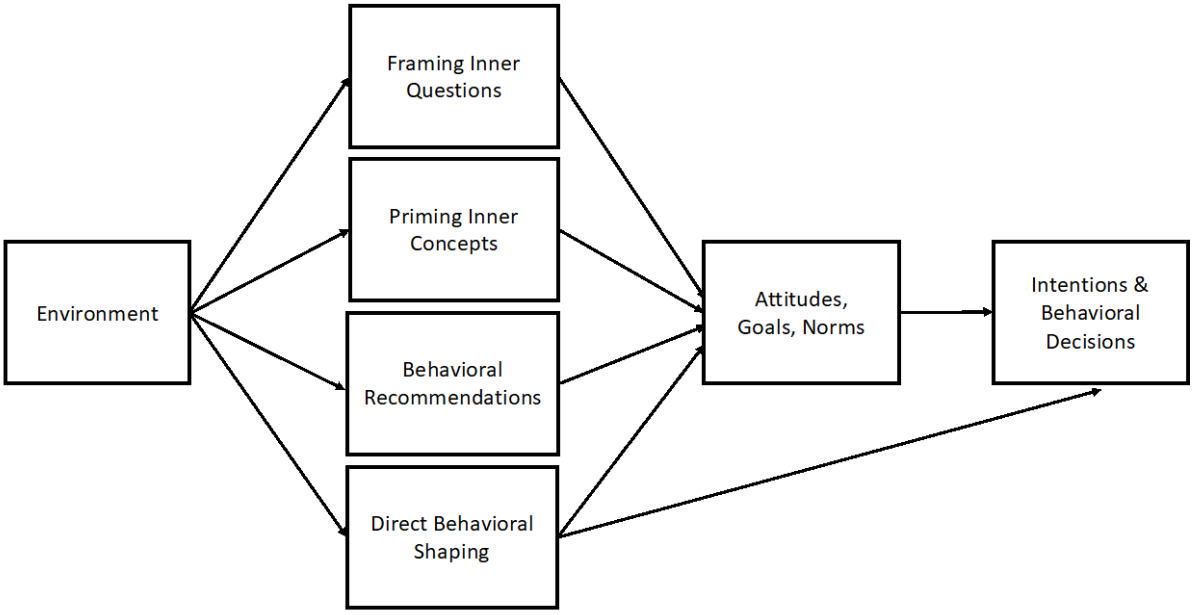


Figure 2. Continuum of Automatic-Deliberative Decision Making Showing Automatic Responses to Questions in the Middle

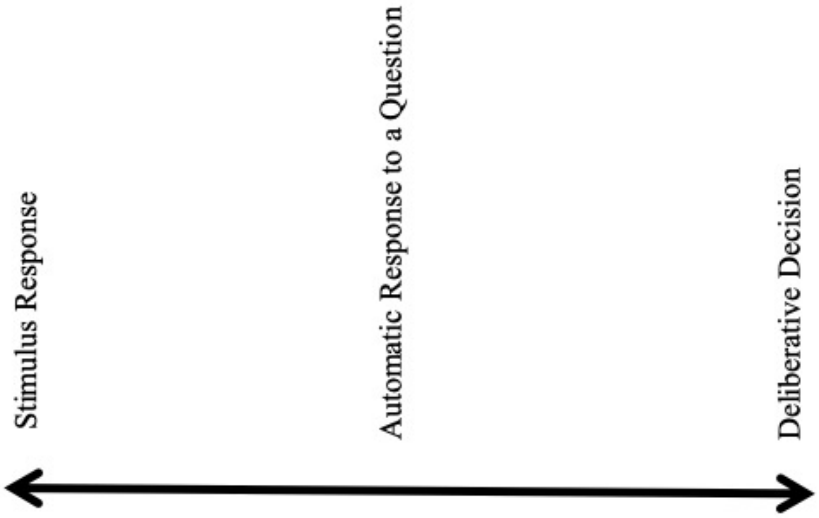


Figure 3. Experimental Paradigm for Choice Repetition (Methods Used in Lohmann et al. 2021)

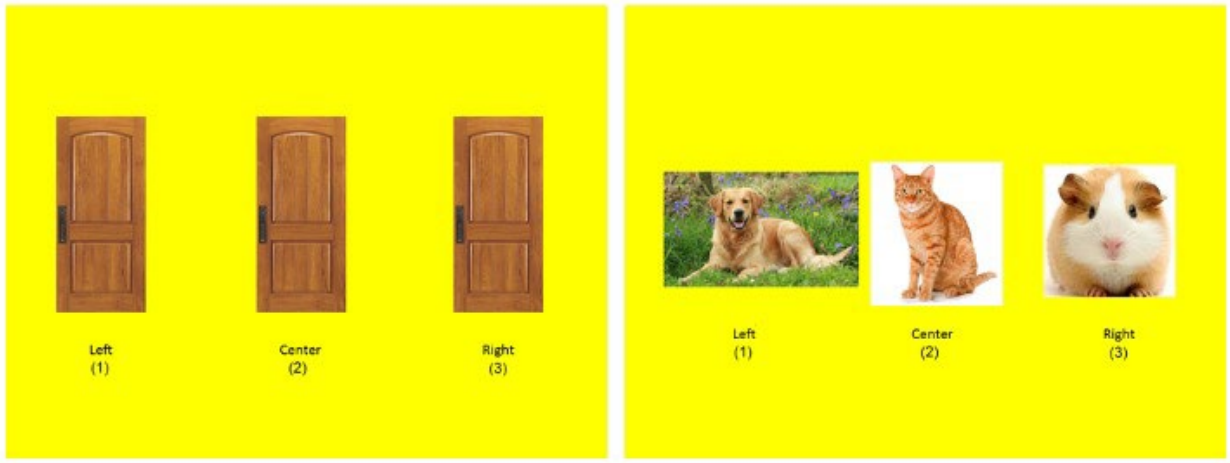
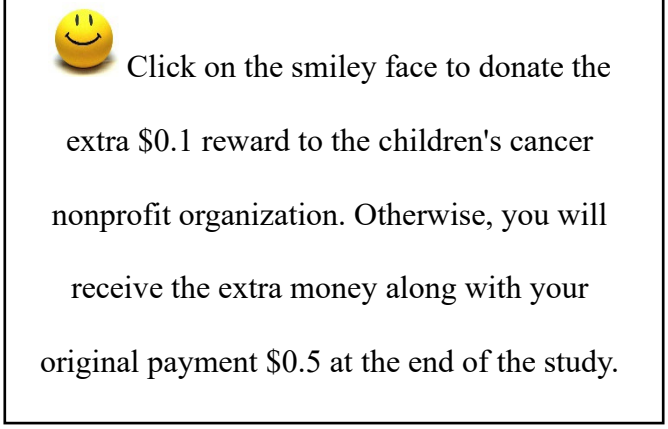


Figure 4. Default Architectures Used in Experiments (White et al., 2020)

Experiment 1



Experiments 2, 3 & 4

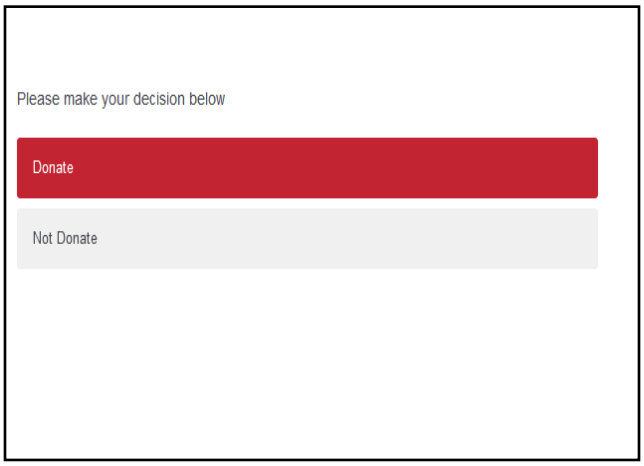
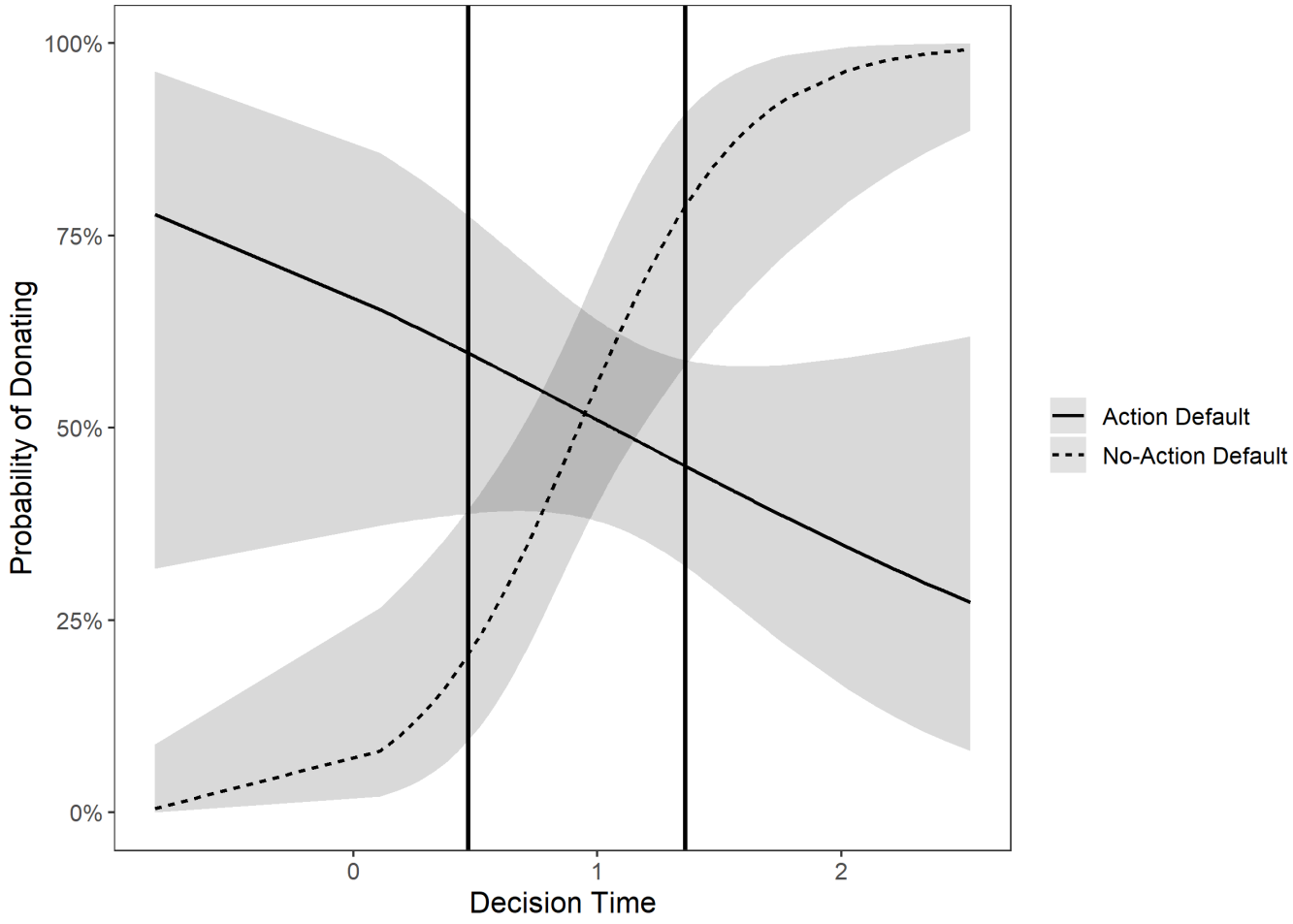


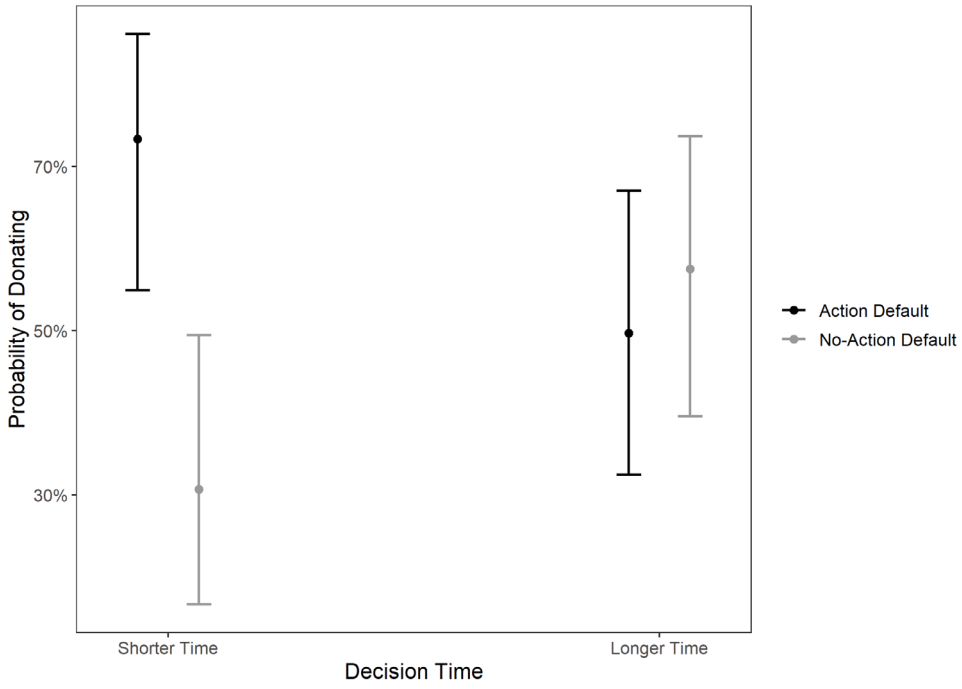
Figure 5. Spotlight Analysis: Experiment 1 (White et al., 2020)



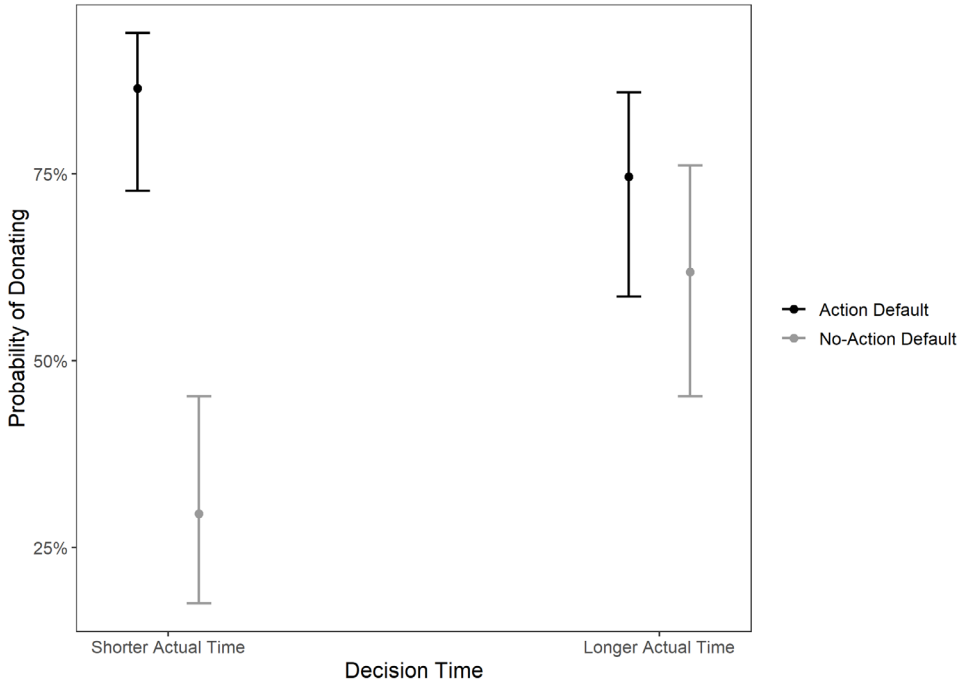
Note. The figure shows regions of significance for Decision Time values on donation behavior. Left Area is before 1.60 seconds and displays an action-default advantage. Middle Area from 1.6 to 3.9 seconds displays no significant difference in effects. Right Area from 3.9 seconds onward displays a No-Action Default advantage.

Figure 6. Probability of Donating as a Function of Time and Default Type: Experiments 2 and 3 (Adapted from White et al., 2020)

Experiment 2

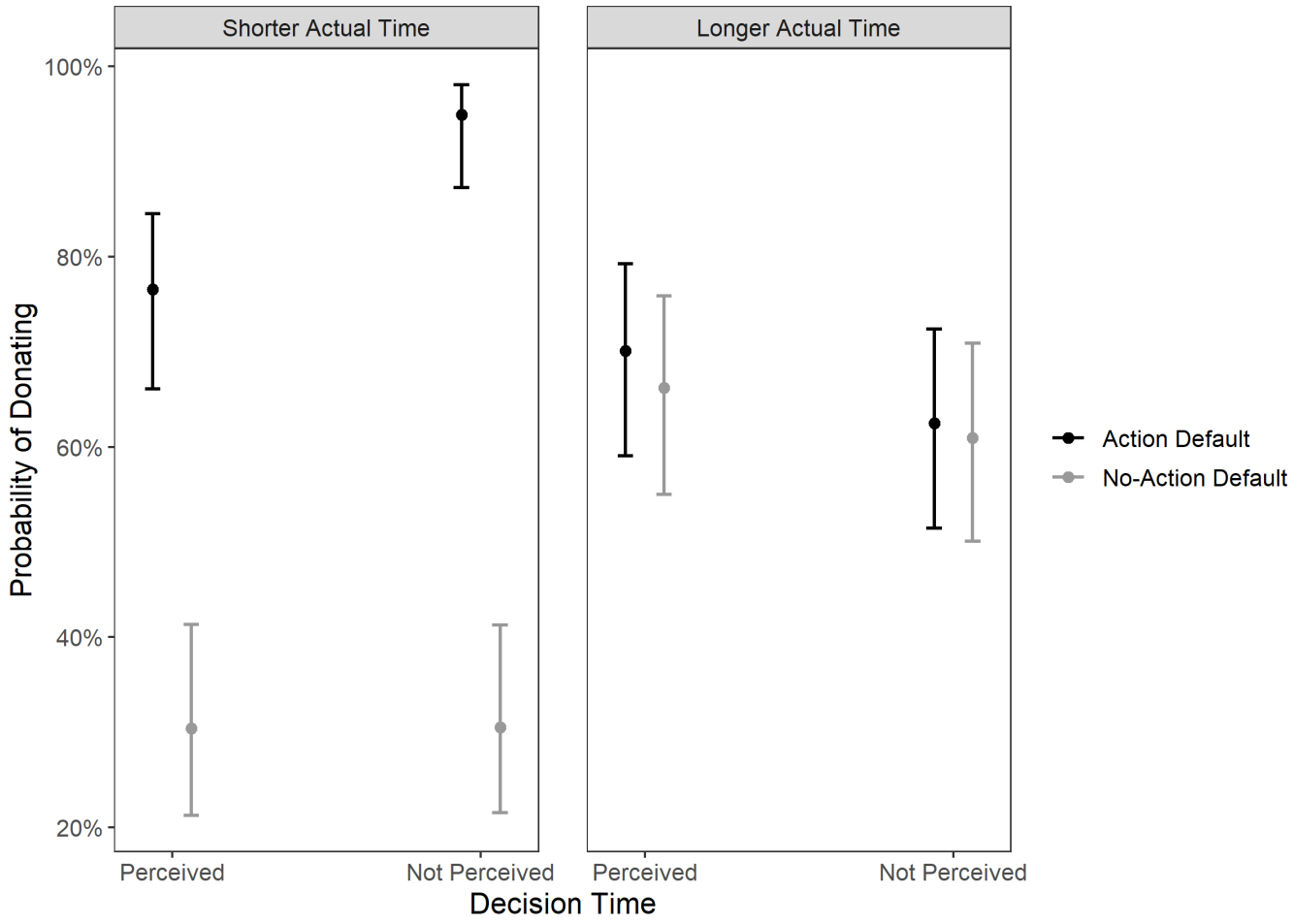


Experiment 3



Note. Y-axis is marginal probability of a participant donating to a non-profit organization in each condition. Longer Time were conditions where participants had no restrictions on their decision time. Shorter Time conditions were conditions where participants had two seconds to make their decision. Graph presents corrected means and 95% confidence intervals accounting for participant attitudes to donation behavior.

Figure 7. Probability of Donation as a Function of Actual and Perceived Time and Default: Experiment 4 (White et al., 2020)



Note. Y-axis is marginal probability of a participant donating to a non-profit organization in each condition. Graph presents corrected means and 95% confidence intervals accounting for participant attitudes to donation behavior.