Running Head: *The ASOC Effect*

**What Drives Asymmetric Attention to Intertemporal Opportunity Costs?**

**A Cognitive Process Analysis of the ASOC Effect**

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**Abstract**

Recent studies have uncovered a fundamental asymmetry in the attention (i.e., or decision ‘weights’) given to intertemporal tradeoffs between immediate gratification (choosing smaller-sooner rewards) and delaying gratification (waiting for larger-later rewards): people are naturally aware that waiting for larger-later rewards means forgoing immediate benefits, but they seem to pay less attention to the fact that choosing smaller-sooner rewards means having to forgo greater benefits in the future. This asymmetry hinders self-control and the ability to delay gratification. However, merely reminding people that opting for the smaller-sooner option means they get “nothing later” counteracts the asymmetry and boosts patience—a phenomenon called the “ASOC” effect. The ASOC effect is highly robust, yet little is known about the underlying processes that govern it. In this paper, we use a combination of process measures (e.g., choice times and thought-listing), experimental manipulations (e.g., putting decision-makers under cognitive load), and cognitive modeling techniques (drift diffusion models) to clarify the psychological nature of this asymmetry and the ASOC effect. Our studies reveal that the ASOC effect is driven by an implicit (‘System 1’) process, that its influence spills over to future decisions, and that it works by getting decision-makers to more carefully consider the future consequences of choosing immediate gratification.

Keywords: self-regulation; self-control; decision-making; process measures; cognitive modeling; ASOC effect

**Introduction**

Self-regulation has long been (and continues to be) a core topic of interest within social psychology (e.g., Bandura, 1991; Carver & Scheier, 1981; Karoly, 1993; Vohs & Baumeister, 2016), and for good reason: Self-regulation is critical to achieving desirable outcomes and accomplishing long-term goals. In particular, self-control, or the ability to override short-term desires to achieve larger but more distant goals, is necessary for maintaining health (e.g., dieting and exercising), wealth (e.g., saving money and investing in education), and indeed most determinants of long-term happiness. Greater self-control has been shown to predict desirable outcomes in many different domains, including finances (e.g., Lawrance, 1991; Meier & Sprenger, 2010; Reimers et al., 2009), health (e.g., Moffit et al., 2010; Reimers et al., 2009), and education (e.g., Duckworth, Quinn, & Tsukayama, 2012; Duckworth et al., 2019; Reimers et al., 2009). Studies have also found that children who are better able to resist temptation grow into more successful and well-adjusted adults (Mischel et al., 2010; Watts, Duncan, & Quan, 2018). In fact, the tendency to focus on the future (as opposed to the present) even predicts national differences in economic output (Noguchi et al., 2014). Understanding the processes that govern self-control and the factors that facilitate or inhibit it has thus attracted much attention; not just from psychologists, but also economists (e.g., Hoch & Loewenstein, 1991; O’Donoghue & Rabin, 1999; Olivola & Wang, 2016; Thaler & Benartzi, 2004; Tirole, 2002), biologists (e.g., Kacelnik, 2003; Sozou & Seymour, 2003; Stephens & Kreps, 1986), and philosophers (e.g., Elster, 2000; Kennett & Smith, 1996; Mele, 1998; Read, 2001). The central question of interest is how people navigate the many, incessant tradeoffs between consuming smaller rewards that can be obtained sooner and waiting for larger rewards that can only be obtained later. To give just a handful of examples, during his/her lifetime, a person might have to choose between studying hard and partying with friends, between a longed for vacation and achieving the down payment on a house, between working through the difficulties in a relationship and abandoning it in haste, between the pleasures of smoking and the pleasures of a long life, or between taking a full paycheck now versus investing it and receiving a larger pension upon retirement. Unfortunately, the recurring finding has been that people tend to be overly impatient, requiring too large a future reward to forego present gratification (e.g., Frederick et al., 2002; Read, 2004; Urminsky & Zauberman, 2014). This naturally raises the question: why are we so impatient? Or, to put it another way: what is it about human cognition that often hinders us from forgoing short-term incentives in favor of larger, long-term benefits?

**Asymmetric Attention to Intertemporal Opportunity Costs**

Recently, we presented a novel account of why people tend to be (too) impatient: there is a fundamental asymmetry in the attention given to the opportunity costs[[1]](#footnote-1) of choosing smaller-sooner (*SS*) versus larger-later (*LL*) rewards (Read, Olivola, & Hardisty, 2017). We proposed that when decision-makers are faced with a choice between *SS* and *LL* options, they are naturally fully aware that choosing *LL* means they will have to forgo *SS*. By contrast, while they “know” (at some level) that choosing *SS* means forgoing *LL*, less attention is given to this latter fact. As a result, the negative consequences (i.e., opportunity costs) of opting for the more immediate reward are less influential in the decision-making process than the negative consequences of delaying gratification. This, in turn, produces a de facto bias that favors *SS* options, and thus hinders the ability to delay gratification.

To test this asymmetry, we compared the degree of patience exhibited (i.e., proportion of *LL* options chosen) when one or both of the opportunity costs associated with the *SS* and/or *LL* option were explicitly highlighted. We presented participants with two-option intertemporal tradeoffs (i.e., choices between an *SS* and an *LL* option), and we framed these choices in one of four ways (see Table 1):

1. A standard (“hidden zero”) frame that did not highlight either opportunity cost.
2. An *SS* opportunity cost (“*SS* zero”) frame that *only* highlighted the consequences of choosing the *SS* option (e.g., receiving $0 at the future date).
3. An *LL* opportunity cost (“*LL* zero”) frame that *only* highlighted the consequences of choosing the *LL* option (e.g., receiving $0 today).
4. A combined (“explicit zero”) opportunity cost frame that highlighted *both* the consequences associated with the *SS* option *and* the *LL* option.

The resulting pattern of preferences could hardly be clearer: Participants were more patient (i.e., more likely to choose *LL* options) when the *SS* opportunity cost was highlighted (e.g., the “*SS* zero” was present), suggesting that the logical consequence of opting for the impatient option (receiving nothing later) does not, naturally, receive our full attention. By contrast, participants became no less (nor more) patient when the *LL* opportunity cost was highlighted (e.g., the “*LL* zero” was present), suggesting that the consequence of waiting for the larger, delayed reward (receiving nothing sooner) already receives our full attention. In fact, there was essentially no interaction between the two opportunity cost reminders, so that including both of them (“*SS* zero” and “*LL* zero”) was equivalent to only including the “*SS* zero”, whereas only including the “*LL* zero” reminder was equivalent to having no reminders. In sum, people responded to the addition of the “*SS* zero” by becoming more patient, but they did not respond to the addition of its symmetric counterpart, the “*LL* zero”.

We showed that this pattern is robust to variations in the way options are laid out (e.g., vertically or horizontally), the way opportunity costs are described (e.g., as “$0” or “nothing”), and the participant population (e.g., students or online samples; respondents in the US, the UK, or India; see also Wu & He, 2012, who observed the same pattern in Chinese participants). We also showed that it holds for both hypothetical and real payoffs, for gains and losses, and for non-monetary outcomes (e.g., chocolates, air quality, and lives saved). Importantly, this pattern of preferences clarified the findings of previous studies, which had only examined the effect of simultaneously highlighting both opportunity costs (Loewenstein & Prelec, 1991; 1993; Magen, Dweck, & Gross, 2008; Magen et al., 2014; Radu et al., 2011; Read & Scholten, 2012). We found these earlier effects were, in fact, entirely driven by the “*SS* zero”, a single opportunity cost reminder (see also, Scholten, Read & Sanborn, 2016; Wu & He, 2012).

More importantly, we believe this asymmetric effect of highlighting opportunity costs associated with *SS* and *LL* options (the “asymmetric subjective opportunity cost” or “ASOC” effect) reveals a fundamental asymmetry in our cognitive system, regarding the amount of attention (or ‘weight’) allocated to the consequences of pursuing smaller, short-term goals versus larger, long-term goals. However, although the behavioral data are consistent with the mechanism we propose, they do not, by themselves, provide much insight concerning the nature of the processes that characterize this asymmetry. Thus, important questions remain concerning the underlying psychological processes that drive the ASOC effect: Does the ASOC effect reflect a more explicit, deliberate (‘System 2’) process or a more implicit (‘System 1’) process? Is the ASOC effect limited to the immediate decision or can its influence spill over to future decisions? Finally, does the ASOC effect ‘blindly’ shift preferences toward the *LL* option (without considering its attributes) or does it lead decision-makers to more carefully consider the advantages and disadvantages of the *SS* and *LL* options? The goal of this paper is to shed light on the mechanistic nature of the ASOC effect. In particular, we use a combination of experimental manipulations, process measures, and cognitive modeling strategies, to clarify how the ASOC effect works at a cognitive level.

**Outline of Study Goals**

First, we examine whether the ASOC effect reflects a more explicit, deliberate (‘System 2’) process or a more implicit (‘System 1’) process. Study 1 uses a cognitive load manipulation to see whether the ASOC effect is mitigated or eliminated when decision-makers have limited cognitive resources to process the implications of the *SS* zero. Study 2 uses a thought-listing protocol to see whether the *SS* zero leads to greater explicit consideration of the *SS* opportunity cost during the decision process. Finally, Study 3A examines (among other things) whether participants anticipate the behavioral effects of the *SS* zero. To the extent that the ASOC effect is an explicit, deliberate (‘System 2’) process, it should have less impact on decisions when participants are under cognitive load (in Study 1), lead to more explicit thoughts about *SS* opportunity costs (in Study 2), and participants should be able to anticipate its influence on decisions (in Study 3A). On the other hand, to the extent that the ASOC effect is an implicit (‘System 1’) process, it would be immune to cognitive load (in Study 1), would *not* produce explicit thoughts about *SS* opportunity costs (in Study 2), and participants would not anticipate its impact (in Study 3A).

Next, we examine whether the ASOC effect reflects a short-lived mechanism that only operates on the current, immediate choice context, or whether it instead reflects a longer-acting process whose influence persists in the decision-maker’s mind, even after the *SS* opportunity cost reminder is no longer presented. Studies 3A and 3B address this question by having all participants complete two separate blocks of choices: a ‘treatment’ block that follows our standard experimental design (so that some participants are exposed to one or both opportunity cost reminders), and a ‘control’ block (in which choice items are all presented without any opportunity cost reminders). To the extent that the ASOC effect reflects an ephemeral mechanism, its influence should be limited to immediate choices, and thus disappear once *SS* opportunity cost reminders are removed. On the other hand, to the extent that the ASOC effect reflects a more enduring process, we should see its influence spill over to future choices, even after *SS* opportunity cost reminders are removed.

Finally, we utilize a cognitive process modeling approach (drift diffusion modeling or “DDM”) and choice time data to contrast two alternative processes by which the ASOC effect could be operating. Specifically, we examine whether *SS* opportunity cost reminders increase patience directly, by biasing preferences toward the *LL* option without any (additional) consideration of the *SS* or *LL* attributes—a preference “priming” effect—, or whether these reminders increase patience indirectly, by promoting consideration of the negative consequences of selecting the *SS* option—an attention (to information) “priming” effect. Study 4 presents participants with a large number of choice items (27 per person), which allows us to simultaneously estimate all four DDM model parameters for each decision-maker. In Studies 5A and 5B, we edit the choice items so that the *SS* and *LL* opportunity cost reminders are perfectly matched in terms of their word and character counts. Doing so allows us to eliminate any systematic differences in reading time (at the item level) that might be associated with our manipulations, and which could otherwise influence choice time. Finally, we also apply DDM modeling to all prior ASOC studies in which we collected choice times.

**Overview of Study Methodology**

The studies we report generally followed the same design and employed similar methodologies. All studies consisted of (at least) a 2 (*SS* opportunity cost reminder: present vs. absent) by 2 (*LL* opportunity cost reminder: present vs. absent) between-subjects factorial design, with each participant randomly assigned to one of the four resulting conditions: hidden zero (both opportunity cost reminders *absent*), *SS* zero (only *SS* opportunity cost reminder present), *LL* zero (only *LL* opportunity cost reminder present), and explicit zero (both opportunity cost reminders *present*).

All participants received payment for completing their respective study. Participants were presented with a series of binary choice items (ranging from 7 to 31 items, depending on the study), which appeared one at a time, and in random order. Each choice item consisted of two options: a smaller-sooner payoff (the *SS* option) and a larger-later payoff (the *LL* option). Within each choice trial, participants indicated which option they would choose, and clicked on a button to submit their choice and move onto the next choice trial. For each trial, we recorded the option that participants selected, as well as the time[[2]](#footnote-2) they spent deciding; specifically, we measured the choice time for each decision item as the time between when the options appeared on the screen and when a participant submitted his/her choice.

Using their choices across choice items, we calculated each participant’s patience score as the proportion of *LL* options he/she selected. Similarly, we used their choice times to calculate their average decision time. As we explain in more detail, later, the combination of choices and choice times were used in drift diffusion modeling (DDM) analyses to estimate four separate parameters that correspond to different aspects of the underlying cognitive decision-making process. The main results (choice proportions and DDM parameter estimates) of our studies are presented in Figures 1-2, and Table 2.

The specific set of choice items used in each study—including the wording used and their parameter values (i.e., the specific payoff amounts and delay lengths associated with the *SS* and *LL* options)—are detailed in the supplementary materials.

After all choice trials were completed, participants were asked to report a variety of demographic characteristics (e.g., age, gender, etc.).

All measures, manipulations, and exclusions in every study are disclosed. Total sample sizes were set to be greater than 160, so that we would have more than 40 participants per condition (in our standard 2×2 design), and more than 80 participants in our two critical groups (*SS* zero: present vs. absent). Such sample sizes allow us to detect effects that are somewhere between small and medium in size ( < .048; *d* < .45) with 80% power. Note that in prior studies, we obtained *SS* zero effects that were, on average, of medium size (average *d* = .54 – see Table 7 in Read et al., 2017). Further data collection was not continued after data analysis. For each study, we report the sensitivity power analysis for the key hypothesis test --specifically, the minimum effect size that can be detected with 80% power at the standard alpha significance criterion (α = .05, two-tailed).

**Study 1**

In Study 1, we put participants under varying levels of cognitive load to see whether doing so would moderate the ASOC effect. Specifically, participants completed the intertemporal choice items while holding either a simple (low load) or complex (high load) visual pattern in memory. Critically, participants completed both low- and high-load choice trials, which allowed us to compare any potential cognitive load effects within-person.

**Methods**

**Participants.** 382 British residents (65% female; Age: *Range* = 18-73, *M* = 39.1 *SD* = 11.7, *Median* = 36) were recruited through Prolific Academic (an online sample). We excluded data from one participant who failed to recall any of the visual matrices associated with one of the cognitive load tasks.

**Design and Procedure**. As our cognitive load manipulation, we used a variant of the visual dot memorization-and-recall task (e.g., Białek & De Neys, 2017; Bonnefon, Hopfensitz, & De Neys, 2013; De Neys, 2006; Trémolière, De Neys, & Bonnefon, 2012). At the start of the experiment, participants were given a fake cover story regarding this cognitive-load task. Specifically, they were led to believe that the study was designed to examine the effect, on decision making, of holding emotionally evocative images in memory. They were further told that they would be randomly assigned to hold either emotionally evocative or neutral images in memory, while they made decisions, and that we expected emotionally evocative (but not neutral) images to influence decision making. In reality, all participants were “assigned” to the neutral images and the real goal of the study was to test whether a visual cognitive load task (holding the neutral images in memory while making intertemporal choices) would moderate the ASOC effect. Participants completed a total of 16 different choice items (see supplementary materials), with the word “nothing” replacing “£0” for the opportunity cost reminders (see Study 5 in Read et al., 2017). These 16 choice items were divided into two separate sets of 8 items (Set A and Set B). Each trial consisted of three parts: an encoding phase, a choice phase, and a recall phase. First, in the encoding phase, a visual matrix appeared for 2 seconds (see Figure 3), and participants had to hold this visual item in memory until the recall phase. Next, in the choice phase, they responded to one choice item (presented in the frame they were assigned to). Finally, in the recall phase, participants were presented with the target visual matrix and with three decoy matrices, and they were asked to identify the one they had been asked to hold in memory. After completing the recall phase they advanced to the next trial. Unbeknownst to the participants, the 16 choice trials were divided into two blocks (of 8 trials): One block consisted of *low*-cognitive-load trials, in which the visual matrices presented a simple pattern (three Xs in a line or along a diagonal – see Figure 3). The other block consisted of *high*-cognitive-load trials, in which the visual matrices presented a more complex pattern (four Xs lacking an obvious arrangement – see Figure 3). Half the participants were assigned to complete the low-cognitive-load trials first, followed by the high-cognitive-load items. The other half were assigned to the opposite ordering of high- then low-cognitive-load trials. In addition, we counterbalanced which item set (A vs. B) was used in the low- vs. high-cognitive-load trials. Thus, in addition to the standard framing manipulation, our design consisted of two within-subject factors (low- vs. high-cognitive-load; Set A vs. Set B), and two between-subjects factors (load ordering and item set ordering).

**Results**

For our analyses, we only considered trials in which participants correctly recalled the visual matrix. As expected, recall accuracy was higher in low-load trials than high-load trials (99% vs. 89%, within-subject t-test: *t*(380) = 14.74, *p* < .0001). The sensitivity power analysis (for 80% power and α = .05, two-tailed) revealed that the minimum effect size for our key hypothesis tests (of the ‘*SS* zero’ effect on choices) was = .020.

The ASOC effect was observed for both low- and high-load trials (see Figure 1). We conducted a pair of 2 (*SS* nothing: present vs. absent) by 2 (*LL* nothing: present vs. absent) by 2 (choice items: Set A vs. Set B) by 2 (block ordering: high-load first vs. high-load second) ANOVAs: one for low-cognitive-load trials and a second one for high-cognitive-load trials. We obtained a main effect of *SS* nothing in both the low-cognitive-load trials (*F*(1, 365) = 14.55, *p* < .0003, = .04) and the high-cognitive-load trials (*F*(1, 365) = 11.26, *p* < .001, = .03), but no main effect of *LL* nothing nor an *SS*-by-*LL* interaction in either load condition (all *p*s > .2). Incidentally, we also found a three-way interaction between *SS*-nothing, *LL*-nothing, and choice-set in both the low- and high-cognitive-load trials (.038 < *p*s < .040), as well as a main effect of choice-set in the high-cognitive-load trials only (*p* = .025).

These results show that the ASOC effect occurs regardless of whether people can allocate all of their cognitive resources (in our other studies), most of their cognitive resources (in the low-load condition), or only some of their cognitive resources (in the high-load condition) to the decision. This indicates that the ASOC effect is more of an implicit phenomenon. Moreover, the fact that we obtain the ASOC effect in this study rules out a demand effect account, which would be mitigated under high cognitive load (since one needs cognitive resources to try to infer the experimenter’s motivations). Indeed, both the visual-matrix task and our elaborate cover story would have drawn attention away from the fact that the *SS*-nothing and *LL*-nothing reminders were the central manipulations of interest.

**Study 2**

**Overview**

In Study 2 participants stated their thoughts while choosing, and we investigated whether the *SS* zero makes people explicitly consider the opportunity cost of choosing *SS*—i.e., the fact that doing so means forgoing *LL*—, or whether it only produces an implicit awareness of the *SS* opportunity cost.

Participants listed their thoughts while deciding. This “type-aloud” protocol has been successfully employed to study the psychological mechanisms underlying intertemporal choice (Appelt, Hardisty, & Weber, 2011; Hardisty et al., 2013; Weber et al., 2007). We used a total of seven choice items (see supplementary materials). One of these (described below) was employed for the thought-listing task.

**Method**

**Participants.** 202 participants (52% female; Age: *Range* = 18-80, *M* = 34.6 *SD* = 12.4, *Median* = 31) were recruited from Amazon Mechanical Turk (MTurk).

**Design and Procedure.** Participants first completed training in thought listing and a warm-up task to familiarize them with the interface, by listing the words “one” through “seven”. Next, they read the following instructions (with the bracketed sections visible or hidden, depending on the experimental condition):

*“Please imagine choosing between the following two options:*

|  |  |
| --- | --- |
| *$24 today [and $0 in 29 days]* | *[$0 today and] $35 in 29 days* |

*Please tell us everything you are thinking of as you consider this decision. Please enter your thoughts one at a time in the box below and hit the* ***Enter*** *key to submit each thought.”*

Participants could list as many thoughts as they wanted, but were required to list at least one. They next chose between the two options above, and then made the six remaining choices from the remaining choice items.

**Results**

**Thought coding.** Participants listed 3.1 thoughts on average (*SD* = 2.3). Two independent coders (blind to the experimental design and hypotheses) were trained to code each thought according to two criteria: “Did this thought mention receiving $0 today? (or receiving nothing today?),” and “Did this thought mention receiving $0 in the future? (or receiving nothing in the future?)”. Inter-rater reliability was good for both criteria (Cohen’s kappa = .89 and .70, respectively). In cases of disagreement, both raters discussed the thought and reached a consensus.

**Analysis***.* Our main analyses of participants’ preferences were limited to the six choice items for which we did *not* collect thoughts. The sensitivity power analysis (for 80% power and α = .05, two-tailed) revealed that the minimum effect size for our key hypothesis test (of the ‘*SS* zero’ effect on choices) was = .038. As expected, participants were more patient when given the Explicit zero and *SS* zero frames (see Figure 1). A 2 (*SS* zero: present vs. absent) by 2 (*LL* zero: present vs. absent) ANOVA confirmed a main effect of *SS* zero, *F*(1, 198) = 7.00, *p* < .009, = .03, but no effect of *LL* zero and no interaction (both *F*s < .8), replicating earlier studies (we obtain similar results if we do include the first choice item for which we obtained thoughts: main effect of *SS* zero, *F*(1, 198) = 6.68, *p* = .01, = .03, no effect of *LL* zero, *F*(1, 198)= 0.06, and no interaction, *F*(1, 198)= 1.21, *p* = .27).

Overall, explicit thoughts about opportunity costs were extremely rare. Across all conditions, approximately 1% (7 out of 622) of listed thoughts mentioned receiving nothing now; likewise, approximately 1% (8 out of 622) mentioned receiving nothing in the future.

We coded each participant as 0 or 1 to indicate whether or not they mentioned immediate opportunity costs (e.g., “nothing now”) at least once. A logistic regression with main effects of *LL* zero, *SS* zero, and their interaction found no significant differences between conditions in the likelihood that participants mentioned immediate opportunity costs (all *p*s ≥ .99)[[3]](#footnote-3). Likewise, we compared the likelihood of mentioning future opportunity costs, and also found no significant differences (all *ps* ≥ .19). Due to the low counts involved, these inferential tests should be interpreted with caution, but the main finding is clear: participants are extremely unlikely to mention[[4]](#footnote-4) immediate or future opportunity costs, regardless of whether zeros are hidden or explicit. Moreover, even when they do mention opportunity costs, they show no tendency to mention the ones highlighted by the particular frame they are assigned to. The raters also coded the thoughts for a range of other potentially relevant thoughts, but no comparisons were significant (all *p*s > .2).

In sum, the *SS* zero does not promote explicit consideration of *SS* opportunity costs, further indicating (along with Study 1) that the ASOC effect is more of an implicit phenomenon.

**Study 3A**

**Overview**

In Study 3A we asked all intertemporal choice questions twice, once with the Hidden zero frame (we call this the “Hidden zero control” or “HiddenC”), and once with one of the original four zero frames (the treatment condition). In this way we could test if participants who first made choices in the Hidden zero frame would become more patient when they were later provided with the *SS* zero or Explicit zero frame. Conversely, we could test if being first exposed to the *SS* zero or Explicit zero frame (in the first block) would make participants more patient when they were subsequently presented with the same choice items in the Hidden zero frame (in the second block). This order manipulation tells us whether the ASOC effect is ephemeral and local, only applying to a single choice, or whether it creates a more broad and persistent change in thinking about intertemporal tradeoffs. Study 3A was also designed to examine whether people anticipate the *SS* zero effect; that is, whether they are aware of, or at least have accurate intuitions regarding, the effect of the making the *SS* zero explicit. Specifically, we asked participants whether (and how) they thought the three alternative frames (Explicit zero, *SS* zero, and *LL* zero) would influence patience relative to the standard Hidden zero frame.

**Method**

**Participants.** 495 British residents were recruited through Maximiles (an online sample). Prior to our analyses, we excluded participants who failed to complete both blocks of trials (explained below). Our final sample consisted of 468 participants (46% female, 41% male, and 13% not reporting gender; Age: *Range* = 18-92, *M* = 47.5 *SD* = 14.9, *Median* = 48).

**Design and Procedure***.* Every participant responded to two blocks of choice items, each comprised of the same 15 items (see supplementary materials). One block was the Hidden zero *control* (denoted HiddenC), in which all items were presented in the standard frame (no added zeros). For the other, *treatment* block, all items were presented in one of the four zero frames (randomly assigned). Block order was counterbalanced so that half the participants received the HiddenC block followed by the treatment block, while the remainder received them in reverse order. There were thus eight conditions defined by the order of the HiddenC block (first or second), and the framing in the treatment block (*LL* zero present or absent; *SS* zero present or absent). Note that a quarter of participants were presented with the Hidden zero frame twice (once as HiddenC and once as a treatment). The two blocks were separated by a brief estimation filler task that was unrelated to the intertemporal choice questions (e.g., “Is the number of black rhinos in the world smaller or larger than the last four digits of your telephone number?”). In contrast to all other studies we report in this paper, we did not record choice times in Study 3A.

After completing both blocks, participants proceeded to the part of the study designed to examine their intuitions concerning the effect of making the opportunity costs (zeros) explicit. First, they were presented with a single intertemporal choice item in the standard Hidden zero frame: “£49 today OR £60 in 89 days”. This item was selected from the Kirby items because earlier studies showed participants are evenly split between the two options (i.e., approximately half prefer *LL*) when these are presented in the Hidden zero frame. After making their choice, the participants in our study were (truthfully) informed that “[p]revious surveys have found that approximately 50% (half) of people prefer the larger, later payoff.” They were then presented with the same choice item in one of the three alternative frames (Explicit zero, *SS* zero, or *LL* zero), and asked: “If people saw the choice presented in this manner (instead of the way it was originally presented), do you think they would be more likely, less likely, or equally likely to prefer the larger, later payoff?” Finally, participants were asked: “Would presenting the choice in this manner (instead of the way it was originally presented to you) influence YOUR preference?”, to which they could respond “Yes” or “No”. They also indicated how confident they were in their answer to this question (on a 0-100% scale).

**Results**

The sensitivity power analysis (for 80% power and α = .05, two-tailed) revealed that the minimum effect size for our key hypothesis tests (of the ‘*SS* zero’ effect on choices) was = .033. We begin by testing whether we replicate the basic *SS* zero effect when the treatment block was presented first. As with earlier studies, we conducted a 2 (*SS* zero: present vs. absent) by 2 (*LL* zero: present vs. absent) ANOVA. The results were as expected (see Figure 1), showing a main effect of *SS* zero (*F*(1, 231) = 25.97, *p* < .0001, = .10), but no main effect of *LL* zero nor an interaction (both *F*s < .2).

**Does the *SS* zero effect occur after exposure to the Hidden zero frame?** We next tested whether the effect of *SS* zero occurred in Block 2, when participants had already made their choices in the HiddenC frame. The main effect of *SS* zero was obtained even in Block 2, after exposure to the HiddenC frame, although it was weaker (see Figure 1). A standard 2×2 ANOVA analysis showed this effect to be significant: *F*(1, 229) = 5.39, *p* = .021, = .02), with no main effect of *LL* zero nor an interaction (both *p*s > .27).

**Does the *SS* zero effect spill over into future choices?** We also tested whether the *SS* zero effect would spill over from the treatment frames into the HiddenC frame, when the latter came second. That is, would exposure to the *SS* zero in Block 1 influence participants’ choices in the Block 2 HiddenC frame? To examine this question, we focused on participants who were assigned to see the treatment frame first (in Block 1), and we compared their patience levels in the HiddenC frame (in Block 2), as a function of the zero framing condition they had previously been exposed to. The standard 2×2 ANOVA, with Block 2 HiddenC patience as the dependent variable, revealed a main effect of *SS* zero (*F*(1, 231) = 7.98, *p* = .005, = .03), but no main effect of *LL* zero nor an interaction (both *F*s < .4). In other words, participants who were assigned to the Explicit zero or *SS* zero frame in Block 1 went on to make more *LL* choices in Block 2, under the standard Hidden zero frame, than did participants assigned to the Hidden zero or *LL* zero frame in Block 1 (see Figure 1). Thus, the impact of making the *SS* zero explicit can extend beyond one’s current choices to influence future intertemporal choices in which the *SS* zero is absent[[5]](#footnote-5).

One potential issue with this last analysis is that it may reflect a tendency for participants to prefer consistency in responding rather than a spillover effect on patience. To address this possibility, we carried out an additional analysis, in which the dependent variable was the within-participant *difference* in patience between the HiddenC and treatment frames (specifically: treatment patience minus HiddenC patience). We conducted a 2 (Block ordering: HiddenC first vs. HiddenC second) by 2 (*LL* zero present or absent in the treatment frame) by 2 (*SS* zero present or absent in the treatment frame) ANOVA. Again, only the main effect of *SS* zero was significant, *F*(1, 460) = 35.65, *p < .*0001, = .07 (all other *F*s < 1). Relative to the HiddenC frame, people showed much greater patience when the *SS* zero was present, regardless of whether HiddenC was presented in Block 1 or Block 2. Adding the *SS* zero increased patience over the same set of choices participants saw without the *SS* zero, regardless of whether exposure to the *SS* zero came before or after the standard (HiddenC) frame. In other words, and contrary to a mere consistency account, the *SS* zero effect cuts both ways.

**Do people predict the *SS* zero effect?** Approximately half the participants predicted that the proportion of *LL* choices made by other people would be the same in the alternative (non-Hidden zero) frame as it was in the Hidden zero frame (48%, 56%, and 52%, of those assigned to the Explicit zero frame, *SS* zero frame, and *LL* zero frame, respectively). In contrast, just over a quarter of participants (28% and 28%, respectively) predicted that the Explicit zero and *SS* zero frames would increase the proportion of *LL* choices relative to the Hidden zero frame. Similarly, just over a quarter of participants (28% and 29%, respectively) anticipated that their own choices would be influenced by the Explicit zero or *SS* zero frame (compared to 20% of those who evaluated the *LL* zero frame). Moreover, participants who anticipated that the Explicit zero frame would influence their own choices were less confident in their predictions (76% confidence) than those who predicted that it would *not* do so (85% confidence; t-test for unequal variances: *t*(73.78) = 2.36, *p* = .021, *d* = .61). The same was true of those assigned to evaluate the *SS* zero frame (72% vs. 82% confidence; t-test for unequal variances: *t*(103.22) = 2.59, *p* = .011, *d* = .56). In sum, most participants did not anticipate the effect of making the *SS* zero explicit, and those few who did were less confident in their predictions.

These results, along with those of Studies 1 and 2, lend further support to the hypothesis that the ASOC effect is more of an implicit phenomenon –so much so, that people fail to anticipate its impact. Moreover, these results reveal that the ASOC effect lingers in the mind of decision-makers long enough to impact their future intertemporal choices (even after a filler task). Finally, it is worth noting that the fact that the ASOC effect was weaker when the treatment block came second further rules out experimenter demand as a plausible account of the ASOC effect, since making the manipulation even more transparent (by having it appear after the control condition) decreased (rather than increased) the effect.

**Study 3B**

**Overview**

One potential limitation of Study 3A, when it comes to evaluating a spillover effect, is that participants were presented with the same set of items in both blocks, which may have biased their responses (e.g., by increasing consistency – our prior analysis notwithstanding). Study 3B addressed this issue by presenting participants with different sets of items in each block. In addition, Study 3B only focused on examining the spillover effect, so all participants were assigned to a treatment block followed by a HiddenC (i.e., Hidden zero) block.

**Method**

**Participants.** 439 British residents were recruited through Prolific Academic. Prior to our analyses, we excluded participants who failed to complete both blocks of trials. Our final sample consisted of 415 participants (63% female, 35% male, and 2% not reporting gender; Age: *Range* = 18-75, *M* = 34.0 *SD* = 12.7, *Median* = 31).

**Design and Procedure***.* The design and procedure were similar to Study 3A, with three main differences. First, *all* participants completed their assigned treatment frame in Block 1, followed by[[6]](#footnote-6) the HiddenC frame in Block 2 (i.e., no participants saw a treatment frame in Block 2 or the HiddenC frame in Block 1). Second, participants were presented with *different* sets of choice items in each block (see supplementary materials). Specifically, we used a total of 24 choice items, which we then divided into two sets of 12 items (Set A and Set B) that were matched (based on data from past studies) to be comparable in terms of the preference patterns they yielded. Half the participants were randomly assigned to receive Set A in the treatment block, followed by Set B in the HiddenC block. The other half first received Set B in the treatment block, followed by Set A in the HiddenC block. Finally, participants in Study 3B were not asked about their intuitions concerning the effect of making the opportunity costs (zeros) explicit.

**Results**

The sensitivity power analysis (for 80% power and α = .05, two-tailed) revealed that the minimum effect size for our key hypothesis tests (of the ‘*SS* zero’ effect on choices) was = .019. We begin by testing whether we replicate the basic *SS* zero effect when the treatment block was presented first. We conducted a 2 (*SS* zero: present vs. absent) by 2 (*LL* zero: present vs. absent) by 2 (treatment items: Set A vs. Set B) ANOVA. The results were as expected (see Figure 1), showing a main effect of *SS* zero (*F*(1, 407) = 21.00, *p* < .0001, = .05), but no main effect of *LL* zero nor an *SS*-by-*LL* interaction (both *F*s < .02). Incidentally, we also found a main effect of item set (*F*(1, 407) = 6.54, *p* = .011, = .02), but (importantly) no interactions between the set and the other factors (all *F*s < .6).

**Does the *SS* zero effect spill over into a *different set* of future choices?** We also tested whether the *SS* zero effect would spill over from the treatment frames (in Block 1) into the HiddenC frame (in Block 2) when these contained different choice items. To examine this question, we compared their patience levels in the HiddenC frame (Block 2), as a function of the zero framing condition they had previously been exposed to. The 2×2×2 ANOVA, with Block 2 HiddenC patience as the dependent variable, revealed a main effect of *SS* zero (*F*(1, 407) = 8.50, *p* = .004, = .02), but no main effect of *LL* zero nor an *SS*-by-*LL* interaction (both *F*s < 1). In other words, participants who were assigned to the Explicit zero or *SS* zero frame in Block 1 were also more patient in Block 2, under the standard Hidden zero frame, than participants assigned to the Hidden zero or *LL* zero frame in Block 1 (see Figure 1). Thus, the impact of making the *SS* zero explicit extended to a *different set* of future intertemporal choices in which the *SS* zero is absent. In sum, exposure to the *SS* zero in Block 1 influenced participants’ choices in the Block 2 HiddenC frame, despite the two blocks containing different sets of choice items. As with the treatment block, we found a main effect of item set (*F*(1, 407) = 11.85, *p* < .001, = .03), but no interactions between set and the other factors (all *F*s < .07).

**Drift Diffusion Modeling (DDM) Analyses**

Finally, we used drift diffusion modeling (DDM) to estimate four parameters related to the cognitive process involved in decision making. In particular, we sought to determine whether the ASOC effect operates by automatically shifting (i.e., “priming”) preferences toward the *LL* option without any additional consideration of the option attributes—corresponding to an effect on the DDM’s *bias* parameter—, or whether it instead operates by increasing processing of the option attributes (e.g., consideration of the opportunity costs associated with *SS* and *LL*), so that greater deliberation time is associated with more *LL* choices—corresponding to an effect on the DDM’s *drift* parameter. Since DDM requires not just choice data, but also choice times, we ran three additional studies (described below) in which we collected choices and choices times across a large number of choice items for each participant (Study 4) and equalized the lengths of the opportunity cost reminders (Studies 5A and 5B). We start by presenting these three new studies, then describing the methodology and results of our DDM analyses.

**Study 4**

**Methods**

**Participants***.* 241 participants (51% female; Age: *Range* = 18-64, *M* = 32.1 *SD* = 11.1, *Median* = 29) were recruited from Amazon Mechanical Turk.

**Design and Procedure***.* Participants were presented with 27 choice items (see supplementary materials). This large number of choice items allowed us to estimate patience levels (i.e., % *LL* choices) and DDM parameters with greater precision.

**Choice Results.**

The sensitivity power analysis (for 80% power and α = .05, two-tailed) revealed that the minimum effect size for our key hypothesis test (of the ‘*SS* zero’ effect on choices) was = .032. We replicated the ASOC effect on choices (see Figure 1), as confirmed by a 2 (*SS* zero: present vs. absent) by 2 (*LL* zero: present vs. absent) ANOVA showing a main effect of *SS* zero, *F*(1, 237) = 14.42, *p* = .0002, = .06, but no effect of *LL* zero, *F*(1, 237) = 1.65, *p* > .2, nor an interaction *F*(1, 237) = 0.05.

**Studies 5A and 5B**

A potential concern with the interpretation of choice times in our prior studies is that the *SS* zero and *LL* zero manipulations are not matched in terms of reading length. Specifically, the standard *SS* zero (“… and $0 in *X* days”) generally consists of more words and characters than the standard *LL* zero (“$0 today and…”). In the Study 4, for example, the *SS* zero consisted of 5 words and 17-19 characters (including spaces), whereas the *LL* zero only consisted of 3 words and 13 characters. This discrepancy in word and character counts creates an important confound, and the finding that *SS* zero increases choice times to a greater extent than *LL* zero could simply be due to the fact it is longer.

To address this issue, in Studies 5A and 5B, we altered the wordings of the *SS* and *LL* opportunity cost reminders so that they would be *exactly equal* (on an item-by-item basis) in terms of their word and character counts. Specifically, we employed two types of *SS* and *LL* opportunity cost wordings, divided across two sets of choice items.

For the first set of choice items (‘Immediate *SS*’), only the *LL* payoffs were delayed, and they either occurred in 1 week (7 days), 1 month (30 days), or 1 year (365 days). Although *SS* payoffs all occurred immediately, we matched their word and character counts to the *LL* delays they were paired with, as follows: *SS* payoffs occurring “right now” were paired with *LL* payoffs occurring “next week” or “next year”, whereas *SS* payoffs occurring “right away” were paired with *LL* payoffs occurring “next month”. Thus, for *LL* payoffs occurring in [1 week / 1 month / 1 year], the *SS* opportunity cost reminder (“…and nothing next [week / month / year]”) was paired with an *LL* opportunity cost reminder of identical length (“…nothing right [now / away / now]”).

For the second set of choice items (‘Delayed *SS*’), both the *SS* and *LL* payoffs occurred in the future (with the latter occurring twice as many days later), and the wordings for the *SS* opportunity cost reminders (“…and nothing in *X* days”) and *LL* opportunity cost reminders (“…nothing in *Y* days and…”) consisted of nearly identical structures, with only the difference being the location of the word “and”. Moreover, although the exact delay length (i.e., number of days) was necessarily larger for the *SS* than the *LL* option, they were matched in terms of their numbers of digits.

Every participant was presented with both sets of choice items within the same study. Studies 5A and 5B consisted of identical designs, with the only difference being that they were administered to two different populations (UK and Canadian participants) and consisted of different payoff currencies (British pounds and Canadian dollars).

**Methods**

**Participants – Study 5A***.* A total of 416 British participants (57% female; Age: *Range* = 18-71, *M* = 33.7 *SD* = 11.8, *Median* = 31) were recruited through Prolific Academic.

**Participants – Study 5B***.* A total of 189 participants (59% female; Age: *Range* = 18-65, *M* = 23.7 *SD* = 6.4, *Median* = 22), mainly students, were recruited both within and around a large North-American University campus.

**Design and Procedure***.* Participants were presented with 31 choice items (see supplementary materials). Of these, 15 choice items were from the first set (described above), and the other 16 were from the second set (also described above). Given their differences, we separately analyzed the responses associated with each set.

**Results – Study 5A**

The sensitivity power analysis (for 80% power and α = .05, two-tailed) revealed that the minimum effect size for our key hypothesis tests (of the ‘*SS* zero’ effect on choices) was = .019. The first set of matched choice items (‘Immediate *SS*’) replicated the ASOC effect: the standard 2×2 ANOVA analysis revealed a main effect of *SS* zero, *F*(1, 412) = 18.86, *p* < .0001, = .04, no effect of *LL* zero, *F*(1, 412) = .54, and a marginally significant interaction *F*(1, 412) = 3.70, *p* = .055, = .01. The second set of matched choice items (‘Delayed *SS*’) also replicated the ASOC effect (see Figure 1): the standard 2×2 ANOVA analysis revealed a main effect of *SS* zero, *F*(1, 412) = 22.53, *p* < .0001, = .05, no effect of *LL* zero, *F*(1, 412) = .33, and a marginally significant interaction *F*(1, 412) = 3.05, *p* = .082, = .01.

**Results – Study 5B**

The sensitivity power analysis (for 80% power and α = .05, two-tailed) revealed that the minimum effect size for our key hypothesis tests (of the ‘*SS* zero’ effect on choices) was = .040. The first set of matched choice items (‘Immediate *SS*’) replicated the ASOC effect: the standard 2×2 ANOVA analysis revealed a main effect of *SS* zero, *F*(1, 185) = 6.36, *p* = .013, = .03, but no effect of *LL* zero and no interaction (both *F*s < .7). The second set of matched choice items (‘Delayed *SS*’) essentially replicated the ASOC effect (see Figure 1): the standard 2×2 ANOVA analysis revealed a marginally[[7]](#footnote-7) significant main effect of *SS* zero, *F*(1, 185) = 3.67, *p* = .057, = .02, but no effect of *LL* zero and no interaction (both *F*s < .3).

**Drift Diffusion Modeling (DDM) – Details & Results**

For studies in which we collected decision times (all studies except 3A), we modeled the underlying decision-making process by applying the drift diffusion model (DDM) to participants’ *SS* vs. *LL* choices. The DDM is a mathematical sequential sampling model that treats binary choice as being determined by the accumulation of evidence over time until a decision threshold is reached. The DDM approach has been used to model decision dynamics across a wide variety of tasks (e.g., Koop & Johnson, 2013; Krajbich & Rangel, 2011; Nosofsky & Palmeri, 1997; Pleskac & Busemeyer, 2010; Pleskac et al., 2019; Yu, Pleskac, & Zeigenfuse, 2015), and in recent years has started to be applied to social psychological topics and methods (e.g., Bhatia & Pleskac, 2019; Johnson et al., 2017; 2018). Figure 4 illustrates the logic of the DDM process, and the role of its various parameters, in the context of *SS* vs. *LL* choices. In intertemporal choice, evidence refers to reasons for or against a choice of *SS* or *LL*. The ASOC effect, as we have described it, suggests that the presence of the *SS*-zero leads the decision maker to consider more reasons for choosing *LL*, and against choosing *SS*. In the DDM, the process of evidence accumulation is noisy, akin to a random walk, so that the choice made and the time taken to make that choice has a stochastic element. Since the DDM is estimated using the distribution of reaction times for the two choices, it can predict both the choices made and the time taken to make those choices (Ratcliff & McKoon, 2008; Ratcliff & Smith, 2004).

The DDM can be defined in terms of four parameters. Examining how these parameter values vary across contexts and framing conditions provides insights into the processes underlying intertemporal choices and the ASOC effect. The first parameter is the drift rate (*v*), or the *rate* at which evidence accumulates. In general, the distribution of reaction times (RTs) across trials are right-skewed (towards zero), and the DDM is able to estimate the drift rate (*v*) because smaller drift rates result in longer RTs that stretch out the tail of the distribution with no substantial changes to the leading edge (Ratcliff & Smith, 2004). For example, Wedel and Pieters (2015) find that the drift rate decreases when individuals identify more blurry images, indicating that less information per unit of time is extracted from these images. In addition, Pleskac et al. (2018) find that police officers engaged in a simulated first person shoot vs. no-shoot training task exhibit higher drift rates (i.e., faster accumulation of evidence to shoot) for armed Black targets compared to armed White targets. In our application, variations in drift rate would indicate corresponding variations in how easy it is to extract information from the options presented to the participants, and/or in the strength of that evidence. Thus, changes in the drift rate in the presence of the *SS-*zero that are consistent with the observed main effect on patience (i.e., greater percentage of *LL* choices) would indicate greater consideration of information provided by the *SS-*zero; namely, the future opportunity cost associated with the *SS* option. In particular, it would imply that the *SS-*zero increases patience by promoting consideration of the opportunity costs of choosing *SS*, thereby favoring *LL* and shifting preferences accordingly.

The second parameter is the difference between the boundary for choosing *LL* or *SS*, denoted as the threshold parameter (*a*). Once the accumulation of evidence reaches one of the boundaries, the decision-maker makes a choice. Greater threshold separation corresponds to more evidence needed to make a choice, or greater time/effort that participants are willing to spend on making the choice, and thus higher RTs. Boundary separation has also been observed to decrease across trials as participants become familiarized with the stimuli (Zhang & Rowe, 2014). Examining whether the threshold is affected by intertemporal opportunity cost reminders (i.e., the addition of ‘zeros’ or “nothing”) allows us to determine whether individuals require more or less evidence accumulated before making a decision. For example, if differences in the threshold are driving the effects of the *SS*-zero on patience, then we would expect to see larger boundaries in the conditions with *SS-*zero, which would result in more *LL* choices.

Third, the nondecision time parameter (*t*) captures the time that is not directly involved in choice deliberation, including stimulus encoding (e.g., reading text) and response execution (e.g., motor movements to respond). Thus, the DDM’s predictions of RT include both the deliberation time for the decision process plus the nondecision time. The model is able to estimate nondecision times because variability in nondecision times determine the shape of, and variation in, the leading edge of the RT distribution (Ratcliff & Smith, 2004). For example, Zhang and Rowe (2014) find that nondecision time is higher when participants are asked to focus on accuracy (rather than speed) during motion discrimination tasks. For the *SS* vs. *LL* decisions in our studies, nondecision time encompasses all the time *not* spent on evidence accumulation towards these options. Since there is little variation in the appearance of the stimuli across conditions in our studies (moreover, Studies 5A and 5B are designed so that all stimuli contain the same numbers of characters for both *SS* and *LL* options), we don’t expect nondecision time to vary substantially across conditions. Nonetheless, explicitly accounting for potential differences in nondecision times across conditions (e.g., if it takes slightly longer to read the extra text associated with opportunity cost reminders) allows us to isolate response time effects that are unrelated to the decision process, and thus to identify any existing effects on deliberation time.

Finally, at the onset of the trial, there may be an initial bias (*z*) towards either the *SS* or *LL* option. If the initial bias starts closer to the *LL* option, then we should see *LL* choices occur faster and with higher probability. A critical distinction between bias (*z*) and drift rate (*v*) is that bias represents a prior tendency towards an option, while the drift rate represents a dynamic bias that evolves with evidence accumulation (Dunovan et al., 2014). In our studies, differences in the bias across conditions that are consistent with the effects of the *SS*-zero on patience would indicate an initial gestalt preference for the *LL* option, prior to any consideration of the options’ attributes (including their associated opportunity costs). In particular, it would suggest that the *SS-*zero increases patience by “priming” a “mindless” preference for *LL* (i.e., absent any consideration of the *SS* and *LL* option attributes).

Thus, by examining whether and how these four parameters vary with the presence (vs. absence) of the *SS-*zero and *LL-*zero, we can determine which of these mechanisms best explains the ASOC effect of the presence of the *SS-*zero on patience. Specifically, we are interested in testing whether the *SS*-zero induces an initial bias (*z*) (i.e., a priori tendency) towards *LL* options, and/or whether it changes the drift rate (*v*) towards *LL* options, indicating greater consideration of future opportunity costs as the decision evolves.

We fit a hierarchical drift diffusion model (HDDM; Wiecki et al., 2013) on the participant data from each study. HDDM is a Python toolbox that uses a hierarchical Bayes formulation of the DDM and a Markov chain Monte Carlo estimation algorithm, under the assumption of a normal distribution of the parameters across participants (Gelman et al., 2013). The HDDM gives us individual-level posterior samples for each of the four parameters. In order to determine which DDM parameters can provide an explanation for the behavioral pattern we observe—specifically, the asymmetric effects of adding the *SS-*zero and *LL-*zero on patience—we also allowed each of the four parameters to vary by condition within each study (i.e., Hidden zero, *LL-*zero, *SS-*zero, Explicit zero). For each model, we generated 5,000 samples and used 4,000 samples as burn-in. We checked for convergence via visual inspection, and also by running multiple MCMC chains from different values and checking that the Gelman-Rubin statistics was below 1.2 for all parameters (Gelman & Rubin, 1992). Note that we excluded participant trials where the response time exceeded 100 seconds, which comprised less than 5% of the data.

For each study that we applied the HDDM analysis, Figure 2 plots the population-level posterior means (and estimated standard errors) of each parameter for each condition (drift, threshold, nondecision time, and bias). Note that for Studies 1 and 2, we only collected a small number of responses per participant (≤ 8 choice items per condition), and thus we dropped the bias parameter, *z*, from the estimation (i.e., we set *z* = 0.5). Moreover, among the studies in which we did estimate the bias parameter, we did not observe substantial, nor directionally consistent, deviations from 0.5 or variation across conditions (see Figure 2). Since the hierarchical Bayes estimation procedure of the HDDM yields individual-level posterior samples, we took the means of these samples to use as point estimates for comparing between conditions. We then used these values to conduct our standard 2×2 ANOVA analyses, shown in Table 2.

The results of these analyses, presented in Figure 2 and Table 2, reveal that, of the four DDM parameters, only the drift rate follows the pattern that corresponds to the ASOC effect. Specifically, the drift rate mainly responds to the presence of the *SS* opportunity cost reminder, but not to the *LL* opportunity cost reminder, nor to their interaction. Recall that a higher (i.e., more positive) drift rate indicates that evidence accumulates faster over time to favor *LL*, and in particular, it appears that evidence accumulation is faster when the *SS-*zero is present: Figure 2 shows that in every case, the addition of the *SS* opportunity cost reminder leads to a higher drift rate toward the *LL* option, and Table 2 reveals that this ‘*SS-*zero’ drift rate effect is significant in 9 out of 10 analyses (moreover, Figure 2 shows that the only non-significant result is directionally consistent with the other effects). By contrast, we only observe a single case of a significant ‘*LL-*zero’ effect on the drift rate and a single case of a significant interaction. Thus, the pattern for the drift rate perfectly mirrors what we observe for patience (the proportion of *LL* choices). Indeed, the first-column graphs in Figure 2 (drift rate results) resemble those in Figure 1 (choice results).

Across most studies, we also observe that the *SS-*zero and *LL-*zero both (independently) increase the threshold (*a*), which suggests that individuals either need more information to make choices, or are more willing to put time/effort into making choices, when either zero is present. We do not see consistent effects of the conditions on nondecision time (*t*) or bias (*z*). In particular, nondecision time (*t*) is rarely (reliably) influenced by the opportunity cost reminders, while bias (*z*) is roughly equally likely to respond to the *SS-*zero and *LL-*zero, though not always in a consistent way.

In sum, of the four DDM process parameters, only the drift rate can explain the asymmetric *SS-*zero effect. The unique effect of the *SS-*zero on drift rate, but not bias, indicates that rather than experiencing an immediate, a priori bias towards the *LL* option in the presence of the *SS*-zero, participants experience a dynamic preference shift, with stronger evidence accumulation over time leading to increased choice of the *LL* option (Dunovan et al., 2014). In particular, the evidence people accumulate comes from the *SS-*zero, which is a reminder of the future opportunity cost (i.e., forgone future payoff) associated with choosing *SS*.

**Discussion**

The ability to focus on the future and inhibit short-term desires is associated with a variety of desirable outcomes for individuals (e.g., Duckworth, Quinn, & Tsukayama, 2012; Duckworth et al., 2019; Lawrance, 1991; Meier & Sprenger, 2010; Moffit et al., 2010; Reimers et al., 2009), societies (e.g., Noguchi et al., 2014), and future generations (Hardisty et al., 2012; Hardisty & Weber, 2009). Yet, people nonetheless tend to be far too impatient, even when they stand to gain much from delaying gratification (e.g., Frederick et al., 2002; Read, 2004; Urminsky & Zauberman, 2014). This naturally raises the question of why we tend to be patient and, in particular, what features of human cognition hinder the ability to delay gratification.

We recently proposed a novel explanation for this generalized tendency toward impatience (Read et al., 2017): a fundamental asymmetry in the attention given to the opportunity costs of immediate versus delayed gratification. Specifically, whereas people are naturally attuned to the fact that waiting for larger-later (*LL*) outcomes means forgoing smaller-sooner (*SS*) ones, they are far less attentive to the “obvious” fact that choosing *SS* means forgoing *LL*. This “tipping of the scales” by our cognitive system, in favor of attending to immediate, but not future, opportunity costs, helps explain why self-control often requires significant mental effort (Inzlicht, Shenhav, & Olivola, 2018). Moreover, it parallels long-held ideas (Akerlof, 1991; Böhm-Bawerk, 1890; Fisher, 1930) and recent theorizing (Pronin, Olivola, & Kennedy, 2008; Urminsky, 2017) that the current concerns of our ‘present self’ typically outweigh those of our ‘future selves’. Fortunately, this asymmetry can be corrected (at least to some extent) by simply highlighting the future opportunity cost that people tend to neglect: merely reminding them that choosing the *SS* payoff means they will receive “nothing” or “$0” later (i.e., they will have to forgo the *LL* payoff) increases patience. By contrast, and in line with the proposed asymmetry, similarly highlighting the opportunity cost of choosing *LL* (e.g., receiving “nothing” or “$0” today) has no impact on their preferences, presumably because this information is psychologically redundant (i.e., people are already fully aware of this consequence). This pattern of results doesn’t just reveal a useful “nudge” for increasing self-control and patience; the asymmetric effect of highlighting opportunity costs associated with *SS* and *LL* options (the “asymmetric subjective opportunity cost” or “ASOC” effect) also points to the hypothesized fundamental asymmetry in our cognitive system, regarding the amount of attention (or ‘weight’) the mind allocates to the costs of pursuing short-term versus long-term goals.

Until now, we could only indirectly speculate about the nature of the process that drives this asymmetry. In this paper, we used a combination of process measures, experimental manipulations, and cognitive modeling to clarify the psychological nature of this asymmetry and the ASOC effect.

First, we examined whether the ASOC effect reflects a more explicit, deliberate (‘System 2’) process or a more implicit (‘System 1’) process. Study 1 used a cognitive load manipulation to show that the ASOC effect is observed even decision-makers have limited cognitive resources to process the implications of the *SS* zero. Study 2 used a thought-listing task to show that the presence of the *SS* zero cost reminders does not lead to greater explicit consideration of the *SS* opportunity cost during the decision process (hardly any of our participants mentioned anything related forgoing an option). Finally, Study 3A revealed that participant fail to anticipate the ASOC effect. Taken together, these studies indicate that the ASOC effect is the product of an implicit (‘System 1’) process.

Next, we examined whether the ASOC effect reflects a short-lived mechanism whose impact is limited to the immediate decision context, or a longer-acting process whose influence persists. Studies 3A and 3B revealed that the ASOC effect spills over from an initial set of decisions (in which participants were exposed to the *SS* zero) to a separate set of decisions (in which the *SS* zero was absent), indicating that the ASOC effect is a ‘sticky’ phenomenon that endures in the mind, and can thus impact unrelated, future choices, even after *SS* opportunity cost reminders have disappeared.

Finally, we used drift diffusion modeling (“DDM”) to test two alternative psychological accounts of the ASOC effect. Specifically, the *SS* zero could increase patience either directly, by biasing preferences toward the *LL* option without promoting further consideration of the *SS* or *LL* attributes, or indirectly, by increasing consideration of the negative consequences of selecting the *SS* option. We started by collecting choice and choice time data in three new studies: Study 4 presented participants with a large number of choice items (27 per person), so that we could precisely estimate all four DDM model parameters for each decision-maker; Studies 5A and 5B, went a step further by presenting choice items for which the *SS* and *LL* opportunity cost reminders were perfectly matched in terms of their word and character counts, to eliminate systematic differences in reading time. We carried DDM analyses of these three new studies, as well as our other, prior studies (which measured choice times). The DDM analyses revealed that the *SS* opportunity cost reminder increases the drift rate, indicating that evidence accumulates faster over time in the presence of the *SS* zero, and that this evidence accumulation favors the *LL* option. By contrast, the bias parameter, which tracks preference shifts unrelated to deliberation, is not uniquely influenced by the *SS* zero. Taken together, these results indicate that the ASOC effect operates by increasing consideration of the opportunity costs associated with *SS* and *LL*, so that greater deliberation time is associated with more *LL* choices, and *not* by automatically shifting (i.e., “priming”) preferences toward the *LL* option (without any additional consideration of the option attributes).

In sum, our studies clarified the psychological process underlying one of the contributing factors[[8]](#footnote-8) to people’s tendency toward impatience: A fundamental asymmetry in the consideration given to the consequences of satisfying immediate desires versus delaying gratification. We showed that people are spontaneously, and implicitly, attuned to the latter, but far less so to the former. Moreover, these findings help explain why people don’t correct this asymmetry: because it operates implicitly, they fail to notice that their attention is biased toward short-term rewards.

**Conclusion**

Our ability to self-regulate is hindered by a fundamental asymmetry in the attention (i.e., or ‘weights’) we give to the opportunity costs of pursuing smaller short-term gains versus larger, future benefits. Fortunately, merely reminding ourselves of the “obvious” fact that smaller sooner rewards come at the cost of forgoing larger later earnings is enough to rebalance our attention, so that we are more attuned to the consequences of impulsive choices. The rebalancing by subtly highlighting the opportunity costs of choosing smaller-sooner rewards—the ASOC effect—is driven by an implicit (‘System 1’) process that works by getting decision-makers to more carefully consider the future consequences of choosing immediate gratification, and whose influence lingers in the mind to impact future decisions.

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Table 1 - The four core opportunity cost framing conditions

|  |  |  |  |
| --- | --- | --- | --- |
|  |  | *SS* opportunity cost (*SS* zero) | |
|  |  | Implicit (Absent) | Explicit (Present) |
| *LL* opportunity cost (*LL* zero) | Implicit (Absent) | Hidden zero:  $100 today  OR  $150 in one year | *SS* zero:  $100 today and $0 in one year  OR  $150 in one year |
| Explicit (Present) | *LL* zero:  $100 today  OR  $0 today and $150 in one year | Explicit zero:  $100 today and $0 in one year  OR  $0 today and $150 in one year |

Table 2 – 2×2 ANOVA analyses of the drift diffusion model (DDM) parameter estimates.



*Note*. Dark grey cells represent significant effects (*p* < .05) and light grey cells represent marginally significant effects (*p* < .1).

Figure 1



Choice proportions, as a function of condition, for each study. Error bars represent standard error of the mean.

Figure 2



Drift diffusion model (DDM) parameter estimates, as a function of condition, for each study. Error bars represent standard error of the mean. For Studies 1 and 2, there were too few trials (i.e., choice items) per participant to estimate all four DDM parameters, so we only estimated (and report) the first three parameters.

Figure 3

|  |  |  |
| --- | --- | --- |
| Low cognitive load pattern |  | High cognitive load pattern |
|  |  |  |

Examples of visual matrices used in low cognitive load trials (left) and high cognitive load trials (right), in Study 1.

Figure 4



Illustrations of the drift diffusion model (DDM) process and parameters for a single decision between an *SS* and *LL* option. (A) depicts a DDM choice process with basic threshold boundary (*a*) and nondecision time (*t*) parameter values. (B) depicts a shorter nondecision time (*t*), adds a negative bias (*z*) favoring *SS*, and adds a positive drift rate (*v*) favoring *LL*. (C) depicts a longer nondecision time (*t*), a wider threshold boundary (*a*), adds a positive bias (*z*) favoring *LL*, and adds a negative drift rate (*v*) favoring *SS*.

1. In economics parlance, *opportunity costs* are what we have to forgo whenever we choose particular options or actions. More formally, the opportunity cost of choosing a particular option is the value of the best alternative option we forgo (given that we can’t have everything). Thus, for a decision-maker faced with two options, *X1* and *X2*, the opportunity cost of choosing *X1* is *X2*, and the opportunity cost of choosing *X2* is *X1*. In standard intertemporal choice tradeoffs, a smaller outcome that occurs sooner (*SS*) is pitted against a larger one that occurs later (*LL*). In these two-option intertemporal decisions, the opportunity cost of choosing *SS* is *LL* and the opportunity cost of choosing *LL* is *SS*. [↑](#footnote-ref-1)
2. Study 3B was the only study in which we did not record choice times (this study was carried out before all the others, and before we had thought to examine choice times). [↑](#footnote-ref-2)
3. The extremely high p-values are the result of the extremely low base-rates (7 out of 622). Two cells had values of zero, which created extremely large standard errors when looking at differences between conditions (for example, the beta estimate for the effect of *LL* zero on mentions of “nothing now” was 18.1, with a standard error of 5,684.1). Therefore, the Wald statistics were near (or at) zero, and the p-values were near (or at) one. Overall, the results of the model should be interpreted with caution, as it is difficult to draw any kind of inference with such low base-rates. [↑](#footnote-ref-3)
4. Also, a text-based sentiment analysis of the thought contents revealed no significant effects of the *SS* zero on the concreteness or affective valence of the thoughts that participants generated. [↑](#footnote-ref-4)
5. Unsurprisingly, when HiddenC came first (in Block 1) there were no effects of Block 2 (future) treatments on (current) HiddenC patience: no effect of *SS* zero, *F*(1, 229) = .08, no effect of *LL* zero, *F*(1, 229)= 1.86, *p* = .17, and no interaction, *F*(1, 229)= .21. [↑](#footnote-ref-5)
6. The two blocks were separated by the same filler task used in Study 3A. [↑](#footnote-ref-6)
7. Unsurprisingly, the main effect of *SS* zero is (highly) significant if we combine data from Studies 5A and 5B (since participants in both studies were presented with essentially equivalent choice items), or if we combine the first and second set of choice items. [↑](#footnote-ref-7)
8. Of course (and this goes without saying), we are *not* claiming that the asymmetry in attention to intertemporal opportunity costs explains all, or even most, of the variance in the ability (or lack thereof) to exercise self-control and delay gratification. Clearly, a host of other affective, environmental, and individual factors also play important roles. [↑](#footnote-ref-8)