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Time Isn't of the Essence: Activating Goals Rather than Imposing Delays Improves Inhibitory Control in Children

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Abstract

Is it easier to inhibit inappropriate behaviors if we pause before acting? An important finding for theory and intervention is that children's inhibitory control improves if an adult imposes a delay before children can act. Such findings have suggested that the passage of time allows impulsive urges to passively dissipate. However, prior studies that imposed delays also added reminders about what children should be doing, which may have aided children's activation of goal-relevant information. We tested this possibility by independently manipulating delays and task-reminders, and measuring 3-year-old's abilities to inhibit opening boxes in a go/no-go box-search task. Task-reminders, but not adult-imposed delays, improved children's response inhibition. However, as in prior work, children's spontaneous delays predicted better response inhibition. These results challenge a causal role for the passage of time, suggest that spontaneous delays index other processes that improve inhibitory control, and highlight the importance of goal-activation in developing inhibitory control.

Keywords

cognitive development; response inhibition; self control

Consider sitting at a table and encountering a tempting bowl of sweets that you know you should avoid. The bowl is directly in front of you. Or, your path is briefly blocked by a glass pitcher, which is then removed. Which scenario makes it easier to avoid snacking?

Questions about inhibitory control are often investigated in children, who struggle to avoid impulsive actions, whether eating cookies right before dinner or running after a ball into the street. Impulsive actions can be triggered without careful thought, because the actions are well-practiced or compelling for other reasons (e.g., salience, pleasure, biological predisposition (Macrae & Johnston, 1998; Simpson & Riggs, 2007)). Children persist in such behaviors even when they can indicate how they *should* behave instead. For example, despite answering questions about task rules correctly, children often open boxes that should

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Author Contributions
J.E. Barker and Y. Munakata developed the study concept and design. Data collection and analysis were performed by J.E. Barker. Both authors contributed to the conceptualization and writing of this manuscript.

not be opened in a box-search task (Figure 1a) and give incorrect, well-practiced responses in a day-night Stroop task (Figure 1b) (Simpson & Riggs, 2005, 2007).

Understanding why children struggle to inhibit inappropriate behaviors is of great interest to scientists, educators, parents, and policy-makers (Diamond & Lee, 2011). Inhibitory control is not only important in the moment, but predicts important outcomes decades later (Moffitt et al., 2011), such that targeting interventions in childhood could yield benefits. Moreover, clear failures in cognitive processes during development may provide insight into processes that persist in more subtle forms into adulthood (Diamond & Kirkham, 2005; Hermer & Spelke, 1996).

A key finding for theory and intervention is that children's inhibitory control seems to improve when a delay is imposed before they can act, as in the pitcher-blocking-sweets example above. Children make fewer errors when an experimenter sings a ditty after showing each day-night Stroop image (Diamond, Kirkham & Amso, 2002), and when an experimenter reveals the box-search box without a task cue, then pauses before placing the cue on the lid (Simpson et al., 2012). Children's *spontaneous* delays before acting also predict better inhibitory control (Gerstadt, Hong & Diamond, 1994; Wiebe, Sheffield & Espy, 2012).

Why do delays help? One view is that delays allow urges to passively dissipate (Simpson et al., 2012). Specifically, impulsive actions (such as darting into the street) and correct responses (looking both ways first) compete within a race model. Impulsive responses are activated early, and rapidly reach threshold for emitting a response, before beginning to weaken. Correct responses are activated later, and reach the response threshold more slowly. Delays allow time for an impulsive response to passively dissipate, so that the correct response can outcompete it. Such dissipation is thought to occur even when the stimulus that caused the initial impulsive response is no longer visible or attended to. Consistent with this idea, delays improve inhibitory control in the box-search task even when children are engaged in a distractor task that leads them to look away from the target box during the delay (Simpson et al., 2012).

However, delays may have appeared to benefit inhibitory control only because they were confounded with factors that improved children's activation and maintenance of goal-relevant information, which can support appropriate actions in the face of impulsive alternatives (Bunge & Zelazo, 2006; Miller & Cohen, 2001; Munakata et al., 2011). Specifically, manipulations of delays in childhood inhibitory control tasks have also included additional reminders – akin to a dieting sticker affixed to the pitcher. In the day-night Stroop task, the delaying ditty instructed children to "Think about the answer, don't tell me!", which may have cued participants to retrieve task-relevant information (Beck, Carroll, Brunsdon & Gryg, 2011; Diamond et al., 2002). In box-search delay conditions, 1) the experimenter stated additional instructions (e.g., "You mustn't open the box until I put the shape on top because only when the shape is on can you tell if there is a sticker inside"); 2) the experimenter drew attention to the cue by placing it on the box in view of the child; and 3) the distractor task was to find the cue hidden in one of the experimenter's hands – again drawing attention to it (Simpson et al., 2012). Similarly, children's spontaneous delays may predict better inhibitory control because the passage of time co-occurs with goal-oriented processes that support inhibitory control.

We tested these contrasting perspectives by independently manipulating delays and task-reminders, within a box-search task (Table 1). Three-year-olds were allowed to reach either immediately or after a delay, and were either reminded of goal-relevant information or not reminded. Children's spontaneous delays before acting were also measured. Within a passive-dissipation framework, both experimentally-imposed and spontaneous delays should predict inhibitory control. Within a goal-oriented framework, task-reminders and only spontaneous (but not experimenter-imposed) delays should predict children's inhibitory control.

Method

Participants

One-hundred-fifty 3-year-olds (M_{age} = 3.52; range= [3.00, 3.99]; males = 72) participated in the study, excluding 14 children dropped for non-participation (i.e., not opening any practice or test boxes during the entire experiment) (*n* = 7), failure to comprehend study instructions (*n* = 3), experimenter error (*n* = 2), fussiness (*n* = 1), and parent interference (*n* = 1). Sample sizes (*n* = 30 per condition) were selected based on those reported in Simpson et al. (2012). Conditions were matched for age and gender. All children spoke English as a first language. Participants were recruited from either a laboratory subject pool (20 per condition) or from families visiting a children's museum (10 per condition). Museum participants completed the experiment in a quiet museum area; laboratory participants completed the experiment in a standard testing room. Informed consent was obtained for all participants, and parents were notified that they could cease participation at any point during the study. Parents of children recruited through the laboratory pool received $5 for travel expenses. All sessions were videotaped, except in cases of parent refusal (*n* = 4). Accuracy data for children whose parents refused taping was hand-recorded after each trial (as in Simpson et al., 2012).[1]

Materials

The box search apparatus replicated that described in Simpson and colleagues (2012). For each of two sets of test boxes, eight white boxes (each 60 mm cubed; lids 65 mm wide × 65 mm long) were spaced equally along a white cardboard mounting strip (75 mm wide × 700 mm long). Across all trials, the 'go' cue was a blue square and the 'no-go' cue was a red triangle (each 40 mm per side). Four practice boxes (of the same dimensions) were mounted to a shorter piece of cardboard (75 mm wide × 250 mm long). Separate strips of cardboard were used to cover test and practice boxes so that boxes remained hidden until revealed by the experimenter. In two conditions, the experimenter used an opaque, colored screen (125 mm width by 150 mm length) to obscure children's view of the target box (prior to cue placement in the Delay with Reminders condition, and during cue placement in the Delay No-Reminders condition).

[1]Accuracy data for museum participants was captured using both hand-coding and video, because camera angles occasionally obscured child reaches.

Procedure

Each child completed one of five conditions: Immediate No-Reminders, Immediate with Reminders, Delay No-Reminders, Delay with Reminders, or Delay with Reminders with screen. (The final condition was included to test whether the introduction of the screen in the Delay No-Reminders condition independently influenced child performance, over and above any effects attributable to delays.) Assignment to the first three conditions (Immediate with Reminders, Immediate No-Reminders, and Delay with Reminders) was random. Recruitment for the fourth (Delay No-Reminders) and fifth (Delay with Reminders, with screen) conditions occurred later, and was partially random: approximately 25 percent of participants in the former condition and 50 percent of the participants in the latter condition were assigned while no other conditions were being run.

The box search apparatus and general procedure replicated Simpson and colleagues (2012) (Figure 1). Each condition included demonstration and practice phases. During the demonstration phase, the experimenter showed the child example 'go' and a 'no-go' boxes and explained that boxes with squares on top contained stickers, whereas boxes with a triangle on top did not. The experimenter then stated condition instructions (Table 2) and sequentially presented the child with four practice boxes, alternating between go and no-go trials. Standardized feedback was provided after each practice trial.

After the practice trials, the experimenter repeated the condition task instructions, and presented the child with the first of two sets of test boxes. Go and no-go cues (8 go, 8 no-go) were presented in the same pseudorandom order in all conditions. Children were given 3 s to initiate a reach towards the box. If the child did not initiate a reach during the 3 s interval, the experimenter revealed the next box. The experimenter provided no feedback in test trials, except in cases when children erroneously opened boxes before the cue was placed on the lid in the Delay with Reminders condition (Table 2). Experimenters recorded errors if, during the 3 s interval following cue presentation, a child failed to open a box on a 'go' trial, or erroneously opened a box on a no-go trial.

Conditions (Table 2)

Immediate No-Reminders (as in Simpson et al., 2012)—The experimenter revealed the box and the cue simultaneously (Figure 2). Children were allowed to reach immediately, and received standard verbal instructions.

Immediate with Reminders—The experimenter revealed cues and boxes simultaneously, just as in the Immediate No-Reminders condition. However, two manipulations were introduced to mimic the reminders incorporated in Simpson et al.'s (2012) delay condition ('Delay with Reminders' in the present study): children received additional instructional reminders during practice trials, and the experimenter quickly tapped the cue once with her right index finger as she revealed the box (consistent with placement of the cue in view of the child in the Delay with Reminders condition; Figure 2).

Delay No-Reminders—The experimenter revealed the box, ensuring it was momentarily visible to the child, so that according to passive-dissipation accounts, the impulsive response to reach should be activated and then passively dissipate. The experimenter then placed a screen between the box and the child so that the box lid was not visible from the child's perspective (Figure 2). Next, the experimenter placed the cue on the box lid, and removed the screen so that the child could reach. The delay period between box reveal and screen removal lasted ~2.5 s. Children received standard instructions, slightly modified to indicate that they would see a screen.

Delay with Reminders (as in Simpson et al., 2012)—The experimenter revealed the box, waited ~2.5 seconds, then placed the cue on the box lid in view of the child (Figure 2). During practice trials, children received additional instructional reminders relative to the Immediate No-Reminders condition. Boxes opened during the delay period were counted as errors.

Delay with Reminders, with screen—This condition was conceptually and methodologically like the Delay with Reminders condition, but added a screen manipulation to make this condition more comparable to the Delay No-Reminders condition. The experimenter revealed the box, immediately placed the screen between the box and the child so that the box lid was not visible from the child's perspective, then removed the screen before placing the cue so that the child could observe cue placement (Figure 2). The delay period between box reveal and cue placement lasted ~2.5 s. Children received instructions as in the Delay with Reminders condition, modified as in the Delay No-Reminders condition to indicate that they would see a screen.

Coding

Two trained coders who were blind to all experimental hypotheses coded videos for 137 participants. (Videos for 9 participants were not coded because camera angle obscured reaching behavior (n = 5) or equipment malfunctioned (n = 4).) A third trained, blind coder coded videos for 40% (n = 54) of participants. For each participant, coders assessed accuracy and reaction times on the trials where children reached. Reaction times were measured in terms of the interval on each trial between the moment that the cue was visible on top of the box and the time that children began to lift the box lid. Inter-rater reliabilities were high (primary coding pair r= .95; primary-secondary coding pairs = .87; .91). RTs were averaged across raters, and then averaged across trials within participants to form composite go-trial and no-go trial RT scores.

Analysis Approach

Accuracy data were analyzed via generalized linear mixed effects models (GLMM), a preferred alternative to ANCOVA for analyzing repeated, within-subject binary dependent variables (Jaeger, 2008). To determine how trial type and condition influenced the probability of correct performance on individual trials, accuracy (correct=1, incorrect=0) was predicted using trial type (go, no-go), condition (delay yes/no, reminders yes/no), experimental setting (museum/laboratory), and age (in days, mean-centered and treated as continuous) as fixed effects, and subject as a random effect. For group-level comparisons, higher-order condition predictors (reminder and delay indicators) were dropped from the model and replaced with Helmert-contrast-coded group-level categorical predictors.

Accuracy data showed a substantial skew (with many more accurate trials than inaccurate trials), which can bias standard GLMM estimation procedures (i.e., Laplace-approximation-derived estimates; Pinheiro and Chao, 2006). Therefore, model parameters and confidence intervals were generated using Bayesian Markov Chain Monte Carlo (MCMC) estimation via the R statistics package MCMCglmm version 2.18 (Hadfield, 2010). Weak, independent priors were established for each fixed effect coefficient using the Gelman method (Gelman, Jakulin, Pittau, & Su, 2008), specified as a scaled t-distribution with a single degree of freedom, with a scale of 10 for the intercept and 2.5 for the regression parameters. Prior standard deviations were scaled as $pi^2/3+v$, where v is the total variance due to model random effects. Models were run for 30000 iterations. Burn-in rates ranged from 3500 to 4500 iterations. They were adjusted based on visual inspection of model posterior distributions and examination of non-independence between successive iterations.

All reaction time analyses were conducted using standard linear regression using logged, mean participant reaction times. For all analyses, outlying observations were identified (Cook's D > 3 standard deviations above the mean) and removed. This resulted in the exclusion of no more than five cases from any analysis.

Results

We first present preliminary analyses of accuracy and RT focusing on factors of screen presence, age, gender, experimental setting, and go-trial performance. This is followed by the key analyses of no-go trial accuracy, focusing on effects of our experimental manipulations and tests of spontaneous delays as a predictor.

Preliminary Analyses

The introduction of a screen did not influence accuracy over and above other factors ($p > .250$). However, children were slower to respond when a screen was present ($B = .21$; 95% CI = [.10 – .32]; $F(1, 127)=14.61$; $p < .001$), so screen presence was included as a predictor in all RT models.

Younger children (as indexed continuously, in days) made more errors across all trials (Odds-ratio (OR) = .011; 95% CI =[.004 – .018]; $p < .001$) and responded more quickly on go-trials ($B_{Day} = -.0007$; 95% CI =[−.0014 – −.0001]; $F(1, 127) = 4.64$; p = .033) relative to older children. Gender did not significantly predict performance or RTs (ps > .250). Children tested in the museum were less accurate and faster to respond on go-trials than children tested in the laboratory (accuracy: OR = −.84; 95% CI =[−1.55 – −0.14]; $p = .018$; go-trial RT: $B = -.15$; 95% CI =[−.23 – −.07]; $F(1, 127)=13.72$; p < .001). There were no significant interactions between experimental setting and performance in Reminders ($p > .250$) or Delay ($p > .250$) conditions, and pairwise analyses indicated that the effect of museum participation on performance did not vary across conditions (ps > .250). Therefore, we report combined data across settings, and include setting and age as predictors in all models.

Children made few errors on go-trials ($M_{Imm+Rem}$=99%; $M_{Delay+Rem}$=99%; $M_{ImmNoRem}$= 100%; $M_{DelayNoRem}$=97%; $M_{Delay+RemScreen}$=97%), and were more accurate, relative to no-go trials (go-trial M_{Acc}=98%; no-go trial M_{Acc}=75%; OR = 5.58; 95% CI =[4.97–6.21]; $p < .001$). Children in Reminders conditions demonstrated better performance on go trials than children in No-Reminders conditions (OR = .88; 95% CI =[.12 – 1.64]; $p = .027$), and reached more quickly (average Reminders RT=.96 s; $B = -.20$; 95% CI = [−.28 – −.12]; $F(1,127) = 24.58$; $p < .001$). Children in Immediate conditions did not show significant differences in RT (p = .194) or accuracy (p > .250) on go trials relative to children in Delay conditions.

No-Go Trial Performance

Reminders—As predicted by goal-oriented accounts, children erroneously opened boxes less often when given reminders, regardless of length of delay. That is, children in Reminders conditions ($M_{Imm+Rem}$=81%; $M_{Delay+Rem}$=83%; $M_{Delay+RemScreen}$=80%) demonstrated better no-go accuracy than children in No-Reminders conditions ($M_{ImmNoRem}$ =63%; $M_{DelayNoRem}$=68%) (OR = 1.09; 95% CI =[.36 – 1.92]; $p = .014$; Figure 3).

Planned, group-level contrasts also indicate that reminders, but not delays, improved children’s response inhibition. Children in the Immediate with Reminders condition made fewer errors than children in No-Reminders conditions, both within and across levels of delay (Immediate contrast: OR = −2.73; 95% CI =[−5.42 – −.15]; $p = .035$; Delay contrast: OR = −2.59; 95% CI = [−5.10 – −.06]; $p = .044$). Corresponding contrasts between the Delay with Reminders condition and No-Reminders conditions showed the same pattern, though they did not fall below the .05 alpha threshold (Delay contrast: OR = −1.95; 95% CI =[−4.24 – .26]; $p = .093$; Immediate contrast: OR = −1.99; 95% CI = [−4.06 – .19]; $p < .069$). The performance advantage associated with reminders was not improved or attenuated when reminders were combined with a delay (p > .250).

Experimental delays—In contrast to claims based on prior work, delays did not benefit response inhibition independent of task reminders. Children in Delay conditions performed no better than children in Immediate conditions ($p > .250$), and delays did not benefit performance within levels of reminders (Delay with Reminders - Immediate with Reminders contrast: $p > .250$; Delay No-Reminders - Immediate No-Reminders contrast: $p > .250$).

Spontaneous Delays—However, consistent with past findings (Gerstadt et al., 1994; Wiebe et al., 2012), children who committed fewer inhibitory errors on no-go trials spontaneously reached more slowly on go-trials ($B = 0.03$; 95% CI =[.01–.06]; $F(1,127) = 6.52$; $p = .012$). To explore potential implications, we tested whether benefits associated with spontaneous delays varied across Immediate and Delay conditions. Passive dissipation accounts predict that spontaneous delays should be associated with better inhibitory control in Immediate, rather than Delay conditions, because in Delay conditions, experimenters have already imposed the delays that should allow impulsive responses to dissipate. Spontaneous delays were equivalently predictive of performance in Delay and Immediate conditions (Delay condition x accuracy $p > .250$). Thus, spontaneous delays do not seem to improve no-go accuracy by allowing impulsive responses time to passively dissipate.

We considered whether task reminders may have improved inhibitory control by encouraging children to spontaneously delay (or by inadvertently imposing a delay). This was not the case, given that children in Reminders conditions reached more quickly on go-trials than children in No-Reminders conditions.

Discussion

Does the passage of time make it easier to inhibit impulsive actions? What are implications for intervention? Our findings challenge current theorizing that children can better control their impulses if they are simply made to wait before acting. We demonstrate that *imposed* delays do not benefit 3-year-old's response inhibition; instead, goal-relevant reminders drive benefits. This finding is compatible with successful goal-oriented interventions in other domains, including consumption of undesirable foods in dieters (Kroese, Adriaanse, Evers & De Ridder, 2011) and self-regulation during peer-to-peer reading exercises in preschoolers (Bodrova, Leong & Akhutina, 2011). Nonetheless, we also find that children's inhibitory control is predicted by their *spontaneous* delays. Such delays may reflect the time used for goal-oriented processes, such as detecting and responding to conflict (e.g., Kerns et al., 2004; Rueda, Posner, Rothbart & Davis-Stober, 2004). Thus, the passage of time may be a symptom rather than a cause: Children can take time to think, but giving them time does not guarantee that they will think. These results highlight the importance of goal-activation in developing inhibitory control, and challenge a causal role for the passage of time.

Passive dissipation accounts might be revised to try to explain our findings, but we believe that such revisions would negate the explanatory power of these accounts and make them unfalsifiable. For example, revised accounts could posit that it is not simply the passage of time that allows an urge to passively dissipate; instead, that time must be used to think about the impulsive behavior (i.e., an 'active' dissipation account). The opaque screen we used to impose a delay, and to block children's view of the cue-placement to avoid providing a reminder of it, may have prevented children from thinking about their urge to reach, preventing dissipation. Testing this revised account would require imposing a delay before participants could act, while ensuring that they continue to think about the impulsive behavior across the delay — *without* providing them with goal-relevant reminders. These conditions seem difficult if not impossible to achieve. For example, using a transparent screen could allow participants to keep thinking about the impulsive behavior, but would prevent placing the cue out of sight. Furthermore, any attempt to prevent participants from acting (e.g., by distracting or restraining them) could be argued to reduce the extent to which they think about the impulsive behavior, such that failures to find benefits of delays could not falsify these revised theories. Thus, while future work could attempt to explore such ideas, our findings challenge existing, testable dissipation accounts.

A challenge for future work is further specifying the goal-oriented processes that support inhibitory control, how these change with development, and the resulting implications for intervention. For example, we have emphasized the joint influence of verbal and physical reminders, but their relative effectiveness may vary with age. At age 3, children tend to *reactively* engage cognitive control, in the moment as needed. By age 6, children increasingly engage control *proactively*, in advance of needing it (Chatham, Frank, & Munakata, 2009; Lucenet & Blaye, 2014). Thus, the highlighting of cues during the current task may have mattered more, by influencing young children's control in the moment, when they were more likely to engage it; the additional instructions at the outset may have mattered less, since children were unlikely to proactively maintain this information into the testing period.

Conversely, older children who proactively prepare may not need reminders in the moment, or may even be impaired by them. Such patterns have been observed in children's task-switching (Chevalier, Huber, Wiebe & Espy, 2013). Older children may instead benefit more from interventions that target their proactive processing (e.g., Chevalier, Chatham & Munakata, 2014; Winter & Sheridan, 2014), such as a proactive monitoring for environmental cues that signal the need for inhibitory control (Chatham et al. 2012; Dodds, Morein-Zamir & Robbins, 2011), because older children are more likely to engage and improve proactive processes during such interventions. Thus, the interventions that are most effective for inhibitory control may depend upon the temporal dynamics of goal-oriented processes at that age. It is even possible that delays could sometimes benefit older children (and adults), who might be more likely to use this time to adapt their behavior toward their goals (e.g., Wiersema, van der Meere, & Roeyers, 2007). Delays might also benefit performance in more complex tasks where the time could be used to work out answers, which might explain benefits observed with delays in counterfactual reasoning tasks (Beck et al., 2011). Such goal-oriented possibilities should be tested across a broader range of ages, tasks, and real-world contexts, and in other domains thought to tap inhibitory control, such as theory of mind (Carlson & Moses, 2001) and delay of gratification (Eigsti et al., 2006).

An important question for future work concerns the factors that lead some children to spontaneously engage processes that decrease impulsivity. For example, why did only some children in our study show the slowed responding on go trials that predicted better performance on no-go trials? Why do only some children adopt helpful strategies in other situations, such as clasping onto a chair to avoid responding impulsively during a 'Simon Says' game (Jones, Rothbart & Posner, 2003), or re-conceptualizing tempting treats (e.g., by visualizing a marshmallow as a less-tempting cloud) to avoid eating them in favor of larger, future rewards (Mischel & Mischel, 1983)? Some of these spontaneous strategies can be trained to improve children's performance. For example, instructing children to visualize a marshmallow as another, less-tempting object (e.g., a fluffy cloud) increases their willingness to delay gratification (Mischel & Baker, 1975). Thus, attempts to separate the substance of children's spontaneous strategies from their correlates, as in the current study, may help in the design of interventions.

More generally, our findings connect with a broader intervention literature, in which training a correlate of success can fail to yield improvements and can even lead to adverse outcomes. For example, children with high self-esteem show better academic achievement (Davies & Brember, 1999), but efforts to improve academic performance by boosting self-esteem have yielded no changes or declines relative to control subjects (Forsyth, Lawrence, Burnette & Baumeister, 2007). Follow-up work has shown that self-esteem indexes (rather than influences) academic performance, because it is an outcome of strong achievement (Marsh & O'Mara, 2008). Similarly, although individuals with high working memory capacity tend to also have high general fluid intelligence, training studies that improve working memory capacity do not improve fluid intelligence; these capacities are correlated but separable (like height and weight), such that one capacity can be experimentally manipulated without influencing the other (Harrison et al., 2013). In the same way, despite the fact that children's spontaneous delays before acting are correlated with better inhibitory control, we find that imposing delays before children can act does not improve children's inhibitory control, in contrast with prior claims. Our findings instead suggest that investigations of goal-oriented processes will prove more fruitful in understanding and influencing inhibitory control than focusing on the passage of time.

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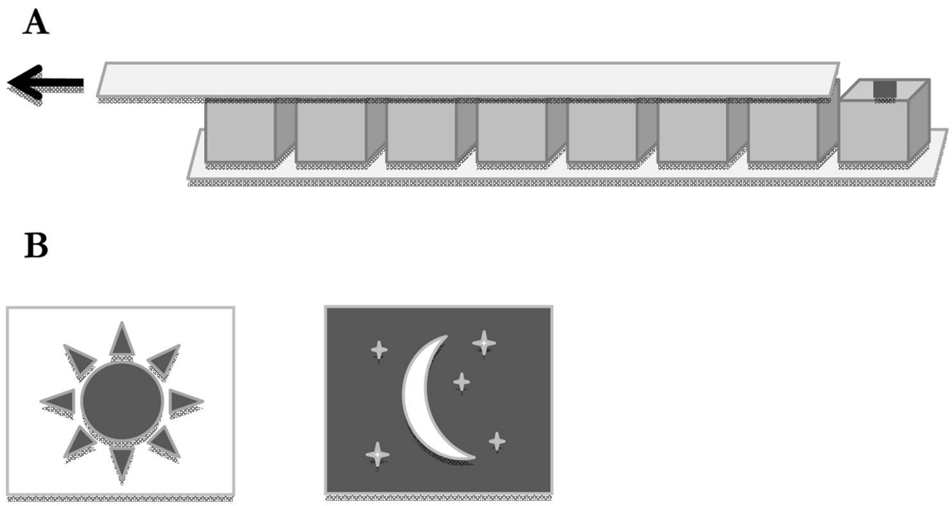

Figure 1.
Schematic representations of childhood inhibitory control tasks. In the box-search task (**A**), children are instructed to open boxes to find stickers, or leave boxes shut, based on cues placed on box lids (Simpson et al., 2012). In the day-night Stroop task (**B**), children are asked to say "day" to a picture of a moon and stars, and "night" to a picture of a sun (Gerstadt, Hong & Diamond, 1994).

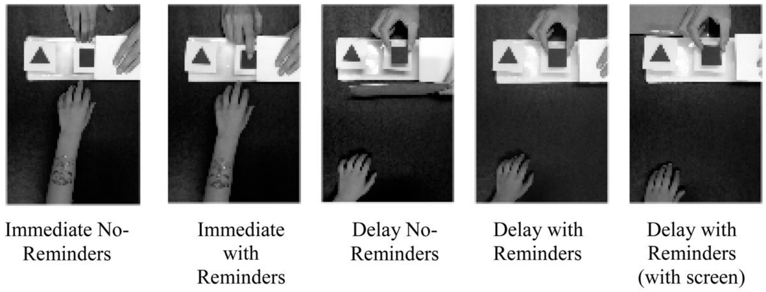

Figure 2.

Visual comparison of box search conditions. From left to right: Immediate No-Reminders; Immediate with Reminders; Delay No-Reminders (note screen blocking cue placement); Delay with Reminders; Delay with Reminders, with screen (briefly introduced during delay, then placed behind boxes, as shown).

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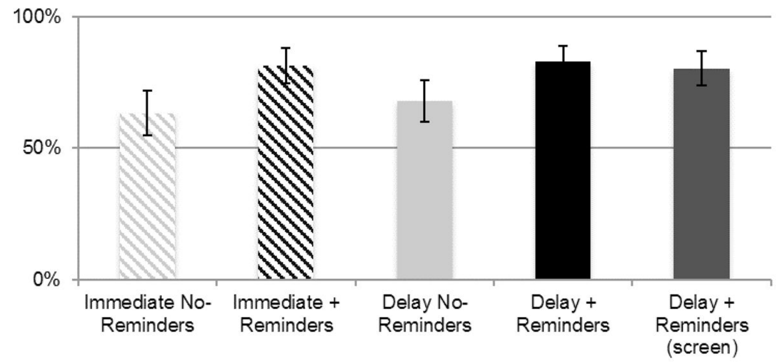

Figure 3.
Average no-go trial accuracy by condition. Children in Reminders conditions (dark shading) demonstrated significantly better performance that children in No-Reminders conditions (light shading). Average performance in Delay conditions (solid) did not differ from average performance in Immediate conditions (striped). Error bars indicate 95% confidence intervals.

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Table 1

Passive-dissipation and goal-oriented predictions for inhibitory control.

Condition	**Predicted Inhibitory Control Performance**	
	Passive Dissipation	***Goal-oriented***
Immediate No-Reminders (as in Simpson et al., 2012)	Low	Low
Immediate with Reminders	Low	High
Delay No-Reminders	High	Low
Delay with Reminders (as in Simpson et al., 2012)	High	High

Table 2

Description of box-search experimental procedure, by condition.

Task Instructions and Cue Handling	No Delay Imposed		Delay Imposed		
	Immediate No-Reminders†	Immediate with Reminders	Delay No-Reminders	Delay with Reminders†	Delay with Reminders with screen
Demonstration Instructions	Standard: "See these two boxes? One has a square and the other has a triangle on the top. If there is a square on the lid then there's a sticker inside the box, but if there is a triangle on top then there's no sticker in the box. So if you open the boxes with squares on top you'll win stickers! But leave the triangle boxes closed because there aren't any stickers in them."	Standard + **"Make sure you wait to open the box until you see the right shape!"**	Standard + **"Now we're going to play a game where we look at this** [shows screen], **then open boxes."**	Standard + **"Don't open the box until I put the shape on top, because only when the shape is on can you tell if there is a sticker inside."**	Delay with Reminders instructions + **"Now we're going to play a game where we look at this** [shows screen], **then open boxes."**
Practice Feedback	Standard: **Correct go:** "That's right! You get a sticker when you open that one." **Correct no-go:** "That's right! You shouldn't open that one because there's no sticker inside." **Incorrect no-go:** "Oops! You shouldn't open that one because there's no sticker inside." **Incorrect go:** "Oops! You should open that one because there's a sticker inside."	Standard	Standard	Standard, **unless child reaches during the delay period, then: "Oops! Wait until I put the shape on top, because only when the shape is on top can you tell if there's a sticker inside!"**	Standard
Post-Practice Instructions	Standard: "Are you ready to find the stickers? Remember, open the boxes with squares on top because they have stickers inside, but leave the boxes with triangles on top closed because they are empty."	Standard + **"Make sure you wait to open the box until you see the right shape!"**	Standard	Standard + **"Don't open any boxes until I put a shape on top, because only when the shape is on top can you tell if there's a sticker inside."**	Standard + **"Don't open any boxes until I put a shape on top, because only when the shape is on top can you tell if there's a sticker inside."**
Test Feedback	None	None	None	**If child opens a box during the delay: "Don't open the box until you see the shape on top!"**	None
Experimenter handling of the cue	Fixed to lid before box revealed	**Tapped as box revealed, in view of the child**	Fixed to lid before box revealed	**Placed on lid as box revealed, in view of the child**	**Placed on lid as box revealed, in view of the child**

†Procedure as in Simpson et al., 2012.