Habitual Control of Goal Selection in Humans

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**Abstract**

Goal-directed planning is a hallmark of human behavior. Yet, formal models of goal selection show that it often carries severe computational costs. A key challenge is to understand how humans efficiently select goals from the infinite space of potential candidates. We describe a solution grounded in computational models of reinforcement learning: Habitual control over the process of goal selection. This approach exploits the computational efficiency of habits to select a goal, while preserving the flexibility of planning processes once a goal has been selected. We find experimental evidence that human participants spontaneously employ this solution. While many existing treatments of the distinction between habitual and goal-directed action emphasize their competition over behavioral control, our results illustrate a codependence between the systems in guiding human action.

**Introduction**

The distinction between habitual and planned action is fundamental to behavioral research (1-4). Habits enable computationally efficient decision making, but at the cost of behavioral flexibility. They form as stimulus-response pairings are “stamped in” following reward, as in Thorndike’s law of effect (3). Planning, in contrast, enables more flexible and productive decision-making. It accomplishes this by searching over a causal model that links candidate actions to their expected outcomes, ultimately selecting actions as a function of their anticipated rewards. However, it carries a severe computational cost: The number of candidate plans to be evaluated rapidly becomes intractable as the complexity of the model grows.

Much research aims to find computationally tractable forms of planning for complex tasks. One important result is that computational costs can be partially mitigated when planning is organized around a task-appropriate goal (5, 6). The resulting goal-specific policies are then available for reuse and recombination, further reducing computation in future planning episodes (7). Recombination is especially productive when goals are organized hierarchically (e.g., turn on the machine to grind the beans to make the coffee) (8). Thus, it is not surprising that human planning makes widespread use of hierarchical goal structure (3, 9, 10).

In one crucial respect, however, planning around hierarchical goals maintains a severe computational constraint: Although planning is easier with a goal, first an appropriate goal must be selected. Suppose, for instance, that your goal is to make a cup of coffee. What is an appropriate sub-goal to select? In principle, an infinite number of possibilities might be entertained, and each one evaluated for its long-term utility. But this merely recapitulates the dilemma of planning: Exhaustive search is not feasible. How, then, do humans efficiently alight upon an appropriate sub-goal: getting ground beans?

One potential solution to this problem is to exploit the computational efficiency of habit learning for the purposes of goal selection (11-13). Traditionally, habits are modeled as a learned association between a perceptual stimulus and motor response. This form of learning can be extended, however, to the relation between superordinate and subordinate goals: A superordinate goal can serve as the internally represented “stimulus” triggering a cognitive “response” of subordinate goal selection. Thus, for instance, the goal of getting ground beans might be “stamped in” due to the history of reward associated with selecting this goal in past coffee-making episodes. In contrast to many past treatments of habit and planning, which emphasize their competition over behavioral control (14, 15), this proposal implies a co-dependence between them (see also 16).

In order to provide a precise model of habitual control over goal selection, and to support it with experimental evidence, we adopt a formal account of habit versus planning derived from the reinforcement learning (RL) framework (17). Model-based RL maintains an explicit causal model of the world and uses it to choose actions by assessing their likely consequences. Thus, it enables goal-directed planning. In contrast, model-free RL does not maintain an explicit causal model, and therefore does not allow planning. Rather, it assigns value to candidate actions based on their context-dependent history of reward. The resulting cached policies (akin to stimulus-response habits) are globally adaptive, but may exhibit local irrationality (18, 19).

RL models are widely used in cognitive research because they capture several core features of learning and choice, including in humans (1, 15, 19). Elements of model-free RL, including prediction-error updating and temporal difference learning, are implemented in the midbrain dopamine system (20-22). Human behavior also relies extensively on model-based planning towards goals, which depends on diverse cortical and subcortical regions (4, 19, 23-25).

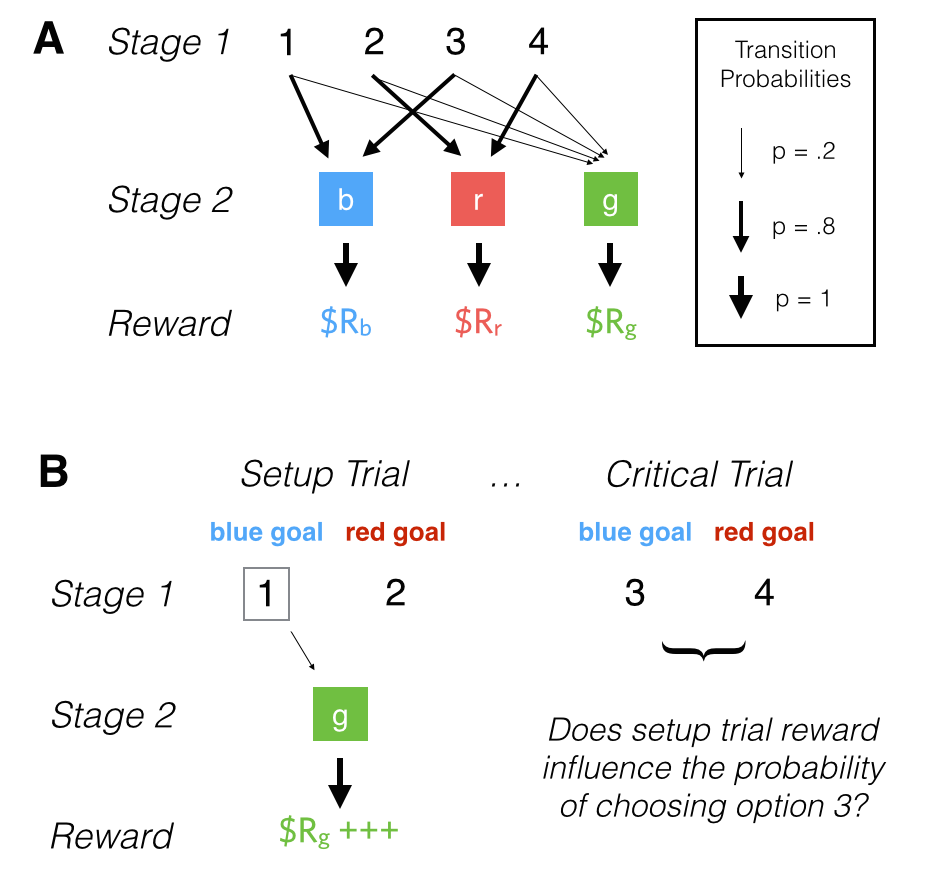
We use the RL framework to formalize and test for model-free control over goal selection. Once a goal has been selected, subsequent planning to achieve it could proceed in a model-based fashion, or by other methods. This enables computationally tractable goal selection, while maintaining the potential for planning towards the selected goal.

Colloquially, this captures the notion of a “habit of thought”: Model-free control can contribute to the effective deployment of model-based cognitive routines that facilitate productive and flexible cognition. Consistent with this proposal, recent research emphasizes the pervasive role of model-free control in related elements of higher-level cognition (26, 27), including the gating of working memory (28) and the construction of hierarchical task representations (7). These models offer an appealing functional explanation for the neuronal connections between striatum and frontal cortex (29).

The possibility of habitual control over goal selection complements several existing models, both in RL and psychology (11). Some past computational models of RL implement model-free control over hierarchical goal selection, and with promising results (12). This formal approach to model-free control over model-based planning has not, however, received a direct experimental test in humans. Meanwhile, psychological models of hierarchical planning recognize the problem of goal selection and have implemented a number of solutions, varying in scope and specificity. These include the use of hidden-layer backpropagation networks (9), Pavlovian search heuristics (30), procedural learning mechanisms (31), the chunking of action sequences (32), and other dedicated or domain-specific solutions (3, 33). Here, we aim to explicitly link a formal model of habitual control over goal selection to experimental data.

Our task is adapted from a multistep choice paradigm used in prior research (19). The original paradigm behaviorally dissociates the influence of habitual (model-free) and goal-directed (model-based) control on choice. The key feature of this paradigm is to exploit low-probability connections between behavior and reward. A mechanism employing model-free methods is sensitive to such rewards, stamping in the participant’s prior choice. In contrast, a model-based mechanism planning over a known causal model of the task would discount the link between such actions and rewards because of their known low probability of occurrence. By observing participants’ choices, the influence of model-free and model-based control can be dissociated. Several lines of convergent evidence support the alignment of these mechanisms with habitual and goal-directed control, including functional neuroimaging (19), transcranial magnetic stimulation (34), and manipulations of cognitive load (14) and stress (35), among others (36, 37).

We modified this task so that it can index not only model-free value assignment to actions (as in the original task) but also model-free value assignment to goals, which may be subsequently pursued via model-based planning (Figure 1A). At Stage 1 of each trial participants make a choice between two actions drawn from the set [1,2,3,4]. These choices trigger stochastic transitions to Stage 2 states from the set [blue, red, green]. Finally, Stage 2 states deterministically transition to three unique reward distributions. The rewards change gradually over the course of the experiment. Thus, participants are motivated to choose Stage 1 options that maximize the likelihood of transitioning to the current reward-maximizing Stage 2 state. Participants received detailed instructions and practice trials, including information about the stochastic transitions between Stage 1 and Stage 2.



**Fig. 1.** (**A**) In Experiment 1 participants performed a two-stage Markov decision task. They were presented with two possible Stage 1 actions drawn from a set of four. These transitioned with variable probabilities to a set of Stage 2 actions, which then transitioned deterministically to a set of drifting reward distributions. (**B**) The logic of the experiment depends on a subset of trials. For instance, participants might be presented with the choice set (1,2) in a setup trial. Upon selecting action 1, they experience a low-probability transition to the green state followed by a large reward. A model-free influence on goal selection uniquely predicts an increase in the selection of action 3 on the subsequent critical trial, because actions 1 and 3 share the common goal state of blue.

Our analysis depends on a critical subset of trials (Fig. 1B). For example, a participant is presented with the choice set (1,2) at Stage 1 and chooses action 1. Because 1 typically leads to the blue state, we assume that this participant’s goal was to transition to blue. On our critical trials, however, they experience a low-probability transition to the green state, and then experience a very large reward. A model-based system would discard this information because transitions to the green state are equally likely from all Stage 1 options. This renders forward planning toward green irrelevant. In contrast, model-free value update would increase the likelihood of selecting 1 on subsequent trials due to the positive reward history (19). Our interest, however, is in the model-free assignment of value to a goal; in this case, the goal of transitioning to blue. If the experience of reward increases the likelihood of selecting blue as a goal, then participants should exhibit a greater likelihood of choosing 3 on the subsequent trial (when paired with either 2 or 4). Conversely, the experience of punishment should decrease the likelihood of choosing 3. This influence of the reinforcement history of choosing 1 on the subsequent choice of 3 cannot be explained by model-free update of a value to the specific action (choosing 1); rather, it depends on the assignment of value to their shared goal (getting to blue).

**Results**

*Experiment 1*

We assessed choice on critical trials by comparing instances when the participant experienced reward vs. punishment on the preceding setup trial (i.e. following low-probability transition to the green state). Consistent with our prediction, the mean proportion of trials on which participants selected the shared-goal action following positive reward (89%) was significantly greater than the proportion following negative reward (69%) *t*(134)=-12.5, *p*<.0001 (Fig. 2A).



**Fig. 2.** Bars represent the proportion of trials on which participants chose the shared-goal action, averaged across participants. Whiskers indicate the standard error of the mean of these proportions across participants. (**A-C**) Results from Experiments 1-3, respectively.

In order to capture trial-by-trial variation in the magnitude of the reward obtained on the setup trial, we regressed participants’ critical trial choices on the reward using a logistic mixed-effects model, estimating both random intercepts and random slopes at the subject level. (Following past research (CITE), this model approximates the value representation of a prediction error update mechanism as the most recently observed reward. In simulations presented below we validate this approximation.)

The reward obtained on the setup trial significantly predicted choice (*β* = .191; Wald z-test, z = 12.1, p < .0001). The model was preferred to a null model without the reward (Likelihood ratio test, *χ*2(2) = 266.0, p < .0001). In a parametric bootstrap analysis, 0 out of 1000 randomly resampled null models had a likelihood as large as the full model.

To control for other influences on choice, we re-estimated this model while including additional regressors for model-based and model-free action values (described in Methods). In this analysis the setup trial reward again significantly predicted choice (*β* = .200, z = 12.3, p < .0001). The model was preferred to the null model (*χ*2(4) = 298.2, p < .0001). In a bootstrap analysis, 0 out of 1000 randomly resampled null models had a likelihood as large as the full model. The model based action predictor (*β* =.221, z = 7.3, p < .0001) was significant, and the model-free action predictor (*β* = .054, z = 1.87, p = .062) predictor had a trending effect.

*Experiment 2*

The evidence from Experiment 1 is ambiguous between two interpretations. It may be that people assign value to the selection of a goal (e.g., “choose blue”), or it may be that people assign value directly to the shared-goal Stage 1 action (e.g., “choose option 3”). Experiment 2 was designed to disambiguate these possibilities (Fig. 3).

Specifically, Stage 2 states were arranged in a 2(color: red vs. blue)×2(shape: circle vs. square) design, with a fifth state that differed on both dimensions (a green triangle). Each trial was defined as a “color trial” or “shape trial”, with the trial type dictating the deterministic transitions to drifting rewards. Participants were cued to trial type at the beginning of each trial. Thus, three reward distributions were accessible on color trials, while three independent reward distributions were accessible on shape trials. This made color goals relevant only to color trials, and shape goals relevant only to shape trials. If model free value is assigned to goals, it should only influence choice on subsequent trials of the same type.

Consistent with this prediction, we replicated our result from Experiment 1 for same-type critical trials: After a low-probability transition to the green triangle state, participants were more likely to choose the shared-goal Stage 1 action on a subsequent same-type trial following reward (83%) compared with punishment (76%) *t*(302)=-4.82, *p*<.001. On different-type trials, however, there was no significant difference (positive: 50%; negative: 47%) *t*(282)=-.94, *p=.35* (Fig. 2B).

Following our analytic approach in Experiment 1, in a mixed effects model on same-type trials the model-free goal regressor was significant (*β* = .056, z = 4.51, p < .0001). The model was preferred to a null model (*χ*2(2) = 27.8, p < .0001; by bootstrapping, p < .001). This effect remained after controlling for model-based and model-free action values, as in Experiment 1. In the corresponding mixed-effects model on the different-type trials, the model-free goal regressor was not significant (*β* = .009, z = .784, p = .433). The model was not preferred to a null model (*χ*2(2) = .615, p = .74; by bootstrapping, p = .55). We also estimated a model with both same- and different-type critical trials, which included the model-free goal value and an interaction between that value and the trial type. The interaction was significant (*β* = .049, z = 2.62, p < .01), and the model was preferred to a null model with the interaction term removed (*χ*2(4) = 10.7, p < .05; by bootstrapping, p < .01). Same and different-type trials were coded as 1 and 0, respectively. Therefore the positive interaction term indicates that the model-free goal effect was significantly stronger for same-type trials.



**Fig. 3.** In Experiment 2, Stage 2 states varied along two orthogonal dimensions: shape and color. On each trial the participant was cued whether rewards would be determined by shape or by color. We predicted that rewards obtained following low-probability transitions to the green state would only influence subsequent choice on critical trials of the same trial type (shape vs. color). This is because the goals selected in each of two trials can only match when their trial types are identical.

*Experiment 3*

The results of Experiments 1 and 2 are consistent with model-free control over goal selection in which the habitually selected goal then participates in a process of forward planning over a causal model of the task’s transition structure. They are also consistent, however, with some hierarchical reinforcement learning models that do not invoke true model-based planning (8, 38). These models assume that “goal states” establish internally represented contexts that bias model-free stimulus-response associations. We designed Experiment 3 to test whether the goals selected in our paradigm could be flexibly integrated with knowledge of independent state transitions, a hallmark of true planning.

Participants were first trained on a deterministic set of transitions between four Stage 0 options [A,B,C,D] and the same four Stage 1 options used in Experiment 1 (Fig. 4A). During this training phase the Stage 1 options comprised the terminal states. Then, participants trained on and performed the same task used in Experiment 1, without any involvement of Stage 0 choices. Finally, we tested each participant in a set of critical trials—those in which a shared-goal choice follows a low-probability transition—but presented participants on critical trials with a pair of Stage 0 choices in place of the ordinary Stage 1 choices (Fig. 4b). In order to integrate information about a desired goal with the set of Stage 0 choices, participants were required to engage in forward planning over the learned transition structure between Stage 0 and Stage 1.

Here, again, we found that participants were significantly more likely to choose the shared-goal action following positive reward (85%) than following negative reward (69%) *t*(172)=-9.17, *p*<.0001 (Fig. 2C).



**Fig. 4.** Experiment 3 was modeled on the design of Experiment 1, except that (**A**) participants performed a pre-training in which they learned deterministic transitions between Stage 0 and Stage 1 choices, and (**B**) on critical trials the Stage 0 choices were selectively reintroduced. Thus, in order to make successful choices on critical trials, participants were required to choose a Stage 0 option that would lead to their preferred Stage 1 state.

In a mixed-effects model the model-free goal regressor significantly predicted choice (*β* = .143, z = 9.62, p < .0001), and the model was preferred to a null model (*χ*2(2) = 238.1, p < .0001; by bootstrapping, p < .001). This effect remained after controlling for model-based and model-free action values, as in Experiments 1 and 2.

The results of Experiment 3 speak against an alternative interpretation of Experiment 1 according to which statistical structure of state transitions could support associative pairings between shared-goal Stage 1 options, and thus associative transfer of reward values (15). In Experiment 3 there is no such basis for statistical association between Stage 0 actions analyzed on the critical trial and the Stage 1 action rewarded on the setup trial.

*Computational Model*

We specified a computational model of learning and choice that includes traditional model-based and model-free control alongside model-free goal learning. Using this model to generate simulated data in our task, we show that our observed results are obtained only when the model includes model-free goal learning. (We simulate Experiment 2, which entails the key features of 1 and 3).

The task is a Markov decision process with six states: The initial Stage 1 state, and five Stage 2 states. The Stage 1 state had four possible actions (i.e. the four numbers), only two of which were available on any given trial. The Stage 2 states had only one possible action each (i.e. clicking on the object), which led to a reward. The rewards were randomly generated for each agent by the same process as in the behavioral tasks.

Agents behavior was determined by a weighted mixture of three controllers. Model-free control was implemented using a SARSA algorithm with eligibility traces (5). Agents estimated a model-free value of the state-action pair (*s,a*), denoted *MFV(s,a)*. In Stage 1, agents chose an action *a* and transitioned to state *s.* The value update for *MFV(1,a)* occurs by temporal difference learning with learning rate :



where *a’* is the only available action in state *s*.In Stage 2, agents chose *a’* and received reward *r*. Again, value update is given by temporal difference learning:



In keeping with prior computational models of stochastic sequential decision-making paradigms (*6,22*), we also implemented an update of Stage 1 value representations following reward by applying an eligibility trace :



Model-based control was implemented a basic forward planning technique. Agents maintained a model-based value of each state-action pair, denoted *MBV(s,a)*. We assumed knowledge of the trial-type-dependent reward distributions on the part of the model-based controller. Thus, we separately indexed Stage 2 states according to the relevant trial type. To calculate the model-based value of each action from state 1, agents estimated the transition probability from *a* to *s*, denoted *T(a,s)*, by counting the number of observed transitions from *a* to *s* and normalizing the counters. Counters were initialized to 1, yielding a symmetric prior distribution over transition probabilities under the Dirichlet model. Then:



After transitioning to state *s*, performing the only available action *a’*, and receiving reward *r*, the model-based update was:



The third controller implemented model-free learning on goal selection. After a trial with chosen action *a* and received reward *r*, agents inferred the intended goal *g(a)* by:



Agents then updated the model-free value of the goal, *MFG(g(a))*, by:



To determine the probability of selecting action *a* out of choice set (*a*,*b*), agents took a weighted average *Wa* of the three controller values and entered it into a softmax function with temperature parameter :



Thus, agents were characterized by five parameters: (the learning rate), (the eligibility trace), (the softmax temperature), (the model-based weight), and (the model-free weight).



In each simulation, 200 agents were generated with parameters sampled uniformly over plausible ranges (see Methods). We ran two simulations: one where agents performed model-free goal learning, and one where they did not. We then analyzed the agents’ behavior by the same process as in the behavioral tasks.

In the simulation with model-free goal learning, on same-type trials agents chose the shared-goal action 65.4% of the time after a reward and 55.5% of the time after a punishment (*t*(199) = -4.68, p < .0001). The simple mixed-effects model on same-type trials estimated a model-free goal coefficient of .063 (z = 5.47,p < .0001), and was preferred to a null model (*χ*2(2) = 40.9, p < .0001). The complete mixed-effects model showed similar results. In contrast, on different-type trials, agents chose what would have been the shared-goal action 49.3% of the time after a reward and 48.3% of the time after a punishment (*t*(191) = -.330, p = .75). Analysis by mixed effects models similarly showed null results for the model-free goal regressor (*p*s > .8).

In the simulation where agents did not perform model-free goal learning agents showed no difference in behavior following a reward versus a punishment on same-type trials (*t*(199) = 1.30, p = .195). Analysis by mixed effect models similarly showed null results (*p*s > .6).

**Discussion**

We find that goal selection in humans is partially determined by model-free value representations derived from reward history. These goals are subsequently used during model-based planning over an internally represented causal model of the task structure. In our simple task this mechanism appears suboptimal, because participants could easily have performed an exhaustive search over candidate goals and thereby attained a higher average rate of reward. Yet, the same mechanism may be crucial in allowing humans to avoid the computational burden of full model-based evaluation for the kinds of complex tasks that we routinely face in everyday life—tasks for which exhaustive search over candidate goals is computationally intractable.

While our proposal relies upon the conceptual distinction between habitual (model-free) and goal-directed (model-based) behavioral control, it also demonstrates a mutual dependence between them. This integration captures several empirical phenomena that blend features of habits and goals. Contextual cues can trigger goal pursuit outside of conscious awareness (39), consistent with the operation of stimulus-response habits in the process of goal selection. In cases of “utilization behavior” among individuals with insult to prefrontal cortex, goal-directed behavior may be intrusive or inappropriately invoked based on contextual cues (40). Among neurotypical individuals, “functional fixedness” describes the tendency to consider a limited set of candidate means-end relationships based on past experience with a tool (41). Finally, it is observed in educational settings that the execution of controlled cognitive processes improves with practice—in other words, that learning complex tasks requires the incremental acquisition of appropriate habits of thought (42, 43).

Habitual control over goal selection reduces the computational demands of hierarchical planning, but there is no free lunch: By relying on habit, an agent forgoes the opportunity for optimal planning. This is apparent in our task, where model-free goal selection reduced participants’ payoff, compared to full model-based evaluation. Thus, humans face the challenge of optimally balancing the efficiency of model-free control against the productivity of model-based control. Several promising avenues of research explore how we accomplish this (44-47).

Within the present framework, one approach to fine-tuning this balance is to select and evaluate multiple candidate goals. The model we implemented allows only a single goal to be retrieved and adopted. A simple extension of this model would retrieve multiple goals with a probability proportional to their model-free value. Then, each of the candidate goals could be evaluated by model-based means. This would permit model-based planning not only at the level of goal execution (as we demonstrate here) but also at the level of goal selection. In this case, the function of model-free value assignment would be to restrict the space of goals subject to evaluation.

The utility of habitual goal selection also depends, of course, on accuracy of the model-free value representation. An agent with highly accurate representations sacrifices little by turning over goal selection to model-free control, while an agent with inaccurate representations sacrifices much. In our experiment, model-free value representations are set by the history of reward. However, obtaining sufficiently accurate representations exclusively by trial-and-error is not feasible for many complex tasks. Critically, past research shows that model-free value representations are established by several other means. For instance, people generate “fictive” reward signals by simulating over an internally represented causal model (e.g., daydreaming about the bonus they will get if they put in extra hours at work). Such fictive rewards can establish model-free value representations (14), including in the striatum (48). In this case, model-based processes cache a reward value in memory prior to decision-making, thus avoiding the need for online planning during behavioral control (12, 45).

Both observational learning and direct instruction by social partners also establish value representations (49-51). The possibility of cultural transmission of goal/subgoal structure by observational learning or instruction stands out as a likely explanation for the efficiency and power of goal-directed behavior in humans. Cached model-free value assignment to goal selection may serve as an important repository for cultural knowledge of this form. This implies a codependence between two capacities that are remarkably developed in humans: cultural transmission (52) and productive and flexible reasoning (53).

**Conclusion**

It is widely recognized that humans’ representations of complex tasks are organized hierarchically into goals and subgoals (3, 9, 10). We find that task proficiency depends partially upon the acquisition of habitual subgoal selection given the contextual state of a superordinate goal. This mechanism may contribute to humans’ ability to strategically deploy cognitive control in a manner that supports flexible and productive thinking, while avoiding the computational demands of full model-based evaluation.

**Acknowledgments:** We thank Michael Frank, Samuel Gershman, Wouter Kool and Josiah Nunziato for their advice and assistance. This research was supported by grant N00014-14-1-0800 from the Office of Naval Research.

**Methods**

*Participants*

A total of 960 subjects were recruited on Amazon Mechanical Turk to participate in a two-stage Markov decision task. Subjects were excluded from analysis if they timed out on more than 50 trials. Following past research on a similar task (24), we also excluded subjects who did not meet a minimum threshold of learning. We ran a Monte Carlo simulation of 10,000 agents performing the task randomly, and determined the 95th percentile of their final scores. We excluded subjects whose final scores were below this cutoff. After applying our exclusionary criteria, there were 135 subjects and 3806 critical trials in Experiment 1, 303 subjects with 4231 same-type critical trials in Experiment 2 (along with 2137 different-type critical trials), and 173 subjects and 4755 critical trials in Experiment 3.

*Experiment Design*

The designs of Experiments 1-3 are depicted in Figs. 1A, 3, and 4A. The two Stage 1 options for each trial were always chosen such that the options led to different Stage 2 states (i.e. (1,3) were never paired in Experiment 1). All rewards distributions were initialized uniformly at random on a range of -4 points to +5 points, and varied according to a bounded Gaussian random walk for the remainder of the experiment. After each round, the drift was sampled from a normal distribution with (μ=0, σ=1.8), rounded to the nearest integer, and added to the current reward level. In cases where drift selected a reward level outside the bounds of [-4,5], the reward would ‘rebound’ by the amount of the excess. (In Experiment 2 only, the lower bound was changed from -4 to -5.) The rewards on setup trials (those immediately preceding critical trials) were boosted to their extremes by adding +2 or -2 points, depending on the reward distribution’s current sign. If the boost selected a reward level outside the bounds, the reward remained at the boundary amount.

After the experiment, participants received a bonus payment based on their accumulated points. Each point was worth 1 cent. Participants were informed of the value of points in the instructions. Participants completed 75 practice trials followed by 175 rewarded trials. The practice trials were divided into three sections of 25 practice trials each. Sections were designed to ease participants into the task by introducing one task element at a time. On the rewarded trials, subjects had only 4 seconds to make their choice between the two numbers. If they did not make a choice within 4s the trial would time out and the next trial would begin. Practice trials had no time limit. A total of 26 critical trials occurred in each experiment. The spacing of critical trials was chosen randomly, with the constraint that they had to be at least three trials apart from each other.

*Analysis*

We regressed critical trial choices on setup trial rewards using a mixed-effects model. We coded choices on critical trials as 1 if participants selected the shared-goal action, and 0 otherwise. In addition to a fixed slope and intercept, the model estimated random slopes and intercepts for each subject. We also estimated a second mixed-effects model, which included model-based and model-free action values as nuisance regressors. The logic of our experiment ensures that the values calculated by these traditional mechanisms are uncorrelated with those calculated by our hypothesized mechanism, making this control not strictly necessary. However, to guarantee the accuracy of our results, we include the complete model as a secondary analysis.

To estimate the second mixed-effects model, we defined an action’s “model-free value” as the last reward the agent received from selecting that action, and an action’s “model-based value” as the last reward the agent received from that action’s associated color. These definitions are consistent with our above analytic approach, which assumes that value representations are dominated by the most recently experienced reward (CITE). Since these rewards could potentially have been received many rounds ago, convergence of our model depended upon temporally discounting rewards. We implemented a discounting factor of *γ* = .85. We subtracted the model-free and model-based values of the non-shared-goal action from those of the shared-goal action to obtain single model-free and model-based regressors, both coded so as to predict choice in the same direction as the model-free goal regressor.

In order to achieve convergence, all models allowed correlation among random slopes but not between random slopes and the random intercept. We also excluded several additional participants who made the same choice on every critical trial (7 in Experiment 1, 2 in Experiment 2, and 8 in Experiment 3). All regressors were grand mean centered. We calculated the significance of the reward regressor using a Wald z-test. We then determined whether the regressor increased the model’s likelihood enough to justify inclusion by calculating a null model with the regressor removed, and comparing models using a likelihood ratio test and parameteric bootstrapping. All mixed-effects analyses were conducted in R (54), making use of the lme4 linear mixed effects package (55).

*Simulations*

Parameters were sampled as follows. was sampled from a uniform distribution from 0 to 1, which we denote as U(0,1). was sampled from U(.5,1). was sampled from U(0,1.5). For the weights, three variables – ,,and – were sampled from U(0,1), and then and .



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