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3 **Selective application of theory does not replace formal**

4 **model tests: a reply to Shenhav et al.**

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In our study¹, we tested predictions of the Expected Value of Control² (EVC) and Predicted Response-Outcome³ (PRO) models in the context of motivated control. Our intent was to engage empirical and computational neuroscientists in an open debate about the broader function of dorsal anterior cingulate cortex (dACC) in control. However, it appears that Shenhav and colleagues⁴ have misrepresented our efforts to implement and test 3 accounts of dACC function, including the EVC model.

Shenhav and colleagues develop a straw model ('EVV') and spend substantial time attacking that rather than our actual implementation of EVC. Shenhav and colleagues claim that 'EVV' commits a sort of computational tautology - specifically, that it uses information about option values to determine the value of options. As is made explicit in our simulation code (available at <https://github.com/modelbrains/EVC-Simulations>), option value information is used to determine the value of a response for a given level of effort:

$$Value(Response\ 1|Effort, Options) =$$

$$P(Response\ 1|Effort, Options) * Value\ 1 =$$

$$\frac{V_1}{1 + \exp\left(-\left(effort_1 + \frac{V_1 - V_2}{\gamma}\right)\right)}$$

Eq. 1 is taken directly from the EVC model equations², with the term $\frac{V_1 - V_2}{\gamma}$ reflecting the idea that, as differences between the value of options increase, choosing the more valuable option becomes easier. This type of mechanism is specifically invoked in Shenhav et al., 2016⁵ (Fig 1A) as evidence in favor of EVC – easy choices between foraging and engaging options do not require control because they are easy. Our behavioral data¹, in which we observe that the drift rate is higher for trials with large differences between options than those with small differences, support this aspect of our implementation. Critically, this mechanism need not depend on an explicit value computation – it could, for example, reflect more efficient integration of information when options are well-distinguished⁶.

Shenhav and colleagues suggest our implementation is 'inappropriately restricted.' To demonstrate the generality of our implementation, we conducted additional simulations (also available on

github) of the model for a foraging task in which the observed pattern of dACC activity is cited as evidence supporting EVC^{5,7,8} (Fig. 1A). When applied to this task, our implementation reproduces the peak in dACC activity (Fig. 1B) as a non-default foraging option becomes more valuable. Shenhav and colleagues's contention that our implementation is an inappropriately restricted version of their more general theory is demonstrably inaccurate.

Figure 1. A) “FVT predicts that dACC should be even more engaged [when value of the foraging option greatly exceeds the current option], whereas EVC predicts the opposite (since the decision is now easy: pick the foraging option).” Text and figure from Shenhav et al., 2016. B) Our implementation captures this relationship, demonstrating that it is not inappropriately constrained only to the speeded decision-making task in Vassena et al., 2020.

Shenhav and colleagues suggest that control is not required to meet a response deadline: the decision boundaries of a diffusion model may be fixed and ‘organically’ collapse to ensure timely responses (Fig 2A). Under this interpretation, the differences we observe in the decision boundary (high for easy trials, low for difficult trials) are an artifact of rapid evidence accumulation during easy trials allowing the diffusion process to reach a decision boundary during the period when high (inhibitory) control persists.

Figure 2. A) If one condition has a lower estimated drift rate and decision threshold, reaction times should be longer for that condition under fixed collapsing boundaries and faster under response invigoration. B) Difficult high reward trials had a lower estimated drift rate (left) and lower estimated decision boundary (right). C) For difficult trials in our task, reaction times were faster for high reward vs low reward trials, consistent with response invigoration and inconsistent with a fixed collapsing boundary.

Fortunately, we can investigate this with our data. Using the HDDM⁹ methods previously described¹, we fit a drift diffusion model to difficult trials (value difference ≤ 10), median split into ‘high reward’ and ‘low reward’ trials, i.e., although the values of each option were similar, subjects were guaranteed a relatively high outcome in one condition, or a low outcome in the other. The response threshold (Fig 2B) was lower for high vs low reward trials ($p=0.035$), while the drift rate was higher for low reward trials ($p=0.01$). Although this pattern could be explained as a collapsing boundary with a fixed within-trial shape as proposed by Shenhav and colleagues, this explanation requires that conditions with lower thresholds *also* have longer reaction times. Instead (Fig. 2C), RTs for difficult high reward trials (mean 507ms) are *faster* than low reward trials (mean 527ms, $t(22)=-3.68$, $p=0.0014$, $CI=-0.307$ —0.0086), consistent with invigoration of a response within the trial.

Finally, Shenhav and colleagues complain that ‘EVC’, under certain parameterizations, predicts low levels of control for trials in which option values are similar. *Normatively*, when two options are likely to produce equivalent outcomes, it makes little sense to incur additional control costs to invigorate or inhibit a specific response. This logic is used to support EVC for situations in which a non-default forage option has approximately the same value as a default engage option – the additional control needed to inhibit the default response is not worth the cost of doing so⁵.

In short, Shenhav and colleagues’s four major criticisms of our test of EVC are not applicable. First, our implementation does not commit the tautology Shenhav and colleagues suggest it does. Second, our implementation generalizes correctly to tasks they have used to support EVC. Third, controlled invigoration of responses is empirically supported in our data. Fourth, the predictions we derive from EVC have clear normative rationales.

Conspicuously, Shenhav and colleagues do not comment on our primary finding regarding EVC: specifically, we find *no evidence* of a neural signal in line with the hypothesized optimal control signal in our data. Under EVC, the purpose of control-relevant signals is the estimation of such an optimal control signal. Without the critical step of estimating a control signal, the ‘expected value of control’ theory reverts to a list of effects that have been observed in dACC (Shenhav and colleagues, Fig. 1), rather than an integrative principle. Conversely, all these effects can be explained by the unifying principle of surprise embodied by the PRO model³ and its extensions^{10–15} in a parsimonious manner and with a testable and falsifiable implementation.

A line of argument that would facilitate the debate and the integration of competing models would entail an implementation of the EVC model by Shenhav and colleagues that could accommodate for our findings. Although intriguing, the theoretical suggestion that under EVC, dACC ‘integrates’ a variety of control-relevant signals (including surprise, conflict and many others) remains computationally under-specified. Shenhav and colleagues identify several obstacles preventing the formulation of testable predictions from EVC; this seems problematic when neural findings can be selectively interpreted either as supporting⁵ or not relevant¹ to the theory. We believe a constructive discussion has great potential to

advance our understanding of dACC's role in surprise signaling and control allocation. However, this requires the development of computationally specific implementations to allow for independent validation of competing predictions for dACC function.

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