Review of: "Antihypertensive Medications Adherence and Its Relationship to Blood Pressure Control Among Healthcare Workers in Jose R. Reyes Memorial Medical Center (JRRMMC): A Retrospective Analytic Study"

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Potential competing interests: No potential competing interests to declare.

Review

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Review of: A brief introduction to the COM-B Model of behaviour and the PRIME Theory of motivation

The reviewer(s) rated it **5**/**5**

Karl Friston[1]

Reviewer(s) details

Declarations

I thought that this was a great little synopsis of the COM–B and PRIME frameworks – for formalizing sentient and motivated behavior. For someone outside the field, it offers a very clear and useful description of the architectures one can

deploy to understand (and model) behavioral interventions.

Although beyond the remit of the current paper, it would be interesting to see how the two theories transcribe into the fields of theoretical neuroscience and ethology. In my world, behavior of this sort is generally modelled in terms of "planning as inference" (Attias, 2003; Baker, Saxe, & Tenenbaum, 2009; Botvinick & Toussaint, 2012; Millidge, 2019): specifically, active inference and learning.

I was struck by the number of phrases and constructs that link the two fields. For example, "belief as processes" that underwrite evaluation "by inference of some form". This is exactly how active inference works, were the process of Bayesian belief updating is used to assimilate evidence from the environment (i.e., perception) to form (posterior) beliefs about states of affairs in the world. The same Bayesian principles are then applied to beliefs about policies or plans: e.g., (Schwartenbeck et al., 2019). Evaluation then becomes a problem of inferring ‘what is the most likely thing I will do?’. This entails the selection of a particular plan following its evaluation in terms of the things that the COM–B model brings to the table, specifically, prior beliefs about the plausibility of a plan and its outcomes (often described in terms of prior preferences or goals).

I can see that capability and opportunity would shape the policy space (i.e., “dispositions”) from which a particular plan is selected and enacted (c.f.: “active beliefs”). In machine learning, this selection is sometimes cast as Bayesian model selection. The evaluation itself is very interesting. In active inference, it inherits from statistical notions of maximizing marginal likelihood (that can be reduced to risk sensitive control in economics or Bayesian decision theory in psychology). I imagine the PRIME theory would specify prior beliefs about the consequences of action and the kinds of actions people originally commit to.

I mention these points of contact because, in principle, it should be possible to implement PRIME theory in silico using partial observed Markov decision processes to simulate agent-based behavior. The second reason is that certain imperatives for behavior figure prominently in simulations of active inference. I am thinking here about the resolution of uncertainty and epistemic affordances – and indeed epistemic habits: i.e., “where do I usually turn to get this kind of information?". This links to another field of affordances in ethology; e.g., (Constant, Ramstead, Veissiere, & Friston, 2019; Gibson, 1977, 1979). In the future, it would be nice to see how the authors would articulate (epistemic or Gibsonian) affordances within their frameworks – perhaps they have already.

Attias, H. (2003). *Planning by Probabilistic Inference.* Paper presented at the Proc. of the 9th Int. Workshop on Artificial Intelligence and Statistics.

Baker, C. L., Saxe, R., & Tenenbaum, J. B. (2009). Action understanding as inverse planning.*Cognition, 113*(3), 329-349. doi:10.1016/j.cognition.2009.07.005

Botvinick, M., & Toussaint, M. (2012). Planning as inference. *Trends Cogn Sci., 16*(10), 485-488.

Constant, A., Ramstead, M. J. D., Veissiere, S. P. L., & Friston, K. (2019). Regimes of Expectations: An Active Inference Model of Social Conformity and Human Decision Making. *Front Psychol, 10*, 679. doi:10.3389/fpsyg.2019.00679

Gibson, J. J. (1977). The theory of affordances. In S. R & J. Bransford (Eds.),*Perceiving, acting, and knowing: Toward an ecological psychology* (pp. 67-82). Hillsdale, NJ: Erlbaum.

Gibson, J. J. (1979). *The ecological approach to visual perception*. Boston: Houghton Mifflin.

Millidge, B. (2019). Deep Active Inference as Variational Policy Gradients. *arXiv e-prints*, arXiv:1907.03876.

Schwartenbeck, P., Passecker, J., Hauser, T. U., FitzGerald, T. H. B., Kronbichler, M., & Friston, K. J. (2019). Computational mechanisms of curiosity and goal-directed exploration. *Elife, 8*, e41703. doi:10.7554/eLife.41703

Comments

Saiful Koya

Post

I would like to thank the authors for manuscript. Some editing for English language is required throughout the manuscript.

For abstract, in my opinion it was quiet weak. ‘Background’ was too long and I feel it was a waste because the authors should focus on the method and the results. For methods, authors did not state what MMAS-8 was used for (e,g: to measure adherence to antihypertensive medications'. Also, no scoring and categorizations was described in relation to MMAS-8 questionaires and the statistical analysis used was also not described. For conclusion, authors stated association between ‘non-adherence to treatment and antihypertensive treatment disruption’. However, ‘antihypertensive treatment disruption’ was never mentioned anywhere in the abstract, either in the backgroud or method. Finally, the references in the abstract should be removed because they were not supposed to be there in the first place. Full name for abbreviations for ARB, CCB should be stated clearly.

For the whole manuscript, I feel that the the manuscript was poorly written. Data and vital information was poorly presented. Below are the opinion that I have on each part of the manuscript:

For ‘Method’, author never mentioned if they have received any permission or license to use MMAS-8. If they have, then it should be clearly stated in ‘method’ or ‘acknowledgement’. Unauthorized use will have legal ramification. The scoring system for MMAS-8 that separate participants into non-adherence or adherent categories was also never stated. The analysis technique was too briefed. Authors should describe how they conduct the univariate and logistic regression in detail. The guideline/s used for hypertension was not described too. Pooling all participants into one target blood pressure can produce results that are confusing/ misleading since target blood pressure are different between those who were only hypertensive or those with hypertension and other comorbidities such as diabetes.

For ‘Results’, there were excessive number of tables. Authors should minimise this. For instance, table 6,7 and 8 can be pooled into one table. The use of two decimal places for percentage was also unnecessary. the 95% confidence intervals should be shown on the tables and written in the results as well. The titles for all the tables was also too long and

unncessary and it needed to be simplify further.