Reviewer’s report

Title: The Role of Habit in Predicting Fruit Consumption

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Reviewer: Benjamin Gardner

Reviewer’s report:

I am grateful for the opportunity to review this revised manuscript. To be clear, there is much to like about this paper. Particularly interesting is the analysis of the stability of habit strength and behaviour over time; the assumed stability of habit (i.e. learned automatic cue-responding) over time underpins recent theorising around the relevance of habit formation for behaviour maintenance (e.g. Lally & Gardner, 2013; Rothman, Sheeran & Wood, 2009), making the imperfect relationship between habit over the two timepoints noteworthy. I still have reservations around this paper, though these are more focused on the rationale for the study, and the interpretation of the findings, than from the analyses themselves. For pragmatic reasons, I have explicitly suggested how the most salient concerns might be addressed.

Major compulsory revisions

1) The authors call for tests of the difference between behavioural frequency and habit (e.g. p3). I previously raised the concern that this is unnecessary, as there is ample evidence that habit is different to behavioural frequency. My concern remains, but my point is essentially about wording.

Suggested revision: the authors should argue for tests of the different *impacts of* behavioural frequency and habit *on future behaviour*, rather than for tests of the difference between the two concepts per se. (Habit, as the authors define it, is necessarily different to behavioural frequency, and no empirical data are needed to test such a difference.)

2) A more fundamental concern is that the study design is not conducive to determining causality, nor can it reveal true mediation. The authors have said that they chose not to address this, but it is of utmost importance for interpreting findings. A compelling body of theory and empirical evidence suggests that habits form through repeatedly performing (in a consistent context) behaviours towards which one has positive intentions (i.e. intention > initiation of behaviour > repetition of behaviour > habit; Lally & Gardner, 2013). Thus, in the early stages of habit formation, repeated behaviour strengthens habit, and as habit strengthens, habit comes to prompt subsequent behaviour, which in turn reinforces the habit (see e.g. Gardner, in press, Health Psych Rev, Fig 1). It is while habit forms that the direction of relationships between cognitions, behaviour and habit can more reliably be inferred. The key problem is that it is highly likely that the present data were collected when the cognitions, habits and behaviours of the sample had become settled and established, and so differ minimally over
time. (Although correlations between measures over time are imperfect, they are nonetheless very strong, and the instability could well reflect measurement error rather than true changes in the constructs.) So, from these data alone, we cannot tell which variables ‘came first’. It also means that mediation cannot be reliably tested, as the independent variable (e.g. past behaviour) within the mediation relationship must precede the mediator (habit strength; p10). Reverse mediation (where habit is the IV, past behaviour the mediator, and future behaviour the DV) is also difficult to justify, as different assumptions must be made around temporality for such an effect to be explored.

A similar concern is that one cannot conclude from these data that habit is less important than past behaviour simply because past behaviour is a better predictor of future behaviour than is habit. Consider a hypothetical case of someone eating fruit out of habit, which means that they consistently eat fruit on the same occasions and in the same contexts. Their past behaviour would be a perfect predictor of their future behaviour, in which case there would be no residual variance in future behaviour for habit to explain. This would not mean that habit is not a determinant of behaviour, or that it should not be considered for intervention development. It is difficult to draw meaningful conclusions from the comparison of effects of past behaviour vs habit on future behaviour.

Suggested revision: The authors must acknowledge these problems. I would recommend that the paper explicitly states (in both Introduction and limitations section in the Discussion) that the tests presented here are tentative and statistical, and are designed to generate ideas for testing using more rigorous analyses of habit formation processes.

3) I previously argued that past behaviour is an ‘empty’ construct (Ajzen, 2002), which in and of itself is not of interest. The authors replied that both past behaviour and habit can be viewed as ‘empty’ constructs, and that the predictive value of past behaviour suggests it is not ‘empty’. I disagree. Habit is a conceptually meaningful construct, and decades of research have shown that people can acquire automatic responses to situations, which can then guide responses in those situations (see e.g. Neal et al, 2012, JESP). Ajzen did not suggest that habit was an empty construct, but rather argued against using past behaviour as a measure for habit. The point still stands that past behaviour can predict but not explain future behaviour; it is nothing more than a proxy measure for unmeasured variables that likely have continued influence on behaviour. Its ‘emptiness’ becomes clear where the authors suggest that ‘previous behaviour … [should be an] educational objective for behaviour change programs’ (p2), and that ‘educational interventions need to address the importance of [past behaviour]’ (p14). What would this entail in practice? We cannot change behaviour that has already occurred.

Suggested revision: The authors should give more consideration to the meaning of effects of past behaviour over and above habit, and should remove assertions that past behaviour should be an objective or target for interventions.

4) I argued that there is no evidence that habits influence attitude and efficacy
beliefs, which the authors contest. I agree with the authors (and reviewer 1) that performing behaviour can influence attitudes, norms and self-efficacy, but we do not have any evidence that the development of automaticity (i.e. habit) influences cognitions. The direction of relationships between habit and cognitions is an interesting empirical question, but because of the study design problem discussed above, we cannot infer from these data that habits (rather than behaviour) changes cognitions.

Suggested revision: Remove the claim that habits influence cognitions; or at the very least, state that this is a tentative hypothesis requiring more rigorous testing.

Minor essential revisions:

5) I argued that the authors’ statement that more work is needed around the construct validity of SRHI subscales was redundant, to which they responded that another reviewer suggested that different versions of the scale should be tested. I agree that these tests are needed, but the statement that ‘more work is needed’ is not.

Suggested revision: remove the sentence ‘research is needed to illuminate whether abbreviated versions of the SRHI have the same construct validity as the original version’ (p13). No such work is needed – it has been done and is reported in Gardner et al, 2012 (IJBNPA). Also, move the sentence ‘When excluding the items in the habit scale...’ (p12) to the final paragraph later on this page, for added coherence.

6) On p13, the authors refer to Lally et al’s finding that habit formation peaked at 18-254 days and state that ‘our measurement periods may have been too short to outline the impact of automaticity’ (p13). This is not a study of habit formation, so Lally et al’s time period is not relevant. Additionally, the study design precludes examining the true impact of automaticity.

Suggested revision: Remove these two sentences.

7) The authors suggest that past behaviour may be an adequate measure of habit, and that more research needs to explore whether two constructs are needed to measure the influence of past behaviour. This regressive argument undermines the 15 years of careful efforts by Verplanken, Wood and others to design conceptually coherent measures of habit that are distinct from behaviour frequency.

Suggested revision: No such work is needed. Remove this sentence.

Discretionary revisions

8) The title could be changed to better reflect the content of the paper. (Several studies have documented the role of habit in predicting fruit consumption – what is the novelty of this piece?)

9) A power analysis should be provided.

10) Several references are made to a variable being ‘mediated’ (e.g. p4:
‘mediation of previous behaviour via motivational factors did not occur as expected’). Given that three variables are needed for a mediation effect, for clarity all three variables should be included in such sentences (e.g. ‘mediation of *the effect of* previous behaviour *on future behaviour* via motivational factors did not occur as expected’).

11) More could be made of the finding that habits were not found to be especially stable over time, as it has profound implications for the field; if habits are not stable, then forming habits may not sustain behaviour as is thought (e.g. Rothman et al, 2009). Findings from Judah et al’s (2013, BJHP) flossing study would support this. Alternatively, the findings might reflect a lack of test-retest reliability in habit measurement, so raising questions around whether habit can be measured reliably using self-report.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests