Author’s response to reviews

Title: Does the Discrepancy between Implicit and Explicit Attitudes Moderate the Relationships between Explicit Attitude and (Intention to) Being Physically Active?

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BMC Psychology

Dear Editor,

We would like to thank you for giving us the opportunity to resubmit our scientific article “Does the Discrepancy between Implicit and Explicit Attitudes Moderate the Relationships between Explicit Attitude and (Intention to) Being Physically Active?”. We appreciate the constructive feedback of the reviewers and have addressed each of their concerns as outlined below.

Editor Comments:

1) In the Background and Discussion please discuss the larger study that this current manuscript is a part of, what has been published to date and the contribution that this current manuscript will make.

AUTHORS: We added on page 7 (introduction) the results and reference to the former study and explain the added value of the current manuscript. Also on page 20 (discussion), we added the information that the current study extends an earlier study and describe its added value.
BMC Psychology operates a policy of open peer review, which means that you will be able to see the names of the reviewers who provided the reports via the online peer review system. We encourage you to also view the reports there, via the action links on the left-hand side of the page, to see the names of the reviewers.

Reviewer reports:

Tanya Berry (Reviewer 1): This is interesting work, largely well written and presented. The authors have presented a prospective study that adds to the literature regarding how both implicit and explicit processes may interact to inform physical activity behaviour. I have a few comments for the authors to consider:

AUTHORS: Thank you for your positive feedback.

1. In several places (e.g., p. 5, lines 54 - 59) the authors discuss the relationship between intention and behaviour but present it only positively, implying that intention leads to behaviour. However, there is the well-known intention-behaviour gap that should be acknowledged.

AUTHORS: On pages 5 and 6, we added the information that intention does not always result into behavior and explain the intention-behavior gap. A relevant reference for that was also added.

2. Is it possible that participants recruited from the BeeLab are very familiar with the tasks (or variants of them) used in this research? If so, this could have implications for the findings because of practice or similar effects.

AUTHORS: It is very unlikely that participants recruited from the BeeLab are familiar with the tasks or with variants of the tasks used in the study as the sample and lab are normally used for economic studies. According to the BeeLab coordinator, our study was the first study that used a SC-IAT and this type of questionnaire.

3. P. 9. Is it possible that the discrepancy and low correlation (r = .11) between implicit and explicit attitudes is because the positive and negative words used in the SC-IAT are not related to physical activity? According to the APE model, implicit attitudes represent memory associations and so I wonder if words more reflective of positive and negative feelings about physical activity would have generated different results.

AUTHORS: Thank you for providing this alternative explanation. We added this point to the limitations section in the discussion (page 23).
4. P. 14 - does it matter which type of attitude (implicit or explicit) is higher when attitudes are discrepant, particularly when the correlation was quite small as reported on p. 15? The APE model, which the authors cite describe scenarios for when one or the other change which makes me wonder about the stability of these constructs over the course of the study, in addition to the direction of the discrepancy.

AUTHORS: We added an additional analysis in the post-hoc section. In this analysis, we assessed whether the direction of the discrepancy plays a role in this regard (page 19). As this was the case for PA at T1, our assumption put forward in the discussion that dissonance is likely a result of a more favorable explicit attitude towards PA than the implicit attitude, was supported (page 23).

5. P. 15 - this is a very active sample - what are the implications for the findings? This is an important point that I think needs to be addressed.

AUTHORS: On page 23, we added this point and we mention that results regarding the relationship between explicit attitude and intention might be different in a less active sample and even be more influenced by IED.

Julie C.S. Boiche (Reviewer 2): The article presents a complementary analysis to a study already published in BMC Psychology on the explicit and implicit determinants of physical activity derived from a longitudinal study in Dutch college students. While the research question raised appears interesting, I have several concerns related to the article.

AUTHORS: Thank you for your constructive feedback.

First, there is a lack of rationale in the manuscript: the idea that there can be a discrepancy between explicit and implicit attitudes which can impact behavior adoption is announced page 4. However, there is no explanation or theoretical background explaining why this should happen; and the next paragraph directly presents the results of previous studies on the subject - including experimental research in social psychology which may not be directly transferable to the issue at hand.

AUTHORS: We added a new paragraph describing factors that have been discussed in the literature to cause dissonance between implicit and explicit attitudes. Also theoretical assumptions put forward in the APE regarding this issue are described on page 4 now.

The authors mention several theoretical frameworks (RIM, APE) but do not develop the assumptions on which the hypotheses of the current article are based. It is not clear for example why the IED score should moderate the explicit attitudes -intention relationship (and not the intention-behavior relationship)
AUTHORS: The RIM and the APE were used to explain that both explicit attitudes and implicit attitudes can be associated with behavior. We now added the information why dissonance between implicit and explicit attitudes can exist according to the assumptions put forward in the APE (see page 4). The hypotheses, however, are based on a former study conducted by Karpen et al. (described on page 5), who found explicit attitudes to be a weaker predictor for alcohol consumption when IED was high. Based on this finding, we assumed that IED weakens the relationship between explicit attitude and behavior and tested this assumption for the relationship between explicit attitude and physical activity. In addition to that, we extended this approach and we also looked at the relationship between explicit attitude and intention as (a) explicit attitude is also strongly linked with intention and (b) intention is the proximate determinant for (PA) behavior. This reasoning is explained on page 5-7.

A consequence of this is that it is not clear why absolute discrepancy was investigated and not only the difference between implicit and explicit scores - indeed, the example that spontaneously comes to mind, considering the strong social norms that generally exists in favor of exercise, is that people usually hold strong positive explicit attitudes toward physical activity, but may not have implicit attitudes that are as favorable; hence a strong discrepancy could reflect a particular configuration (positive explicit attitudes + less favorable implicit scores). This seems plausible as the mean scores for explicit attitudes are very high (56 on a 70 point measure) and the implicit score close from 0 (a score indicating no strong implicit association). From my point of view a thorough description of the studies that dealt with this issue is missing; there are three previous studies in the context of exercise (Karpen et al., Brand & Antoniewicz; Berry et al.), but their method and results are not presented sufficiently in detail. The method used in the present study presents several similarities but also differences with previous research; unfortunately those aspects are not developed.

AUTHORS:
- We have addressed the abovementioned issue by adding the additional post-hoc analysis, in which we investigated whether the direction of the discrepancy is important (see page 23).
- Moreover, parts of the study of Karpen et al. are described on pages 5 and 7. We mention the outcome of the study to indicate that IED has been shown to moderate the relationship between explicit attitude and alcohol consumption. Karpen et al. also looked at IED in the context of PA, however, the question they addressed was ‘Does self-perceptual information (regarding PA) have a greater impact on the self-concept for those with greater IED than on those with lower IED?’. As this is fundamentally different form our hypotheses, we do not describe the respective method or results. Yet, the way IED was calculated is the same way as done in our study.
- The two other studies (Brand & Antoniewicz; Berry et al.) also investigated IED in the context of PA. The way IED is measured in the study of Brand and Antoniewiecz (2016) is relatively new and, as far as we know, is not established yet. However, we added this way of discrepancy assessment to the limitation section as an alternative way to assess
IED (page 25). In the study of Berry et al. (2018), IED is calculated in the same way as we did, namely ‘[E]IED scores were calculated as the absolute value of the difference between standardized implicit health and exercise evaluations relative to standardized explicit health motives ([E]IED health), and standardized implicit appearance/body shape and exercise evaluations relative to standardized explicit appearance motives ([E]IED appearance’). As this study has the focus on whether IED predicts dropping-out of an exercise program, we do not explain other parts of the method or result section. The study is, however, mentioned on page 6 of our manuscript to indicate that IED has been shown to be related to dropping-out from an exercise program.

The authors used the SC-IAT and cite articles demonstrating evidence of validity of this measure; however, there were adjustments to translate the material from English to Dutch and German; the authors present statistics for the test internal consistency; however other properties may not be verified; for instance temporal reliability of PA-related SC IAT was already reported in past literature.

AUTHORS: We added more references, which demonstrated an adequate internal reliability and predictive validity of the SC-IAT used in the context of PA (page 10). In addition to that we added test-retest correlations of the SC-IAT used in the present study. Those correlations are reported on page 12 and results are similar to former studies, which are added as references there.

The method used to compute a discrepancy score seems basic compared to the analyses performed in previous research.

AUTHORS: The method used to compute discrepancy scores is based on former studies, such as


We performed the same calculations as done in the studies cited above and we regard this a suitable method to calculate the discrepancy for the purposes of the current study.

Regarding the scales used to assess the sociocognitive variables, since the Theory of Planned Behavior is cited in introduction, it is surprising to see items that refers to slightly different concepts (social modeling; barrier self-efficacy).

AUTHORS: To assess socio-cognitive variables, we used the I-change model, which has been used in former studies to assess socio-cognitive variables related to PA. Although the core of the I-change model and the Theory of planned behavior show great similarities, they assess the abovementioned concepts slightly differently. The according references are now added in the introduction to prevent a possible confusion (page 6).

Yours sincerely, on behalf of the co-authors,

Carolin Muschalik