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On the evaluation of competing theories: A reply to van der Maas and Kan

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ABSTRACT

van der Maas and Kan (2016) do not believe in the premise underlying the indirect test of the mutualism theory of general intelligence (*g*) reported in Gignac (2016). Additionally, van der Maas and Kan (2016) do not believe in the interpretation of the results reported in Gignac (2014). As a response to van der Maas et al. (2016), it is noted that, contrary to van der Maas et al. (2016), the *g*-factor was not proposed by Gignac (2014, 2016) as the only factor of intelligence. Secondly, there is arguably no reason to consider the *g* factor a mysterious latent variable. Thirdly, the simulation results reported in van der Maas and Kan (2016) simply demonstrated that a very highly parameterised mutualism model may be a possible alternative representation of the positive manifold, rather than a plausible one that may be expected to replicate across samples. Intelligent researchers will likely continue to disagree about *g*. Based on individual differences research relevant to non-intellective predictors of the inclination to accept various scientific propositions, I conclude with the suggestion that belief in the plausibility of the *g* factor (or mutualism) may be impacted significantly by individual differences in personality, attitudes, and worldviews, rather than rely strictly upon logical and/or empirical evidence.

van der Maas and Kan (2016) should be commended for extending the simulation work relevant to the mutualism theory of *g*. Specifically, the original mutualism simulation work was restricted to *g* factors with essentially equally weighted *g* loadings (van der Maas et al., 2006). However, van der Maas and Kan (2016) demonstrated that a mutualism model can be found to be just as well-fitting as a higher-order model, even when the data are associated with unequal *g* loadings, as was the case with the data presented in Gignac (2016). Although a worthwhile incremental advancement to the area, as I argue below, I do not believe the simulation results or arguments reported by van der Maas and Kan (2016) help support the plausibility of the mutualism theory of *g*. Instead, I believe the results reduce the plausibility of the theory.

1. Correction

First, I would like to make a correction to van der Maas and Kan (2016). Specifically, van der Maas and Kan (2016) stated that Gignac (2014, 2016) has taken an extreme, unpragmatic view by suggesting that the that the *g*-factor is "…the common cause of 'everything'…" (2016, p. 7). However, the orientation adopted by Gignac (2014, 2016) was consistent with a contemporaneous multi-factor model of intelligence:

> "… *g* factor theorists do not claim that *g* is the only factor of intelligence. Instead, *g* is considered the most substantial factor, alongside several, smaller, group-level factors…" (Gignac, 2014; p. 89)

> "…it is widely acknowledged that there are approximately 10 group-level factors of intelligence, in addition to *g*…" (Gignac, 2016; p. 69).

Thus, it should be made clear that the results reported in Gignac (2014, 2016) do not support the notion that the *g*-factor is the only factor of intelligence. Instead, the main purpose of both papers was to evaluate the plausibility of the mutualism theory of *g*.

2. Attempt at clarification

It is unfortunate that many researchers, including van der Maas and Kan (2016), appear to be under the impression that the theorized process underlying *g* is an as yet unidentified, "mysterious" (p. 7) construct. Spearman (1927) theorized that *g* was the result of individual differences in "mental energy". Arguably, Spearman's (1927) theory of *g* was a victim of the relatively unsophisticated language of the time, as the term "mental energy" does not exactly inspire thoughts of scientific rigour in today's age. Lykken (2005) reframed Spearman's theory of mental energy into a theory of sustained concentration. Arguably, all intelligence tests require the capacity to attend to stimuli in a concentrated manner for successful completion. Furthermore, individual differences in the capacity to sustain concentration exist (e.g., Unsworth, 2015). However, no single test would be expected to measure comprehensively individual differences in sustained concentration, as any single test would be constrained by stimulus modality and task type. Instead, a battery of tests based on a diverse source of stimuli and task types would be ideal for such purposes

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(Jensen, 1998). In light of the above, what is mysterious about a latent variable specified to represent individual differences in the capacity for sustained concentration to account for the positive correlations between cognitive ability tests?

3. Possibility versus plausibility

van der Maas and Kan (2016) have provided simulation evidence to suggest that the correlation matrices used by Gignac (2016) can be shown to be equally well accounted for by a mutualism model of *g*. The mutualism model specified to account for the data as well as the higher-order model is displayed in Fig. 2 (middle panel) of van der Maas and Kan (2016). Strictly speaking, I do not object to the veracity of the results reported in van der Maas et al. Statistically, the extraordinarily complex network model displayed by van der Maas and Kan (2016) in Fig. 2 is a *possible* representation of the data. However, by my count, there are >250 estimated parameters associated the mutualism model depicted in Fig. 2. By contrast, the well-fitting higher-order model depicted in Fig. 2 (lower panel) by van der Maas and Kan (2016) is associated with 36 freely estimated parameters.

It is a longstanding tradition in model testing to favour a simpler model over a more complex model (Bollen & Long, 1993; Bozdogan, 1987). Contrary to van der Maas and Kan (2016), the preference in science for simplicity is not "…a religious view…" (p. 7). It is a view predicated principally upon evidence relevant to generalizability: the parameters associated with simpler models tend to replicate better than the parameters associated with a more complex models (Browne, 2000; Forster & Sober, 1994; Myung, 2000). Consequently, indices such as the Bayesian Information Criterion, the Akaike Information Criterion, and the Cross Validity Index are used to evaluate competing models, as they incorporate penalties for model complexity (Browne & Cudeck, 1989; Pitt & Myung, 2002). From this perspective, it is interesting to point out that van der Maas and Kan (2016) reported the RMSEA associated with the higher-order model (well-fitting, <0.05). However, they did not report RSMEA for the much more unrestricted mutualism model.[1] The results of Gignac (2016) suggest that many of the positively directed parameter estimates associated with a mutualism model would not replicate.

Finally, whereas the higher-order model of intelligence is an a priori theorized model that can be falsified, the mutualism model depicted in Fig. 2 of van der Maas and Kan (2016) appears to contain a very large number of ad-hoc model modifications, a model generation approach that has been criticised (e.g., Bentler, 2007). Again, the mutualism theory of *g* is possible, but on grounds of widely recognised competing model evaluations (e.g., parsimony, generalizability, falsifiability, post-hoc specifications), it does not seem preferable to a model which incorporates a *g*-factor, such as the higher-order model or the bifactor model.

4. Intelligent people can be expected to disagree

van der Maas and Kan (2016) also took issue with a previously published paper of mine relevant to the mutualism theory of *g* (i.e., Gignac, 2014). They accepted the logic of the indirect test, but not my interpretation of the results. Specifically, van der Maas and Kan (2016) contended that the results reported in Gignac (2014) should not be considered evidence contrary to mutualism, because children less than the age of 2.5 years were not included in the analysis. Although certainly a limitation, and identified as such in the discussion of the manuscript, the available evidence, particularly Fig. 4 in Gignac (2014), lead me to conclude that the results were a failed confirmation of the mutualism theory of the *g*. The reviewers agreed. Many individuals to whom I have presented the results have also agreed. Importantly, however, some individuals did not agree that the results were a failed confirmation of theory. In fact, a small number of individuals viewed the results as a confirmation of the mutualism theory of *g*!

How intelligent people can disagree strongly upon the interpretation of the same evidence is an interesting question. Recently, it has found that belief in scientific propositions such as global warming, vaccinations, and genetically modified foods, for example, can be predicted substantially by a combination of individual differences variables such as conservatism, free-market orientation, and the propensity to believe in conspiracy theories (Heath & Gifford, 2006; Lewandowsky, Gignac, & Oberauer, 2013).

In light of these results, it may be hypothesized that a person's inclination to believe or disbelieve in the plausibility of the general factor of intelligence is related meaningfully to individual differences in personality, attitudes, and worldviews, rather than rely strictly, upon logical and statistical arguments. Consequently, as personality, ideology, and attitude type variables relate to intelligence only relatively weakly (<|0.30|; DeYoung, 2011; Heaven, Ciarrochi, & Leeson, 2011), very intelligent individuals may be expected to disagree on the matter of the plausibility of *g*, despite the possibility that they have examined precisely the same evidence. Although the continued generation of empirically testable hypothesis relevant to the mutualism versus *g*-factor debate is certainly encouraged, it may be time for differential psychologists to also turn their attention onto themselves from a psychological perspective to help understand why some researchers are more inclined to endorse a particular theoretical account of the data than others.

5. Conclusion

One of the great challenges faced by differential psychologists is that many of the constructs of interest are not amenable to experimental manipulation. Consequently, research designs which make available the possibility for clear and essentially incontrovertible results are left for want. Currently, the problem of choosing between the mutualism theory of *g* and a modern *g*-factor theory of *g* with a high degree of confidence is that there is no experimental evidence to which to consult. Consequently, coupled with the problem of equivalent models (MacCallum, Wegener, Uchino, & Fabrigar, 1993), the mutualism versus *g*-factor debate is very unlikely to be resolved with any particular investigation. On balance, currently, I believe the evidence is not in favour of the mutualism theory of *g*. Hopefully, over time, an accumulation of evidence will emerge which will facilitate the possibility of the formation of a consensus within the scientific community on this matter, one way or the other (or another!).

[1] RMSEA incorporates a penalty for model complexity (Curran, Bollen, Chen, Paxton, & Kirby, 2003).

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