Continued methodological imprecisions, nontransparency, and misinterpretation in a meta-analysis of semantic fluency in schizophrenia: a comment on “Corrigendum to: Semantic memory impairment across the schizophrenia continuum: a meta-analysis of category fluency performance” by Tan et al.

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Tan, Neill, Tomlinson, and Rossell [2020 (*Schizophr Bull Open* 1(1), sgaa054)] conducted a meta-analysis of semantic fluency in people with high schizotypy, recent-onset patients with psychosis, chronic patients with schizophrenia, and first-degree relatives. For the first time, a meta-analysis of clustering on semantic fluency in patients with schizophrenia has been performed, among other analyses. In a comment on their study, I expressed serious concerns about the methodological validity of the clustering meta-analysis (Gabrić 2021). Specifically, I have criticized (1) errors in the terminology, (2) errors in the descriptions of the studies used for the meta-analysis, (3) the combination of different (incompatible) clustering variables and different thresholds for cluster classification, (4) the selective use of the statistics from these studies where important confounding factors were not controlled (despite the presence of statistics where the confounding factors have been controlled), (5) the unexpectedly small

number of analyzed studies (despite the presence of other studies), as well as (6) nontransparency about points four and five. In a response to my comment, Tan and associates issued a corrigendum to their study (Tan et al. 2021). Blaming “unintentional manual entry errors in clustering variable definitions and participant groups [that] occurred during the data screening stage”, the authors corrected points (2) and (3) in their study, leaving points (4)–(6), however, completely unaddressed, while point (1) remains an issue of its own. Despite this, Tan et al. (2021) boldly, and perhaps unusually, assert that “[t]he interpretation of the data remains *correct*¹”. Furthermore, Tan et al. (2020) still erroneously maintain that “mean cluster size” (pages 1, 4, and 8) was significantly smaller in patients with schizophrenia compared to healthy subjects, even though this variable was not analyzed in their meta-analysis. In the present comment, I briefly argue that Tan et al.’s (2020) meta-analysis contains serious methodological imprecisions, it remains methodologically nontransparent, and their interpretations are not backed by the conventional ontology of semantic fluency.

Tan and associates recognized from my first comment (Gabrić 2021) that they erroneously described the clustering studies as analyzing the “mean cluster size”. Specifically, Berberian et al. (2016), Bozikas et al. (2005), and Kosmidis et al. (2005) calculated the *raw number of clustered words*, not *mean cluster size*. Although this has been corrected in certain parts of their paper, the authors still maintain in the abstract, description of the statistical procedures, and discussion that they analyzed the “mean cluster size” from these studies. An important distinction should be made between the raw number of clustered words and mean cluster size, as only the latter reflects the specific mechanism of semantic clustering. This is because the total number of words produced is indirectly controlled by calculating the mean cluster size, which is

¹ Emphasized by PG.

not the case if only the raw number of clustered words is calculated. This is relevant because a higher number of words produced greatly (perhaps even unavoidably) increases the subject's chances to produce a higher number of clustered words (cf. Troyer 2000; Troyer & Moscovitch 2006; Troyer et al. 1997). In other words, the raw number of clustered words reflects more the subject's productivity rather than clustering ability. This is why in a number of studies this variable (as well as the raw number of *switches*) is analyzed by controlling the total output.

Furthermore, as discussed in Gabrić (2021), Tan et al. (2020) analyzed only those statistics from the studies where the total output was not controlled (inferring from Table 1), even though two studies offered both the non-controlled and controlled statistics (Berberian et al. 2016; Bozikas et al. 2005). Tan et al. (2020) did not explicate this for the readers of their paper and this has yet again not been explicated in the corrigendum. This approach by Tan and associates is especially intriguing given that Bozikas et al. (2005) reported no significant differences between patients and healthy subjects after controlling for total output, while the test values (and thus presumably effect sizes) were considerably smaller in both studies when controlling for total output compared to when the output was not controlled. Thus, Tan et al. (2020) failed to specifically investigate the mechanism of clustering in patients with schizophrenia compared to healthy subjects. This has, however, not stopped the authors from asserting that the putative clustering deficit in schizophrenia “may be more aligned with characterizations of a degraded semantic store with less category exemplars available, and supports previous propositions that the [semantic memory] deficit in the chronic stages of the disorder are [sic] more storagerelated [sic]” (p. 9), as well as that their clustering analysis “provide[s] a more sensitive gauge of [semantic memory] processing and function in real-time” compared to the sole productivity score

(p. 11). It is unclear to me how such inferences can be drawn from calculating the raw number of clustered words.

Another unaddressed issue from Tan et al.'s (2020) meta-analysis, also discussed in Gabrić (2021), is the unexpectedly small number of studies included in the analysis. Originally, Tan and associates included four studies in their clustering meta-analysis. This was reduced to three studies after the publication of the corrigendum and removal of the study (Piras et al. 2019) which analyzed a different clustering variable from the ones analyzed in Berberian et al. (2016), Bozikas et al. (2005), and Kosmidis et al. (2005). Tan et al. (2020) write they included in their meta-analysis “relevant studies published up to October 2019”. It remains unclear why, for example, the studies by Allen et al. (1993), Batty et al. (2015), Elvevåg et al. (2002), Landrø & Ueland (2008), Moore et al. (2006), Okruszek et al. (2013), Popescu & Micluția (2006), Rinaldi et al. (2013), Robert et al. (1998), and Veleva et al. (2019) should be considered “irrelevant”. Even if they were “irrelevant”, it remains unclear why they were not at least included in Table 1 (and why at least the productivity scores were not extracted from these studies) and it would be important to transparently explicate why these studies were not included in the meta-analysis. The non-inclusion of Batty et al. (2015) is especially interesting given that Susan Lee Rossell, the second author of the criticized meta-analysis, was the second author in that study as well.

In conclusion, even after issuing a corrigendum, Tan et al.'s (2020) clustering meta-analysis contains serious methodological imprecisions, it remains methodologically nontransparent, and the interpretations are not backed by the conventional ontology of semantic fluency.

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