

The Scales and Sample Were Adequate for the Purpose: Reply to Lawrence's (2014) Critique of My Taxometric Analysis of the Sexuality of Transsexual Women

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Blanchard (1989b) proposed that there are two categorically-distinct types of transsexual women (male-to-female transsexuals) and this typology is based on one of these types having sexual attraction to oneself as a woman. He labeled this type of sexual attraction *autogynephilia*. Blanchard proposed that those who do not experience autogynephilia are sexually attracted only to men (*androphilic*) and those who experience autogynephilia are sexually attracted to women only, both men and women, and neither men or women (*non-androphilic*). I conducted a taxometric analysis (Ruscio, Haslam, & Ruscio, 2006) to assess whether the sexuality of transsexual women more was consistent with a dimensional or two taxa (category) structure (Veale, 2014). In opposition to Blanchard's theory, the study's findings were more consistent with a dimensional structure.

Lawrence (2014) critiqued my study, suggesting that the scales I used were not appropriate for the task and that the sample I used lacked sufficient numbers of androphilic transsexual women. In this letter, I respond to Lawrence's criticisms.

Suitability of the Scales Used

Response Options for the Scales

Lawrence (2014) noted that some of the measures used in the study—notably adaptations of Blanchard's Core Autogynephilia Scale and Autogynephilic Interpersonal Fantasy scale and scales measuring androphilia and gynephilia—asked participants to respond with the amount of time that they are sexually attracted to these things. While I had noted that this is a limitation because it is “not consistent with standard definitions of sexual attraction” (Veale, 2014, p. 1184), Lawrence pointed out that this underestimated these sexual attractions because she believed that participants are unlikely to endorse one of the response options that they experience these sexual attractions “all the time.”

This underestimation would be a problem if I had been trying to estimate the overall prevalence and intensity of these sexual attractions in the sample. Having a potentially lower mean score generally would not affect the results of taxometric analysis as long as this effect could not be expected to impact either of the types differently, if any taxa do exist. There would only be a problem if the response options resulted in a restriction of range of the scores. However, there was no evidence of this, as illustrated in Figure 1 in the article (Veale, 2014) and reproduced

as Figure 1 here, which shows scores for two of these scales that span the breadth of possible range, from 0 to 20.

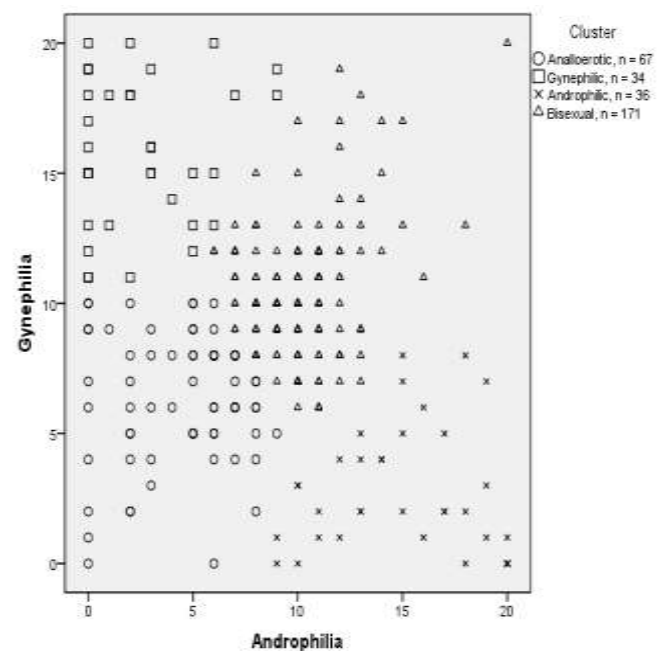


Figure 1. The results of the K-means cluster analysis of scores of sexual orientation in the sample

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Content Validity of the Scales

Lawrence (2014) believed the study was invalid because three out of the four scales that were used in the taxometric analysis “lacked content validity with respect to the fundamental typology that Blanchard (1989) described” (p. 2). By this, she means that the Autogynephilic Interpersonal Fantasy, Attraction to Feminine Males, and Attraction to Transgender Fiction scales did not sufficiently measure what Blanchard proposed would distinguish the two subtypes, autogynephilia.

My response to this is twofold: Firstly, the initial analysis was exploratory in nature. Working on the theory that there are two

categorically distinct types (taxa) of transsexual women's sexuality and my knowledge of the empirical work of Blanchard to link a number of aspects of sexuality (including masochism and sexual attraction to femininity in males) to this distinction (Blanchard, 1992, 1993; Blanchard & Collins, 1993; Chivers & Blanchard, 1996), I decided to conduct the analysis by entering the twelve sexuality scales into the first part of the analysis (see Veale, Clarke, & Lomax, 2008 for a list of these scales) to see which of these possessed sufficient validity to separate the proposed taxa in the analysis. The four sexuality scales that are more unique to transgender and transsexual people's sexuality and more similar to Blanchard's concept of autogynephilia were the ones that had the distributional properties to be able to distinguish two putative groups at the recommended level of $d > 1.25$. This is a finding that is in accordance with Blanchard's typological theory.

Lawrence's (2014) belief that the Core Autogynephilia Scale is the only scale that I should have used in this analysis reflects a belief that autogynephilia, or sexual attraction to oneself as a woman, is the sole facet of this type of sexual attraction. The high correlation between autogynephilia and these other sexual attractions could be explained by supporters of Blanchard's theory as this core autogynephilia causing these other types of supposedly more peripheral attractions. However, this is a theoretical position that I do not hold. It seems to me that a more parsimonious explanation is that the variation on each of these scales could be accounted for by a single underlying sexuality factor. Researchers have given this construct many names, among others, I have called this *cross-gender eroticism* (Veale, Lomax, & Clarke, 2010), Serano (2010) called this *female/feminine embodiment fantasies*, and Ekins (1997) called this *erotic femaling*. Indeed, this conceptualization of a broader construct was supported by my finding that a broader range of measures than just the Core Autogynephilia Scale distinguished putative groups and the Attraction to Transgender Fiction Scale had the largest effect size for doing so (Veale, 2014).

Lawrence (2014) rightly pointed out that content validity is a key requirement for taxometric analysis. This means using measures that "assess all relevant facets of the target construct" (Ruscio et al., 2006, p. 72). By only using a measure of core autogynephilia in this analysis, as Lawrence suggested, I believe I would have actually been overlooking some relevant facets of the construct and the analysis would have lacked the requisite construct validity.

The breadth of the underlying construct is a position of theoretical difference between Lawrence and myself that remains untested empirically.

Suitability of the Sample

The reason that taxometric analysis is needed to assess whether there are categorically-distinct groups is because latent constructs like sexuality are assessed with measurement error. This means that even when there are two distinct groups (or taxa), the measurement error will decrease the precision of the measurement, causing overlap in how the members of the different taxa score on the scales used to measure these categories (see Ruscio et al., 2006, pp. 37–45 for further discussion). To illustrate this, I alert the reader to the fact that

there were no clear boundaries between androphilic and non-androphilic transsexual women in measures of sexual orientation in my sample (see Figure 1) and in Blanchard's (1989b) original sample (see Figure 1 in Blanchard, 1989a). This does not necessarily mean that transsexual women's sexual orientation is dimensional (not taxonic) because the measurement error can cause the overlap and not show a clear categorical boundary. This is why more complex taxometric analyses are needed to distinguish if categories exist or not.

Keeping in mind that each participant's individual response on the androphilia and gynephilia scales is influenced by measurement error as well as their sexual orientation, Lawrence's (2014) painstaking judgement of whether each case is worthy of its categorization based on each individual response for those in only one of the two putative taxa is unhelpful. Firstly, it is treating each response as if it is not influenced by measurement error. We use scales rather than single items to measure constructs that are not directly measurable to average out any error and any inconsistencies a single item might have (DeVellis, 2003) meaning any critique of participant classification should be based on scale total scores rather than individual item responses. The similarity in the categorization in my study with Blanchard's (1989a) study is notable—both were arrived at using cluster analysis and the androphilia and gynephilia scale cut-points were similar. Secondly, Lawrence's inspection of the individual responses was imbalanced as only one of the proposed two categories underwent this scrutiny.

Nevertheless, out of the 36 (12%) of the sample that were categorized as androphilic based on the results of a cluster analysis, Lawrence (2014) raised concerns about up to 21 of these cases. Foreseeing this criticism, I also performed the inchworm consistency test. This test is used to detect distinct taxa that might be smaller than the 10% that is the recommended minimum for the other taxometric analysis procedures (Ruscio et al., 2006). With the cases Lawrence raised concerns about recategorized as not androphilic, this still leaves 5-6% of the total sample. As I had noted in the original, the results of the inchworm consistency test showed no evidence of any smaller taxa (Veale, 2014).

Conclusion

Lawrence (2014) critiqued my taxometric analysis of the sexuality of transsexual women on the grounds that the scales used would underestimate sexual attractions and were not consistent with Blanchard's core typology. She also suggested that the sample contained insufficient numbers of androphilic respondents. In this letter, I responded that any underestimation, if it did occur, would not have affected the results of the taxometric analysis. I also countered that the measurement sexual attraction to oneself as a woman is a matter of theoretical perspective and gave discussion of why a broader assessment of the construct might be required to achieve the adequate content validity required of taxometric analysis. In response to Lawrence's criticism of the sample, I argued that the sample was sufficient by outlining problems with interpreting each individual response in a scale among only one of the two proposed categories in the way that Lawrence did. Finally, I pointed out that there still no sign of an even smaller distinct taxon of

androphilic transsexual women in the analysis. Nevertheless, I stand by my original claim that this analysis should be replicated on a larger sample of all groups (Veale, 2014).

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