Response to Tozdan and Briken´s (2016) “Accepting Sexual Interest in Children as Unchangeable: One Claim Fits for All?”

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In their Letter, Tozdan and Briken (2016) discussed and criticized the results of our study, which investigated the stability of sexual arousal to minors via self-reports in a non-forensic sample (Grundmann, Krupp, Scherner, Amelung, & Beier, 2016). Tozdan and Briken raised a concern that the treatment strategies applied in the context of the studies have a crucial impact on the outcome of the results. Specifically, the aim of the Berlin Dissexuality Therapy Program (Berlin Institute of Sexology and Sexual Medicine, 2013) is to obtain behavioral control over sexual impulses and does not aim at intervention in terms of a direct change of pedo-/hebephilic arousal. This notion is in line with common clinical and scientific practice, as the success of enduring modifications of pedo-/hebephilic arousal has not been demonstrated convincingly thus far (Seto, 2012). However, we cannot estimate what effect this attitude has on our clients' expectations or the outcome measures of this study. Tozdan and Briken argued that this approach may hinder remission of a pedophilic disorder. A remission specifier was added to paraphilic disorders in DSM-5 (p. 687). Sadly, following the board of trustees rejection of the proposed criteria for Pedophilic Disorder and retaining the DSM-IV-TR criteria, this remission criterion was not added for Pedophilic Disorder (see also Blanchard, 2013). Briken, Fedoroff, and Bradford (2014) warned of iatrogenic fixation of patients suffering from Pedophilic Disorder following this decision, and we agree that omitting the "in remission" specifier in the DSM-5 criteria for Pedophilic Disorder was regrettable.

Still, the data of our study do not necessarily contradict their view as the remission criterion included in the DSM-5 is applicable for a paraphilic disorder if "the paraphilic interest has ceased to cause dysfunctions for at least five years in an uncontrolled environment" (Beech, Miner, & Thornton, 2016). The criterion thus refers to a level of social functioning but not to the intensity of experienced sexual arousal, as it was measured via self-reports in our study. Therefore, our data cannot

be used to support the decision on the lack of any “in remission” specifier in the recent version of the DSM-5.

In their Letter, Tozdan and Briken (2016) continue to discuss the methods used in the study and the interpretation of results and suggest further analyses. For example, concerning Study 1, Tozdan and Briken argued that a (one-sample) chi-square test of independence could have supported the interpretation of a mostly early age of onset of arousal to an age-gender category. In the one-sample chi-square test, a categorical variable is tested against a hypothesized distribution. In our case, the Null-Distribution tested against was a random distribution of early and late onset. Rejection of this distribution indicates over-representation of one of the two categories. Gladly following their recommendation, we calculated the respective chi-square tests to the following results: $\chi^2 = 6.42$, $p = .011$ for prepubescent females; $\chi^2 = 23.61$, $p < .001$ for prepubescent males; $\chi^2 = 5.75$, $p = .016$ for early pubescent females; $\chi^2 = 20.83$, $p < .001$ for early pubescent males; $\chi^2 = 121.00$, $p < .001$ for adult females; $\chi^2 = 8.45$, $p = .004$ for adult males. Early versus late onset of all age-gender categories can thus be assumed to be unequally distributed with early onset being over-represented according to the relative frequencies as reported in the article. This additionally supports the hypothesis of a mostly early onset of sexual arousal to pedo-/hebephilic contents.

Tozdan and Briken (2016) further commented that there were methodological restrictions to our study, which would not allow one to come to conclusions concerning stability. For example, they argue that for the investigation of stability of arousal scores over three assessments, the short average observation periods were a limiting factor and suggested a comparison between T0 and Tpost (after treatment). We agree that for this comparison the average observation periods were rather short (T0-Tpre: 10.9 months, *SD* = 7.0; Tpre-Tpost: 13.8 months, *SD* = 2.8).

However, in a first step, we examined arousal scores over a maximum observation period between T0 and T* (28.8 months, *SD* = 13.3). Our study did thus include a comparison with an observation period that we saw fit to allow for first conclusions. In the second set of analyses in Study 2, we aimed to investigate the changes of arousal in an observation period with and without treatment. For this, we decided not to include the comparison between T0 and Tpost. The comparisons requested by Tozdan and Briken bring no different picture than the ones published in the original article: The *z*-scores of the Wilcoxon tests (two-sided) and the Spearman's rho between the T0 and Tpost assessment (average time 25.3 months, *SD* = 8.0) for *n* = 31 are as follows: *z* = -0.24 and ρ = 0.62 for prepubescent females; *z* = -1.13 and ρ = 0.95 for prepubescent males; *z* = -0.36 and ρ = 0.81 for early pubescent females; *z* = -0.63 and ρ = 0.78 for early pubescent males. All Spearman's rho are significant with $p < .001$. Overall, the values are comparable to those given in Table 3 and Table 5 of the article.

Tozdan and Briken also claimed that the detected significant change in arousal scores to prepubescent males between T0 and Tpre (before treatment) ($z = -2.41$, $p < .05$), i.e., a significant decrease in mean-level for arousal to prepubescent males within the sample of *n* = 31 individuals with complete data for three subsequent assessments had not been addressed sufficiently. It is indeed not easy to interpret this finding satisfactorily as in the longer observation period between T0 and T* and within a larger sample a similar drop in mean-level arousal scores for prepubescent males was not found. Also, within this category the greatest between-group correlation (ρ = 0.89, $p < .001$) was observed, suggesting a highly consistent within-group change. No comparable data for changes within an age-gender-category are available. Given the dissolution of the effect in the larger sample, a selection bias appears probable. Hypothetical factors that may have contributed to

the change of sexual arousal to prepubertal males in this subsample include statistical artifacts as well as potentially therapeutically accessible processes. Their clarification must remain to future studies.

In their critical comment, Tozdan and Briken highlighted interesting yet still unpublished results of their own work that focus on an individual's "Specific Self-Efficacy for Modifying a Sexual Interest in Children" (Tozdan et al., 2016a, b). However, until the finalization of this Letter, their data were unfortunately unavailable impeding us to dispute their arguments (a request was declined as their article was under revision).

Perhaps most importantly, we feel that Tozdan and Briken (2016) incorrectly equate the concepts of stability and immutability. However, stability and immutability cannot be used synonymously. Only a careful separation of concepts as immutability vs. mutability on the one hand and the investigated stability or variability of arousal on the other will allow for a differentiated discussion. We believe that the results of our study are an important contribution to the ongoing discussion on the understanding of pedophilia (and hebephilia) in the area of clinical diagnosis and forensic research. To come to a broader understanding of the course of sexual preferences, findings of other work groups are needed to add to our data.

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