Response to "Is Basson's Model of Sexual Response Relevant? A Commentary" by Dr. Balon

Gunst, Annika

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Response to "Is Basson's Model of Sexual Response Relevant? A Commentary" by Dr. Balon

Gunst, Annika (Ph.D., Psych.)

Dr. Balon raises several important points in his commentary on the relevance of Basson's sexual response model. Among these are the importance of empirical evaluation for the development and improvement of medical guidelines and the notion of mixed receptions concerning the FSIAD diagnosis. I have decided to highlight a few points I think deserve further attention. These points mainly circle around the questions raised by Dr. Balon: *"Is Basson's model more reflective and suitable for women with unsatisfying sexual response or with FSD? Is the linear model used at present more reflective of "normal", trouble-free response? Why use a model of presumably normal sexual response which reflects more pathology than "normalcy?" Why has the model not been validated/empirically tested in solid multi-center field trials?"*. In order to keep my response concise, I have intentionally left out the discussion on the conceptualization of sexual desire versus sexual arousal and the discussion on whether sexual desire can exist in the absence of external stimuli (i.e., the incentive motivation theory; Toates, 2009).

First of all, Dr. Balon questions why Basson's model has not been empirically tested in multicenter field trials. It remains unclear to me what type of evaluation Dr. Balon is looking for: self-identification with Basson's model, evidence for the existence of motivations to engage in sex other than 'spontaneous' desire, or evidence that sexual stimuli can trigger sexual desire? Although there may not be any coordinated multicenter studies, there is not a complete lack of empirical evidence. For instance, in their recent article on the development of a measure for assessing responsive desire, Velten et al. (2020) provides a summary of studies assessing the relationship between sexual stimuli and increased sexual desire. Several studies also investigate various motives for having in sex (e.g., Cooper et al., 1998; Meston & Buss, 2007). Dr. Balon states that online-based studies are difficult to interpret but gives no further clarification on why this would be the case. Further, considering the recent publication by Velten et al. (2020), the possibilities for studying responsive desire in multicenter designs are perhaps better than ever at present.

If, however, by lack of multicenter studies, Dr. Balon refers to studies investigating how certain sexual response models associate with distress or dissatisfaction, there are some potential pitfalls when interpreting such results that need to be considered. Take, for instance, the study by Sand and Fisher (2007). First of all, the study did not include personal distress

among the study measures. Thus, we are only talking about lower scores on the Female Sexual Function Index (FSFI), which is indicative of sexual function difficulties, but insufficient for dividing participants into sexually functional and dysfunctional. The authors, however, did assess sexual and relationship satisfaction, and found that women relating to the linear model were more satisfied sexually and in their relationships. Although this satisfaction could (at least partly) stem from 'spontaneous' desire being more rewarding in itself compared to responsive desire (in my opinion, this is still not a sufficient argument for claiming 'spontaneous' desire as the norm for healthy desire), I think we should also consider alternative explanations. Part of this lower satisfaction could arguably stem from the stigma around responsive desire due to cultural scripts and portrayals of 'spontaneous' sex as superior (Dune & Shuttleworth, 2009). Distress or dissatisfaction about low sexual desire should be interpreted with caution, keeping in mind potential distress due to social deviance (i.e., distress due to violation of social norms; see for instance discussion by Abouelleil & Bingham, 2014). The fact that those who identified with Basson's model reported lower scores on the FSFI could possibly also be explained by how these participants (and their partners) dealt with the situation: was enough time provided after initiation of sexual activity in order to attain sufficient arousal and lubrication (and thereby also prevent pain difficulties)? These are, of course, somewhat speculative explanations, but nonetheless aspects that have commonly been neglected when drawing conclusions about healthy sexuality based on reports of distress and dissatisfaction.

Moreover, although there are slight disagreements on the operational definition of sexual desire (e.g., the inclusion of sexual fantasies and the overlap between sexual desire and arousal), a commonly agreed upon definition of sexual desire includes some form of motivational state and interest in sexual behaviors and experiences (e.g., Brotto, 2010). As such, the inclusion of sexual desire in any model of the sexual response is deemed to be unfit, as we are talking about a broad cognitive and motivational aspect of sexuality that does not necessarily have a specific temporal position in the context of sexual activity. A motivational state can arguably be present or absent throughout any sexual activity—or in the absence of sexual activity. Thus, its conceptualization as part of the sexual response is questionable. This is not to say that sexual desire should be neglected when considering individual sexual health, personal wellbeing, and intimate relationships. After all, sexual desire difficulties are often reported as the most common sexual difficulty in women (Burri & Spector, 2011; Shifren et al., 2008; Witting, 2008).

I would want to take the discussion in a different direction by raising another question: Are we approaching (women's) sexual desire difficulties from the right point of view? To my understanding, one of the main reasons for including responsive desire in the FSIAD diagnosis was to normalize experiences of desire other than 'spontaneous' desire (Basson, 2014; Brotto, 2010). However, a more overarching question than whether Basson's model is relevant is whether the HSDD and/or FSIAD diagnoses are, in their current form, relevant. There are at least two major problematic aspects of the HSDD/FSIAD diagnoses. First, it is unrealistic to find efficient 'one-size-fits-all' treatment alternatives for a diagnosis that has its basis in such a broad operational concept of sexual desire. This is supported by the vast body of literature suggesting that the etiology of desire/arousal difficulties is multifactorial, and that there are significant individual differences in treatment outcome (e.g., Gunst, 2019). For instance, Basson (2014) points out that it would be important to differentiate between physiological difficulties (e.g., closer to genital arousal) and difficulties stemming from more contextual factors (e.g., interpersonal/educational). The issue with group-to-individual generalizability has also been highlighted more generally in the context of human subjects research (Fisher et al., 2018), suggesting a shift towards idiographic approaches. Secondly, categorizing (long-lasting and distressing) low/absent motivation to engage in sex or to be recipient to sex as a sexual dysfunction has the potential to further contribute to the stigmatization and pathologization of both healthy reactions to the environmental context and individual variations in sexual desire. I wonder if the time is soon ripe for a paradigm shift. One step in that direction would be a similar solution for desire difficulties in the DSM as has been suggested for the upcoming ICD-11 manual (Reed et al., 2016), that is, moving desire difficulties (or desire difficulties without a clear physiological etiology) to a separate chapter for sexual health conditions. By doing so, the need for support in health care would still be acknowledged, while potential stigmatization and pathologization would be minimized. Such a solution should, of course, be preceded by an extensive evaluation of the potential advantages and disadvantages.

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