Critique of Prause Study

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There has been a lot of media attention to a recent study conducted by Dr. Nicole Prause and her colleagues titled “Sexual desire, not hypersexuality, is related to neurophysiological responses elicited by sexual images” published in *Journal of Socioaffective Neuroscience & Psychology*. My mailbox has been flooded with inquiries from colleagues, patients, and media about my reaction to this study. I’ve responded to some media requests such as Time Magazine to provide a balanced perspective. First, let me say that Dr. Prause is a credible researcher and her office is right next to mine here at UCLA. We have things we agree on and certainly have had our differences which we respectfully debate with each other on a regular basis. One of my initial reactions to this paper is that we should be thanking her for raising the bar on the debates around the phenomenon of hypersexual behavior. While most of my colleagues know I don’t advocate an “addiction” model per se for hypersexuality, this is merely based on scientific evidence which I believe is lacking to characterize it as such at the present time. I have published this position with colleagues elsewhere for review (Kor, Fogel, Reid, & Potenza, 2013). I also work with patients seeking help for hypersexual behavior and many of these individuals perceive themselves as having an “addiction” and I don’t discount their beliefs in therapy based on scientific nomenclature. Although Dr. Prause and I have both been trained in the scientist-practitioner model, she is more of a scientist and does not currently see patients although she is qualified to do so and taught doctoral practica on the topic in the past. Subsequently, she is looking at this issue through the lens of a scientist and using scientific methods to investigate sexually dysregulated behavior. I suspect Dr. Prause would acknowledge there are individuals who struggle with regulating their pornography consumption or the frequency of their sexual behavior with partners, commercial sex workers, and so forth; in fact, she seems to be acknowledging exactly this in all her media appearances. However, she would diverge from a common position that such patterns of behavior should be characterized as a “disease” or “addiction” without scientific evidence. So her recent study is challenging the validity of an addiction model or a theory of addiction to explain this phenomenon of sexually dysregulated behavior. An extension of her study would raise a larger question for debate: *what is an addiction*? This is all very important to understand given her present study at its foundation does not address the issue of whether individuals seeking help for sex addiction, hypersexuality, etc… are experiencing a legitimate problem. It asks whether an addiction theory is the best explanation for this problem or whether there are alternative explanations that help us better understand this phenomenon. That’s it! Somewhere in the mix up, the media has taken this and distorted it to suggest Dr. Prause’s study discounts the existence of sexual problems when it might have been more accurately described as a study challenging addiction as a theory to best explain what is happening with individuals who experience sexually dysregulated behavior.

There are of course, other relevant points to be made. The first is whether a brain marker of any kind (e.g. P3, BOLD activation in fMRI studies, etc…) can or should be consider evidence for the presence or absence of a disorder. This is a significant assumption in many imaging studies that is often overlooked, yet, it’s at the heart of how we might explain and interpret results of science utilizing measures of EEG, fMRI, DTI, and so forth. Keep in mind however, that this also works both ways. We have to be careful suggesting that imaging studies “prove” that hypersexuality or sexual addiction is a legitimate disorder.

Some critiques and commentaries have emerged on the internet on sites like *Psychology Today* (e.g., Mr. Gary Wilson; Dr. Brian Mustanski). As I’ve looked at some of the critques, I quite frankly disagree with some of them and think they are inaccurate. I’ll address a few of these and then go on to make some points I think we should raise in response to Prause’s study. [Note: Mr. Wilson's posting on *Psychology Today* has since been removed]

Mr. Wilson has attempted to assert that Dr. Prause has failed to sufficiently analyze an SDI subscale used in her study. Mr. Wilson has erroneously missed information in her article. The Solitary SDI subscore was computed, analyzed, and reported alongside the Dyadic Scale as described in the paper. The paper states “Both are investigated,…” and “Effects that did not reach statistical significance, defined as $p < 0.05$, are not discussed.” The Solitary scale was not related to the P3. The Dyadic subscale is far more commonly used in the literature and thought to be less subject to reporting bias (“I cannot wait to go home and masturbate” is not as acceptable as “I cannot wait to find an attractive person to have hot sex with”.) The data were fully represented from a widely-used, well-characterized scale. I’m sure Dr. Prause and her colleagues would share their non-significant finding values if anyone requested that data, however, non-significant values are often omitted from scientific papers. While they used three different measures of hypersexual problems, they acknowledge in their paper “Although several scales were analyzed in this study to increase the likelihood of identifying a scale that would be related to P300 variance, more scales exist (e.g. Reid, Garos, & Carpenter, 2011) that might better include the proposed core feature of high sexual drive.” For example, the Sexual Compulsivity Scale (SCS) might have been under endorsed by participants who were recruited for “problems regulating their viewing of sexual images” if they did not also feel out of control regarding their relational sexual behavior. Since the SCS has items related to relational sexual behavior, such items may not have been endorsed lowering scores on the SCS and may have possibly influenced results. This is one of the reasons why my research team developed the Hypersexual Behavior Inventory (Reid, Garos, & Carpenter, 2011) to overcome this limitation. Interestingly, Dr. Prause argues that her method of recruitment “appears to have successfully recruited participants with scores comparable to those labeled as ‘patients’ with hypersexual problems” citing Winters, Christoff, & Gorzalka, 2010 as a comparison. However, I’ve also indicated on other occasions that Winter’s method of classifying hypersexual patients fell short of what we might use in clinical practice. Moreover, I looked at the data from our DSM-5 field trial (one of the only studies published where a diagnostic interview based on the proposed hypersexual disorder criteria was to classify patients as ‘hypersexual’) and ran the descriptive statistics for our SCS data. These numbers were not part of our publication on the DSM-5 field trial (Reid, et al, 2012), but the SCS data for patients in our study yielded means (*Mean* = 29.2, *SD* = 7.7) that would be considered statistically significantly higher than the participants SCS scores in Prause’s study (*Mean* = 22.31, SD = 6.05). Subsequently, I would raise the issue that Prause’s sample does not parallel patients we normally see in treatment and she does appear to also acknowledge this in her paper where she concedes that samples may have differed from treatment seeking ‘sex addicts’ in other ways. In fairness to Dr. Prause, the proposed DSM-5 criteria for hypersexual disorder were not available to her at the time of her data collection.

Some have criticized the analysis, again, appearing to misunderstand statistics tests. In their study, the tests were regressions, not correlations. Correlations were titled “exploratory” in the article to investigate possible relationships that might have been missed with the regressions. These tests assume error in different terms, so are complementary, but different. For some reason, the main finding in the regression analysis is never described in any of the critiques by Mr. Wilson or others. The paper consistently describes these as “relationships” appropriately so these critiques aren’t particularly helpful and suggest Mr. Wilson misunderstands these statistical tests.

Some of the internet critiques mentioned above have also misrepresented how science works. Ideally, a theory is presented, and falsifiable predictions are made from that theory. The addiction model is consistent with an enhanced P3, whereas high sexual desire alone is not. It is, therefore, important that the results of those constructs were different. So, yes, the high sexual desire and the addiction models make different predictions, which allowed an examination of their separable effects.

Some have criticized the participants recruited in this study. They were apparently recruited as described in the study, stratified across scores on several measures of hypersexuality that have been used (and instruments such as the Sexually Compulsivity Scale which I have also used in my own early research in the field). This stratification allows for appropriate distribution of scores necessary for a valid analysis and is a common practice in research. The participants were required to report attraction to the opposite sex. I’m assuming that Dr. Prause did this to establish that the stimuli presented could be argued as relevant for all participants in the study. One point I might debate with Dr. Prause on this is the degree to which the standardized sexual stimuli used elicited sufficient sexual response, and thus in turn, influenced variance in P3 data. For example, it’s plausible that although sexual arousal was elicited by the sexual stimuli, we have no way of knowing how it might have differed if more explicit, more intense, or stimuli that better mapped to personal preferences were used instead. This issue is discussed at length among sex researchers and is actually very complex. Certainly a replication study using personal preferenced sexual stimuli could be conducted to see if the results remained the same. Prause would likely respond by stating that the stimuli have been used in hundreds of neuroscience studies and were extremely tightly controlled. She’d also likely state that speculations about the necessity of erotica matching specific preferences seems to rest on the assumption that these would be more arousing. She’d further argue that is indeed what was represented in the stimuli: lower and higher intensity sexual stimuli were presented. Visual sexual stimuli ratings were known, characterized, and have been published elsewhere already. This being said, she can’t discount the possibility that specific preference stimuli of a hypersexual population may have some caveats and it’s a future research question to determine if this would make a difference. She appears to acknowledge this since in her paper and interviews with the media she states that the study does need to be replicated.

One important issue that Dr. Prause did not report in her study was whether these patients were assessed for other comorbid psychopathology (e.g., ADHD), history of head trauma, medications, etc… that might have impacted P3 scores. I see this is a possible limitation in her findings. Not screening for such concerns has the advantage of testing a group that might look more like real patients, who we certainly do not refuse help on the basis of these, but has the disadvantage of possible affecting the P300. For example, P300 is affected to positive stimuli in depression, and we do not have depression diagnoses for her participants. A few critique's suggesting some of Prause's participants had "no problems" are likely inaccurate. She reported score values (see Table 2 in the paper). Variation in the level of problems is necessary for conducting regressions, which make assumptions such as Gaussian distributions. She also tried to cover her basis using three measures to capture "hypersexuality." It is difficult to claim all three have no utility. Again, I would argue, as noted above that SCS scores fall short of reflecting a patient population.

I've noticed some people mention Prause had no control group. Not sure this is a valid concern. She used a "within-subject" design and while old-school science might make people believe a separate group is necessary in a regression analysis, using a person as their own control, as occurs in a within-subject design, is actually is a stronger statistical approach. Control groups would be more appropriate for a longitudinal study such as whether pornography consumption is harmful. So, we can't fault her for issues with "control group" or argue that this approach was insufficient to address her research question. However, it might be argued that the within-subject control that they use is insufficient to make between-subject designs could answer other questions.

Criticisms of the cue-reactivity research protocols are likely not valid. I suspect they were likely precisely followed. Prause is very particular in this regard with her research. In substance abuse, eating, and gambling studies, people are presented with pictures of the objects they are struggling with and are not able to interact with them. Similarly, participants in her study were instructed not to masturbate or advance the images in the present study. There are thousands of cue-reactivity studies, many using within-subject designs that resemble the design in her study. It's an interesting criticism, but without further research, it's hard to assess if this would really make a substantial difference.

One online critique suggested that the P3 findings presented are conflicting? Not sure why this was concluded. This isn't true at all. For example, researchers have studied P3 among alcoholics to alcohol cues and to errors on a task. These are entirely different phenomena and are completely misrepresented in the critique. It's equivalent to calling "EEG" a measure of anything and suggests a lack of fundamental knowledge of EEG and neuroscience. Consider how Prause analyzed her data. First, the replication of the general P3 to emotional stimuli is shown. This has been shown thousands of times and is merely noted as replicated. "Given that this replicated expected, previous findings, the next planned test was conducted." Then, the relationship with sexual desire is examined, which has been studied before by others. Finally, the relationships with sexual problem measures are examined. As she has stated in her interviews, there was no relationship between the P3 measure and the measures of sexual problems. The study shows a very nice result linking P3 to erotic stimulus responses over other stimuli, but we don't know whether the relationship between P3 and the behavioral measures is indirect through other variables not measured in her study which could potentially offer alternative explanations for her findings.

One issue I might raise is my discomfort with Mr. Wilson's dismissal of EEG as a technology. EEG is still used in numerous laboratories across the world, and in some cases concurrently with fMRI. It's not that EEG doesn't have its limitations as noted by others (Polich, 2007), but they aren't the ones mentioned by Mr. Wilson in the context of Prause's study. A fair criticism might be that EEG is ideal for finding early, fast differences in brain response, where fMRI is ideal for finding where slower differences occur. Neither EEG nor fMRI is inherently a "best" measure. Again, however, as I noted at the beginning of this critique, it is questionable whether brain markers of any kind can or should be consider evidence for the presence or absence of a disorder.

Dr. Don Hilton, in a SASH ListSrv posting raises questions about the nuances of P3 but I think his stronger argument lies in how constructs such as "desire" and "craving" are operationalized and whether such operationalizations are a good proxy for the latent variable of interest.

Conclusions

So, in summary, I think the salient points are as follows:

- Prause's study attempts to ascertain whether a theory of addiction has explanatory power in predicting hypersexual behavior over high sexual desire alone. It doesn't address whether the phenomena of sexually dysregulated behavior is legitimate, only whether an addiction model offers a plausible explanation for such behavior.
- Prause makes a meaningful contribution to the literature insofar as she's starting to tackle questions related to a possible cohesive theory to characterize dysregulated sexual behavior. The sex addiction field and even my own work on hypersexual behavior has largely failed to contribute to a theoretical model of dysregulated sexual behavior. Some of the limitations of Prause's study are a direct result of our own limitations to actually define a testable theory of dysregulated sexual behavior whether it be an addiction model or some other model. Interestingly, no one has asked Dr. Prause if she has her own hypothesis of a model or whether she's simply going to continue to focus her efforts on falsifying other models.
- Her study assumes that her measures of desire and hypersexuality capture the latent variable she is studying. Although this is an assumption inherent in many studies including my own, we must remind ourselves that it is, nevertheless, an assumption.
- EEG is best for finding fast, early differences in brain activity, whereas other imaging techniques offering more detail about where differences happen. These other imaging approaches might bolster arguments for or against an addiction theory. Regardless, replication studies are necessary to provide further support of Prause's position, as from her study "As ever, these results warrant replication with different participants and protocols more focused on external validity."
- Questions about the sample of the participants in used in the study have some merit. Prause attempted to recruit patients, but was prevented from doing so by her local IRB. Any future replication studies should consider using the methods to classify hypersexual patients as per the methods in the DSM-5 field trial for hypersexual disorder. Future studies might also consider investigating concerns about the given study and specific preference stimuli of a hypersexual population. Future studies will also need to control for relevant comorbidity, psychopathology, history of head trauma, and medication

effects, although it is still difficult to know which are more important to control and the trade-off is external validity.

- The media has misconstrued some of Prause's findings. While she has some responsible to ensure the accuracy of such reports, many of us can relate to the media misquoting or erroneously reporting things we've said and should take this into consideration as we read reports about this study.

Note: Mr. Wilson's page on *Psychology Today* has been removed. *Psychology Today* will remove information from their website pages when it's considered erroneous, inappropriate, or in violation of copyright. There were certainly a substantial amount of errors in Mr. Wilson's work so perhaps someone at *Psychology Today* elected to remove it.

References

Kor, A., Fogel, Y. A., Reid, R. C., & Potenza, M. N. (2013). Should hypersexual disorder be classified as an addiction? *Sexual Addiction & Compulsivity*, *20*(1-2), 27–47.

Polich, J. (2007). Updating P300: An integrative theory of P3a and P3b. *Clinical Neurophysiology*. *118*(10), 2128-2148.

Reid, R. C., Garos, S., & Carpenter, B. N. (2011). Reliability, validity, and psychometric development of the Hypersexual Behavior Inventory in an outpatient sample of men. Sexual Addiction & Compulsivity, 18(1), 30–51.

Reid, R. C., Carpenter, B. N., Hook, J. N., Garos, S., Manning, J. C., Gilliland, R., Cooper, E. B., McKittrick, H., Davtian, M., & Fong, T. (2012) Report of findings in a DSM-5 Field Trial for Hypersexual Disorder. *Journal of Sexual Medicine*, *9*(11), 2868-2877.

Winters, J., Christoff, K., & Gorzalka, B. B. (2010). Dysregulated sexuality and high scexual desire: Distinct constructs? Archives of Sexual Behavior, 39(5), 1029-1043.

