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A response to Brody, Costa and Hess (2012): theoretical, statistical and construct problems perpetuated in the study of female orgasm

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A string of publications by Brody and colleagues purport to identify health implications in women who report reaching orgasm from vaginal/partnered versus clitoral/solo stimulation. Brody, Costa and Hess (2012) responded to a number of critiques of this line of research, including one by this author (Prause, 2011). Brody, Costa and Hess (2012) elected not to address two of the most critical problems raised: (1) a lack of any theoretical foundation and (2) the absence of psychometric support for their primary measurement. Rebuts are provided for the issues addressed. Also, new data are presented: (1) demonstrating the inadequacy of their methods, (2) falsifying of the vaginal-is-always-optimal prediction, (3) failing to replicate previous reports by Brody and colleagues and (4) documenting confounds of Brody and colleagues measures. Approaches for researchers interested in testing a vaginal-is-always-optimal prediction are suggested in an effort to raise the standards of the science in this area of study.

Keywords: orgasm; clitoris; female sexual response; orgasm source; sexual psychophysiology

Introduction

In reviewing the psychological consequences of human orgasm, Prause (2011), discussed a recent spate of publications proclaiming different health consequences specific to the source of orgasm. These articles originate primarily from Brody, and follow equivalently remarkable publications purporting that, for example, vaginal sex does not transmit HIV (Brody, 1996) and a defense of the mental integrity of adult stuffed toy owners (Brody, Costa, & Hess, in press). While certainly effective in generating controversy, these publications are neither theoretically grounded nor investigated with much scientific rigor. In a recent controversial clamor, Brody and colleagues published several articles on the topic of female orgasm. In these articles, it is claimed that orgasms resulting from penile-vaginal intercourse (PVI) or vaginal stimulation are associated with better physical and mental health outcomes than orgasms resulting from self-stimulation or clitoral stimulation. A recent issue in this journal focused on female orgasm. It included several papers from different researchers in this field pointing out numerous problems with the rationale, methods and interpretation in these publications, to which Brody et al. (this issue) have partially responded.

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Most regrettably, Brody et al. (this issue) elected not to respond directly to two central critiques raised by myself and others. First, they offer no proximal theory to suggest why one might ever predict a relationship between health and orgasm type. They eventually offer a vague evolutionary possibility as an explanation, but this is not adequate theory. Evolutionary theory has been described as a meta-theory of psychology that requires specific sub-theories (Duntley & Buss, 2008) and has been critiqued for a lack of falsifiable predictions (for response, see Buss, 1998). Even Brody et al. describe the evolutionary framework as “At a distal level” (p. 3) and then fail to offer a proximal theory. Lacking a proximal theory is a major problem, because science advances by falsification of the tenets of theories (Popper, 1963). Brody et al.’s perspective precludes falsification when they offer no proximal theory. Their story appears to begin and end with a single prediction that women who orgasm from PVI (or vaginal stimulation alone) have better physical and mental health relative to women who orgasm from self-stimulation (or clitoral stimulation). Since this is easily tested, it was falsified with data in Prause (2011). Additional falsifying data are offered below. The second major issue to which Brody and colleagues failed to respond is the absence of psychometric data supporting the self-report measure to assess orgasm. Data presented below show that the self-report measure they use is inadequate to test their predictions due to multiple confounds.

Responses to critiques

Brody et al. (2012) imagined that Prause (2011) concluded that “orgasm has no primary role in rewarding women’s sexual behavior”, where what actually was written was “At this time, data do not support the role of orgasm as a reward in women” (p. 3) where reward was defined, consistent with other literature, as “any cue that increases approach behavior” (p. 2). Strangely, the literature Brody et al. (2012) cite in support of their position does not, in any identifiable way, address the concept of reward. For example, reports of correlations between orgasm and “sexual satisfaction” provide no data relevant to whether orgasm prompts actual increases in sexual behavior, much less any evidence mapping its connection to actual reward. This example also demonstrates Brody et al.’s propensity to infer causal interpretations of correlational data. They continue by misrepresenting my data stating that, because women who self-stimulate their vagina “had more desire for sex with a partner”, these data “conform to the pattern” (presumably of vaginal orgasm being optimal). Actually, these data were not presented to address, and do not address, the rewarding nature of orgasm. Increased desire might reflect any number of things, such as a lack of sexual satiation, a shorter period of satiation, a priming
or upregulation of sexual arousability or other constructs not measured in that particular study. None of these potential components are inherently superior, as the context would likely determine which outcome is best. For example, sexual arousal increases intentions to take sexual risks (Prause, Staley, & Finn, 2011), which would be problematic only when sexual risk opportunities are available. Also, each possibility suggests somewhat different conclusions about the rewarding nature of orgasm. For review on current definitions of “reward”, readers are redirected to the original literature review of the definition of a reward in Prause (2011).

Brody et al. (2012) also declare that Prause (2011) misinterprets a poor association between orgasms and partner intimacy, stating: “when they are differentiated, relationship satisfaction is consistently associated with more frequent vaginal orgasm”. In fact, Prause (2011) sought evidence for orgasm serving as a reward by associating sexual behaviors with an intimate partner. The studies Brody et al. (2012) cite supporting this claim are not prospective, in contrast to the largely laboratory and prospective work Prause (2011) cited, which are scientifically relevant to make such causal attributions. As noted above, this is consistent with a pattern in which Brody fails to differentiate correlation and causality.

Brody et al. (2012) claim that attempts to replicate their research are not allowed. For example, they regard the results of Prause’s (2011) study showing that those who report reaching orgasm clitorally show greater sexual arousal to sexual films as “surprising” and of “doubtful ecological validity”. Replicability is a fundamental principle of science. The study also included (in addition to direct measures) self-report methods: the same approach used by Brody et al. Prause (2011) not only failed to replicate their curious findings, but actually reported the opposite response pattern. Brody et al.’s findings were not replicated, but they were falsified. Oddly, Brody et al. seem to fixate on the sample size in the original Prause (2011), although it exceeded (e.g., $n = 19$ [Brody & Kruger, 2006]) or met (e.g., $n = 38$ [Brody, Laan, & van Lunsen, 2003]) the sample size of many of Brody et al.’s efforts. Of course, appropriate sample size should be determined by power calculations, not subjective “smallness”. These failures to replicate and contradictory findings now have been extended in a larger sample with additional variables (see below), the full results of which are in preparation. Finally, if laboratory studies are irrelevant due to their lesser external validity, Brody will need to withdraw a few of his own publications and discount a considerable database of experimental work. While this is not the place to defend such a self-evident position, laboratory studies, of course, are valuable for the greater control and internal validity over self-report, questionnaire measures. As exemplified in the review of physiological, behavioral, clinical, qualitative and other methodological approaches in Prause (2011), different methods provide different types of evidence and cannot be discounted out of hand due to approach alone.

Remarkably, Brody et al. (2012) continue to misunderstand statistical power. Prause (2011) critiqued that the study by Brody, Laan and van Lunsen (2003) did not have enough statistical power. Brody et al. (2012) attempt to defend their work by stating “In the study in question, the small sample led to significant results” (p. 11). Actually, this is the opposite of power. Power refers to the ability to detect a difference when it is in fact present. Power is a function of effect size and sample size. The authors highlighted the absence of an effect of clitoral orgasm as evidence: “The concordance was unrelated to orgasm consistency during either masturbation or noncoital partner sex. Thus, it is specifically PVI orgasm consistency that is related
to integrating of vaginal response into the appraisal of arousal” (Brody, Laan, & van Lunsen, 2003, p. 21). This is an interpretation of a non-significant result. This is an error. Praise (2011) also showed in a repeated-measures power assessment that this was underpowered, so it should not be interpreted. It is unclear on what basis Brody et al. (2012) believe the power analysis of their own data “miscalculates power” (p. 11), since they provided no power analysis of their own to contradict the report in Prause (2011).

The measures of defense mechanisms are poor both theoretically and psychometrically, as shown by the evidence Brody et al. (2012) cite in their defense. Defense mechanisms are clearly portrayed as an indirect measure of mental health in the original publications (e.g., Bond, 2004), which defies the Brody et al.’s (2012) claim of interest in mental health. Why are direct measures not used? Furthermore, the measure is described in Brody et al. (2012) as “robust”, a term that is neither used in the cited article (Bond, 2004) nor supported by the failure to differentiate those with social phobia or obsessive-compulsive disorder from controls, underwhelming (<.5 SD) differentiation from panic disorder, and only one standard deviation difference in an unusual sample of child-abusing parents from controls (Andrews, Singh, & Bond, 1993). Brody et al. (2012) also fail to respond to the observation in Prause (2011) that they selectively reported only one of the scales, or that their other primary measure (from Lewin, Helmius, & Månsson, 2000) is psychometrically poor. As shown below, direct measures of mental health (anxiety and depression) do not differentiate orgasm groups. This represents yet another failure to replicate the pattern of data reported by Brody and colleagues.

The self-report measures used to assess orgasm origin in Brody et al.’s publications remain suspect. They mischaracterize Praise (2011) as arguing “against self-report studies”. In fact, Praise (2011) stated “Self-reports are of unusually limited utility in the study of female orgasm” (p. 7). This statement is supported by the data presented below. Brody et al. (2012) also disagreed with Praise’s (2011) description of their question to women as relying on women “to report whether their orgasm was attributable to PVI or clitoral stimulation” (p. 11). In fairness, this was my own summary of the nature of those questions, so, in the following paragraph, I further support my original critique by directly quoting the questions asked of women. As stated above, however, it is striking that Brody et al. (2012) continue to offer no psychometric data to support their primary independent/predictor self-report variables.

Women have been asked to report on their orgasm experience in a variety of ways in this literature (see Table 1). However, as self-report, women must be: (1) able and (2) motivated to report their experience. Brody has conceded this point in the past. Carey and Kalichman (1995) summarized Brody’s position on self-report questions as “opines that self-report of sexual and drug use history is unreliable due to intentional lying, social desirability responding, and poor recall” (p. 656). The questions (see Table 1) specifically rely on women being able to report whether orgasm was due to “orgasm from the activity” or “orgasm produced by PVI”. As also pointed out by Levin (2011), there are many reasons to believe women cannot provide this report due to physiological overlap in vaginal and clitoral structures. The data in Prause (2011) are supplemented below, adding psychological evidence showing that women cannot accurately report this information. Since no psychometrics have ever been reported by Brody on the orgasm questions he
regularly uses (see Table 1), including their correspondence with diary data, physiological indicators of orgasm presence, test-retest reliability and more, there remains no argument to support their use for this purpose.

Finally, Brody et al. (2012) make a strong claim that Prause (2011) “misleads the readership by falsely claiming…” an exclusion criterion in a study was not empirically supported. Later, Brody et al. (2012) show that the readership was not mislead by Prause (2011) – they do not quote any part of that publication supporting the criteria. Instead, they cite a different publication, apparently trying to bait and switch the reader to thinking the rationale was actually provided in the article. As stated in the original Prause (2011), this rationale did not appear in the article cited. The exclusion of female participants with shorter intercourse in the other article was rationalized as possibly indicative of sexual dysfunction. Reported length, then, represents only a proxy for one problem male sexual problem and an indicator known not to classify men well (Rowland, Cooper, & Schneider, 2001). This
rationale was not, as stated accurately in the original Prause (2011), provided in the manuscript.

New data

To further demonstrate the questionable nature of the methods used in Brody and colleagues’ line of research, additional evidence is provided. Levin (2011) addresses the physiological reasons why self-reported orgasm source is not reasonable, so here I address the psychological and statistical reasons why the approach is untenable. I use a larger yet \( n = 69 \) sample in a continuation of data collection from Prause (2011), the full results of which are in preparation. Briefly, this study invited women to the laboratory to complete a series of questionnaires. Women then viewed a series of short films during which they attempted to increase, decrease or not change their sexual arousal level.

Evidence of methodological inadequacy

First, women’s reports of a source of stimulation for orgasm are psychometrically unsound. Some women reported experiencing orgasm (“very sure” or “pretty sure”, \( n = 61 \)), being unsure \( n = 13 \) or that they did not experience orgasm (“probably do not” or “definitely do not”, \( n = 13, 1 \) did not respond). As research by Brody never reports continuous certainty of orgasm, results reported here also do not distinguish further on this basis. Also, the astute observer will notice that women’s sexual organs consist of more than a vaginal canal and clitoris. Women indicated the main area of stimulation that they felt “usually” contributed to their orgasm the most and the area (if any) that they felt “usually” contributed the second most to their orgasm (see Table 2). They could select “I do not experience orgasms” instead. As can be seen, women reported several sources of orgasm other than clitoral or vaginal (e.g., nipple) and most identified a secondary site.

The secondary-site responses show another problem with this method of assessment: “vaginal” or “clitoral” generated orgasms, consistent with the physiology, usually are felt to come from both structures. In fact, collapsed as shown in Table 2,

<table>
<thead>
<tr>
<th>Table 2. Self-report area(s) of stimulation contributing to orgasm.</th>
</tr>
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<tbody>
<tr>
<td><strong>Primary</strong> ( n ) (%)</td>
</tr>
<tr>
<td>Skin over/above the clitoris(^1)</td>
</tr>
<tr>
<td>Very tip (glans) of the clitoris(^1)</td>
</tr>
<tr>
<td>Labia</td>
</tr>
<tr>
<td>Side of the vagina towards my belly (may include g-spot)(^2)</td>
</tr>
<tr>
<td>Side of vagina towards my back(^2)</td>
</tr>
<tr>
<td>Opening of my vagina</td>
</tr>
<tr>
<td>Nipples</td>
</tr>
<tr>
<td>Somewhere else</td>
</tr>
<tr>
<td>I don’t know</td>
</tr>
<tr>
<td>Do not experience orgasms</td>
</tr>
<tr>
<td>Did not answer</td>
</tr>
</tbody>
</table>

Note: \(^1\) Collapsed as “clitorally generated”; \(^2\) Collapsed as “vaginally generated”.

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there are a significantly greater number of women who change stimulation sites from vaginal to clitoral, or clitoral to vaginal, when describing the first and second areas contributing to their orgasm than select the same site ($\chi^2 [1, n = 44] = 7.6, \Phi = .42, p = .006$). Specifically, 28 of 44 women who identified either clitoral or vaginal as primary stimulation site identified the other as their secondary stimulation site. Women did not have to select any second site, which was requested as “second area, if any”, so this difference is not likely due to women describing different orgasms at different times.

A third problem with this approach is that women’s general impressions of the area that usually contributes the most to their orgasms may not be reliable. Women also were asked which area contributed the most to their orgasms “usually” ($\chi^2 [1, n = 59] = 10.9, \Phi = .43, p = .002$). However, 3 of the 13 women who reported that their orgasms were “usually” generated from vaginal stimulation reported that their most recent orgasm was mainly attributable to clitoral stimulation. Given that these reports were not consistent, the site of “last” orgasm is used in later analyses. In summary, there is now reasonable evidence women’s orgasms are not only generated by only vaginal or clitoral stimulation, that women who report their vagina is their primary source of stimulation for orgasm usually experience their clitoris as the second most important area of stimulation and that recall of orgasm source is not stable as contrasted with the reported source of the most recent orgasm.

“Optimal” functioning in those reporting orgasms from clitoral stimulation

Although Brody and colleagues’ self-report measure is likely neither reliable nor valid, these self-report measures are now used to attempt replicate or falsify their vagina-is-always-optimal prediction. However, a different orgasm question was required. In addition to the problems above, the “percentage” and “consistency” approach often used by Brody and colleagues to classify women as vaginal or clitoral fails to correct consistently for base rates (in addition to the critiques above). Although the questions are not always clearly described in the studies (see Table 1), available information shows that a woman who did not engage in PVI with clitoral stimulation who did not reach orgasm at one episode would be classified with women who did not engage in PVI with clitoral stimulation and women who did not have orgasms over 30 episodes of PVI with clitoral stimulation. Given the evidence (above) of the potential advantages of using the most recent orgasm as a model, that question is used for analyses presented below. It is similar to a last-event analysis common in research on infections spread by sexual contact (Rosenberger et al., 2011). Brody et al. (2012) described Prause’s (2011) work as “presents as evidence of superiority of clitoral orgasms is higher self-reported arousal in response to erotica (rather than an interpersonal situation) in a small study (Prause, 2012) of doubtful ecological validity” (p. 6). Results show three measurements of health that suggest optimal health in women with clitorally-generated orgasms and two measurements of major psychopathology that do not differentiate women by orgasm type.

Women who attributed their most recent orgasm to stimulation of clitoral areas ($n = 42$) reported that the orgasm was more intense ($t [69] = 1.78, d = .81, p = .04$) than those who experienced their most recent orgasm from PVI alone ($n = 29$).
Second, women who reported orgasm from vaginal intercourse alone were more likely to report that they were sexually abused than women who reported orgasm from clitoral stimulation ($\chi^2 [1, n = 69] = 5.85, \Phi = .29, p = .03$). To be clear, having been a victim of childhood sexual abuse is not indicative of a problem in the women who were victims. Given the association of childhood sexual abuse and a variety of problems (Rind, Tromovitch, & Bauserman, 1998), it warranted inclusion as one health indicator. In addition to the greater sexual arousal to visual sexual stimuli reported in Prause (2011), women whose last orgasm was attributed to stimulation of clitoral areas also continued to report higher sexual arousal during subsequent neutral films ($t[56] = 2.87, d = .74, p = .01$) than women experiencing orgasm from PVI alone. Thus, sexual arousal in women who reported that their last orgasm was due more to clitoral stimulation appears more easily activated, and sustained, relative to women who reported that their last orgasm was due to penile vaginal stimulation. These groups did not differ on any questions about feeling sexual deprived (e.g., “How sexually deprived do you feel right now?”). Hence, this sustained sexual arousal after neutral films seems most likely to represent a benefit of high sexual arousability (lower stimulation threshold for experiencing sexual arousal). In summary, women who report that their last orgasm was due more to clitoral stimulation appears more easily activated, and sustained, relative to women who reported that their last orgasm was due to penile vaginal stimulation. The two groups also were compared on measures of depression (Radloff, 1977) and anxiety (Beck, Epstein, Brown, & Steer, 1988). No significant differences were identified. To ensure a very small effect was not missed given the current sample size, it would be useful to replicate this finding in a larger sample.

**Confounds**

Orgasms generated primarily by clitoral, vaginal or other stimulation do not differ solely in their stimulation site. For example, women who reported that their last orgasm was due primarily to clitoral stimulation reported significantly less frequent sexual intercourse ($t[66] = 2.8, p = .0007$), stronger orgasms ($t[65] = -2.1, p = .04$) and less sexual contact with a partner ($t[67] = 2.4, p = .02$). Partner context also is likely to differ between these groups. Women in the present study were asked to indicate on a continuous scale whether they experienced their orgasms from 1 = all/mostly during masturbation to 7 = all/mostly with a partner. Women who reported that the area of stimulation that contributed to their most recent orgasm as clitoral were significantly more likely to report that their orgasms tended to occur during masturbation ($t[58] = 2.84, d = .81, p = .01$). Since questions in the research by Brody and colleagues have unknown reliability or validity, many other factors might better explain the relationships being reported. For example, if the base rates of sexual activities differ, this should have been controlled. Having 100% orgasm consistency during PVI intercourse “only” that occurred once in the time period in question (see Table 1) differs from 100% orgasm consistency during PVI “only” occurring 16 times in the same time period. Similarly, women engaging in PVI only appear likely to be engaging in much less frequent clitoral-only stimulation, which represents another confound. Simply put, PVI and clitoral stimulation may be proxies for many different factors that have not been adequately assessed nor controlled in this series of problematic studies.
Recommendations for moving forward

Brody and colleagues have a history of previous publications committing similar errors. In this previous series of publications, they suggested that HIV was not transmitted by penile-vaginal intercourse. That series of publications provoked a response from other scientists that “Interpretations such as these have the potential for grave outcomes” (Carey & Kalichman, 1995). Carey and Kalichman also described Brody’s publications in that area as “his criticism does not reflect a sophisticated appreciation of the state of the science” (p. 656), he “omits relevant literature” (p. 657) and “misinterprets the findings” (p. 657). This provoked a response by Brody recommitting the same errors (Brody, 1996). Thus, one can reasonably anticipate that Brody and colleagues are most likely to continue exactly as before in response to the critiques in this issue. However, their vaginal-is-always-optimal prediction is easy to falsify (see above) and suggestions for scientists who may have an interest in providing rigorous tests to further falsify Brody’s declarations are now discussed.

The psychometric properties of orgasm self-report should be characterized. This could include, for example, assessing the presence of anal contractions in women who report that they experience orgasms to support construct validity. Unfortunately, physiologically differentiating “vaginal” from “clitoral” orgasms is so far impossible. Other investigators started mapping somatosensory brain areas for female genitalia (Michels, Mehnert, Boy, Schurch, & Kollias, 2010), but a strange step back was taken when the present investigator attempted to differentiate sites. Specifically, “self-stimulating, by hand or personal device, using ‘comfortable’ intensity, the clitoris, anterior wall of the vagina, the cervix, or the nipple, in separate, randomized-sequence trials” with their own devices ensured that stimulation site could not be attributed (Komisaruk et al., 2011, p. 2824). This means that brain activations reported in that study may represent shifts in stimulation intensity, a shift to perception of combined site stimulation, affective modulation, or something else due to a complete lack of control for these possibilities. The result has not been replicated, but is in contrast to the considerable physical evidence (Levin, 2011) that penile-vaginal intercourse necessarily stimulates both vaginal and clitoral structures. Finally, the horrific comment made by Brody (2012) about spinal cord-injured women: “The women with spinal cord injuries below T-10 do not present much movement artifact in fMRI measurements of orgasm (bypassing the problem that Prause claims)” (p. 14) is also spurious. These women likely retain full mobility of their upper body and are capable of much of the movement associated with high states of sexual arousal (e.g., of the face, as in Hughes and Nicholson [2008]) and these are the exact areas that would create artifacts in an MRI brain scan. Since the questions currently have not been examined psychometrically, much progress could be made in this area by examining convergent validity with other measures of orgasm (e.g., Cronbach & Meehl, 1955).

There are now a number of published, known problems with such questions. These include the common presence of a secondary stimulation site that directly contradicts the first reported stimulation site, a frequent lack of confidence in the presence of orgasm, instability in the site of primary orgasm genesis and the frequent contribution of areas that are not strictly vaginal or clitoral to orgasm. Clearly, a much more detailed, nuanced assessment of these areas is needed to rule out a number of likely alternative explanations for some results reported to date. In
addition, data suggest that adult women are often unaware of the existence of both internal and external reproduction-related structures (Blum, 1978). Lack of knowledge about anatomy would limit a woman’s ability to report what type of sexual stimulation they are receiving. Such information would be useful to include in future investigations.

Finally, many third variables remain that should be controlled in future investigations. Relationship variables are scarcely assessed in the existing publications, where the length of relationship with a partner, relationship status (e.g., married, dating, cohabitating) with the partner and their sexual repertoire/history (e.g., previous attempts at clitoral or vaginal orgasm) could be especially useful. The quality (e.g., intensity, depth), recency (e.g., last 24 hours) and character (e.g., multiple) of the orgasms could also be considered.

There remains no evidence that women’s sexual health is “optimized” by portraying women’s most sensitive and common area for stimulation, above and over the clitoris (Schober, Meyer-Bahlburg, & Ransley, 2004) as a pathological stimulation site. There are a number of serious methodological challenges to adequately test these predictions and the basic science is not in place to even determine whether these sites are truly separable. I hope that the science-minded reader might be motivated to correct the many errors in the “You gotta have a gimmick” (from Gypsy) approach to orgasm stimulation site in the series of studies by Brody and colleagues.

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Notes
1. Note that the \( n \) will vary depending on how many women had the experiences being analyzed.
2. “Have you had sex (oral, anal, genital) before age 16 when you did not want to because someone forced you in some way or threatened to harm you if you did not?” (from Rellini & Meston, 2007).
3. Statistic is corrected for unequal variance, uncorrected df shown.

Notes on contributor
Nicole Prause, PhD, is a member of the research faculty at the University of California, Los Angeles and Research Scientist at the Mind Research Network in Albuquerque, New Mexico. She was trained as a clinical scientist with concentrations in statistics and neuroscience from Indiana University, Bloomington and The Kinsey Institute for Research in Sex, Gender, and Reproduction. Her research focuses on HIV risk behaviors, substance use and sexual functioning, approach motivation, and reward processing in depression using a variety of psychophysiological methods.

References


