

Oral Contraceptives and Sexual Desire: Replies to Graham and Bancroft (2013) and Puts and Pope (2013)

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In our Letter (Roberts, Cobey, Klapilová, & Havlíček, 2013), we proposed that insights from studies on mate preferences may provide new avenues for understanding the relationship between use of oral contraceptives (OCs) and women's sexual desire. In the context of growing evidence from studies by evolutionary psychologists that indicate OCs alter women's partner preferences, we presented evidence to suggest that quantification of effects of OCs on sexual desire will be improved if (1) the target of women's desire is more clearly defined when it is being measured and (2) women's history of OC use, specifically at the time of initiating her current relationship, is also considered alongside current use. In commentaries on our Letter, Graham and Bancroft (2013) and Puts and Pope (2013) raised a number of points, which we seek to address here.

Response to Graham and Bancroft (2013)

Graham and Bancroft made three main points, at least two of which appear to be based on misconceptions of our argument,

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so here we aim to clarify what we suggested. Their first point is related to our introductory paragraphs in which we described the mixed evidence for an association between OCs and sexual desire, both across studies (different studies report different effects) and within studies (there is considerable variability in women's experience of OC use within samples). Graham and Bancroft argued that "variability in women's response *within* a sample probably underlies the mixed evidence *across* studies." They then stated that we failed to mention that much previous research used cross-sectional designs, presumably implying that this leads to some of the differences between studies.

Our point, however, was simply to highlight the indisputable fact that no clear picture of the effects of OC on sexual desire has yet emerged, either within or across studies (e.g., Pastor, Holla, & Chmel, 2013). We agree with them that more longitudinal studies are needed and indeed we often take this approach in our own work (e.g., Cobey et al., 2012; Little, Burriss, Petrie, Jones, & Roberts, 2013; Roberts, Gosling, Carter, & Petrie, 2008). We also agree that differences in sampling might be one cause of between-sample differences in findings although neither we nor they can be sure of this. Even if it is, however, this still leaves within-sample variability in OC effects on desire in prospective studies as an outstanding issue; to be understood, this likely requires more data and possibly new perspectives such as the one we introduced. In any case, and a point they appear to have missed altogether, our proposal provided a coherent and testable framework for understanding patterns of results in both cross-sectional and prospective studies. We explicitly showed, for each kind of study design, how the congruency hypothesis can account for both increased and decreased sexual desire (in different women) for a woman's primary sexual partner, depending on her OC use at the onset of the relationship (Roberts et al., 2013, Fig. 1).

Second, Graham and Bancroft argued that we overlooked previous studies and measures that distinguished between general desire and partner-focused desire. As evidence of this, they cited our interpretation of the Sexual Desire Inventory (SDI). Unfortunately, their comments betray a misconception about our argument, so we will attempt to clarify it here.

By partner-specific desire, we mean desire specifically for the individual with whom a woman is in a long-term relationship as opposed to any other sexual partner (real or imagined). To illustrate why this is important, imagine two women in long-term relationships who take part in a research study on sexual desire. As part of this study, each is asked to answer the following question: “Over the past 4 weeks, how satisfied have you been with your overall sexual life?” Imagine each indicates that they are very satisfied. They are then asked another question: “Over the past 4 weeks, how satisfied have you been with your sexual relationship with your partner?” Here, one indicates a similar response as to the first question, but the other woman indicates that she is not satisfied. The difference between the questions is the emphasis on “your partner.” This second woman may be satisfied in her overall sexual life, but not with her long-term primary partner, perhaps because she is fantasising about or even having sex with someone other than that partner. Now, finally, imagine that the second question was instead worded as follows: “Over the past 4 weeks, how satisfied have you been with your sexual relationship with a partner?” Here, both women would probably respond positively, as they did with the first question; in her mind’s eye, the second woman might now be imagining her extra-pair partner rather than her long-term partner.

The first two questions come from the Female Sexual Function Inventory (FSFI) and respectively typify what we would describe as items measuring general satisfaction (“overall” satisfaction in the FSFI) and partner-specific satisfaction. In our original Letter (Roberts et al., 2013, Table 1), we therefore summarized the FSFI as asking about both general and partner-specific satisfaction. The difference between the second and third questions is the extent to which the item’s phrasing distinguishes the target of the question and the ambiguity about the target that is contained in the phrase “a partner.” Had the FSFI contained the third question in place of the second (that is, asked about “a partner” rather than “your partner”), we would have indicated that the FSFI lacked a partner-specific item on sexual satisfaction.

In the same way, we classed the SDI as containing items about general desire but not partner-specific desire. For example, it contains the questions “During this last month, how often would you have liked to engage in sexual activity with a partner?” and “During this last month, how often have you had sexual thoughts involving a partner?” Graham and Bancroft suggest these items are partner-focused, but our point is that these items do not specify sufficiently clearly that the desire is targeted at her current

long-term partner rather than any sexual partner (real or imagined).

Although the difference between items asking about desire for “your partner” rather than “a partner” may seem trivial, our notional scenario of two participants shows how it might easily produce discrepancies in response. Furthermore, other items are even more ambiguous about the target of desire; for example, the SDI also includes items such as “When you spend time with an attractive person (for example, at work or school), how strong is your sexual desire?”, which is another measure of what we term general desire.

In addition to its utility in obtaining a more precise measure of desire which takes into account variation in women’s individual circumstances, appreciation of the distinction between general and partner-specific desire becomes particularly important when considering the congruency hypothesis (Roberts et al., 2013). Imagine, for example, a woman who meets her long-term partner while using OC. At some later time, she ceases use but remains in this relationship. The evidence on OC effects on partner preferences suggests that, at this point, she may be less attracted to her partner than she would be had she remained on OC, because her preference has changed and may have more fantasies (and even be more likely to have affairs with) other people. If this woman takes part in the aforementioned research study on sexual desire a few months after ceasing OC use, her answer to the question: “During this last month, how often have you had sexual thoughts involving your partner?” may be rather different to how she would have answered had she not ceased use. On the other hand, a question not adequately specifying the target of desire may not yield a difference, because she may instead be thinking of other sexual partners. Note that the congruency hypothesis allows for several other routes to congruency and non-congruency, as shown in Fig. 1 in our original Letter.

We have rather labored our response to this second point because it is critical to interpretation of our proposal and we thus wish it to be clear to researchers interested in the relationship between OC use and sexual desire and because Graham and Bancroft (as two such researchers) appear to have misunderstood it. Likewise, their third point also indicates misconception of our argument. According to them, we misled readers by arguing that previous researchers have not adequately studied autosexual behavior and we ignored instruments that take this aspect of sexuality into account. What we actually said was very different. We said that autosexual behavior is likely a different and motivationally distinct facet of sexuality from arousal, satisfaction, and so on, and that it may even be inversely related to partner attraction and hence also to partner-specific sexual satisfaction and desire. We were thus arguing not that autosexual behavior had been inadequately addressed, but rather that it should be treated differently than other items in the commonly used questionnaires. To be absolutely clear, our intended point is simply this: that its inclusion in calculation of women’s sexual

desire scores (from instruments that record autosexual behavior) might introduce noise into estimates of partner-specific desire, because frequency of masturbation may be more closely linked to unconsummated desire for and sexual fantasies about extra-pair partners than it is towards actual long-term partners (Hicks & Leitenberg, 2001).

Finally, as well as suggesting that we misrepresented the SDI, Graham and Bancroft advocated use of the Interviewer Ratings of Sexual Function (IRSF), used by four of the 36 studies reviewed by Pastor et al. (2013). It was not our intention to systematically review every possible instrument, but we will briefly describe and then comment on the IRSF. In the introductory information, interviewers are asked to inform participants that the interview will focus on the sexual relationship with the partner and, indeed, many of the questions do specify that women should respond in relation to “your partner.” In total, 16 questions covered in four sections (sexual activity, enjoyment/arousal, vaginal response, partner variables) are partner-focused and we agree that this constitutes an excellent measure of sexual activity and satisfaction with, but not desire for (see below) the woman’s primary partner. However, the interview then moves to a new section (sexual interest/masturbation) containing three questions which are not (or at least much less) partner-focused. These are: “Apart from the times that your partner approached you wanting to make love, how often have you found yourself thinking about sex with interest or desire?”, “Do you find your interest or enjoyment of sex varies with your menstrual cycle?”, and “Have you masturbated on your own over the past ___ months?”

Based on the arguments presented in our original Letter, we would make several points about use of the IRSF. First, notwithstanding the information given to participants prior to the interview, the wording of the first of these three items almost precludes partner-specific desire, arguably steering women towards extra-pair targets of desire and fantasy. Indeed, additional rubric for the item clarifies what is meant by interest/desire by adding that “This includes times of just interest, daydreaming, and fantasizing, as well as times of lovemaking.” We would thus argue that both the position of the item in the questionnaire and its wording implies that this particular item does not (either by intention or accident) specifically measure desire for the individual with whom the woman is in a long-term relationship, but could instead (or also) apply to any sexual partner; in other words, it is more a measure of general desire than partner-specific desire. Second, as these three questions in the sexual interest section are not partner-focused, they should be treated separately from the preceding 16 items. Third, nowhere in the preceding sections that do deal with partner-specific sexual activity and satisfaction is there an analogous item asking about frequency of sexual thoughts specifically about the woman’s partner. Thus, the IRSF is not ideal in distinguishing general and partner-focused desire. Perhaps because of this problem, it is the first of these three items that is used as the principal measure of

sexual interest in at least one previous study using the IRSF (Graham, Bancroft, Doll, Greco, & Tanner, 2007) rather than one that is explicitly partner-focused. Finally, Bancroft and Graham (2011) noted that, in their experience of using the IRSF, women have difficulty in answering this question. This may, as they suggest, be because expressing desire is inherently difficult, but perhaps it could also be because participants are confused as to whether the question is asking about their primary partner or not.

Response to Puts and Pope (2013)

In contrast to Graham and Bancroft, Puts and Pope understand the distinction that we made between general and partner-specific desire, commented on the merit of our proposal, and appreciated its potential to contribute to our understanding of the relationship between OC use and women’s sexuality. Indeed, they suggested extending the ideas we proposed to include other contextual influences such as the partner’s perspective on aspects of relationship functioning and the extent to which women are able to express their mate preference during mate choice, and presented one caveat, which was that our proposal was less relevant to situations in which women are unable to exercise completely free choice of long-term partners. We agree with the spirit and the gist of much of what they say.

In particular, we agree with their point that partners may also be influenced by a woman’s OC use. We deliberately constrained our argument to women’s preferences and desire in order to make our ideas as clear as possible to researchers unaware of the evolutionary literature, but the point is well-taken. Indeed, in addition to the literature they cite, it is known that perception of attractiveness of women’s faces (Roberts et al., 2004) and odor (Havlíček, Dvořáková, Bartoš, & Flegr, 2006) varies according to menstrual cycle phase (for a review of such evidence, see Haselton & Gildersleeve, 2011) and that odor perception, at least, is influenced by OC use (Kuukasjarvi et al., 2004). Furthermore, variation in female perceptions of in-pair sexual attractiveness across the menstrual cycle were moderated by the woman’s rating of their male partners sexual attractiveness (Larson, Haselton, Pillsworth, & Gildersleeve, 2013), meaning that women rating their male partner to be low in attractiveness reported even less in-pair sexual attraction near to ovulation. If men detect such changes in their partner’s feelings and, if their own assessment of their female partner is altered by OC use (Cobey et al., 2013), then this could have reciprocal effects on men and women, potentially influencing various aspects of relationship functioning, including women’s own sexual satisfaction (Roberts et al., 2012), levels of relationship jealousy (Cobey et al., 2012; Cobey, Roberts, & Buunk, 2013), and ultimately also in women’s reports of sexual desire for their partner.

Furthermore, Puts and Pope suggested that factors such as a partner's masculinity may also influence women's desire. Again, we agree and indeed we outlined ways in our original Letter that OC use might affect partner choice for a number of such male characteristics, including altering affinity for male facial masculinity (Little et al., 2013). However, although both we and they find this interesting, a potential concern is that such information might not hold significant practical utility for sexual desire researchers (our target audience) because the characteristics affected may be numerous and the ways in which they alter with OC use are likely to be complex and variable. Incorporating the detail of such factors in research on sexual desire is therefore likely to be arduous, at least currently, for clinical researchers and practitioners. In contrast, the congruency hypothesis—the idea that women's desire and relationship functioning is higher when a woman's current OC use matches her OC use at the time of meeting her partner—distills this complexity of women's preference and choice into a simple dichotomous variable (women's OC use is either congruent or not). The practical benefit of this idea is that congruency is easily determined, its effects are easily tested, and we believe it might be intuitive to both researchers and OC users, while at the same time accounting for the complex and evolutionarily adaptive aspects of partner choice.

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