Using 26 thousand diary entries to show ovulatory changes in sexual desire and behaviour

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Abstract

Previous research reported ovulatory changes in women's appearance, mate preferences, extra- and in-pair sexual desire and behaviour, but has been criticised for small sample sizes, inappropriate designs, and undisclosed flexibility in analyses. In the present study, we sought to address these criticisms by preregistering our hypotheses and analysis plan and by collecting a large diary sample. We gathered over 26 thousand usable online self-reports in a diary format from 1043 women, of which 421 were naturally cycling. We inferred the fertile period from menstrual onset reports. We used hormonal contraceptive users as a quasi-control group, as they experience menstruation, but not ovulation. We probed our results for robustness to different approaches (including different fertility estimates, different exclusion criteria, adjusting for potential confounds, moderation by methodological factors). We found robust evidence supporting previously reported ovulatory increases in extra-pair desire and behaviour, in-pair desire, and self-perceived desirability, as well as no unexpected associations. Yet, we did not find predicted effects on partner mate retention behaviour, clothing choices, or narcissism. Contrary to some of the earlier literature, partners' sexual attractiveness did not moderate the cycle shifts. Taken together, the replicability of the existing literature on ovulatory changes was mixed. We conclude with simulation-based recommendations for reading the past literature and for designing future large-scale preregistered within-subject studies to understand ovulatory cycle changes and the effects of hormonal contraception. Interindividual differences in the size of ovulatory changes emerge as an important area for further study.

Keywords: ovulatory cycle shifts, sexual desire, diary study, hormonal contraception, evolutionary psychology

1 Introduction

1.1 Theoretical Background

Personality, behaviour, sexual desire, attractiveness, mate preferences and mate choices vary between and within persons (Fleeson, 2001, 2004; Gerlach, Arslan, Schultze, Reinhard, & Penke, in press). While copious research has identified antecedents of interindividual variation (Zietsch, Lee, Sherlock, & Jern, 2015), it is still often viewed as mere chance fluctuation or response to situational demands. Systematic endogenous causes of intraindividual variation are worthy of further study.

In the evolutionary psychology literature, the menstrual cycle has been suggested as one such influence on psychological state fluctuations in women (Gangestad & Thornhill, 2008). Menstrual cycle changes in attractiveness, mate preferences, and sexual desire, as well as men's reactions to those changes have been interpreted as evidence for adaptations formed by sexual selection and sexually antagonistic coevolution, i.e. arms races between the sexes. However, to this day debate continues over the existence and extent of such changes (W. Wood, Kressel, Joshi, & Louie, 2014).

In this paper, we have the twin goals of reviewing methodological problems with commonly used approaches and addressing them in a high-powered, preregistered replication study. Because our study was preregistered in March 2014, the introduction of this manuscript reflects our reading of the literature at that point in time. We review recent theoretical and empirical developments in the discussion.

1.1.1 Do human females show oestrus?

Human women do not develop garish sexual swellings or other prominent changes around ovulation, unlike their closest cousins, the chimpanzees (Deschner, Heistermann, Hodges, & Boesch, 2003). Moreover, human women and several other primates exhibit *extended sexuality*, that is they have sex outside the fertile window, not just during a period of oestrus or *heat* (Dixon, 2012).

However, other, less conspicuous endocrine, behavioural, physiological and psychological changes happen over the course of the menstrual cycle and some peak when women are fertile (Gangestad & Simpson, 2000; Haselton & Gildersleeve, 2016). This led (Gangestad & Thornhill, 2008) to argue that the differentiation of functional and physiological aspects of fertile phase sexuality merits being called oestrus.

1.1.2 The good genes ovulatory shift hypothesis

The ovulatory shift hypothesis posits that women's mate preferences and choices vary with their fertility status. It is a central functional differentiation predicted under the human oestrus perspective (Gangestad & Thornhill, 2008). According to this theory, women would optimise their reproductive potential by choosing to be with partners who will invest in offspring during non-fertile times and choosing, if necessary, other, extra-pair, males with *good genes* to provide their offspring's genes, i.e. to have sex with during the fertile phase. To differentiate this theoretically predicted *ovulatory shift in mate preferences to obtain good genes, potentially from extra-pair copulations* (Pillsworth & Haselton, 2006a) from simpler, generalized increases in sexual drive or

libido in the fertile phase, we will call this theory *good genes ovulatory shift hypothesis* (GGOSH).

The concept of *good genes* is meant to index genetic qualities that women should want their offspring to inherit. The concept includes dyadic genetic fit (e.g. good immunocompetence genes), genetic fit to the current environment, and few harmful mutations. It has no direct correspondence in the evolutionary genetic literature and some purported indicators of *good genes* are controversial (Arslan & Penke, 2015).

Several male characteristics have been argued to indicate *good genes*. Cycle studies have then reported fertile phase increases in preferences for these traits, which include masculinity, low fluctuating asymmetry (Scheib, Gangestad, & Thornhill, 1999), and various measures of attractiveness (Gildersleeve, Haselton, & Fales, 2014a; Haselton & Gangestad, 2006; Larson, Haselton, Gildersleeve, & Pillsworth, 2013; Pillsworth & Haselton, 2006b). In laboratory studies, fertile phase shifts towards preferences for male stimuli with such characteristics (photos, videos, voice samples), have been cited as support for GGOSH (Gildersleeve et al., 2014a).

1.1.3 Rationale for the present study

In our study, we sought to replicate and extend previous results from field studies of naturally cycling women commonly cited as evidence of a differentiation of fertile phase sexuality. These field studies reported evidence for changes in female sexual interests and appearance across the cycle. Central results in these studies served as the rationale for the preregistration of our study.

1.1.3.1 *Extra-pair desire and behaviour*

(Gangestad, Thornhill, & Garver, 2002) asked 51 naturally cycling women (i.e. not using hormonal contraceptives) to report their sexual interests and fantasies once in the fertile and once in the non-fertile phase. Women reported substantially greater attraction to and fantasies about men other than primary partners when fertile.

In a sample of 54 couples and using the same study design, (Gangestad, Thornhill, & Garver-Apgar, 2005) additionally reported support for a predicted moderator effect. Women showed stronger fertile phase increases in attraction to other men if paired with relatively asymmetrical primary partners. In a diary study, (Haselton & Gangestad, 2006) asked 38 naturally cycling women to provide daily reports of sexual interest and feelings for 35 days. Women reported that they were more attracted to and flirted more often with men other than primary partners on higher fertility days, if their partner's sexual attractiveness was low.

1.1.3.2 *In-pair desire and behaviour*

According to the ovulatory shift hypothesis, women whose long-term partners display indicators of “good genes” do not benefit from engaging in what (Pillsworth & Haselton, 2006a) call a dual-mating strategy. The authors predicted such women should instead experience ovulatory increases in in-pair desire. Findings were mixed, with some showed the predicted moderated shifts (Gangestad et al., 2005; Pillsworth, Haselton, & Buss, 2004) others did not (Gangestad et al., 2002; Pillsworth & Haselton, 2006b). (Gangestad et al., 2002) found that women did not experience significantly

higher levels of overall sexual desire when fertile, but tended to initiate and have more sex with their partners as ovulation neared.

1.1.3.3 Male mate retention

Because female extra-pair sex might lead her primary partner to involuntarily invest parental care and resources into offspring sired by an extra-pair mate, counter-adaptations to the aforementioned shifts were predicted (Pillsworth & Haselton, 2006a). (Gangestad et al., 2002) correspondingly found that *prohibitive* (i.e. jealousy) and *persuasive* (i.e. affection) male partners' mate retention tactics increased during the fertile phase. (Haselton & Gangestad, 2006) replicated these results. These tactics were exhibited primarily by partners of women who perceived their partners to be low in sexual attractiveness relative to investment attractiveness.

1.1.3.4 Self-perceived desirability and clothing choices

Although obvious outward signals of fertility are absent in humans, some studies report evidence of subtle ovulatory cues in human females and conclude that ovulation may not be perfectly concealed. (Haselton & Gangestad, 2006) reported that women perceived themselves to be more attractive when fertile. Haselton et al. (2007) further predicted and found fertile phase increases in grooming and attractive clothing choices in a sample of 30 partnered women who were photographed at high and low fertility. (Schwarz & Hassebrauck, 2008) replicated and extended this study. In a sample of 40 women who completed a daily questionnaire over 31 days, participants rated their perceived attractiveness, and their clothing style on the dimensions "figure-hugging", "sexy", and "permissive". They were also instructed to take one photo of themselves

each day. Men then rated these photos for clothing style and physical attractiveness. Women perceived themselves and were perceived by men to be dressed more provocatively on their fertile days. In another replication, using 88 women tested twice, (Durante, Li, & Haselton, 2008) reported evidence that women prefer clothing that is more revealing and sexy during the fertile phase, as shown in full-body photographs and drawn illustrations of what they would wear to a hypothetical social event that evening.

1.1.3.5 *Intrasexual competitiveness*

Durante et al. (2008) interpreted their results discussed above as evidence of increased intrasexual competitiveness, i.e. women altering their physical appearance to enhance their ability to compete with other women. We speculated that, if intrasexual competitiveness during the fertile phase were increased, we might detect this in narcissistic personality states, as conceptualized in the two-dimensional narcissistic admiration and rivalry concept (NARC(Back et al., 2013). Narcissistic admiration is thought to be linked to the desire to attain social status, and evoke social interest. Narcissistic rivalry is thought to be linked to motivations to defend one's social status against others. In the context of our study, to test the prediction of increased intrasexual competitiveness in the fertile phase (Durante et al., 2008) in a novel way, we reformulated narcissistic state items for both NARC dimensions to refer to other women only.

1.2 Methodological issues

The psychological literature on ovulatory changes has been criticised and hotly debated. Two meta-analyses based on overlapping data both concluded that publication bias afflicts research on ovulatory shifts in mate preferences, as may be the case for most of the scientific literature (Fanelli, 2011; Ferguson & Brannick, 2012). However, one team of investigators (Gildersleeve et al., 2014a) concluded that all evidence taken together suggested replicable shifts in mate preferences, even after including studies freed from the file drawer and adjusting for bias. Another team (W. Wood et al., 2014) concluded further bias and methodological artefacts implied that any non-negligible effects were, in fact, overestimated. Our study focuses on different outcomes than these meta-analyses, but many of the criticisms and problems pertain to the designs commonly used to study ovulatory change, irrespective of outcomes and research questions. Thus they also influenced our approach. In the following, we summarise several methodological issues brought to the fore by this debate.

1.2.1 Researcher degrees of freedom can lead to false positives

Many psychological studies do not replicate in exact replications (Open Science Collaboration, 2015). Potential sources of bias are *researcher degrees of freedom* in specifying hypothesis, methodology, and statistical approach after seeing the data. Journals and researchers tend to preferentially publish and cite significant counter-intuitive results, leading to warped incentives (Simmons, Nelson, & Simonsohn, 2011).

Recent debate in the menstrual cycle literature has specifically highlighted flexibility in the definition of the fertile window, but more general problems such as reporting only

significantly associated measures and stopping data collection conditional on significance could also affect the literature. As surveys of psychological researchers show that some research practices now deemed questionable were widespread (John, Loewenstein, & Prelec, 2012) and meta-analyses show publication bias. Both sides in the ovulatory cycle debate acknowledge bias (Gangestad, 2016; Harris, Pashler, & Mickes, 2014; W. Wood et al., 2014) but do not agree on whether and how it can be adjusted for (Gildersleeve, Haselton, & Fales, 2014b; Harris et al., 2014) in order to obtain trustworthy bias-corrected estimates (Inzlicht, Gervais, & Berkman, 2015; van Elk et al., 2015). The debate surrounding this has at times turned vitriolic, because the often used term *p-hacking* has connotations of intentional mischief, but it is clear from simulations (Smaldino & McElreath, 2016) and intuition (Gelman & Loken, 2014) that flexibility will lead to bias even without ill intentions, as long as odds of publication and tenure can hinge on whether results turn statistically significant. Ultimately, although methods such as the *p-curve* (Gildersleeve et al., 2014b) can offer suggestive evidence of replicability, the true tests of replicability are *preregistered* replication studies in which hypotheses, methods and statistical approach are fixed before the data are collected, preventing *researcher degrees of freedom* from skewing results.

1.2.2 Estimating the fertile window

There is wide variability in the approaches used to estimate women's fertile windows. (Gildersleeve et al., 2014a) reviewed these approaches and problems associated with them. (Gangestad et al., 2016) recommend that researchers abandon windows altogether and instead estimate continuous probabilities of being fertile.

Flawed recall of the last menstrual onset, accuracy being as low as 57% (Wegienka & Baird, 2005), remains a problem. Moreover, menstrual cycle lengths vary within person, so that recalled average cycle length correlates only $\sim .5$ with the length of individual cycles (Blake, Dixson, O'Dean, & Denson, 2016; Gangestad et al., 2016). Because the follicular phase leading up to ovulation is more variable than the luteal phase (Fehring, Schneider, & Raviele, 2006), the more convenient method (forward counting from the last menstrual onset) is also more imprecise (Gangestad et al., 2016). Backward counting to ovulation from the next menstrual onset should hence be more accurate, with a validity for estimated fertility as high as $\sim .7$ (Gangestad et al., 2016). (Blake et al., 2016) report much lower validities, using luteinising hormone (LH) surges as the criterion in a small sample of 140 women, but re-analyses of their data using a hedged fertile window estimate, as in Gangestad et al. (2016), show comparable validities.

For researchers, backward counting has the added benefit that women who count days as part of their contraception regiment cannot do it prospectively, perhaps reducing awareness and thus demand characteristics. Still, counting-based estimates of conception probability derive from forward-counted actuarial values which are then reversed (Gangestad et al., 2016), ideally actuarial estimates would be backward-counted too.

1.2.3 Between-subject designs to study a within-subject process

Many past studies have used between-subject designs to study a within-subject process, ovulation (Gangestad et al., 2016). Even when sample sizes are large, selection bias could confound any identified effects. One possible scenario could be that a common cause, for instance genetic makeup or a disease, makes women anovulatory and lowers their sexual desire. This could lead researchers to mistake a between-subject difference for an ovulatory change. Another potential problem might be that increased social activity during the fertile phase (Haselton & Gangestad, 2006) could make fertile women less likely to participate in a survey study, biasing the sample towards women who experience smaller changes. Further, cross-sectional designs can never reliably measure individual differences in the size of ovulatory changes. They may also lead to the use of outcome measures that measure a trait component, but not a state component, reliably. This can be avoided by using established measures tested on within-subject data. Indeed, many of the above problems are minor and could potentially be avoided or adjusted for, but given that within-subject studies do not have these problems and are no longer hard to implement, they seem the superior option. Most crucially however, typical between-subject studies have far too low statistical power at typical samples sizes, as shown by (Gangestad et al., 2016).

1.2.4 Lack of power or implausible effect size expectations

The average menstrual cycle study is underpowered to detect anything but very large changes (Gangestad et al., 2016). At the same time, most researchers seem to agree that ovulatory changes are, if anything, subtle. In this situation, many plausible and interesting effect sizes will be missed, and reported effects will tend to be

overestimates. If we desire theoretical progress, we need to narrow down effect sizes to disambiguate between theories that predict no, minimal, small, medium, or large ovulatory changes in certain outcomes. Thus, the literature would benefit from narrower confidence intervals to resolve theoretical debates over evolutionary function. Even for larger effects, typical cycle studies are underpowered, because of the combination of suboptimal design aspects and small sample size (median $N = 48$ in Gildersleeve, Haselton, and Fales, 2014, mean $N = 49$ in the studies we sought to replicate). For between-subject studies planning to achieve 80% power to detect a Cohen's d of 0.4 with a backward-counted conception probability estimate, Gangestad et al. (2016) recommend a sample size of 1,143.

1.2.5 No differentiation of women by reproductive intentions and contraception method

(W. Wood et al., 2014) pointed out that the most uniquely human aspect of menstrual cycles may be women's exertion of control over their cycle and fertility to adapt to cultural, societal and their own needs. Although they provide no specific recommendations how this should change research practices, we note that most studies do not report differentiating between naturally cycling women who use barrier methods, awareness-based methods, or simply no contraception. Among women who do not use contraception, there may be women who are actively trying to conceive and would usually be excluded, but also those who do not mind risking a conception. Most studies also do not report asking women whether they track their fertility or menstrual cycle by counting with an app or calendar in addition to a primary contraceptive. If

women are aware of their fertility status, their answers in the fertile phase might differ spuriously due to changed behaviour (e.g. avoiding sex or using condoms, or seeking sex to conceive), heightened self-awareness for sexual thoughts and fantasies, demand characteristics, or personal theories on how their menstrual cycle affects them.

1.2.6 Directly assessing hormones may create demand characteristics

Test strips to assess ovulation via luteinising hormone surges in urine are more precise than counting methods. However, these strips are familiar to many adult women, making it easy for them to infer that a study employing these strips aims to assess effects related to ovulation and conception risk. If the participants are undergraduates at the same institution as the research team, they may accurately guess the researchers' hypotheses and consciously or unconsciously change their responses (Harris, Chabot, & Mickes, 2013). Similar worries are justified when oestrogen and progesterone are measured in saliva, blood, or urine and if women are invited back to the lab based on their menstrual cycle. In an online diary study, the study intention can be kept opaque to participants, or at least less dominant in participants' minds, especially when many other items are included. In our study, one benefit presumably was that our laboratory had not yet published research on ovulatory changes.

1.2.7 Lack of control group

Changes in oestrogen and progesterone levels around ovulation are usually hypothesised and sometimes tested as the mediating mechanism for observed

changes mid-cycle (Roney & Simmons, 2013, 2017). Unfortunately, many studies exclude women using hormonal contraceptives (HC) from taking part or from analysis, even though they can serve as a quasi-control group that experiences menstruation but not ovulation and the concurrent hormonal changes. A quasi-control group is also useful as an empirical baseline for the false discovery rate: if researchers found as many 'ovulatory' changes among HC users as among naturally cycling women, this would serve as feedback that the analysis procedure might entail false positives or invalid conclusions about the hormonal processes driving the changes. Apart from being a helpful methodological feature, including HC users allows researchers to more directly test whether, say, shifts in mate preferences or extra-pair desire do not happen among HC users. This may, simply put, be highly relevant for the many women who use HC and who might consider the absence of ovulatory cycle shifts desirable or undesirable side effects (Alvergne & Lummaa, 2010).

1.2.8 Ecological validity may be lacking

In Western societies, although female infidelity is not uncommon, with a 12-month prevalence of 2-4% and an occurrence of 20-25% per marriage (Fincham & May, 2017), few women have children with an extra-pair mate (1-2%, (Larmuseau, Matthijs, & Wenseleers, 2016)). This makes it difficult to collect the data necessary to ascertain that ovulatory shifts in extra-pair sex lead to offspring with increased fitness. Still, few instances may suffice to exert the necessary selective pressure, the low rate may be a evolutionarily recent cultural innovation (Larmuseau et al., 2016), and there has been some evidence against nonadaptive explanations of extra-pair mating in women

(Zietsch, Westberg, Santtila, & Jern, 2015). Still, most studies, lab and field, were conducted chiefly in western, educated, industrialised, rich, democratic populations (Henrich, Heine, & Norenzayan, 2010) and ours is no exception. Many studies on GGOSH have further issues with ecological validity, because women rate artificial stimuli, like morphed pictures of men, in the laboratory without consequences to their love lives. These male stimuli may highlight certain characteristics and display them in a way that exaggerates the variation from which the sampled women usually choose. Thus, effects may be overestimated and responses may not map to mate choice in the real world.

2 The present study

In the present study, we sought to replicate central findings on cycle shifts in extra- and in-pair desire, attractiveness, clothing choices and competitiveness while also improving on methodological shortcomings in the cycle research literature. By preregistering our study and main analysis plan before data collection, we reduced our own researcher degrees of freedom and thereby the risk of false positives. By using an online diary with up to 35 days reported per woman, we increased our power to detect any effects and our ability to isolate them from confounders. This design also allowed us to obtain daily reports of menstrual onset, avoiding recall error, and to do backward-counting from actual next onsets, decreasing error in the estimation of conception probability. Because diaries were filled out on participants' personal electronic devices we could assess women's reported behaviour and experiences close in both place and

time to actual behaviour. We automated the study process, decreasing our own ability to influence women's participation and responses. Because there was no cost per participant we recruited a large sample and included women regardless of contraception status, providing both a quasi-control group and making it less clear to participants what we were studying. We also assume that the automated, encrypted, minimal-contact online study made women feel more anonymous and hence comfortable to report, for instance, extra-pair desire and sex. However, using this approach implied we could not directly measure hormones, obtain photos of women, or collect ratings by their partners.

Because there is little agreement on best practices and standard operating procedures for doing this research (Blake et al., 2016; Gangestad et al., 2016; Gildersleeve et al., 2014b), we also used a variety of robustness checks to test the consequences of different decisions during data processing and statistical modelling, especially conception probability estimation, exclusion criteria and control variables.

2.1 Preregistered hypotheses

We registered the following hypotheses on the Open Science Framework on the day that data collection began. We reworded and reorganised them slightly here for space and clarity.

1. Ovulatory changes (increases during fertile window among naturally cycling women in a heterosexual relationship, but not for hormonal contraceptive users) occur in
 1. female extra-pair desire and behaviour

2. female in-pair sexual desire
 3. having and initiating in-pair sexual intercourse (if circumstances allowed, e.g. partner was close by)
 4. subjective feelings of attractiveness
 5. choice of clothing (self-rated on the dimensions “sexy”, “figure-hugging”, “seductive”)
 6. reported male partner mate retention tactics
 7. narcissism on both dimensions of the NARC (admiration and rivalry)
2. Moderation or *shift* hypotheses: The ovulatory increase in women’s extra-pair desires and reported male mate retention behaviour is strongest (and the in-pair desire increase is weakest) for women who perceive their partners
1. as low in sexual and physical attractiveness
 2. as low in sexual attractiveness relative to long-term partner attractiveness
 3. as less attractive compared to themselves
3. Predicted ovulatory changes are larger than, and independent of, potential ovulatory shifts in self-esteem.

In addition, we preregistered to test extraversion (4.1.), shyness (4.2.) and neuroticism (4.3.) as potential ovulatory change moderators. We called these

moderators exploratory in the preregistration to differentiate them from those already tested in the existing literature. We expected that the ovulatory increase in extra-pair desire (e.g. desire to attend social gatherings where they might meet men) may possibly be stronger for extraverted/outgoing than for introverted/shy women. Further, we expected that neuroticism may influence strength of the ovulatory increase in extra-pair desires and subjective feeling of attractiveness, though we did not specify a direction (4.4.).

3 Methods

3.1 Study description

3.1.1 Power analysis

Because we used multilevel analyses for our within-subject data, we conducted simulations to assess our study's statistical power. We simulated data under a number of different scenarios, varying among others the effect size associated with conception probability, the sample size, the number of days sampled per participant, the standard deviation of the day of the ovulation (i.e. by how much our estimated conception probability missed the correct day on average), the trait component of the outcome, and whether participants were scheduled for sampling on predicted fertile vs. non-fertile days or on random days. We did not simulate between-subjects analyses, because these should be avoided not only because of their low power (Gangestad et al., 2016) but also for reasons of validity.

3.1.2 Researcher degrees of freedom simulation

Because researcher degrees of freedom have been discussed as a source of problems in the literature, we repeated our power analysis with an effect size of zero and the following procedure simulating a hypothetical researcher engaging in the following *questionable research practices*: a) optional stopping (stop 20 or 10 participants earlier if $p < .05$), b) control for an irrelevant covariate if $p > .05$, c) try up to five correlated items as outcomes, d) start with a continuous predictor, then try broad and narrow window if $p > .05$ and combinations of these practices and determined the number of false positives.

3.1.3 Preregistration

We preregistered our study's hypotheses and methods on March 19, 2014 and added a planned amendment to our exclusion criteria and fertility estimation method to the preregistration on May 10, 2014, when data collection was already underway (Schilling, Straus, Arslan, Gerlach, & Penke, 2014). Participants enrolled from March 19, 2014 to July 2, 2015. The last diary entry was made on December 3, 2015. The preregistration had a second part, which pertained to hypotheses related to oestrogen dosage effects in hormonal contraceptives and which we plan to discuss in a separate manuscript.

In our initial preregistration, we specified that we would use backward counting from the observed next menstrual onset to estimate a narrow fertile window (reverse cycle days 15-19 vs. 2-11). After the publication of Wood et al. (2014), we decided to also test

a broad window (reverse cycle days 14-22) in order to compare results using the two approaches. Moreover, we preregistered that we would descriptively show results based on continuous curves centred on the estimated day of ovulation. We preregistered the personality and daily diary items we would use. For sample size calculations, we did not preregister a fixed sample size, as this is hard to control in online studies and power analyses based on a biased literature are of limited use. Instead, we preregistered a complex procedure under which we tried to ensure that we would obtain an adequate sample size even if recruiting proceeded slowly and that students could finish projects based on this study in a reasonable timeframe. We stopped recruiting when we were unable to find further participants and we honoured our promise not to stop data collection depending on results.

3.1.4 Participants

We recruited women via university mailing lists in Germany, newspaper articles about our group's work (without references to ovulation-related work), our online study site psytests.de, word-of-mouth, and among local students in exchange for course credit at our university. Only participants who self-reported their sex as female and reported currently being in a heterosexual relationship were allowed to participate. Out of the 1,720 participants who signed up for the study, 259 were ineligible to participate according to these criteria, 253 did not complete the demographics and personality survey preceding the diary, 54 completed no diary entries, 41 were sterilised, infertile or pregnant, and fertility was never estimable for 70 due to few or patchy diary entries. Out of the remaining participants, 60% ($n = 631$) were using some form of hormonal

contraceptive and 40% ($n = 428$) were naturally cycling. Specifically, 5% ($n = 53$) used a fertility-awareness-based-method, 28% ($n = 291$) used only barrier methods, mostly condoms, and 6% ($n = 67$) reported no contraception. We preregistered several exclusion criteria that we deemed useful to exclude women with potentially anovulatory cycles. Applying the strictest criteria proved to be over-exclusive, as only 13% of the naturally cycling sample would have been retained. Hence, we differentiated our exclusion criteria into four strictness levels and examined the effect of applying these levels in robustness checks. The participant flow and exclusion criteria are shown in Figure 1.

Figure 1. Participant flow. The figure depicts the various exclusion criteria and the number of participants affected by each (if not already excluded for a preceding reason).

The 1,043 eligible participants were on average 25.5 years old ($SD = 6.3$, 18-53) and had been in a relationship for 3.8 years ($SD = 4.3$). Most (71%) were students, 24%

were working, 3% were not working or described themselves as homemakers, and 3% were in secondary or vocational school. A majority reported their religious denomination as Christian (56%) and 42% described themselves as nonreligious. Twelve percent were married and a further 4% engaged to be married. Four percent of the sample reported not yet having had sex with their partner. Most (88%) had no children. The largest group co-habited with their partner (41%), but a sizeable fraction had a long-distance relationship (31%), with the remainder living in the same city. Of those who did not live with their partner, 34% lived in a flatshare and 25% lived alone. We present more detailed data on the distance between partners, how often they saw each other and spent the night in the online supplement. Geographically, only our university town seemed visibly overrepresented. Hormonal contraceptive users differed from naturally cycling women in a number of ways (see Table 1 for continuous variables and online supplement for all others). Most importantly, they were almost 5 years younger on average, and consequently tended to be unmarried and not to co-habit, to be in relationships for a shorter time (approximately 2 years), to have had 3.5 fewer lifetime sexual partners, to be students and have lower income. However, when simultaneously predicting hormonal contraception status from 28 demographic and personality predictors in a probit regression, only lower age, low openness, high conscientiousness, and being unmarried were significantly predictive at $p < .05/28$. For the sample used in our preregistered analyses, the only differences remaining significant in the regression were that women on the pill were approximately 3 years younger and lower in

openness. Hormonal contraceptive users also had shorter and more regular cycles, which might be consequences rather than causes.

Table 1. Descriptive statistics by hormonal contraceptive use.

Variable	Mean (Standard deviation)		Hedges' g	p
	HC user	Cycling		
Age	23.6 (4.4)	28.4 (7.6)	1.10	< .001
Religiosity	2.0 (1.1)	2.0 (1.2)	0.01	.891
Age at first time (years)	16.9 (2.3)	16.9 (2.4)	-0.01	.886
Age at menarche (years)	13.0 (1.3)	13.0 (1.5)	-0.06	.557
Relationship duration (years)	2.9 (3.0)	5.0 (5.5)	0.70	< .001
Cycle length (days)	27.9 (2.9)	29.1 (3.6)	0.41	< .001
Life no. sexual partners	5.7 (7.2)	9.3 (14.9)	0.50	< .001
BFI Extraversion	3.5 (0.8)	3.5 (0.8)	0.03	.638
BFI Agreeableness	3.6 (0.6)	3.6 (0.6)	0.00	.964
BFI Neuroticism	3.1 (0.7)	3.0 (0.8)	-0.14	.037
BFI Conscientiousness	3.6 (0.7)	3.5 (0.7)	-0.15	.024
BFI Openness	3.6 (0.6)	3.8 (0.6)	0.31	< .001
Relationship satisfaction	4.2 (0.7)	4.0 (0.8)	-0.20	.003

Notes. Constructs in bold remained significant after multivariate adjustment in a

probit regression. BFI = Big Five Inventory. HC = hormonal contraceptive.

3.1.5 Procedure and implementation

3.1.5.1 Procedure

3.1.5.1.1 Intake form and consent

Participants filled out web-based questionnaires on their personal electronic devices (27% used a mobile device). They were informed that the study's purpose was to

examine the relationships between everyday life, relationship events, psychological well-being, and sexual behaviour. They were told that each diary day they filled out would add one more lot in a lottery for four Amazon.com coupons worth 20€ each and that they would receive extensive feedback on their personality and the longitudinal co-development of their mood, self-perceptions, and clothing choices over week days.

Students of our university could earn course credit instead. They were informed that, although the study required their email address to send diary invitations, data would be stored separately and anonymously and that the feedback would also be generated anonymously and automatically. Research that only entails self-reports does usually not require IRB approval under German regulations.

3.1.5.2 Demographic and personality survey

After obtaining consent, we asked participants for their sex, age, and relationship status. Only self-identified females in a heterosexual relationship could proceed. Next, the women reported various demographics, details about their relationship, their menstrual cycle and contraception status and completed several measures of personality, relationship satisfaction and jealousy (see Table 2).

3.1.5.2.1 Diary

On the next day and until at least 30 entries were obtained over a period of at least 40 days, women were invited to fill out the diary via email and, if possible, text message at 5 pm German time. They could fill out the diary until 7 hours after the invitation was sent. Participants completed the diary in a median time of 6.5 minutes. In each diary entry, they responded to 58 items about their relationship, interactions with their partner,

clothing style, self-esteem, narcissism, sexual desire and behaviour, and menstrual cycle (see below). They were asked to refer to the period since their last entry or 30 hours ago, whichever happened sooner. They could also give free-text responses to provide context for their entry.

3.1.5.2.2 Follow-up survey

After completing the diary (usually immediately after the last day), women were invited to a follow-up survey. In this survey, we asked several questions which we expected to relate to the validity of the results, namely what they thought the purpose of the study was, whether they were ill, took medication, lost weight, smoked, broke up with their partner, started a new relationship, switched contraception methods, or felt extraordinarily stressed. They then received their feedback. If they had not menstruated during the last 14 days of the diary, we sent them reminders every other day inviting them to tell us about their next menstrual onset, continuing until they did.

3.1.5.3 Implementation

The study was implemented using the online open-source survey framework formr.org (Arslan & Tata, 2016). The software permitted us to automate all repetitive aspects of the study, such as administering surveys, sending email and text message invitations and to generate graphical feedback for participants. The study administrators communicated with participants through an email account and could send manual reminders and administer service requests in case of problems without seeing the participants' data.

3.1.6 Measures

We documented all items for all surveys in the online supplement. To assess reliability for cross-sectional measures we computed Cronbach's alpha. For within-subject measures, we computed the generalizability of within-subject change aggregated across items (Shrout & Lane, 2012) using the *psych* package (Revelle, 2017). We documented the main outcome measures for the diary and their reliabilities in Table 2. We used measures from previous studies where possible, but previous studies often could not or did not test the relevant generalizability metric for a within-subject process, namely whether the scale measured within-subject change reliably. Unfortunately, this did not appear to be the case for the mate-retention-related measures, and generalizabilities for the other outcomes were lower than optimal. The cross-sectional measures of personality, i.e. the Big Five Inventory (Lang, Lüdtke, & Asendorpf, 2001) and shyness (Asendorpf & Wilpers, 1998), had Cronbach's α s ranging from .81 to .88. Agreeableness, which we did not use in this study, was an exception with $\alpha = .73$. Confidence intervals (95%) for these α s had a width of 0.02-0.04. The reported physical attractiveness of the partner was based on two items (taken from (Haselton & Gangestad, 2006)) asking about his physical attractiveness and his sexiness ($\alpha = .80$). The reported short-term attractiveness of the partner included the physical attractiveness scale, plus an item about his attractiveness for an affair or one-night stand and an item asking about sexual satisfaction with this partner ($\alpha = .62$). To compute the partner's attractiveness relative to oneself (Haselton & Gangestad, 2006) we first computed a five-item mate value scale (Landolt, Lalumière, & Quinsey, 1995) for the partner and the participant. We omitted two items in both scales because they

used tortuous sentences and counterfactuals and exhibited low to negative scale loadings. Own mate value ($\alpha = .84$) correlated .25 with partner mate value ($\alpha = .78$). We then tested whether the four-point Likert item “Who does better with the opposite sex? You or your partner?” favoured the partner most when his mate value exceeded hers. This was the case. Thus, we standardised and summed the mate value difference and the latter item ($\alpha = .74$). The relative measure was uncorrelated with the various absolute measures ($|r| < .05$). Further details on scale construction and reliabilities are available online. Confidence intervals (95%) for α s of the attractiveness-related scales had widths from .04-.07.

Table 2. Outcome measures in the diary.

Construct	Scale Origin	Items	Rcn	Example item
Female Jealousy		3	.00	“I have asked my partner with whom he spent his day.”
Relationship satisfaction		1	.85	“How satisfied were you with your relationship?”
“Sexy” clothing	Schwarz & Hassebrauck, 2008	3/8	.60	“Would you describe your chosen clothes today as sexy?”
Extra-pair desire	Haselton & Gangestad, 2006	12	.60	“I had sexual fantasies about men other than my partner.”
Partner mate retention	Haselton & Gangestad, 2006	4	.00	„My partner asked my with whom I spent my day.“
Female mate retention	Haselton & Gangestad, 2006	6	.17	“I told my partner I love him.”

Narcissistic admiration and rivalry	NARQ-K (Back et al., 2013)	3+3	.57/.55	"I felt worthy of being seen as a great personality."
Self-esteem	RSES Rosenberg, 1965	1	.86	"I was satisfied with myself overall."
Self-perceived desirability		1	.85	"I felt sexually desirable."
In-pair desire		3	.75	"I found my partner particularly sexually attractive."

Notes. Rcn = Reliability of change or generalizability of within-person variations. For clothing choices, three of eight items asked about "sexy" clothing choices.

3.2 Analysis

3.2.1 Menstrual onset computation and fertile window inference

On each diary day, women reported whether they had had their period on that day or in the preceding 6 days. As this meant that women could report the same menstrual onset multiple times and hence incorrectly recall a menstrual onset a few days later, we always used the report closest to the reported onset. Women also reported a last menstrual onset in the survey preceding the diary and a next menstrual onset in a follow-up survey after the diary. We used these dates to generate time series for each participant. We then counted forward and backward from each menstrual onset to the next or respectively last menstrual onset. If the next menstrual onset was not available, because women did not complete the follow-up survey, we could infer it from the reported average cycle length, but only did so for our robustness checks. We then inferred a narrow and a broad fertile window. For our robustness analyses, we

additionally computed a continuous estimate of the probability of being in the fertile window according to the method advocated by Gangestad et al. (2016), who based their estimates on (Stirnemann, Samson, Bernard, & Thalabard, 2013), among other data. This method accounts for the fact that the luteal phase length is less variable than the follicular phase. Further details can be found in the online supplement. This procedure resulted in seven different predictors which allowed us to include a varying number of diary days, see Table 3.

Table 3: The different conception probability estimates that were used as predictors.

Description	fertile window	n (days)	% of days	n (women)
all days		28,493	100	1043
narrow window, backward counted	15-19	9501	33.35	794
broad window, backward counted	14-22	11,497	40.35	796
narrow window, forward counted	11-15	12,171	42.72	973
broad window, forward counted	8-16	15,880	55.73	997
continuous, backward counted	n/a	17,614	61.82	817
continuous, backward counted from reported cycle length	n/a	26,580	93.29	1043

Notes. To make effect sizes across predictors comparable, we dummy-coded windowed predictors as being 0.053 on non-fertile days and 0.44 (broad)/0.51 (narrow) on fertile days. These were the averaged probabilities for those days from the continuous estimate, which varied from 0.01 to 0.58. Days were counted from the menstrual onset, starting at 1. The non-fertile window was defined as days 4-12 (backward-counted) or respectively days 18-26 (forward-counted).

3.2.2 Statistical approach

To test our hypotheses we fitted multilevel models in *lme4* (Bates, Mächler, Bolker, & Walker, 2014) with a random intercept per person, interacting our fertility estimate with a dummy for hormonal contraceptive use. Defining the model in this way allowed us to both test whether any ovulatory change among naturally cycling women was different from zero, as well as whether it was different from any changes occurring among hormonal contraception users. For Likert-scaled outcomes we fitted linear multilevel models and for categorical outcomes we fitted generalized linear multilevel models with a binomial family using a probit link. In Wilkinson notation (Bates et al., 2014, p. 4; Wilkinson & Rogers, 1973), the model equation can be formalised as

$$\text{outcome} \sim \text{fertile_window} * \text{hormonal_contraceptive_user} + (1 \mid \text{person})$$

Here, *fertile_window* refers either to the backward-counted narrow or broad fertile window in the preregistered analyses. To test H3.1 we also refitted models with self-esteem as a covariate. Because we did not preregister it, we did not fit random slopes for the fertile window effect. We instead examine the effect of doing so in our robustness checks (Bates, Kliegl, Vasishth, & Baayen, 2015).

3.2.3 Robustness checks

To test our results for robustness, we used a variety of approaches. First, we built a baseline model that deviated from our preregistered procedure but implemented the best practices published after we preregistered (Blake et al., 2016; Gangestad et al., 2016).

In Wilkinson notation, the model can be formalised as

outcome ~ (*fertile_window_probability* + *premenstrual_phase* + *menstruation*) *
hormonal_contraceptive_user + *average_fertile_window_probability* + (1 | person)

Here, the probability of being in the fertile window was continuously estimated from backward counting from the next menstrual onset, according to Gangestad et al. (2016). In cases where the next menstrual onset was not observed, we fell back to the next menstrual onset inferred from the average cycle length that women reported in the screening survey (see Table 3). Because using a continuous predictor means that days on which women were menstruating or in the premenstrual phase were also included, we included dummy variables for the reported menstruation and the inferred premenstrual phase (the six days before the menstrual onset). We also adjusted for average probability of being in the fertile window per woman as an additional predictor, to ensure within-person estimates (Bafumi & Gelman, 2006). We let our fertility and menstruation predictors interact with hormonal contraception status.

In this baseline model, we included all usable data (from 1,043 women, 421 naturally cycling) instead of excluding many women based on our preregistered criteria.

We tested robustness by fitting numerous variations on the baseline model described above. We then examined the effect size and standard error of the fertile window predictor across many models, which we outline in the following.

In model *M_r1*, we allowed a varying slope per participant for the fertile window and the two menstruation dummy variables, a “maximal” specification that is somewhat

controversial because of the potential for overparameterisation (Barr, Levy, Scheepers, & Tily, 2013; Bates et al., 2015). We tested four levels of stringency for exclusion (“all”, “lax”, “conservative”, “strict”, see Fig. 1) in models M_{e1-4} and M_{m5} . We also tried to implement a post-hoc criterion (M_{e5}) for data reliability, under which we excluded 1251 diary days (4% of all) where participants a) gave the same answer to all Likert items ($n=23$) or b) accessed the diary later or earlier than intended due to technical problems ($n=896$) or c) took more than 24 hours ($n=376$) or less than a minute ($n=30$) to finish filling out the diary. We took these steps to reduce the number of careless responses and to remove days on which the assigned cycle day might be off. We also tested (M_{e6}) whether the effect of excluding women who were trying to get pregnant, an exclusion criterion we had not preregistered.

In models M_{p1} to M_{p11} , we tested different estimates of the fertile window as our predictor to address the concerns described in section 2.2.2. We compared all combinations of a narrow window, broad window, continuous estimates, and backward- and forward-counting. To address section 2.2.3 and 2.2.4 empirically, we then tested whether effects could be shown using only a single day per participant, two days (at low and high fertility) or four days (two each).

To transparently show how much modelling decisions that might be considered researcher degrees of freedom (section 2.2.1) matter, we fitted models M_{c1} to M_{c5} . In these, we added adjustments in one model each for M_{c1} self-esteem, M_{c4} week day and week number, and M_{c5} the time when the diary was started and how long it took to fill out, or we omitted adjustments for M_{c2} average fertile window probability, or

M_c3 both average fertile window probability and menstruation. This allowed us to see the effect these adjustments had on the estimated fertility effect. In *M_c6* to *M_c7*, we tested two different temporal autocorrelation models as opposed to the unstructured random effect correlations in our main model. In *M_c9*, we tested whether measurement reactivity might confound our results, by adjusting for splines for the number of days since the diary beginning (a variable for days filled out and one including missing days), by hormonal contraceptive use.

We then tested various moderators to prod different methodological issues. To partially address the issues pointed out in section 2.2.5, in *M_m1* we compared four groups of contraceptive methods (hormonal, awareness-based, barrier-based, none). For women who combined multiple methods, the order of the list above determined precedence. To test generalizability, we tested moderation by participant age (in groups 18-20, 20-25, 25-30, 30-35, 35-45, 45 and older, *M_m2*), and whether the weekday (*M_m3*) or the weekend (*M_m4*) moderated effects (Roney & Simmons, 2013). Because the validity of fertility estimates from counting methods depends on accurate reporting and regular cycles, we tested for moderation by cycle length (*M_m6*), by self-reported certainty about menstruation parameters (*M_m7*), and by self-reported cycle regularity (*M_m8*). To further test generalizability, we also tested for moderation by cohabitation (*M_m9*) and by marital status (*M_m10*).

We also ran Bayesian regression models using Stan (Bürkner, in press; Carpenter et al., 2015) to be able to appropriately model the positively skewed distribution of the Likert items for extra-pair desire (i.e. many respondents indicated minimal extra-pair

desire) in an ordinal regression using a cumulative outcome distribution and random effects for items and participants. In the Stan models, we also tested for heterogeneity of effect sizes across participants and items. In additional Stan models, we fitted a thin-plate regression spline (S. N. Wood, 2003) over backward-counted cycle days to examine whether the continuous probability of being in the fertile window would be a good fit to the shape of the estimated effect. In exploratory analyses, we also fitted one Stan model per item and graphically summarised the posterior densities for the conception probability estimates. Because of computational limitations, we fitted models separately instead of pooling information across items and scales.

In our robustness checks, a null hypothesis testing approach would have been inappropriate, given the wide-ranging exploration and varying questions asked across outcomes and models. Instead, we focused on visualisations and the fertility effect's point estimate and confidence interval. We inspected effects to look for evidence that an effect was not robust (i.e. shifts in estimates that might not be explainable by sampling error). We summarise what we consider the main patterns, but made the detailed results available online (see below).

3.3 Data, code, results, and materials availability

We released all code, both for implementation and analysis, materials, and full statistical results pertaining to this study openly in the online supplement (https://rubenarslan.github.io/ovulatory_shifts/). We partially anonymised the data and uploaded them to the Open Science Framework for safekeeping. However, because sexual diary

data are hard to completely de-identify and extremely sensitive, we did not request consent from participants to share their data openly and cannot share these data publicly. Therefore, we can only share the partially anonymised data with anyone who has a valid reason and agrees not to attempt to re-identify the data. We have also generated a synthetic dataset using synthpop (Nowok, Raab, & Dibben, 2016). This dataset attempts to replicate many of the central features of our data, such as means and bivariate associations, but is anonymous. Others can use this to test and build models using realistic fake data, which we can then easily test on the real data.

4 Results

4.1 Power analysis and researcher degree of freedom simulation

We documented our power analyses and researcher degrees of freedom simulations and results in more detail online. They showed that under reasonable assumptions, power was a function of the number of usable days multiplied by the sample size.

To detect a regression coefficient of the fertile window of .2 with an alpha level of .01 in a sample of 150 naturally cycling women measured over 30 days, we had a power of .84 using a windowed predictor, because using windows meant not being able to use many of the measured days. Using a continuous predictor increased power to .99. In a sample of 500 women measured over 30 days, power approached 1. Power to detect an effect half/a quarter this size was still .97/.36 using a continuous predictor.

4.2 Preregistered analyses

To adjust for multiple comparisons, we set the significance level to .01 (see below). After applying our “lax” exclusion criteria (see robustness checks for further tests of stringency), we could use data from 143 naturally cycling women and 374 hormonal contraceptive users. Using the narrow (broad) fertile window predictor, we could use 6,378 (7,740) diary days, or 12 (15) days per woman (see Table 3).

All outcomes are summarised in Table 4. For three outcomes, effects of the fertile window were significantly positive for naturally cycling women but absent for hormonal contraceptive (HC) users, a pattern we will refer to as *fertile window increases* in the following. When the interaction between HC use and the fertile window is of the same size as the fertile window effect, but negative, it indicates an absence of the change among HC users.

We found small fertile-window increases in extra-pair desire and behaviour. Effects were significantly positive for all extra-pair subscales except the compliments subscale. We examined this pattern in more detail in the robustness analyses. Actual instances of intimate contact or sex with another person were very rarely reported (48 women reported extra-pair sex on 127 days, 112 women reported extra-pair intimate contact on 383 days), so that the log-odds-ratios seem large, but estimates were not significant (p s > 0.17).

We also found small fertile window increases in in-pair desire, similar in size to the increase in extra-pair desire. On average, women did not have significantly more sex

during the fertile window, but there were two consistent but only marginally significant moderators of the ovulatory increase in having sexual intercourse, namely cohabitation and average number of nights spent with the partner. Cohabitation moderated the changes, so that we observed no ovulatory increases among women in long-distance relationships ($p = .020$). Women who spent more nights per week with their partner also showed stronger ovulatory increases ($p = .048$). The increases were not stronger on the specific nights that the couple spent together ($p = .58$). Women did not initiate sex significantly more often.

We also found small fertile window increases in self-perceived desirability, but not on wearing “sexy clothes”. The predicted effects were not significant for initiating sex, male mate retention, narcissistic admiration, and narcissistic rivalry (all $ps > 0.21$).

The changes in self-perceived desirability, in- and extra-pair desire were also clearly apparent when plotting a smoothed spline over reverse-counted cycle days (Figure 2). The pattern of results held independently of whether we used a narrow or broad fertile window as the predictor. As predicted, there were no significant effects on self-esteem and adjusting for self-esteem did not weaken any other tested associations.

Figure 2. Smooth thin-plate splines (S. N. Wood, 2003) fitted over days until next menstruation with three central outcomes. The dashed line shows the estimated probability of being in the fertile window for each day. The shaded areas reflect 95% confidence bounds pooling days over participants for simplicity. To account for the cyclical nature of the data, we spliced in duplicates of the time series at both ends before estimating the splines and then dropped them afterwards.

None of the three main predicted moderators, i.e. the partner's short-term, sexual, and relative attractiveness, significantly exhibited the predicted pattern for any outcome (p s > 0.07), and some patterns went descriptively in the opposite direction of the prediction. Also, none of the personality variables moderated changes in extra-pair desire and behaviour (p s > .32). A test of whether neuroticism moderated shifts in self-perceived desirability was significant ($p = .002$), but inspection of marginal effect plots showed this to be driven by significant increases in desirability among highly neurotic hormonal contraceptive users, an unpredicted and likely spurious result.

Because we had not preregistered a procedure to correct for multiple comparisons due to multiple outcomes and believed Bonferroni to be too conservative, as many outcomes were highly correlated, we tested whether we would have ever rejected the null hypothesis of no effect in our HC control group with the significance threshold of .01. Although this would have been the case for one outcome, follow-up analyses showed that this result would not have survived our robustness analyses, so we concluded that our chosen threshold was appropriate. The pattern of significant results here would not have been different using the uncorrected threshold of .05 or when using a Benjamini-Hochberg (Benjamini & Hochberg, 1995) correction (see online).

Table 4. Preregistered associations, using the narrow fertile window

Outcome	Intercept	fertile	HC user	HC user x fertile
Extra-pair desire and behaviour				
extra-pair (EP)	1.75±0.05	0.27±0.06	-0.05±0.06	-0.30±0.07

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desire & behavior		$p < .001$	$p = .373$	$p < .001$
- EP compliments	2.37±0.08	0.25±0.11 $p = .023$	-0.11±0.10 $p = .267$	-0.37±0.13 $p = .005$
- EP flirting	1.36±0.04	0.15±0.06 $p = .006$	-0.09±0.05 $p = .078$	-0.22±0.07 $p < .001$
- EP going out	1.99±0.09	0.24±0.15 $p = .113$	0.24±0.10 $p = .019$	-0.31±0.18 $p = .088$
- EP sexual fantasies	1.50±0.06	0.49±0.09 $p < .001$	-0.19±0.08 $p = .012$	-0.43±0.11 $p < .001$
- EP desire	1.65±0.05	0.34±0.06 $p < .001$	-0.13±0.06 $p = .047$	-0.31±0.07 $p < .001$
extra-pair intimacy ^{pb}	-4.47±0.30	0.89±0.42 $p = .033$	-0.22±0.37 $p = .554$	-0.57±0.72 $p = .431$
extra-pair sex ^{pb}	-4.60±0.39	0.60±0.56 $p = .282$	-0.44±0.57 $p = .444$	0.17±1.08 $p = .873$
In-pair desire and behaviour				
in-pair desire	3.48±0.08	0.31±0.12 $p = .010$	0.24±0.09 $p = .010$	-0.39±0.14 $p = .008$
sexual intercourse ^{pb}	-0.98±0.07	0.12±0.17 $p = .483$	0.17±0.08 $p = .026$	-0.26±0.20 $p = .203$
sex initiated by partner vs. woman ^{pb}	0.26±0.09	-0.14±0.31 $p = .642$	0.12±0.11 $p = .276$	0.11±0.37 $p = .775$
partner mate retention	2.86±0.07	0.05±0.09 $p = .569$	0.00±0.08 $p = .954$	-0.12±0.11 $p = .255$
Self-perceived desirability and clothing choices				
self-perceived desirability	3.72±0.08	0.37±0.13 $p = .004$	-0.07±0.09 $p = .477$	-0.38±0.15 $p = .012$
sexy clothing	3.16±0.07	-0.14±0.10 $p = .169$	0.02±0.08 $p = .831$	0.09±0.12 $p = .492$
Narcissism				
narcissistic admiration	2.69±0.10	-0.05±0.08 $p = .551$	-0.14±0.11 $p = .214$	-0.09±0.09 $p = .335$
narcissistic rivalry	1.29±0.04	-0.03±0.05 $p = .535$	0.05±0.05 $p = .322$	-0.02±0.06 $p = .747$

Notes. Coefficients significant at $p < .01$ (before rounding) are bold. Associations with outcomes marked ^{pb} were estimated in a probit regression. The number after the ± is a standard

error. Scales starting with EP are subscales. The sex initiation item asked whether it was rather the partner or rather the participant who initiated sex, in a forced-choice question. Positive effects reflect that it was rather the partner.

4.3 Robustness checks

Our robustness check results are documented fully in the online supplement, here we verbally and visually summarise the most important patterns. We were able to include 421 NC women and 622 HC users. We used a continuous measure of the probability of being in the fertile window, estimated from backward-counting from the actual next menstrual onset, falling back to the next menstrual onset inferred from the average cycle length when necessary. This way, we were able to include 25,948 diary days, i.e. on average 25 days per woman and more than 3 times as many days as in the preregistered analyses.

We repeated all preregistered tests using this bigger dataset and the adjusted model. Unless otherwise mentioned, results were robust to including more data and to the various checks listed in the method section. Specifically, estimates of fertile window increases in extra-pair desire and behaviour, in-pair desire, and self-perceived desirability were robust, but standard errors shrunk by about half. Further, none of the predicted moderation patterns turned significant when adding more women, and using slightly different items for the partner attractiveness moderator variables did not change the pattern. However, when modelled, random slopes for the fertile window predictor were substantial, larger than for the menstruation predictors and as large as the residual variation and the variation explained by the random intercept. No fertile window

increases emerged for any other outcomes, including further outcomes for which had not predicted increases.

We found that the stringency of our exclusion criteria, designed to exclude women with potentially anovulatory cycles, did not moderate the effect sizes in the expected way, i.e. that effects became stronger with more stringent criteria. When testing for moderation by exclusion criteria in *M_m5*, the pattern validated our post-hoc decision to keep only the truly necessary constraints. When applying stricter exclusion criteria, some effects weakened or confidence intervals overlapped zero, but this seemed to reflect the heavily decreased sample size (see Figure 1). Applying our post-hoc criterion (*M_e5*) to exclude potentially unreliable data also had no noteworthy effect. Excluding women who were trying to get pregnant diminished the effect on in-pair desire, but did not eliminate it.

When we used a continuous fertile window predictor, we also adjusted for premenstrual and menstrual days. We found that including adjustments for menstruation and pre-menstruation (*M_c3*) reduced effect sizes for the fertile window predictor. We could not always adjust for menstruation when using a narrow window predictor because of model convergence problems. After taking this into account, we found no systematic pattern in which certain predictors (narrow or broad window, forward or backward counted) had larger effect sizes than others across outcomes (see Figure 3). However, continuous curves over backward-counted days (Figure 2) matched

the predicted pattern more closely than curves over forward-counted days (online).

Figure 3. Coefficient plot showing a consistent effect of the fertility predictor among naturally cycling women (red) but not HC users (black) across several predictor and model specifications (explained in further detail in the text).

In models *M_p7* to *M_p9*, we found that none of the associations found to be significant in the pre-registered analyses would have been discovered had we used between-subject analyses or a high-low fertility within-subject design with only two days.

There was a complex pattern of results when separately examining contraception methods. The ovulatory increase in extra-pair desire tended to be larger for fertility-aware women (5% of the sample) and this was not merely because they had more regular cycles. Still, women using barrier methods or no contraception also showed

ovulatory shifts. The shifts in in-pair desire and self-perceived desirability, on the other hand, appeared weaker or absent in fertility-aware women but stronger in women using no contraception (6% of the sample). Because women using methods other than hormonal contraceptives and barrier methods made up only a small minority of the sample, we could not rule out sampling variation as an explanation.

Inspecting time series of within-subject change by item (Figure 4) for the three outcomes that were significant in the preregistered analysis, namely extra- and in-pair sexual desire and self-perceived desirability, showed that naturally cycling women tended to exhibit peaks around the estimated day of ovulation, while hormonal contraceptive users exhibited no clear peaks or minor peaks around menstruation.

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Figure 4. Item-by-item plot of within-subject change. The trails in this plot represent within-subject change as a percentage of the maximal peak. Plots are smoothed with a moving average over three days. Items are ordered top to bottom by how late in the cycle the highest peak occurs for naturally cycling women.

Across outcomes, effects tended to be largest for women with cycle lengths between 25 and 30 days, and for women who were more certain about their menstruation parameters, but not for women whose cycles were more regular.

We tested whether the effects of in- and extra-pair desire were different in size and independent of each other, to test whether they were potentially both driven by a third variable, such as increased target-unspecific sex drive. The two categories of desire negatively correlated within each woman, so that adjusting either desire outcome for the other did not diminish the estimated fertile window increase. However, we also conducted simple forward simulations of the realistic scenario that unobserved properties of the object of desire decide whether desire is expressed as in- or extra-pair. These simulations showed that we cannot resolve the question of whether the effects were entirely explained by target-unspecific desire without directly measuring it. Comparing unstandardized effect sizes showed that the fertile window increases in extra-pair ($b = 0.26$ 95% CI [0.17;0.35]) and in-pair desire (0.26 [0.10;0.42]) were comparable in size. Examining item-level effect sizes showed larger heterogeneity across items than across objects of desire (see online supplement).

5 Discussion

In the present large diary study, we aimed to replicate reports of ovulatory changes in extra- and in-pair sexual desire and behaviour, as well as related outcomes, and test several methodological concerns. We could replicate only some of the previously reported ovulatory changes, namely those in the three main outcome categories of

extra-pair sexual desire and behaviour, in-pair sexual desire and behaviour, and self-perceived sexual desirability. In Figure 2 we show the probability of being in the fertile window closely matches the observed changes across the cycle for these three outcomes.

5.1 Main effects of the fertile window

5.1.1 Extra-pair desire and behaviour

We found robust support for a fertile window increase in extra-pair desire and behaviour. This scale was a fairly heterogeneous average of items measuring increased attraction to, fantasizing about, flirting with, receiving compliments from, and going out to meet with men other than the primary partner. In separate analyses, we also examined whether women were more likely to be intimate or have sex with other men during the fertile window. While descriptively supporting the predicted ovulatory shifts, these events were rare and effects were not significant. We also examined effects on the subscale level. Fertile window increases in sexual fantasies were descriptively strongest, but the aggregation of subscales seemed justifiable.

5.1.2 In-pair desire and behaviour

We found robust support for fertile window increases in in-pair desire. Although in-pair desire predicted intercourse with the partner, ovulatory increases in sexual intercourse were not significant in our preregistered analyses. Potentially, we simply had too little power to detect mean shifts in this dichotomous behaviour: Women

reported sex on 21% of days and 67 women who filled out the diary on more than 25 days never reported sex with their partner at all. With added data, we observed increases in some of our robustness tests, but only in comparison to the HC group (which decreased non-significantly). Further, as predicted, two indicators of partner availability moderated the sexual intercourse shifts in the preregistered analyses marginally significantly: ovulatory increases were absent among women in long-distance relationships and among those who reported rarely spending the night with their partner. The daily report of whether the couple spent the night together did not moderate the shift, but the same-day behaviour may act as a mediator, not moderator, of ovulatory shifts in sexual behaviour. We see this pattern as partial support for our hypothesis 1.7., stating that ovulatory increases would be observed if circumstances allowed it, if the partner was close by. This pattern is also consistent with the findings for coupled women in a larger within-subject study on 1,180 women and 37,170 diary days (Caruso et al., 2014), but runs counter to previous results from 20,000 women in a between-subject study (Brewis & Meyer, 2005). Unexpectedly, shifts in in-pair desire also appeared to be stronger for women cohabiting with their partner.

5.1.3 Mate retention, jealousy

We observed no fertile window changes in partner mate retention, but the generalizability of change for these items was very low, making the detection of an effect unlikely. Our questions for these outcomes were based on the previous literature, in which generalizabilities of change were not reported. We had ourselves preregistered a suboptimal procedure for improving outcome reliabilities, based on assessing

Cronbach's alphas, which ignore the multilevel structure of the data. We instead calculated all analyses by item in a purely exploratory format. Based on these analyses and research published after our preregistration (Gangestad, Garver-Apgar, Cousins, & Thornhill, 2014), future research on partner mate retention should more clearly and comprehensively examine *prohibitive* behaviours, as opposed to *persuasive* behaviours, because items measuring the former seemed to show stronger changes.

5.1.4 Self-perceived desirability and clothing choices

We found fertile window increases in self-perceived desirability in our preregistered analyses that were robust to our checks, although standard errors were relatively broad because we used only a single item to assess this outcome. Contrary to our predictions, we found no fertile window changes in self-reported “sexy clothing”, even though this was associated with desirability. As predicted, we also found no change in “flashy/showy” clothes and self-esteem in our robustness checks. These results are consistent with recent large-sample replications of fertile phase increases in facial attractiveness (Jones, Hahn, Fisher, Wang, Kandrik, Han, Lee, et al., 2017), suggesting that day-to-day changes in self-perceived attractiveness might track actual changes in physical attractiveness.

5.1.5 Other outcomes

For all the other outcomes we found no ovulatory changes that were also absent among HC users. Reassuringly, in no case did we observe any significant associations for outcomes for which we predicted none (relationship satisfaction, self-esteem,

spending the night/communication with the partner, female jealousy, and female mate retention). Nor did we find associations for the narcissism outcomes, for which we had indirectly extrapolated our predictions from prior reports in the literature of ovulatory changes in clothing, interpreted as signs of intrasexual competition (Durante et al., 2008). We should reiterate in this context that we did not replicate cycle shifts on clothing choices either. Perhaps this can be interpreted as evidence that the literature suffers more from potential false positives than from false negatives, though it is noteworthy that some previous studies had not found ovulatory increases in in-pair sexual desire and behaviour (Brewis & Meyer, 2005; Haselton & Gangestad, 2006). We would like to emphasize that both negative and positive results were largely robust to the many different analytic approaches that we tested.

5.2 Predicted moderator effects and individual differences

There was insufficient evidence for moderation of male mate retention behaviour, extra-pair or in-pair desire by the partner's attractiveness (no matter if assessed as relative to self, sexual, or physical), as predicted by the *good genes ovulatory shift hypothesis*. Although some patterns descriptively pointed in the predicted direction, none of the predicted patterns were significant, and some were opposite to our predictions. Because only 144 naturally cycling women remained for our preregistered analyses, statistical power may have been insufficient to detect plausible moderation effect sizes. However, we found no evidence for moderation effects in the more inclusive sample of our robustness tests. Although our sample sizes are bigger than many published studies that reported a moderation effect (Haselton & Gangestad, 2006;

Pillsworth & Haselton, 2006b), we would ideally prefer to exceed their power by a wider margin due to winner's curse, i.e. effect sizes being overestimated through selection and publication bias. We should also mention that some of the earlier studies we aimed to replicate (Haselton & Gangestad, 2006; Larson et al., 2013) did not actually report significant main effects of the fertile phase. Increases were reported to be *qualified* by a moderator. In this sense, we replicated neither findings on main nor on moderator effects from these studies. Still, we believe GGOSH can be taken to predict main effects as well, because amplified shifts in some women whose partners lack certain characteristics should, averaged across women, still yield detectable main effects. But taken literally, our findings shed doubt on GGOSH in finding no substantial moderator effects by partner attractiveness.

There are some conceptual similarities between ovulatory shift moderators of extra- and in-pair desire and direct tests of ovulatory changes in mate preferences, because both regard a shift in who is preferred as a mate. Newer, more adequately-powered laboratory research also sheds doubt on ovulatory shifts in preferences for facial masculinity (Jones, Hahn, Fisher, Wang, Kandrik, Han, Fasolt, et al., 2017) and twin studies show that heritable individual differences in this preference dwarf any cyclical changes (Zietsch, Lee, et al., 2015).

We found no evidence for the tentatively predicted moderation of increases in extra-pair desire or self-perceived desirability by neuroticism, extraversion, or shyness. However, because we had on average 25 days for each woman, we could estimate inter-individual differences in ovulatory increases (i.e. random effects for the fertile

window). Random effect variances for the fertile window predictor were substantial. Hence, there might be real heterogeneity in ovulatory increases to be explained. Future research should test and improve the reliability of these inter-individual differences across cycles. Determining if there is inter-individual variation in cycle shifts to be explained should be a precursor step before further attempts to identify both methodological and theoretically substantial moderators of ovulatory increases, such as partner or relationship attributes. Further, until any such moderation patterns are better understood, researchers should probably refrain from testing for moderation in the absence of main effects of fertility if they have not preregistered their approach, because this may lead to (accusations of) overfitting.

5.3 Theoretical implications

Although further tests should be conducted, the *good genes ovulatory shift hypothesis* could be wrong, given that we could not replicate previously reported moderators. More recent theoretical work emphasises that predictions of adaptive extra-pair sex, which (Pillsworth & Haselton, 2006a) call dual mating, should be divorced from predictions of ovulatory changes in mate preferences that do not necessarily precipitate extra-pair sex, but still function to bias sire choice (Gangestad, Thornhill, & Garver-Apgar, 2015). We cannot test all aspects of these recent theoretical developments in our study. An alternative, simpler explanation (Roney & Simmons, 2013) is based on life history theory. It suggests the observed increase in sexual desire during the fertile phase reflects a motivational priority change towards reproduction. The purported function would be to accept higher costs of sex, such as energetic and opportunity costs

or sexually transmitted infections, the more likely it is that sex leads to conception. This theory also predicts fertile phase drops in somatic investment, such as food intake (Fleischman & Fessler, 2007; Roney & Simmons, 2017). In this study, we did not assess any non-reproductive motivations, and we collected no data on single women. Hence, we cannot test whether general, target-unspecific sexual motivation drives the effects on in- and extra-pair desire we find ((Roney, 2009). Future studies should be designed and powered to discriminate between these and other theories. Relatedly, theoreticians should make exact predictions down to what certain statistical models will find, because verbal ambiguity might otherwise preclude the identification of the best supported theory.

5.4 Effect size comparison

Some perspectives (Roney & Simmons, 2013) predict a generalized increase in sex drive with fertility across the menstrual cycle, while others more specifically predict an increase in sexual interest for certain partners (Gangestad et al., 2015). These perspectives differ in predictions of whether the effect on extra-pair desire should be larger than that on in-pair desire. Although testing these competing predictions was not the goal of the present study, we can compare the relevant effect sizes. The continuous backward-counted predictor recommended in (Gangestad et al., 2016) hedges for uncertainty in the estimation of the fertile window. Our effect sizes thus account for uncertainty and reflect the estimated change when certainly in the fertile window, although the predictor never gives a more confident prediction than 58%. In Likert points from 1 to 6, the fertile window effect was 0.26 [0.17;0.35] for extra-pair desire in the

robustness check data. The in-pair desire effect had the same size: 0.26 [0.10;0.42].

We could now standardise the effects by the residual standard deviation in the multilevel model to obtain an effect size estimate of Cohen's d . Since the residual standard deviation of extra-pair desire is much smaller (0.61) than that of in-pair desire (1.1), their standardised estimates would differ by a factor of two. However, our items for in- and extra-pair desire were not comparable and upon inspecting item-level associations in Bayesian models that appropriately account for the ordinal nature of the Likert data, we believe comparisons between the two outcomes are futile. If we can conclude anything, effects were larger on average for items that required no object of desire to be present and no action to be taken. Future studies should attempt to settle the question of whether changes in extra- or in-pair desire are independent and different in size. Most importantly, they should test whether both can be simplified to an increase in sex drive that amplifies interest in all men without affecting their rank order, i.e. mate preferences. To do so, studies should construct parallel items to measure extra-pair, in-pair and objectless sexual desire and behaviour, and test for fertile phase changes in the rank order of ratings of male stimuli.

We suggest not to prematurely ignore the reported effects because of their small size. The effects on in-pair desire are, for instance, comparable with reported effects of a hormonal contraceptive use on sexual desire in a randomised controlled trial (Zethraeus et al., 2016). Moreover, we found evidence for substantial inter-individual variation (see below), so that effects that are small on average might be substantial for some women.

5.5 Hormonal contraception

Whenever we found an ovulatory increase, we also found that it was absent among hormonal contraceptive users. In this sense, we identified one reliable moderator. The absence of these cycle changes probably reflects the suppression of ovulation and concurrent hormonal changes. Moreover, estimated effects of menstruation and the premenstrual phase on psychological outcomes as measured in the diary were also diminished among HC users. In the preregistered analyses, we found only small and statistically non-significant mean level differences between HC users and cycling women in the diary outcomes, as well as in the demographic and personality variables that we tested. These differences are presumably confounded by selection and attrition effects. For example, women who expect their relationship to last may be more likely to start using HC and to show less extra-pair desire, and women who experience libido decreases on HC may go off it again. Thus, the (absence of) mean level differences may not (entirely or at all) speak to causal effects of HC.

There are few randomised controlled trials (RCTs) that can answer questions about psychological changes caused by HC use. Existing ones so far mostly ignore cycle phase (Zethraeus et al., 2016, 2017) thus not yielding the full picture of differences across the cycle. Potentially, this can lead to spurious or misleading conclusions of differences, if women in the naturally cycling control group are measured in different cycle phases across time points. As the effects of cycle phase on sexual desire in our study were similar in size to effects reported for hormonal contraceptives in (Zethraeus et al., 2016), further RCTs should tease cycle phase and HC influences apart.

The suppression of cyclical psychological changes is not currently being pointed out as a side effect of the pill in package leaflets, although they do mention potential effects on libido and appetite. Potentially, decreased fluctuations in extra- and in-pair desire might be seen as less worrisome than e.g. decreased average levels of libido, or altered mate preferences (Alvergne & Lummaa, 2010), but this decision is best left to HC users themselves. Decision making about HC use may vary, e.g. some women may prefer to have cyclical ups and downs, while some may prefer to have a lower but constant mean level. Moreover, individual differences in the actual physiological and psychological response to HC may be more important than differences in side effect preferences and should be a future research priority.

5.6 Limitations

In this study, we relied on self-report, which may mean that social desirability, measurement reactivity and recall error could affect our results. We hope we succeeded in minimising these issues by ensuring privacy and anonymity for participants, preventing access to past responses, asking specific closed-form questions daily, and statistically testing and adjusting for temporal trends (Barta, Tennen, & Litt, 2012). Some women in this sample may have used fertility tracking apps as a supplemental contraceptive method or simply out of interest. Such women may not have reported using these apps, because we only asked about contraception. Potentially, their increased awareness could change our results. An obvious improvement would be to also collect partner- and potentially peer-reports, although this might have negative consequences for the perceived anonymity of responses. To decrease measurement

reactivity and to test its effect, future studies could space out diary invitations over a longer period, for instance by sending them only on odd days or tailoring them to predicted (non-)fertile phases. Ideally, the schedule would be varied randomly by group (Barta et al., 2012).

We overestimated how conscientiously participants would fill out the diary. Hence, some women strung out the participation period over such a long time that menstruation could have occurred in an unobserved period, because women only reported menstrual onsets that occurred fewer than 7 days ago. Therefore, fertility was not estimable for ~6% of days (Table 3). Further, sending daily invitations via email presented a technical challenge. Due to delays in the sending process and spam filters some emails occasionally arrived a few hours late or not at all. We introduced text message reminders approximately halfway through the study and remedied this somewhat. These problems are presumably unrelated to outcomes and cycle position as *M_e5* shows, but still worth avoiding in the future. Because we required 35 complete daily reports before the study could end, some women never concluded our study, leading to 31% dropout in the follow-up survey. Future studies should use a fixed timespan for the diary, so that the follow-up takes place at the same time regardless of participation rate.

We only asked participants whether they had been intimate with someone other than their partner, but failed to systematically ask about the context and sex of the person. Free-text responses showed that several instances of reported extra-pair activity were not cheating with another man, but polyamorous or open relationships, affairs with women, or sex with the partner and another couple or a third person. All of these have

dubious relevance to the research question about adaptive benefits of extra-pair infidelity. We also did not collect data on single women, preventing us from discriminating between an increased propensity for flings in general versus extra-pair infidelity.

The generalizability of change for our outcome scales was sometimes zero and in other cases suboptimal. Previous research, from which we derived our scales, may have suffered the same problem, but did not conduct the appropriate psychometric analyses to find out. We think menstrual cycle research should learn from work on psychometrics and measurement in personality development research (Shrout & Lane, 2012). Mirroring the old person-situation debate (Kenrick & Funder, 1988), the evolutionary literature now debates the relative importance of between and within person variation (Havlíček, Cobey, Barrett, Klapilová, & Roberts, 2015; Jones, Hahn, Fisher, Wang, Kandrik, Han, Lee, et al., 2017; Zietsch, Lee, et al., 2015). However, without improving the methodology and psychometrics used to study within-person variations the debate will not be resolved (Roberts & Caspi, 2001; Shrout & Lane, 2012). Future work should also differentiate sexual activity more than we did here, including not just sexual intercourse and other sexual activity with the partner, but also masturbation and nonsexual intimacy.

Our sample was a convenience sample. Although it included a broad range of women, many (73%) were students, most (87%) had no children, few (12%) were married and all spoke German. Generalizability to older and higher-fertility populations, especially from settings that are not western, educated, industrialised, rich and

democratic (Henrich et al., 2010) may thus be limited. Although we assume universal hormonal changes drive our effects, hormonal levels might differ substantially for women who do not cycle regularly, for instance because they have recently been pregnant or breastfeeding, or because they have worse nutritional status.

Lastly, although we conducted a large number of robustness checks, we fell short of doing a full multiverse analysis or specification curve in which all possible ways to analyse the data are reported (Simonsohn, Simmons, & Nelson, 2015; Steegen, Tuerlinckx, Gelman, & Vanpaemel, 2016). We decided not to do this, because we believe many of our data-analytic decisions are justified properly, and multiverse analyses are most useful if no procedure was preregistered. Hence, our goal here was rather to show the effect of various approaches on the associations, as a guide to interpreting previous work as well as ours.

5.7 Suggestions for planning future and reading past cycle studies

The two most interesting takeaways from our researcher degrees of freedom simulations (see 4.1.2) might be that a) optional stopping and outcome switching had worse impacts than random covariates or switching between narrow, broad, and continuous fertile window estimates, and that b) false positives were acceptably rare (less than 5% in most conditions) if one simply applies a significance threshold of .01. The latter result only holds if researchers behaved as simulated and really stopped at p

< .05 (Nelson, Simmons, & Simonsohn, 2016), but might provide a useful guide to reading the older, non-preregistered literature.

Although it is difficult to compute an equivalent of Cohen's d for multilevel models, our comparable effect size estimates ranged from 0.12 to 0.43. Some were hence only a quarter of the smallest effect size (0.4) considered in (Gangestad et al., 2016). Future research would improve their odds of detecting an effect by improving the reliability of outcomes, predictors, collecting data on more women, more days, or ideally by doing all of this together. Empirically, not a single effect reported here would have been detected if we had collected only the first diary day for each woman in a between-subject analysis. Neither would effects have been detected using only two days per woman in a high vs. low fertility repeated measures design, a common design of previous studies, even though we collected ten times as many women as the average previous study. Whether the fertility predictor was formed based on forward- or backward-counting, narrow, broad, or continuous fertile phases seemed to make less of a difference (Figure 3), except that predictors using more data are preferable and that (pre-)menstruation should be adjusted for.

To fully understand the accompanying cyclical changes going along with ovulation, researchers should collect data over many days per woman (Haselton & Gangestad, 2006; Roney & Simmons, 2013). We have released our survey software and study code to make it easier to conduct online diary studies like this one (Arslan & Tata, 2016). Although online diary studies using counting methods will probably always be most cost-efficient, hormonal assays, especially repeated ones (Jones, Hahn, Fisher, Wang,

Kandrik, Han, Lee, et al., 2017; Roney & Simmons, 2013), are needed as converging evidence and to directly test hormonal mediators. They can compensate smaller affordable sample sizes through the greater validity of their predictors. Potentially the two designs can be fruitfully merged (Roney & Simmons, 2013), so that patchy hormonal assays are used to impute more valid predictors in a larger diary dataset.

Our study was preregistered. We consider this a good way in which researchers can protect themselves from unintentionally generating false positives through selection and publication bias. To combat publication bias even more effectively researchers might also try the *Registered Report* format (Nosek & Lakens, 2014) that is offered by an increasing number of journals. However, preregistration requires that the analysis procedure and ideally the data collection and cleaning procedure are set before data collection. Standard operating procedures are one way to simplify this process and to make it easier for researchers to sufficiently specify their plans, especially in areas in which they have not worked before. We have released our study materials, our data cleaning code and our code for computing menstrual onsets as potential groundwork for a standard operating procedure. We welcome improvements to this procedure that can be publicly shared. We also call for further work to improve inferences of conception probability, tailored to individual cycle lengths, regularities, and potential demographic factors. Although hormonal and sonographic measures of ovulation and fertility will retain their superiority, the use of day counting methods is justified by a much larger amount of data that can be and have already been collected efficiently (e.g. in the numerous cycle tracking apps).

Although we fail to conceive any reasonable non-hormonal or non-causal alternative explanations for the changes we observe mid-cycle, these inferences could be strengthened through a true randomised control group. We suggest that future hormonal contraceptive RCTs collect diary data across several full cycles in both experimental groups. By doing so we would be able to assess differences caused by contraceptive pills across the whole cycle, not just in e.g. the luteal phase (Zethraeus et al., 2016), and we would have sufficiently reliable within-subject data to examine heterogeneity in the response to contraceptive pills. Future studies should also attempt to better test whether awareness of being in the fertile window drives any effects.

5.8 Conclusions

In a high-powered, within-subject diary study, we were able to replicate main effects of ovulatory increases in self-perceived desirability, as well as extra-pair and in-pair sexual desire and behaviour. We failed to replicate reported ovulatory increases in partner mate retention behaviour and clothing style, and found only ambiguous support for increases in sexual behaviour. In contrast to previous reports, we found no evidence that sexual desire shifted more strongly among women who deemed their partner less sexually attractive. Previous studies had inadequate power, sometimes used suboptimal between-subject designs, and none were preregistered. Hence, several previous reports of ovulatory shifts and moderators thereof may have been false positives. We do not rule out changes along other dimensions or moderators that we and others have not tested, but large, well-designed, preregistered studies will be necessary to show these credibly. Alternatively, our data are consistent with the theory that ovulatory increases

reflect generalized changes in sexual motivation, serving the adaptive function to avoid costs associated with sex when it will not lead to conception (Roney & Simmons, 2013). Further work should directly test competing theories against each other.

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