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Sex Drive: Theoretical Conceptualization and Meta-Analytic Review of Gender Differences

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Few spheres in life are as universally relevant for (almost) all individuals past puberty as sexuality. One important aspect of sexuality concerns individuals' sex drive—their dispositional sexual motivation. A vigorous scientific (and popular) debate revolves around the question of whether or not there is a gender difference in sex drive. Several theories predict a higher sex drive in men compared to women, with some theories attributing this difference to biased responding rather than true differences. Currently, there is little consensus on how to conceptualize sex drive, nor does a quantitative summary of the literature exist. In this article, we present a theory-driven conceptualization of sex drive as the density distribution of state sex drive, where state sex drive is defined as momentary sexual motivation that manifests in sexual cognition, affect, and behavior. We conduct a comprehensive meta-analysis of gender differences in sex drive based on 211 studies, 856 effect sizes, and 621,463 persons. The meta-analysis revealed a stronger sex drive in men compared to women, with a medium-to-large effect size, $g = 0.69$, 95% CI [0.58, 0.81]. Men more often think and fantasize about sex, more often experience sexual affect like desire, and more often engage in masturbation than women. Adjustment for biased responding reduced the gender difference ($g = 0.54$). Moderation analyses suggest that the effect is robust and largely invariant to contextual factors. There was no evidence of publication bias. The discussion focuses on validity considerations, limitations, and implications for psychological theory and people's everyday lives.

Public Significance Statement

This article explains sex drive from a scientific, psychological perspective—operationalized as sexual thoughts, desire, and masturbation frequency—and provides support using a meta-analytic review that men have a stronger sex drive than women. Some but not all of these gender differences may be caused by men overreporting and/or women underreporting their sex drive. These findings advance our understanding of sexual dynamics in interpersonal relationships and society at large.

Keywords: sexual motivation, individual differences, sexual thoughts, sexual desire, masturbation

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Past puberty, few spheres in human life are as universally relevant as sexuality. Sexual experiences can bring about intense emotions, positive and negative alike. They can create intense intimacy, feelings of lust and love, but also sadness and anger. Sex can deepen or destroy romantic relationships. In short, sexuality impacts people's everyday lives in myriad ways by influencing their thinking, feeling, and behavior.

A crucial aspect of human sexuality concerns individuals' sex drive. People arguably differ in their dispositional sexual motivation. As a result, scientific research and party conversations alike have long been drawn to the question of whether there is a gender difference in human sex drive.[1] In fact, this question has spurred a vigorous debate, with some authors claiming that there is compelling evidence that men have a stronger sex drive than women (for a review, see Baumeister et al., 2001). Others doubt the validity of this

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[1] We note that the term "gender" typically refers to whether people self-identify as men, women, or other, whereas "sex" typically refers to biological sex assigned at birth. Using these terms with precision is especially important when making a distinction between acculturative versus biological factors. For sex drive, we presume that both biological and cultural influences may be at play. Thus, both the term "sex differences" and the term "gender differences" may apply. For simplicity, we refer to differences between men and women as "gender differences," although this is not meant to imply that any alleged differences are solely caused by cultural influences.

evidence and assume that empirical differences are due to various biasing factors. For example, some have argued that empirically observed gender differences in sex drive may be attributable to factors that are not specific to sex drive, such as socially desirable self-presentation tendencies or a problematic choice of sex drive indicators (Conley et al., 2011; Dawson & Chivers, 2014).

The current research seeks to make two contributions. First, we provide a conceptualization of sex drive grounded in psychological theorizing that has clear implications for what constitutes an adequate indicator of sex drive and what does not. Second, based on this theoretical conceptualization, we conduct a comprehensive meta-analytic review of gender differences in sex drive. Our aims with this meta-analysis are threefold: First, we seek to quantify the overall effect: Do men and women differ in sex drive, and if so, in which direction, and how strongly? Second, we address critical validity concerns. Could potential gender differences in sex drive be the result of biased responding or methodological artifacts (e.g., how participants were compensated or whether the study was advertised as a study on sexuality)? If bias is present, to what degree can it account for potential gender differences? Finally, we investigate the issue of generality. If gender differences exist, do they vary depending on other individual characteristics, such as age, sexual orientation, or relationship status?

The question of whether there is a gender difference in sex drive has substantial practical and theoretical implications. In monogamous relationships, differences in sex drive within a couple may manifest in sexual desire discrepancy, leading to an interdependence dilemma in which both partners may feel that they need to leave their comfort zone. Partners with a lower sex drive may engage in sex more often than they would like; partners with a stronger sex drive may end up having sex less often than they would like. As a result, both partners may question their compatibility with their partner in a potentially key aspect of their relationship. Not all couples manage to successfully resolve this interdependence dilemma and avoid its negative consequences (Day et al., 2015). Although research on differences in sex drive and resultant desire discrepancies in intimate relationships is still scant, preliminary findings suggest that it may lead to negative consequences such as increased conflict, reduced relationship satisfaction, and lower relationship stability (Mark, 2015). Given that many relationships are heterosexual, knowledge about a potential gender difference in sex drive is of great concern.

If an average gender difference in sex drive large enough for people to discern in their everyday lives were to exist, societies would be likely to pick up this difference and incorporate it into their typical gender roles, which are socially shared (Eagly & Wood, 1999). Also, people may form beliefs about characteristics that they perceive to typically go along with a stronger versus weaker sex drive and make corresponding inferences about members of each gender. In this way, gender roles may shape expectations about gender-typical communication, interaction patterns, and behavior in interpersonal relationships, thereby potentially reinforcing and bolstering the gender difference beyond factually existing differences.

Aside from these and other practical implications, on which we elaborate in the discussion, the question of whether there is a true gender difference in sex drive also has pronounced theoretical implications: Whole theories are built on the assumption that such a gender difference exists. For example, a fundamental premise of sexual economics theory (SET) is that men have a stronger sex drive compared to women (Baumeister & Vohs, 2004). In an analogy to an economic market, men's stronger sex drive places them in the societal role of "buyers," who invest resources to acquire sex from women, who take on the role of "sellers" in this theory. If the premise that men have a stronger sex drive than women have does not hold, the theory no longer has ground to stand on and would need to be abandoned or considerably revised.

Previous Empirical Evidence for Gender Differences in Sex Drive

Twenty years ago, a widely received narrative review addressed the question of whether men and women differ in their sex drive (Baumeister et al., 2001). The authors narratively reviewed a large number of outcomes indicative of sex drive, including thoughts and fantasies, spontaneous arousal, desired frequency of sex, the desired number of sex partners, masturbation, willingness to forego sex, initiating versus refusing sex, enjoyment of various sexual practices, sacrificing resources to get sex, favorable attitudes toward sex, the prevalence of low sexual desire, and self-rated sex drive. Men showed evidence of stronger sexual motivation on each of these indicators. The authors concluded that men have a stronger sex drive than women, a pivotal finding that has since been incorporated into theorizing in related fields (e.g., de Ridder et al., 2012; Schmitt, 2005).

The review by Baumeister et al. (2001) strongly focuses on directional evidence to shed light on the question of whether one gender has a stronger sex drive than the other. With the present work, we aim to reconsider these findings quantitatively and extend them by addressing some questions left open by the review's narrative nature, especially regarding the extent of gender differences as well as moderating factors that may influence when the gender difference is more versus less pronounced. In addition, 20 years have passed since the publication of this review. Cultural changes in some societies may have altered average levels of sex drive, how women and men typically respond to questions indicative of sex drive, or both.

Despite a host of research providing directional evidence on possible gender differences in sex drive, studies that explicitly seek to quantify this difference are relatively rare. For instance, Ostovich and Sabini (2004) proposed a four-item scale to assess sex drive, asking participants how often they experienced sexual desire, orgasmed, masturbated, and how they would compare their sex drive to the average person of the same age and gender. They examined gender differences and found a large effect, according to common conventions (Cohen, 1988), indicating a stronger sex drive in men than in women ($d = 1.20$). In three studies (total $N > 3{,}600$), Lippa (2006) found gender differences in the same direction ranging from $d = 0.58$ to $d = 0.84$. In two of these studies, participants responded to a single item ("I have a strong sex drive"), while a third study included four additional items ("I frequently think about sex"; "It doesn't take much to get me sexually excited"; "I think about sex almost every day"; "Sexual pleasure is the most intense pleasure a person can have"). The largest single published study ($N > 200{,}000$ across 53 nations; Lippa, 2009) asked two questions to assess sex drive ("I have a strong sex drive;" "It doesn't take much to get me sexually excited") and found an average gender difference of $d = 0.62$.

Taken together, then, evidence from these and other studies seeking to quantify a potential gender difference in sex drive

suggests a moderate-to-large gender difference, with men having a stronger sex drive than women.

An important observation is that the previous review of the literature (Baumeister et al., 2001) included a diverse array of outcomes as indicators of sex drive. These outcomes cover a considerably broader theoretical scope than the rather focused attempts to directly assess sex drive in the just reviewed studies. At the same time, not all of these outcomes may be equally valid. Even the comparatively focused studies that sought to measure sex drive directly did so in quite different ways. Sometimes a scale comprising several items was used, sometimes only two items or even a single item. In some studies, cognitive aspects such as thoughts and fantasies about sex were focal, in others affective aspects such as sexual desire. These domain-specific estimates were occasionally accompanied by global self-ratings of sex drive strength or self-rated comparisons of one's own sex drive with that of other people. Both domain-specific estimates and global self-ratings left open what precisely was meant by the term "sex drive."

One reason for the large variability in employed indicators for sex drive may be that they were not derived from a coherent theoretical conceptualization of the construct. Instead, they appear to have been created based on face validity considerations. In each case, it is difficult to know why a particular indicator was (not) chosen. What seems missing from the literature is a coherent theoretical conceptualization of sex drive that also has clear implications for the question of which outcomes are best suited to indicate the strength of an individual's sex drive.

Theoretical Conceptualization: What Is Sex Drive?

Sex drive is an individual's intrinsic sexual motivation—the driving force to obtain sexual experiences and pleasure (Baumeister et al., 2001).[2] Although momentary sexual motivation, or state sex drive, clearly varies within persons over time, the present research concerns stable individual differences between persons. Some people are consistently more eager for sexual experiences than others. We are thus interested in sex drive as a trait, where traits are understood as (inter-)individual differences in tendencies to show relatively consistent patterns of thoughts, feelings, and behaviors (Johnson, 1997; McCrae & Costa, 2003; Roberts, 2009).[3] People high in trait sex drive think about sex more often (thoughts/cognition), desire sex more often (feelings/affect), and are sexually active more often (behavior) compared to people lower in trait sex drive.

The seeming conundrum between intraindividual state variability on the one hand, and temporal stability of interindividual trait differences on the other can be elegantly solved by understanding traits as "density distributions of states" (Fleeson, 2001, 2004). This perspective assumes that for any personality trait, a corresponding personality state exists with the same cognitive, affective, and behavioral content as the corresponding trait, thereby constituting the personality an individual manifests from moment to moment (Fleeson & Jayawickreme, 2015). States vary over time within an individual as situational cues interact with disposition, but they vary in consistent, predictable patterns. Specifically, the distribution they form over time is stable with regard to its central tendency and dispersion (Fleeson, 2001). Traits are thus dispositions, not absolute determinants. Someone high in trait extraversion, for instance, does not act in an extraverted way all the time, highlighting the influence of situational circumstances. However, over a longer period, an extraverted person will more often act in an extraverted way compared to someone less extraverted, and this interindividual difference will reliably emerge across several such extended periods. This illustrates the influence of the trait. In sum, this understanding of traits explains the temporal consistency of psychological patterns that vary between individuals, but also explicitly incorporates the notion of cross-situational variability within individuals (Fleeson, 2001; Fleeson & Jayawickreme, 2015; Johnson, 1997; Roberts, 2009).

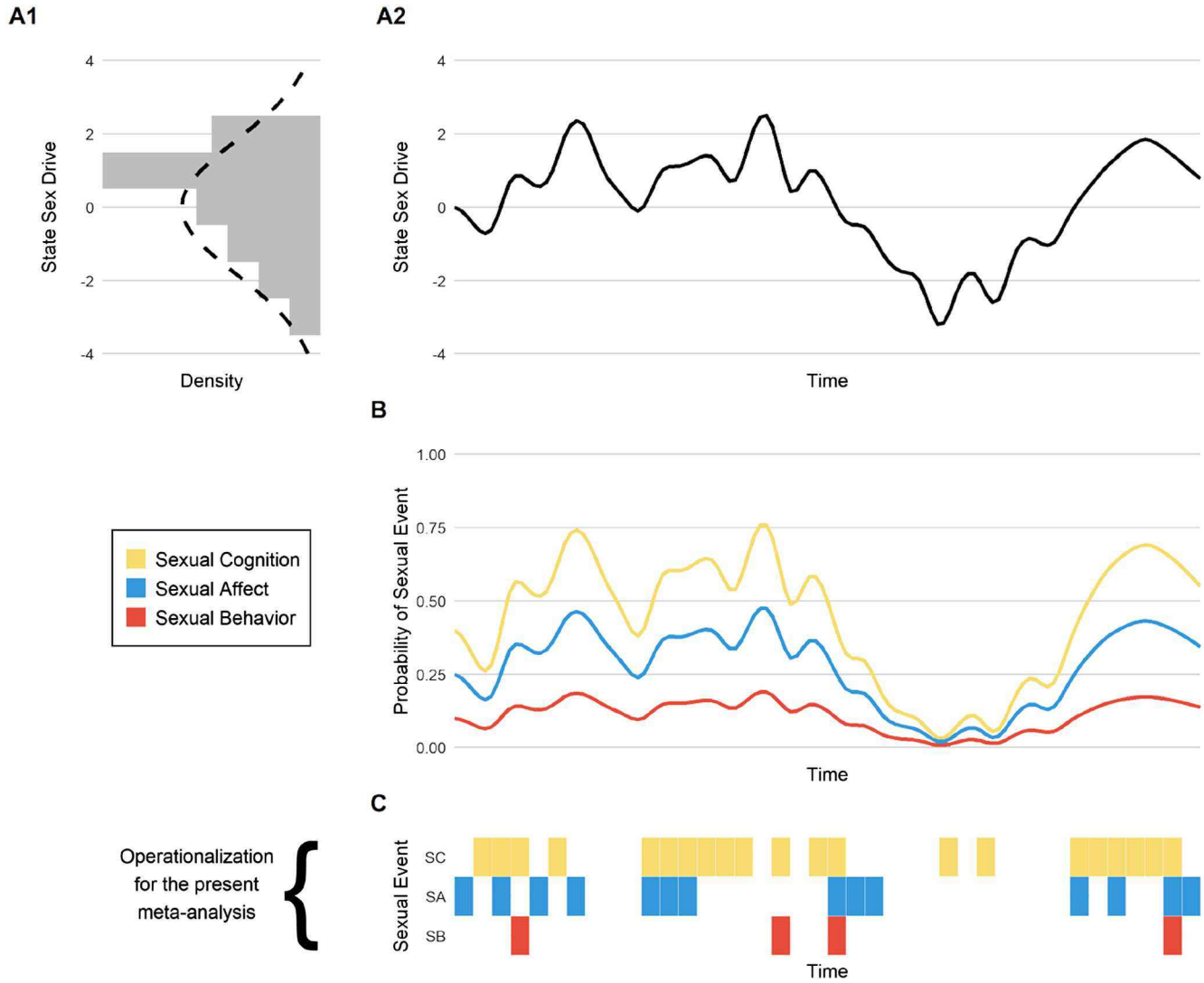
Based on this understanding, we can define (trait) sex drive more explicitly as the central tendency of the distribution of state sex drive, or momentary sexual motivation, across time and situations. Put more simply, a person's sex drive is their average sexual motivation over time. We conceptualize state sex drive as a latent concept that is manifested or reflected in how often people experience three kinds of events: sexual cognitions (e.g., thoughts, fantasies), sexual affect (e.g., desire to have sex), and sexual behavior (e.g., masturbation). The probability that a manifest sexual event occurs at a given point in time will depend on the level of state sex drive at that time (which in turn depends on the interplay between a person's trait sex drive and situational cues).

We illustrate this conceptualization of sex drive in Figure 1. Let us assume that state sex drive varies along an arbitrary scale, roughly between −4 and +4, where 0 represents the average population level. The data presented in Figure 1 have been randomly generated under our assumptions for a hypothetical person whose (trait) sex drive is 0, that is, exactly average. This is illustrated in Panel A1. The density distribution (dashed line) for our hypothetical individual centers directly on zero. Panel A2 illustrates how this person's state sex drive fluctuates over time, while still centering around zero. The "observed" distribution (gray histogram in Panel A1) will never perfectly match the expected distribution for a finite sample of observations, but the correspondence is evident. Panel C illustrates the occurrence of sexual events. Yellow rectangles illustrate that a sexual cognition occurred at a certain point in time, blue rectangles illustrate sexual affect, and red rectangles illustrate sexual behavior. The association of sexual events and state sex drive is visible: After about half the time (*x*-axis), there is a noticeable dip in state sex drive and correspondingly, fewer sexual events occur. The relationship

[2] When we talk about sex drive, we are referring to motivation for sexual experiences and sexual pleasure as an end in itself. The goal is sexual experience and pleasure, not other, potentially (un)related goals. There are many reasons why people may seek out sexual experiences besides the sexual experience itself (e.g., stress relief, procreation, emotional closeness to another person, see Meston & Buss, 2007). We are not referring to these instances, in which sex is a means to achieve other ends. Instead, our definition is confined to intrinsic sexual motivation where sexual experiences are an end in themselves.

[3] We use the terms "sex drive" and "sexual motivation" interchangeably. The term "sex drive" has been criticized as problematic by some theorists (Beach, 1956; Singer & Toates, 1987), especially the notion that an innate need for sex arises independently of external stimuli and builds up over time. In contrast to food, water, or sleep, deprivation of sex is not fatal or directly harmful. We acknowledge these shortcomings of the term. Our use of it is not intended to reflect biological drives akin to those for food, water, or sleep. The reason we retain the term sex drive is that it is widely used and understood in the literature and the general population. Although failure to satisfy one's sex drive is not harmful to an individual, it appears appropriate to say that people may have a drive to pursue certain (sexual) goals or activities.

Figure 1
Conceptualization of Sex Drive



Note. This figure visualizes random data for one hypothetical person generated under our theoretical conceptualization of sex drive. Panel A1 depicts a histogram of the observed density (gray rectangles) and the curve of the expected density (dashed line) of the distribution of state sex drive depicted in Panel A2. In Panel A2, the black line displays the fluctuation of state sex drive over time. Panel B depicts the probability of sexual events over time. Panel C depicts the occurrence of sexual events over time. A colored rectangle indicates that the respective sexual event occurred at a given point in time. The depicted occurrences are the result of random sampling according to the probabilities depicted in Panel B. In Panels B and C, yellow denotes sexual cognition (SC), blue denotes sexual affect (SA), and red denotes sexual behavior (SB). Gender differences in this quantity will be meta-analyzed in the present study. See the online article for the color version of this figure.

between state sex drive and the frequency of sexual events, or Panels A1 and C, is depicted in Panel B. The probabilities for the occurrence of sexual events displayed in Panel B are a direct function of the corresponding level of state sex drive at that time.[4] When state sex drive is relatively low, the probability of sexual events is also low, and fewer events occur. The reverse is true when state sex drive is high. In line with recent calls to incorporate more formal modeling into psychology (Guest & Martin, 2020), we enclosed a preliminary mathematical definition of our conceptualization of sex drive in the Supplemental Materials.

[4] For illustration purposes, we assumed that probabilities for sexual behavior are generally lower than for cognition and affect, and that probabilities for sexual affect are generally lower than for sexual cognition. This, as well as the exact nature of the relationship between state sex drive and the probability of sexual events, currently remains subject of speculation, albeit, in our view, useful speculation. While this is beyond the scope of the present work, future research could seek to devise ways to test and parametrize this model. For example, a recent longitudinal study found that sexual cognition occurs more frequently than sexual affect, and that sexual affect occurs more frequently than sexual behavior, providing tentative evidence for the differential average probabilities we assumed (Weber et al., 2022a).

To summarize what we have described in the previous sections and illustrated in Figure 1, we conceptualize sex drive as average sexual motivation over time, formally described by density distributions of state sex drive. State sex drive, or momentary sexual motivation, manifests in sexual events, specifically sexual cognition, affect, and behavior, that occur more or less frequently, depending on the level of state sex drive. According to this definition, persons high in trait sex drive experience events characterized by sexual thoughts and fantasies, feelings such as desire, and behaviors (or any combination thereof) more often in their daily lives compared to people low in trait sex drive. In other words, people high in trait sex drive think about sex more often (cognition), feel a desire for sexual pleasure more often (affect), and are more often sexually active (behavior) compared to people lower in trait sex drive. This notwithstanding, even someone with a strong sex drive will not constantly think about sex, desire to have sex, or engage in sexual activity. Sex drive strongly varies within persons over time due to stress, time of day, availability of a partner, presence of other persons, conversations, media depictions, sexual satisfaction, and many other factors. Thus, while often influenced by sex drive, sexual experience and behavior can also be driven by other factors. For instance, the relative role of sex drive compared to other contextual variables may be attenuated in romantic relationships, where sexual manifestations such as sexual desire may become a function of each partner's characteristics and characteristics of the relationship itself (e.g., Impett et al., 2008; Regan, 2000). Someone high in sex drive will not always have consistently strong desire for their partner(s), and conversely, a person with a below-average sex drive is not generally incapable of developing sexual desire in a relationship.

Note that our definition of trait sex drive says nothing about the origin of these individual differences and potential gender differences in sex drive. Sex drive (and gender differences in sex drive) may be the result of a complex interplay of various cultural and biological influences. Social roles (Eagly & Wood, 1999) and social learning experiences (Bussey & Bandura, 1999) may contribute to these differences alongside genetic influences. The presence of genetic influences would in turn suggest that sex drive may also be heritable to some extent, similar to other traits (Polderman et al., 2015). We are agnostic toward the respective contributions of these and other possible origins of (gender differences in) trait sex drive. While discussions of the etiology of gender differences in sex drive, that is, how they are shaped by biological and/or social factors, can be found elsewhere (Lippa, 2009), the present work is concerned with the phenomenology of the trait. We neither intend to nor are we able to elucidate the underlying causes of sex drive variability across persons in the present analysis.

Sex Drive Versus Sexual Desire

We understand sexual desire as an emotion, a feeling of wanting sex or sexual pleasure. Sexual desire takes a primary role among the affective manifestations of sex drive. Some scholars have adopted broader definitions that emphasize motivational aspects. Levine (2003) defines sexual desire as "the sum of the forces that lean us toward and away from sexual behavior" (p. 280). Spector et al. (1996) proposed "interest in sexual activity" (p. 178) as a working definition of sexual desire. Brezsnyak and Whisman (2004) define sexual desire as "a motivation to seek out, initiate, or respond to sexual stimulation or the pleasurable anticipation of such activities in the future" (p. 199). Sexual desire according to these views seems closely related to our understanding of sex drive as intrinsic motivation for sexual experiences and pleasure. The definition of Diamond (2004), "a need or drive to seek out sexual objects or to engage in sexual activities" (p. 116), includes an explicit reference to "drive." Notably, these definitions do not define sexual desire as either a trait or a state, yet previous research has suggested that this differentiation is important in regard to gender differences (Dawson & Chivers, 2014). However, our conceptualization of sex drive (Figure 1) which rests on established frameworks specifying the relation between states and traits (e.g., Fleeson, 2001; Roberts, 2009) proposes that this is not an either-or-question, but that trait sex drive manifests in patterns of states of sex drive that are variable across time, but consistent when considering longer periods of time. Our view classifies sexual desire into the affective facet of the triad of sexual cognition, affect, and behavior, and reserves the terms sex drive/sexual motivation for the superordinate construct.

Indicators of Sex Drive

Previous work on sex drive has used a large number of indicators. Without a coherent and theoretically grounded conceptualization of the construct, it is difficult to decide what may or may not qualify as a suitable indicator. The psychological conceptualization of trait sex drive put forward here has clear implications for what does and does not constitute a suitable indicator of sex drive.

According to the present conceptualization, the frequency of sexual cognitions (e.g., thoughts, fantasies, daydreams), sexual feelings (e.g., desire, craving, lust), and sexual behavior (e.g., masturbation, self-stimulation) constitute valid indicators of sex drive. These indicators can thus be directly derived from our sex drive conceptualization. All of them have been used as indicators of sex drive before (Baumeister et al., 2001). Throughout the article, we collectively refer to these three sex drive indicators, that is, the triad of sexual cognition, affect, and behavior, as "facets" of sex drive.

For several reasons, we confine the behavioral facet to solitary sexual activities (i.e., masturbation, self-stimulation) and do not include sex with a partner. First, sex with a partner depends not only on a person's intrinsic sexual motivation but also strongly depends on other influences such as the availability of a partner, their sexual motivation, or interpersonal dynamics between the partners (e.g., desire to feel close and connected to the partner, desire to please the partner). Second, by simple arithmetic, there cannot be a true, objective gender difference in this variable for heterosexual persons on the population level. Every time a woman has sex, a man also has sex, and vice versa. Any appreciable reported difference is likely due to (motivated or otherwise) biased responding. Thus, in line with the present conceptualization, only solitary events can be a meaningful behavioral indicator of gender differences in intrinsic sexual motivation.

The conceptualization of sex drive directly suggests the frequency of sexual cognition, affect, and behavior as suitable indicators of sex drive because they are measurable manifestations of latent, momentary sexual motivation. We refer to these three indicators as "manifestations of sex drive" throughout the article. Our conceptualization also suggests another group of indicators, namely, measures that may directly reflect the latent level. In our survey of the literature, we identified two sets of questions that could serve this

function: self-rated sex drive (e.g., "I have a strong sex drive") and intensity of sexual affect (e.g., "My desire for sex is strong"). Self-rated sex drive may indicate latent sex drive at the highest level of abstraction (Panels A1 and A2 in Figure 1). Intensity of sexual affect may indicate latent sex drive at a more intermediate level as a latent state that is already somewhat differentiated toward sexual affect. We refer to these two indicators collectively as "indicators of latent sex drive" in this study. In our analyses, we will prioritize the sex drive manifestations over the indicators of latent sex drive because the former are directly suggested by our conceptualization (Figure 1). For the indicators of latent sex drive, more detailed psychometric analyses may be needed before concluding that these measures do indeed reflect latent variables according to our conceptualization.

The present conceptualization of sex drive also identifies concepts that have been used as indicators of sex drive in the past, but do not align with our view. For example, favorable attitudes toward sex may or may not be influenced by sex drive, but they are evaluations of sex that are likely influenced by all sorts of cultural and social influences. The frequency of sexual cognitions, affect, and behavior constitute manifestations of sex drive, not individuals' subjective evaluation of sexuality. Second, previous research has utilized the desired number of sex partners as an indicator of sex drive, with higher numbers of desired partners indicating a stronger sex drive. However, in addition to a true impact of latent sex drive, the (reported) number of desired partners may also be influenced by self-verification motives, a desire for social status, or again cultural and social influences. What is more, a person who frequently thinks about and desires sex with only one partner would unequivocally be considered someone with a high sex drive according to the present conceptualization. Desiring sex with *many different* partners is not a defining aspect of a high sex drive according to this understanding (although empirically the two may be correlated). Third, and in a similar vein, enjoying a large variety of sexual practices may or may not be influenced by a strong sex drive. Someone who frequently fantasizes about and desires sex always in the same way would clearly have a strong sex drive according to the present conceptualization. A large variety of sexual practices is not a defining element of the current conceptualization and from this perspective unsuited to serve as an indicator of sex drive.

There are also other extant indicators that seem reasonable downstream consequences of sex drive, but are not directly derivable from the current conceptualization and therefore not considered valid indicators (e.g., unwillingness to forego sex, sacrificing resources to get sex, subjective importance of sex). Finally, because the present meta-analysis is interested in sex drive as a psychological force to obtain sexual experiences and pleasure, we do not regard capacity for physiological reactions as indicative of sex drive (e.g., capacity for sexual arousal or orgasm). Note that we do not exclude the possibility that the constructs discussed in this section may in some way be related to or influenced by sex drive, but rather maintain that they are of subordinate importance as indicators of the construct compared to the frequency of sexual cognition, affect, and behavior.

Theoretical Approaches

This section reviews theoretical approaches relevant to (gender differences in) sex drive, namely sexual strategies theory (Buss & Schmitt, 1993), the sexual double standards hypothesis (Crawford & Popp, 2003), social role theory (Eagly & Wood, 1999), social learning theory (Bussey & Bandura, 1999), the gender similarity hypothesis (Hyde, 2005), and SET (Baumeister & Vohs, 2004).

Sexual Strategies Theory

Rooted in the larger evolutionary psychology framework, sexual strategies theory proposes that humans have evolved a variety of short-term and long-term strategies for passing their genes on to the next generation (Buss, 1998; Buss & Schmitt, 1993, 2019). According to the theory, these strategies differ between men and women, for example, due to the minimum parental investment both sexes have to make to produce a child. The number of offspring women can have is more limited compared to men, and women incur higher biological costs in terms of energy needs, risks during pregnancy, and effort in infant care. In short-term mating contexts, women should therefore be more selective while men should, on average, seek to engage in more casual sexual activities. In long-term mating contexts, men's and women's preferences will be largely similar, and both will be selective. However, according to the theory, women will prefer men who possess resources and/or have qualities that make the future acquisition of resources more likely. Men, by contrast, will be particularly attracted to cues of youth and health in women, both of which are linked to fertility (Buss, 2012).

Sexual strategies theory does not speak directly about gender differences in sex drive. However, it makes some predictions that indicate that the theory assumes a stronger sex drive in men compared to women. For example, the theory predicts that men will be particularly upset when their female partners decline or delay opportunities to have sex, or desire sex less frequently than themselves (Buss, 1998). This implies that, on average, men want sex more often than women do. In addition, some authors have argued that evolution may have favored a weaker sex drive in women compared to men. The higher the sex drive, the more likely a woman will become pregnant, which is associated with higher parental investment costs compared to men (Baumeister et al., 2001). A key tenet of sexual strategies theory is that in evolutionary history, it was likely adaptive for women to withhold sex under certain circumstances. A high sex drive would interfere with this tendency. This tentatively suggests that a higher sex drive in men is more plausible according to sexual strategies theory, and this gender difference would reflect genuine differences on the construct level rather than merely differences on the measurement level.

Sexual Double Standards Hypothesis

The sexual double standards hypothesis (Crawford & Popp, 2003) suggests that men are viewed positively and socially rewarded for sexually permissive behaviors, whereas women are viewed negatively and socially punished for the same behaviors (for a meta-analysis, see Endendijk et al., 2020). Awareness of sexual double standards may lead men to exaggerate their reports of sexual permissiveness and women to underreport their sexual permissiveness. This suggests that some gender differences in reported sex drive may emerge on the measurement level due to biased responding in line with gender roles that in fact may be smaller or even nonexistent on the construct level.

In line with this idea, some studies have found reduced or erased gender differences in reported sexual experiences when participants

were connected to a fake lie detector (encouraging truthful responding) compared to conditions in which participants were led to believe that their responses might be seen by a peer or in which participants were assured anonymity (Alexander & Fisher, 2003). Further indirect evidence comes from findings that men report having had more opposite-sex sexual partners than women (e.g., Mitchell et al., 2019). In heterosexual populations, substantive differences are impossible because every time a woman has sex, a man also has sex, and vice versa.

Social Learning Theory

Social learning theory (Bandura, 1986; Bussey & Bandura, 1999, 2004) suggests that behaviors that are rewarded are more likely to be repeated and behaviors that are punished are less likely to be repeated. This is true both for one's own behaviors as well as behaviors an individual observes other people perform. Learning is particularly likely if the individual perceives similarity to or identifies with the acting person (e.g., because the person is powerful, successful, or admirable). According to this theory, gender differences emerge because boys and girls (a) observe different behaviors in men and women, and (b) observe that men and women are rewarded and punished for different behaviors. Boys and girls pick up these different standards for gender-appropriate behavior and learn to behave in accordance with these gender norms.

To the extent that boys and girls learn (in real life or through media) that men are rewarded and/or women are punished for behaviors indicative of a strong sex drive, they may learn to behave accordingly and adopt corresponding attitudes (see the sexual double standard, in the previous section). Thus, social learning theory makes clear predictions about openly expressed sexual attitudes and behaviors, which reflect genuine differences in this sex drive facet.[5] In other words, boys and girls may learn to *have* a "gender-appropriate" sexuality (indicating true differences on the construct level). At the same time, they may also learn to *express* their sexuality in a norm-conforming way (indicating differences on the measurement level, i.e., biased responding). Whether the theory predicts gender differences in sex drive in the sense of sexual cognition and affect, which are nonobservable for anyone other than the person themselves and arguably more difficult to control than overt behavior, is less clear.

Social Role Theory

Social role theory (also referred to as the biosocial model or sociocultural theory) focuses on social processes instead of evolutionary selection processes to explain gender differences in behavior (Eagly & Wood, 1999; Wood & Eagly, 2012). Specifically, social role theory acknowledges evolved physical differences between the genders such as size, strength, and the capacity to bear and nurse children. In many societies, these differences led to a division of occupational and family labor. Both men and women tended to take on those tasks that aligned with their unique physical properties (i.e., men more often engaged in physically demanding tasks such as hunting and warfare, women more often engaged in less physically demanding tasks requiring care for others). In any given society, people observe the activities typically carried out by each gender and infer that these genders possess not only the physical requirements but also the corresponding psychological characteristics that allow them to excel in gender-typical tasks. This is how gender stereotypes develop and in turn reinforce and perpetuate a gender-stereotypical division of labor, according to social role theory. Gender stereotypes cause gender-typical behaviors because (a) individuals tend to conform to their gender identities, and (b) other people encourage gender-typical behavior. Role-incongruent behavior is more likely to be societally sanctioned. Therefore, role-congruent behavior is perpetuated unless the anticipated benefits of gender-incongruent behavior outweigh the anticipated costs.

Evidence for social role theory comes from observations that typical gender differences in interests, preferences, and even personality characteristics such as agency have decreased over time as the division of labor has become increasingly less polarized and women's social role has shifted (Wood & Eagly, 2012). In a similar vein, a meta-analysis found that typical gender differences are smaller in countries in which gender equality is greater, including differences in the domain of sexuality such as masturbation (Petersen & Hyde, 2010), although recent studies found no support for a moderation of typical gender differences in mate preferences by gender equality (Walter et al., 2020; Zhang et al., 2019).

Social role theory assumes that socialization processes shape individuals' behaviors, beliefs, typical emotional responses, competencies, and personality traits to conform with societal stereotypes about how men and women are. Thus, to the extent that a society views a high sex drive as more typical and normative for men than women, social role theory predicts a higher sex drive in men than in women reflecting genuine differences on the construct level. These differences should be smaller in societies with greater gender equality. Also, gender differences in sex drive should have become smaller over time as gender equality has increased in many societies in recent decades. Differences on the measurement level due to self-presentation tendencies (i.e., biased responding) are also plausible under the assumptions of social role theory: People may adopt self-presentational tendencies on their own accord in order to conform with societal stereotypes, or they may learn them directly during the gendered socialization processes.

Gender Similarities Hypothesis

The gender similarities hypothesis (Hyde, 2005, 2014) is not exactly a theory, but rather a set of observations based on several meta-analyses of gender differences in psychological variables. The hypothesis states that gender differences in most, but not all psychological variables are small or negligible (with "small" being defined as everything up to a meta-analytic effect size of Cohen's $d \leq 0.35$, considered a small-to-moderate effect size according to common conventions). Hence, contrary to many stereotypes and public portrayals, women and men may not be vastly different in many spheres (Hyde, 2014). However, there are exceptions to this general rule. Hyde (2005) reports nontrivial gender differences in physical aggression, cognitive variables such as mental rotation and spatial perception (men score higher), and indirect aggression and some language or verbal skills (women score higher). Relevant to the present purposes, men report masturbating and watching

[5] In line with social role theory (Wood & Eagly, 2012), smaller gender differences in sexual attitudes and behavior should occur in societies where such gender-specific rewards and punishments occur less (e.g., in countries with greater gender equality).

pornography more often than women (*d*s > 0.5, Petersen & Hyde, 2010). Thus, the gender similarities hypothesis, based on previous meta-analytical observations, suggests a higher sex drive in men compared to women with respect to the behavioral facet of the construct. This difference is assumed on the construct rather than purely on the measurement level, thus reflecting genuine gender differences. The hypothesis makes no direct predictions with respect to the cognitive and affective facets.

Sexual Economics Theory

The SET (Baumeister et al., 2017; Baumeister & Vohs, 2004) posits that sex in heterosexual couples is negotiated in an economic marketplace. In this market exchange, women (the "sellers") give sex and, in return, receive sex by men (the "buyers") plus a negotiable amount of nonsexual resources because female sex is—according to the theory—inherently more valuable than male sex. The theory assumes a real gender difference in sex drive on the construct level, not merely on the measurement level. Gender differences in sex drive are, therefore, a fundamental premise rather than a prediction of SET, and considerable theoretical revision would be needed should it turn out that there is no such gender difference. SET does, however, provide prediction and explanation for biased responding. According to the principles of economic exchange in sexuality posited by the theory, female sex is at risk of diminishing in value when distributed freely (or appearing so); hence, women should be motivated to underreport sexual interest and activity. Men, by contrast, should be motivated to exaggerate reports of past sexual activity, since these reflect that they can exchange ample resources to obtain sex. For men's sexual interests, predictions are somewhat less clear. To some degree, interest in a resource may also signal an ability to obtain it, yet this would run counter to the age-old principle of hiding one's true interest in a negotiation. In sum, SET predicts some degree of response bias, since women should understate and men may (or may not) exaggerate.

Summary

Although some of the theoretical approaches reviewed here differ greatly in their core assumptions, they largely converge in the prediction that men have a stronger sex drive compared to women, at least on the measurement level. The distinction between the measurement level and the construct level underlines the importance of considering the possibility of bias as a result of systematically distorted responding. We note that the psychological mechanisms by which the social environment influences sex drive on the measurement or construct level are largely left open by the theories. This influence may manifest in conscious self-presentation tendencies—men overreport to gain social status and women underreport to avoid loss of social status (Jonason, 2008; Mitchell et al., 2019). However, the effect of social influence may also manifest in more subtle ways, for example, memory biases in the form of gender differences in estimating versus actually counting sexual events (Brown & Sinclair, 1999; Mitchell et al., 2019; Wiederman, 1997).

The Present Meta-Analysis

We conducted a preregistered, comprehensive meta-analysis of gender differences in sex drive. Based on the theoretical conceptualization of sex drive presented above, we primarily investigated gender differences in the frequency of sexual cognitions, affect, and behavior. We additionally included analyses on two potential indicators of latent sex drive: self-rated sex drive and intensity of sexual affect.

We put particular emphasis on the possibility that gender differences in sex drive may be (partly) due to biased responding. To this end, a separate meta-analysis examined gender differences in responses to "bias indicators"—that is, questions that logically cannot exhibit a substantive gender difference in heterosexual populations (e.g., a total number of sex partners). The meta-analytic gender difference in these indicators may be interpreted as an indicator of the extent to which gender differences in sex drive in the main analyses may have been driven by biased responding.

Finally, a series of moderation analyses examined the potential impact of a number of either theoretically derived or methodological factors on the magnitude of gender differences in sex drive. These analyses are suited to both test theoretical predictions and examine the stability of potential gender differences.

Method

Effect sizes were drawn from articles, provided by authors, or computed from raw primary study data (two-step individual participant data meta-analysis, see Riley et al., 2010). The unit of analysis is individual questionnaire items, with each effect size indicating the mean gender difference on one particular item.

Inclusion Criteria

Studies were eligible for inclusion in the meta-analysis if (a) they measured the frequency of sexual cognitions, sexual affect, or sexual behavior (sex drive manifestations), or sex drive self-ratings or intensity of sexual affect (indicators of latent sex drive); (b) the sample included male and female participants; (c) participants were at least 14 years of age; and (d) the sample included at least 20 male and female participants each. Studies were excluded if (a) the sample was drawn from a clinical population, an asexual population, or residents of long-term care facilities; (b) the study included an experimental manipulation or other intervention procedure; (c) it took place in the context of pregnancy or abortion; or (d) was published before 1997. The last criterion was imposed post hoc when it proved unrealistic to attain raw data that was necessary to compute item-level effect sizes for older research (see the Meta-Analysis of Item-Level Effect Sizes section, for details). Note, however, that data collection may have taken place before 1997.

Questionnaire items were eligible for inclusion if they reflected a sex drive manifestation or indicator of latent sex drive. In addition, we also included items that reflected a bias indicator (see Table 1). Note that we did not search for and did not include studies reporting only bias indicator items. Items were excluded if they framed sexuality in a negative or clinical way, or invoked perceived social norms (e.g., "I suffer from a lack of desire," "I think about sex more often than I should," "Masturbation sometimes gets in the way of my daily activities"). Another exclusion criterion on the item level was imposed post hoc: Items were not eligible for inclusion if participants were asked to report their sex drive compared to other individuals of their own sex. Such items are designed to eliminate gender differences between men and women—the very purpose of

Table 1
Overview of Included Items

Item	Abbreviation	Role	Example
Frequency of sexual cognitions	CF	Sex drive manifestation	During the last month, how often have you had sexual thoughts?
Frequency of sexual affect	AF	Sex drive manifestation	How frequently do you feel sexual desire?
Frequency of sexual behavior	BF	Sex drive manifestation	How many times did you masturbate during the last week?
Intensity of sexual affect	AI	Indicator of latent sex drive	My desire for sex with my partner is strong.
Self-rated sex drive	SRSD	Indicator of latent sex drive	I have a strong sex drive.
Sexual intercourse frequency	SIF	Indicator of potentially biased responding	On average, how many times per month do you and your partner have sex?
Total one-night stands	ONS	Indicator of potentially biased responding	With how many partners have you had intercourse on one and only one occasion?
Total sex partners	TSP	Indicator of potentially biased responding	With how many partners have you had intercourse in your lifetime?
Total sex partners in last year	TSPY	Indicator of potentially biased responding	How many people have you had sex within the last year?

Note. CF = cognition frequency; AF = affect frequency; BF = behavior frequency; AI = affect intensity; SRS = self-rated sex; SIF = sexual intercourse frequency; ONS = one-night stands; TSP = total sexual partners; SRSD = self-rated sex drive; TSPY = total sex partners in last year.

our meta-analysis—and hence are not suitable to address the present research question.

Literature Search

We used different strategies to identify relevant studies. First, we conducted an electronic literature search using Web of Science (Indices: Science Citation Index Expanded, Social Sciences Citation Index, and Emerging Sources Citation Index), EBSCO (Indices: PsycARTICLES, PsycINFO, and PSYNDEX), and PubMed (Indices: primarily MEDLINE). All databases were searched on the abstract/title level. Searches were done separately for sexual affect, cognition, and behavior. Search terms were constructed with the following pattern: one of "Term 1" AND one of "Term 2" NOT one of "Term 3." Term 1 established the link to sexuality (e.g., "erotic," "sexual"), Term 2 evoked the construct (e.g., "thought," "fantasy," "desire"), and Term 3 excluded clinical studies ("disorder," "dysfunction"). See Table S1 in the Supplemental Materials for the complete set of search terms. Second, we used Google Scholar to screen all publications that cited relevant psychometric inventories. Relevant inventories were identified by searching the *Handbook of Sexuality Related Measures* (T. D. Fisher, 2011) and through unstructured electronic searches (see Tables S2 and S3, for a list of the inventories). Third, we submitted calls for data through the mailing lists of the *International Academy of Sex Research*, the *European Association of Social Psychology*, and the *Society for Personality and Social Psychology*. Fourth, we asked all authors with whom we corresponded for unpublished data. The literature search was restricted to articles in English or German and was completed in 2018. Data collection was completed in 2019.

Screening and Requests for Data

All identified records were screened by one of the authors or a research assistant. First, studies were screened on the title level for eligibility. If studies were deemed potentially eligible, their abstracts were retrieved and screened again. During this screening on the abstract and title level, we adopted a maximally inclusive stance, such that records were only discarded when there was a clear indication that an inclusion criterion was violated or an exclusion criterion fulfilled. Next, full texts were obtained for all studies that passed the initial screening phase and screened again for suitability. For technical reasons, duplicates between different searches were not removed prior to screening. The full results of the search procedure are summarized in the flowchart depicted in Figure 2.

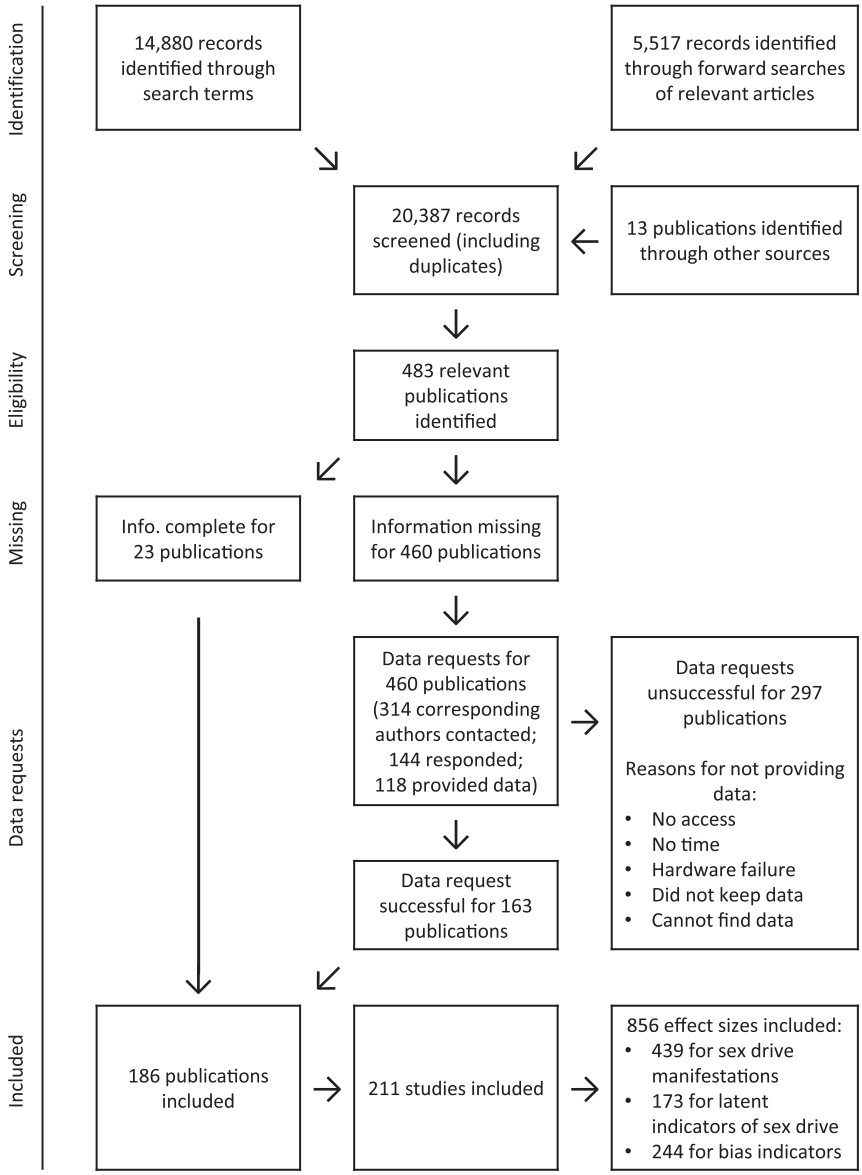
During the full-text screening, we checked if studies reported sufficient item-level statistics to compute effect sizes. If this was not the case, the study's corresponding author was contacted per email with a request for data. Each email also included a request for unpublished data on the subject. A reminder was sent after 2 weeks.

Effect Size Computation

We computed Hedges' g for all outcomes (Hedges, 1981). This effect size indicates the average gender differences in the sample in the metric of the pooled standard deviation. Positive values for g indicate higher values in men. For each relevant outcome, means, standard deviations, and sample sizes for men and women were retrieved from the report or computed from the raw data. Hedges' g was then computed from these summary statistics. If means or standard deviations were missing, Hedges' g was computed from p values, t values, and degrees of freedom for t tests comparing the groups. When measurements were taken multiple times, all time points were averaged prior to effect size computation. For raw data, all values that deviated more than 3.5 *SD* from the mean were classified as outliers and removed.[6] To put findings into perspective, we additionally report a range of natural language interpretations that are more easily interpreted than standardized mean effect sizes (Mastrich & Hernandez, 2021). First, Cohen's U_3, a measure of nonoverlap, indicates which percentage of Population A is surpassed by the upper half of Population B (Cohen, 1988). Second, the overlapping coefficient OVL indicates the overlap between two distributions (Reiser & Faraggi, 1999). Third, the common-

[6] Robustness analyses showed that results did not vary when a smaller (2.5 *SD*) or larger threshold (4.5 *SD*) was chosen.

Figure 2
Flow of Data Into the Research Synthesis



Note. Flowchart of the literature search and study coding. Note that some corresponding authors contributed to more than one publication. Of the 20,387 publications identified during the identification stage, 19,904 did not meet the inclusion criteria (reported at the beginning of the Method section) as became evident either when screening the abstract or the full text.

language effect size CL indicates the probability that an observation drawn at random from Population B surpasses an observation drawn at random from Population A (McGraw & Wong, 1992; Ruscio, 2008).

These effect size statistics are computed as follows:

$$U_3 = \Phi(g), \tag{1}$$

$$\text{OVL} = 2\Phi\left(\frac{-|g|}{2}\right), \tag{2}$$

$$\text{CL} = \Phi\left(\frac{g}{\sqrt{2}}\right), \tag{3}$$

where Φ is the cumulative distribution function of the standard normal distribution.

Meta-Analysis of Item-Level Effect Sizes

A body of research can only be subjected to a meta-analysis when there is a sufficient level of coherence in theorizing and research

methodology. The key challenge for any meta-analysis is to determine if integration is warranted and how it can be achieved. When first reviewing the literature on sex drive, we encountered considerable conceptual heterogeneity. This heterogeneity was reflected in the wide variety of psychometric inventories used to gauge the construct, which rendered conventional meta-analysis of complete inventories unfeasible. To solve this, we developed a new approach that draws on (and separates) the individual inventory items in line with the underlying psychological theorizing. To that end, we first developed the new conceptualization of sex drive outlined in the introduction. This conceptualization served as the theoretical foundation for the integration of previous research. We then selected individual items from existing inventories or ad hoc measurements that reflected the theoretically derived indicators of sex drive. For example, the item "During the last month, how often have you had sexual thoughts involving a partner?" was selected (among others) from the Sexual Desire Inventory (Spector et al., 1996) and classified as the frequency of sexual cognition. All inventories from which individual items were retrieved are listed in Tables S2 and S3. We then collected and synthesized data for gender differences with respect to these items. This novel approach thus combines the advantages of meta-analysis (high generalizability, high statistical power) with high conceptual coherence by exclusively including a set of selected items that adequately fit the theoretical conceptualization of the construct of interest.

Coding

We coded several characteristics of (a) the publication, (b) the study design, (c) the sample, and (d) the outcomes. Some of these were used in statistical tests for the moderation of gender differences in sex drive, others serve descriptive purposes. For some moderators, outliers were removed according to cut-off values prior to analyses. Outliers were determined by visual inspection of the data. We list all such cut-off values when discussing the codings in the following section.

For some characteristics, we derived tentative hypotheses regarding moderation effects based on previous research, but we generally consider these analyses exploratory in nature. Unless noted otherwise, characteristics were coded as "yes" or "no," or "NA" if the information was not available. For categorical characteristics with more than two possible codings, we list all possibilities, except for the country of data collection and the outcome-level codings. For the outcome-level codings, possible categories were generated inductively during the coding process and consolidated after coding.

The coding work was shared among two of the authors. A sample of 21 studies was coded by both coders. Results for interrater reliability are summarized in Table S7. We computed Cohen's κ (Cohen, 1960) for categorical codings with a known number of categories, Pearson's correlation for numerical codings, and percent agreement for categorical codings with an unknown number of categories. Overall, coder agreement was good (mean of all κ values: 0.87) and is classified as an "almost perfect" strength of agreement according to conventions (see Landis & Koch, 1977).

Publication Characteristics

The first set of coded characteristics on the publication level concerns the intent and topic of the article, authors' gender (distribution), and the focus of the journal in which the article was published in (Codings 1–6). The journal focus was inferred by considering the abstract and journal title only. The authors' gender was inferred from their names and institutional web pages. These codings allow for sensitivity analyses for potential biasing effects on the part of researchers. With Coding 7 (focus on anonymity), we aimed to capture whether the authors expressed awareness of the need to create a private and secure environment for participants to respond truthfully to questions about sexuality in order to maximize chances of truthful responses. In Coding 8, we coded the article's publication status to test for potential publication bias, that is, smaller or larger effect sizes for unpublished studies. For unpublished data sets with no article, publication characteristics were coded as missing except for author gender. Thus, the codings for publication characteristics were as follows:

1. Focus on gender differences: Did the authors focus on gender differences in the study?
2. Focus on gender differences in sex drive: Did the authors focus on gender differences in sex drive?
3. Aim to find gender differences in sex drive: Did the authors state that they were aiming to find gender differences in sex drive?
4. First author gender: What was the gender of the first author (male/female/nonbinary)?
5. Mean author gender: Female and male authors were coded as 0 and 1, respectively, and nonbinary excluded.
6. Sexuality journal: Does the journal publish research specifically on sexuality?
7. Focus on anonymity: Was there any general or specific statement about participants' anonymity, confidentiality, or privacy anywhere in the report?
8. Publication status: Had the article been published in a peer-reviewed journal as of October 2020?

Study Characteristics

Codings on the study level were mostly intended to gauge how privacy preserving the study situation and experience was for participants (Codings 1–5). From a theoretical perspective, this seems promising, as empirical sex drive differences may be more pronounced under study situations with less privacy or less subjectively perceived security and anonymity. This is because a lack of perceived privacy, security, and anonymity may promote biased responding, which may manifest as more restrictive responding in women and more liberal responding in men.

We also coded how the study was advertised and how participants were compensated to probe potential selection bias effects (Codings 6 and 7). Studies on sexuality may suffer from volunteer bias, with people willing to participate differing systematically from people not willing to participate. Some evidence suggests that volunteers tend to be more sexually experienced and hold more positive attitudes toward sexuality (Strassberg & Lowe, 1995; Wiederman, 1997), have higher levels of education, are less conservative, and are more novelty-seeking (Dunne et al., 1997).

The year of the study was coded to probe for potential changes in gender differences over time (Coding 8). Social norms change over time, and attitudes toward sexuality are becoming less restrictive (Mercer et al., 2013). This would suggest that, if the results are affected by biased responding, observed gender differences may have decreased over time. Thus, the codings for study characteristics were as follows:

1. Face-to-face interview: Were the questions asked in person by an interviewer?
2. Personal contact: Did participants have personal contact with anyone affiliated with the research team?
3. Group assessment: Were participants tested in groups or not (or a combination thereof)?
4. Electronic data collection: Was the data collection electronic versus not electronic (or a combination thereof)?
5. Participant anonymity: Were participants reassured about anonymity, privacy, or confidentiality? Example for positive coding: "Participants were assured that no IP addresses would be saved to ensure anonymity."
6. Sexuality study: Was the study advertised as a study on sexuality?
7. Compensation: Did participants receive material compensation (money, coupons, etc.), course credit, a combination of both, or nothing in return for participation?
8. Year of study: If the information about the year of data collection was missing for published studies, we entered the year the study was published minus two.

Sample Characteristics

We collected several data points characterizing the sample. For some of these, associations with sexuality have been previously established in the literature. Others were included solely for descriptive purposes or sensitivity analyses. Most codings are related to common demographic characteristics (Codings 1–9). For descriptive purposes, codings for mean age, standard deviation of age, percent heterosexual, and percent single were taken separately for men and women (whenever possible) to collect additional information on potential within-sample differences between men and women on these characteristics. Country-level sex ratio (Coding 12), country-level gender inequality (Coding 13), and country-level gender development (Coding 14) were coded based on the country of data collection (Coding 11). These sets of codings were included to gauge how the social and cultural context may shape gender differences in sex drive. Thus, the codings for sample characteristics were as follows:

1. Mean sample age (in years): Participants' average age was coded. Samples with an average age above 70 were classified as outliers based on visual inspection of the data and removed from the respective moderation analysis (3 effect sizes from 3 studies removed, next closest average age = 51.28).
2. Sexually active: Some studies restricted sampling to sexually active participants, others did not. This was usually defined as having had sex with a partner recently. The definition of "recently" varied across studies.
3. Percent religious: What percentage of participants are religious? Participants were counted as nonreligious if they responded with "none" or equivalent to questions assessing religiosity, faith, and so forth
4. Percent single: What percentage of participants are single? All participants indicating any sort of romantic affiliation were counted as not single.
5. Average partnership duration (in weeks): Relationship length was coded for the subset of participants in relationships. Manifestations of sex drive, including sexual desire, are well documented to fluctuate and in some cases decrease over the course of a long-term relationship (Klusmann, 2002). Recent work has shown sexual desire to decline particularly in wives but less so in husbands during the first couple of years of marriage (McNulty et al., 2019).
6. Percent White: What percentage of participants are of White/European/European American ethnicity? This served as a proxy for the percentage of respondents with minority status in the sample for most studies in the database. More fine-grained coding of ethnicity was complicated by varying definitions across studies. We had no prior hypotheses regarding the association between ethnicity and gender differences in sex drive.
7. Percent heterosexual: What percentage of participants are heterosexual?
8. Percent university students: What percentage of participants are university students?
9. Percent parents: What percentage of participants are parents? Parenthood, especially early parenthood, can impact sexual desire in couples and may affect new fathers and mothers differently (Ahlborg et al., 2005).
10. Contraceptive use: What percentage of the female sample used hormonal contraceptives?
11. Country of data collection: For studies that took place in multiple countries, we retrieved percentages per country.
12. Country-level sex ratio: Previous research suggests that sexual desire is influenced by the number of potential partners available, and that this influence unfolds differently for men and for women (Gebauer et al., 2014). For each country, we coded the number of males per 100 women in the 25–49 age bracket (Population Division of the Department of Economic and Social Affairs of the United Nations Secretariat, 2019), since this bracket was most representative for our data. Values were retrieved for the year closest to the year of the study (see the previous section, on study characteristics). For studies that spanned multiple countries, we entered a weighted score. This coding was not preregistered.

13 and 14. Country-level gender inequality and country-level gender development: Social norms regarding the expression of sexuality may differ for men and women (see the previous sections, on social role theory and the sexual double standard hypothesis). In order to capture how participants may be exposed to differing social norms, we retrieved data for gender inequality (Gender Inequality Index [GII]) and gender development (Gender Development Index [GDI]). Both indices are produced by the United Nations (United Nations Development Programme, 2019). The GII captures discrimination against girls and women in the areas of health, education, political participation, and labor market opportunities. Higher GII values indicate greater gender inequality, with values ranging from 0 to 1. The GDI measures gender differences in human development achievement in health, knowledge, and living standards. GDI values of 1 indicate gender parity, values below 1 indicate discrimination against females, and values above 1 indicate discrimination against males. We again entered the value from the year closest to the year of the study and computed weighted scores for studies spanning multiple countries. For the GDI, values below 0.90 were classified as outliers and removed (6 effect sizes from 3 studies removed, next closest GDI value = 0.94). For context, a value of 1.00 implies equality. In 2018, the country closest to gender parity (Norway) was rated at 0.99, while the world's GDI was estimated at 0.94.

Outcome Characteristics

For outcomes, we coded several characteristics of the item and the response scale. When items were taken from a common psychometric inventory, but no further information was given, we assumed that the item wording and response scale corresponded to the original publication of the inventory. The codings for outcome characteristics were as follows:

1. Item content: What was the content or target of a sexual thought or affect, that is, who or what does one think about or feel desire for? Possible codings were: "no target," typically just containing general references to sex (e.g., "How often do you think about sex?"); "unspecified partner," when a partner is mentioned but not further specified (e.g., "How often do you think about sex with a partner?"); "own partner" for references to one's own partner specifically; "extra-pair partner," when asking about a partner outside of the current relationship (e.g., "How often do you fantasize about having sex with someone other than your current dating partner?"), and "masturbation" (e.g., "How often do you feel desire to masturbate?").
2. Item context: What was the context in which the cognition, affect, or behavior occurred (e.g., sexual thoughts while bored at work, sexual desires in romantic situations)?
3. Item wording: How was the construct labeled (e.g., "self-stimulation" vs. "masturbation," "sexual daydream" vs. "fantasy," "sexual need" vs. "sexual desire")?
4. Aggregation span: For outcomes indicating the frequency of a sexual event: What was the period (in weeks) across which frequency was aggregated? For example, the item "How often did you think about sex in the past four weeks?" would be coded as 4. Values above 60 weeks were removed (10 effect sizes from 9 studies removed, next closest value = 30).
5. Type of response scale: Was the response scale open or closed (e.g., Likert-type scale)?
6. Scale range: For closed response scales, what was the scale range (scale maximum minus scale minimum)?

Statistical Analyses

We aggregated effect sizes using meta-analytic models. In the primary analysis, effect sizes for the sex drive manifestations were modeled as a function of sex drive facet (frequency of sexual cognition, sexual affect, or sexual behavior), akin to a one-way analysis of variance in primary studies. This model estimates the summary effects within subgroups (i.e., per facet) and enables testing for between-group differences. Dependency due to the inclusion of multiple effects per study was handled using robust variance estimation (RVE) meta-analysis (Hedges et al., 2010; Tipton, 2015). Indicators of latent sex drive and bias indicators were analyzed separately using the same model, that is, in a subgroup analysis by type of indicator. To derive a global, cross-indicator estimate for gender differences in sex drive, we fitted a random-effects meta-analysis model with equal weights assigned to the group-wise summary effects for the sex drive manifestations. The same procedure was applied to the bias indicators to obtain a global estimate of biased responding.

We also applied univariate moderation analyses to probe how effect sizes for the sex drive manifestations (i.e., sex drive facets) varied as a function of publication, study, sample, or item characteristics. Models were fitted separately for each type of sex drive manifestation.

Robust Variance Estimation

Due to our approach of meta-analyzing item-level effect sizes, many studies contributed multiple effect sizes. This creates dependence among effect sizes, which constitutes a violation of the assumptions of standard meta-analysis models (Lipsey & Wilson, 2001). Conventional approaches for solving this problem involve either selecting one effect size per study or manually aggregating multiple effect sizes prior to modeling (Borenstein et al., 2009). Both approaches, however, entail a loss of information and severely complicate metamoderation analyses. To illustrate the latter point, consider a study reporting participants' frequency of sexual thoughts on an open scale and their frequency of sexual fantasies on a closed scale. These two effects would need to be averaged manually to satisfy the independence assumption. However, this aggregation would preclude the metamoderation analysis of the effect of closed versus open scales, so effect sizes need to be left unaggregated or be dropped from the analysis. In the former case, a new data set needs to

be created for every single moderation analysis, while in the latter case valuable information is lost. Both options are unsatisfactory. RVE meta-analysis elegantly solves the problem of effect size dependency by estimating a "working" model for the variance–covariance matrix of effect sizes (Hedges et al., 2010), thereby allowing dependent effects to be modeled as a function of one or more predictor variables while minimizing loss of information.

Modelers have a choice between a "hierarchical" and a "correlated" effects model for the dependency structure—the "hierarchical" model is more appropriate when dependence arises predominantly from identifiable clusters of estimates (e.g., multiple studies by the same laboratories or authors), while the "correlated" model is more appropriate when dependence arises predominantly from multiple outcomes per study. Additionally, modelers have to decide on a default value for the correlations between effect sizes, although this usually has no discernable impact on the model estimates. For all models, we selected the "correlated" effects model and a default correlation of 0.8. To test the sensitivity and robustness of the results, we varied the latter correlation value between 0 and 1 in steps of 0.1 for the primary analyses. In no case did this correlation assumption considerably influence the results. We employed small-sample corrections when testing for metaregression by adjusting the degrees of freedom (Tipton, 2015) and using an approximate Hotelling test (AHZ, see Tipton & Pustejovsky, 2015).

Heterogeneity

We report two measures of effect size heterogeneity, τ and I^2 (Borenstein et al., 2009). Although both τ and I^2 are indicators of heterogeneity, they serve different purposes. First, τ is the standard deviation of the true effects. It answers the question of how much the true effects vary independent of variation due to sampling error. Because τ reflects the absolute amount of true variation, it says nothing about the proportion of the observed variation that is due to true variation of effects and not mere sampling error. To facilitate the interpretation of τ estimates, they can be examined relative to τ estimates in other meta-analyses. A recent study examined between-study heterogeneity estimates published in *Psychological Bulletin* between 1990 and 2013 (Van Erp et al., 2017). For studies reporting *d* or *g* effect sizes, the 25%, 50%, and 75% quantiles of τ were 0.12, 0.20, and 0.32, respectively. These may serve as reference points for small, medium, and large heterogeneity.

Second, I^2 indicates the proportion of the variation in observed effects that is due to variation in true effects rather than sampling error (Borenstein et al., 2017). Because I^2 is the *proportion* of variance that is true, it says nothing about the *absolute* amount of variation, as τ does. If all variation in observed effect sizes were only due to sampling error, both τ and I^2 would approach zero.

Meta-Analytic Correlation Analyses

In addition to the summary effects for gender differences, we also investigated the correlations between outcomes. If studies reported multiple outcomes, the Pearson correlations between outcomes (and their respective variances) were retrieved from the article or computed from the raw data and labeled according to the indicators they represented (e.g., cognition frequency–affect frequency [CF-AF] for a correlation between one CF item and one AF item). We then aggregated correlations for all available outcome pairs using the meta-analytic models described previously to create a meta-analytic correlation table. We expected notable correlations among the sex drive manifestations and indicators of latent sex drive (convergent validity). Correlations between sex drive indicators and bias indicators were expected to be lower, but still positive (since on the individual level, a stronger sex drive may well be associated with higher responses to questions that indicate bias on the level of gender differences). All Pearson correlations were transformed to Fisher's *Z* prior to analysis and then back to Pearson correlations for interpretation. Variances for the Fisher's *Z* values were computed from the sample size (Borenstein et al., 2009).

Publication Bias

Publication bias occurs when studies that did not produce the desired outcomes are less likely to be published (Fanelli, 2012; Franco et al., 2014). Authors are less likely to submit "failed" studies for publication, and if they do, reviewers and editors are less likely to favor publication compared to "successful" studies that produced significant outcomes. As a result, most published studies in psychology report hypotheses that "worked" (Fanelli, 2010; Sterling, 1959; Sterling et al., 1995). There is widespread agreement that publication bias exists, that it may bias meta-analytic effect size estimates, and that its prevalence varies across different bodies of research literature (Bakker et al., 2012; Fanelli, 2010; Ferguson & Brannick, 2012; Friese & Frankenbach, 2020).

For the present meta-analysis, publication bias is unlikely to play a role for several reasons. First, the majority of studies providing relevant data primarily investigated a research question unrelated to the one examined in the present meta-analysis. Therefore, the decision to submit and publish the respective studies did not depend on outcomes regarding gender differences in indicators of sex drive. Second, to adequately test our research question, we resorted to comparing responses to individual items instead of complete inventories. These fine-grained data are rarely reported in any article and are therefore unlikely to influence the decision to publish a study.

Despite these reasons to believe a priori that publication bias is rather unlikely to affect the present meta-analysis, we applied two statistical approaches to detect publication bias (Iyengar & Greenhouse, 1988; McShane et al., 2016; Sterne & Egger, 2005). Unfortunately, the toolset for detecting publication bias for meta-analyses with multiple, dependent effect sizes is still limited (Friese et al., 2017). A recent simulation study suggested two approaches—Egger's test and a three-parameter selection model; neither, however, is without drawbacks (Rodgers & Pustejovsky, 2021).

The first approach is a variant of Egger's test for funnel-plot asymmetry (Egger et al., 1997). In this test, effect sizes are regressed on their standard errors in an RVE metaregression. The underlying logic is that studies with smaller sample sizes, and thus larger standard errors, require larger effect sizes to achieve statistical significance than studies with larger sample sizes and smaller standard errors. Consequently, studies with larger standard errors are expected to have larger effect sizes on average, if effect sizes are selected based on statistical significance. Note that these so-called small-study effects can arise from publication bias, but also from legitimate sources (e.g., systematically different populations in smaller compared to larger studies). This approach exhibits nominal Type I error rates, but can have little statistical power when the number of included effect sizes is small (Rodgers & Pustejovsky, 2021).

The second approach is the so-called three-parameter selection model (3PSM; Iyengar & Greenhouse, 1988; McShane et al., 2016; Vevea & Hedges, 1995). This model does not handle dependency due to multiple outcomes natively, so this needs to be addressed beforehand by aggregating effect sizes per study or randomly selecting one effect size per study. The 3PSM approach compares an unadjusted meta-analysis baseline model to an adjusted model in a likelihood-ratio test. The unadjusted model is the standard meta-analysis model and can be any fixed-, random-, or mixed-effects model. We fitted an intercept-only random-effects model. In the adjusted model, the selection process, that is, the process of selecting studies for inclusion based on statistical significance, is explicitly modeled by estimating weights for prespecified p value intervals of interest. We set two intervals: $0 < p < .025$ for significant studies (i.e., a two-tailed p value of .05) and $.025 < p < 1$ for nonsignificant studies. Publication bias is assumed to be present if the inclusion of the selection process significantly improves the baseline model, as indicated by the likelihood-ratio test.

We tested for publication bias using both approaches separately for each sex drive manifestation. For the 3PSM, we created sets of independent effect sizes by randomly drawing one effect size per study. We then fitted the adjusted and unadjusted models, performed the likelihood-ratio test, and retrieved the results. The process was repeated 100 times to reduce the impact of chance during sampling. We report the average p values across repetitions.

Statistical Software

Data handling and analysis were done with the *R* language for statistical computing (R Core Team, 2020). We relied on the *robumeta* package for RVE (Z. Fisher & Tipton, 2015), the *metafor* package for random-effects meta-analysis (Viechtbauer, 2010), and the *tidyverse* package for data preparation (Wickham et al., 2019). Figures, tables, and the results text were produced programmatically for increased reproducibility.

Transparency and Openness

We adhered to the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) reporting guidelines for systematic reviews (PRISMA, see Moher et al., 2009). We made the PRISMA checklist, effect size data, and computer code available in an open online repository at https://osf.io/h4jbx/, following recent recommendations to increase the reproducibility of meta-analyses (Lakens et al., 2016). Note that we are not permitted to share the raw data we collected from the primary authors but do share all aggregate statistics computed from these raw data. Approval by a research ethics committee was not required for this review. The meta-analysis was preregistered on PROSPERO. We included an annotated copy of the protocol in the open repository to denote all deviations. The original protocol can be accessed at https://www.crd.york.ac.uk/prospero/display_record.php?RecordID=72894. The Supplemental Materials to this article contain additional figures and tables, as well as complimentary information on our conceptualization of sex drive.

Results

Search Results

In total, the search team screened 20,397 titles, 3,784 abstracts, and 1,715 full texts (all including duplicates, see flowchart in Figure 2). Four hundred eighty-three publications containing eligible studies were identified. Out of these, 460 did not report all necessary information. We contacted all 314 corresponding authors with requests for data (some contributed more than one publication), out of whom 144 responded and 118 provided data. Those who did not provide data cited lack of time, no access to data, data loss due to hardware failure, not keeping the data, or inability to find the data as reasons. Data could not be obtained for 297 eligible publications. Thus, overall, data could be obtained for 39% of eligible publications. In total, $n = 621{,}463$ participants were included in the analysis. We retrieved $m = 856$ effect sizes from $k = 211$ studies. About half of the effect sizes were sex drive manifestations ($m = 439$, $k = 195$, $n = 225{,}102$), one-quarter were indicators of latent sex drive ($m = 173$, $k = 54$, $n = 444{,}530$), and one-quarter were bias indicators ($m = 244$, $k = 123$, $n = 102{,}634$).

Study and Sample Characteristics

We next present available demographic information on the subset of participants who reported on sex drive manifestations (i.e., frequency of sexual cognition, affect, and behavior).[7] Their average age was 30 years old (information available for 84% of the samples), 89% were heterosexual (50% of samples with information), 76% were White (29% with information), 53% were religious (11% with information), 38% were single (56% with information), and 59% were university students (45% with information). Data for sex drive manifestations were collected between 1992 and 2019 ($M = 2011$, $Mdn = 2012$) and 90% of effect sizes were computed from raw data.

For some samples, codings for sexual orientation, age, and relationship status were available separately for men and women, allowing us to calculate weighted differences scores (i.e., Δs): Male participants were more likely to be heterosexual ($\Delta = 1.74\%$, 36% with information), older ($\Delta = 1.81$ years, 36% with information), and more likely to be single ($\Delta = 1.84\%$, 36% with information). Across all studies that included a sex drive manifestation, 50% were published in sexuality journals (100% with information), 54% had female first authors (98% with information), 88% were published (100% with information), 60% used electronic data collection (85% with information), 34% documented reassuring participants about privacy (88% with information), 49% focused on gender differences in sex drive (87% with information), 35% rewarded participants materially (25% course credit, 9% mixed, 31% no reward; 57% with information), and 80% were advertised as studies on sexuality (47% with information). The studies were mostly conducted in North America (52% total; United States: 79%, Canada: 13%, Mixed: 5%, Mexico: 2%, Costa Rica: less than 1%) and Europe (43% total; Germany: 54%, Portugal: 11%, United Kingdom: 9%, Spain: 9%, Norway: 4%, Croatia: 2%, Estonia: 2%, Italy: 2%, Others: 10%), with some from Asia (4% total; Japan: 38%, China: 31%, Israel: 13%, Turkey: 13%, India: 6%), Oceania (1% total; Australia: 100%), and Africa (less than 1% total; Cameroon: 100%). Further information on the codings is summarized in Table 2 for the sex

[7] We note that in some cases, whether demographic information was reported was likely correlated with the demographic information itself. For example, studies that reported the percentage of college students likely included a larger percentage of students than studies that did not report this information. Consequently, we should be cautious in generalizing these statistics to the full sample.

Table 2
Moderator Overview

Moderator	Total		Cognition frequency			Affect frequency			Behavior frequency		
	m	Compl.	*m*	Compl.	Distribution	*M*	Compl.	Distribution	*m*	Compl.	Distribution
Outcome-level moderators											
Item content	558	62%	282	100%	Unspecified partner ($m = 32$), extra-pair partner ($m = 168$), no target ($m = 82$)	76	81%	Unspecified partner ($m = 20$), masturbation ($m = 1$), no target ($m = 54$), own partner ($m = 1$), NA ($m = 18$)	0	0%	NA ($m = 63$)
Item context	639	71%	282	100%	Asleep ($m = 1$), at work ($m = 4$), before sleep ($m = 1$), being bored ($m = 2$), everyday life ($m = 1$), in everyday life ($m = 1$), not specified ($m = 266$), on the way to work ($m = 2$), traveling on a train or bus ($m = 2$), while reading ($m = 1$), while working at a job ($m = 1$)	94	100%	In contact with an extra-pair person ($m = 1$), not specified ($m = 93$)	63	100%	Alone ($m = 6$), not specified ($m = 57$)
Type of response scale	897	100%	282	100%	No ($m = 268$), yes ($m = 14$)	94	100%	No ($m = 82$), yes ($m = 12$)	63	100%	No ($m = 32$), yes ($m = 31$)
Scale range	732	82%	265	94%	$Q = [2.00, 4.00, 7.00, 8.00, 9.00]$, $M = 6.37$, $SD = 1.72$	81	86%	$Q = [3.00, 6.00, 7.00, 7.00, 9.00]$, $M = 6.14$, $SD = 1.34$	31	49%	$Q = [2.00, 4.00, 5.00, 5.50, 8.00]$, $M = 4.84$, $SD = 1.24$
Item wording	639	71%	282	100%	Daydreams ($m = 10$), dreams ($m = 1$), fantasies ($m = 189$), other ($m = 5$), preoccupation ($m = 3$), thoughts ($m = 74$)	94	100%	Desire ($m = 45$), other ($m = 49$)	63	100%	Masturbation ($m = 49$), other ($m = 7$), self-stimulation ($m = 6$), touch, and explore body ($m = 1$)
Aggregation span	185	21%	48	17%	$Q = [1.00, 1.00, 30.00, 30.00, 183.00]$, $M = 23.67$, $SD = 27.71$	71	76%	$Q = [0.00, 5.00, 30.00, 30.00, 365.00]$, $M = 26.17$, $SD = 42.75$	49	78%	$Q = [7.00, 7.00, 28.00, 30.00, 365.00]$, $M = 48.04$, $SD = 89.43$
Publication-level moderators											
Aim to find gender differences in sex drive	779	87%	243	86%	No ($m = 215$), yes ($m = 28$), NA ($m = 39$)	78	83%	No ($m = 53$), yes ($m = 25$), NA ($m = 16$)	57	90%	No ($m = 44$), yes ($m = 13$), NA ($m = 6$)
Focus on gender differences in sex drive	780	87%	243	86%	No ($m = 201$), yes ($m = 42$), NA ($m = 39$)	78	83%	No ($m = 48$), yes ($m = 30$), NA ($m = 16$)	57	90%	No ($m = 32$), yes ($m = 25$), NA ($m = 6$)
Focus on gender differences	780	87%	243	86%	No ($m = 93$), yes ($m = 150$), NA ($m = 39$)	78	83%	No ($m = 21$), yes ($m = 57$), NA ($m = 16$)	57	90%	No ($m = 11$), yes ($m = 46$), NA ($m = 6$)
Gender of first author	884	99%	275	98%	Female ($m = 102$), male ($m = 173$), NA ($m = 7$)	94	100%	Female ($m = 47$), male ($m = 47$)	63	100%	Female ($m = 20$), male ($m = 43$)
Publication status	897	100%	282	100%	Published ($m = 220$), unpublished ($m = 62$)	94	100%	Published ($m = 61$), unpublished ($m = 33$)	63	100%	Published ($m = 42$), unpublished ($m = 21$)
Sexuality journal	897	100%	282	100%	No ($m = 196$), yes ($m = 86$)	94	100%	No ($m = 65$), yes ($m = 29$)	63	100%	No ($m = 43$), yes ($m = 20$)
Focus on anonymity	815	91%	254	90%	No ($m = 136$), yes ($m = 118$), NA ($m = 28$)	83	88%	No ($m = 32$), yes ($m = 51$), NA ($m = 11$)	63	100%	No ($m = 25$), yes ($m = 38$)
Mean author gender	884	99%	275	98%	$Q = [0.00, 0.50, 0.67, 1.00, 1.33]$, $M = 0.65$, $SD = 0.35$	94	100%	$Q = [0.00, 0.21, 0.50, 1.00, 1.33]$, $M = 0.56$, $SD = 0.43$	63	100%	$Q = [0.00, 0.50, 1.00, 1.00, 1.33]$, $M = 0.73$, $SD = 0.43$
Sample-level moderators											
Mean age	775	86%	235	83%	$Q = [16.78, 20.85, 23.96, 29.89, 70.55]$, $M = 25.89$, $SD = 6.87$	83	88%	$Q = [18.60, 24.05, 26.47, 30.45, 51.28]$, $M = 27.77$, $SD = 5.89$	43	68%	$Q = [15.55, 22.82, 25.95, 30.75, 74.00]$, $M = 29.22$, $SD = 10.29$
Percent White	323	36%	79	28%	$Q = [15.50, 60.00, 78.60, 90.50, 100.00]$, $M = 73.92$, $SD = 19.73$	31	33%	$Q = [15.50, 62.42, 78.60, 88.20, 98.00]$, $M = 70.91$, $SD = 24.69$	16	25%	$Q = [32.20, 50.88, 58.98, 86.25, 100.00]$, $M = 65.90$, $SD = 24.93$

(*table continues*)

Table 2 (*continued*)

Moderator	Total		Cognition frequency			Affect frequency			Behavior frequency		
	m	Compl.	*m*	Compl.	Distribution	*M*	Compl.	Distribution	*m*	Compl.	Distribution
Country-level gender inequality	828	92%	256	91%	Q = [0.05, 0.10, 0.18, 0.25, 0.63], M = 0.18, SD = 0.09	91	97%	Q = [0.08, 0.09, 0.13, 0.24, 0.26], M = 0.16, SD = 0.07	61	97%	Q = [0.05, 0.10, 0.16, 0.25, 0.56], M = 0.18, SD = 0.10
Country-level gender development	828	92%	256	91%	Q = [0.76, 0.97, 0.99, 0.99, 1.03], M = 0.98, SD = 0.02	91	97%	Q = [0.94, 0.97, 0.99, 0.99, 1.03], M = 0.98, SD = 0.01	61	97%	Q = [0.85, 0.96, 0.98, 0.99, 1.00], M = 0.97, SD = 0.02
Percent heterosexual	516	58%	150	53%	Q = [31.66, 85.48, 100.00, 100.00, 100.00], M = 92.31, SD = 12.19	63	67%	Q = [47.41, 77.75, 95.10, 100.00, 100.00], M = 88.13, SD = 14.49	37	59%	Q = [31.66, 77.02, 96.50, 100.00, 100.00], M = 87.93, SD = 15.65
Percent single	548	61%	158	56%	Q = [0.00, 0.00, 37.51, 50.00, 100.00], M = 33.85, SD = 27.67	71	76%	Q = [0.00, 0.00, 35.10, 43.78, 100.00], M = 27.30, SD = 26.11	47	75%	Q = [0.00, 30.27, 45.98, 50.67, 100.00], M = 40.08, SD = 25.04
Percent university students	424	47%	132	47%	Q = [0.00, 89.18, 100.00, 100.00, 100.00], M = 86.02, SD = 26.36	39	41%	Q = [14.20, 58.99, 92.00, 100.00, 100.00], M = 78.59, SD = 28.14	30	48%	Q = [0.00, 53.57, 86.38, 100.00, 100.00], M = 71.67, SD = 33.70
Average partnership duration in weeks	191	21%	46	16%	Q = [1.80, 25.83, 52.52, 58.05, 158.40], M = 56.14, SD = 43.73	21	22%	Q = [3.91, 35.66, 54.94, 78.89, 287.76], M = 70.78, SD = 63.41	9	14%	Q = [2.74, 54.42, 54.94, 55.80, 287.76], M = 82.38, SD = 87.74
Percent parents	147	16%	37	13%	Q = [0.00, 14.55, 20.53, 36.00, 100.00], M = 30.77, SD = 28.51	17	18%	Q = [14.55, 17.30, 25.00, 64.00, 100.00], M = 44.44, SD = 35.03	12	19%	Q = [0.00, 16.37, 19.26, 30.00, 59.70], M = 24.95, SD = 16.81
Study restricted to sexually active	662	74%	201	71%	No (m = 172), yes (m = 29), NA (m = 81)	76	81%	No (m = 64), yes (m = 12), NA (m = 18)	52	83%	No (m = 49), yes (m = 3), NA (m = 11)
Country-level sex ratio	828	92%	256	91%	Q = [92.17, 100.88, 101.19, 103.00, 108.35], M = 101.41, SD = 2.46	91	97%	Q = [93.92, 100.88, 101.23, 103.24, 104.88], M = 101.69, SD = 2.47	61	97%	Q = [92.17, 100.88, 101.36, 103.85, 104.92], M = 101.80, SD = 2.72
Study-level moderators											
Anonymity reassurance	798	89%	250	89%	No (m = 164), yes (m = 86), NA (m = 32)	80	85%	No (m = 43), yes (m = 37), NA (m = 14)	63	100%	No (m = 33), yes (m = 30)
Participant compensation	571	64%	181	64%	Course-credit (m = 53), material (m = 64), mixed (m = 29), none (m = 35), NA (m = 101)	52	55%	Course-credit (m = 7), material (m = 26), mixed (m = 8), none (m = 11), NA (m = 42)	34	54%	Course-credit (m = 5), material (m = 13), mixed (m = 6), none (m = 10), NA (m = 29)
Sexuality study	454	51%	129	46%	No (m = 42), yes (m = 87), NA (m = 153)	57	61%	No (m = 18), yes (m = 39), NA (m = 37)	51	81%	No (m = 20), yes (m = 31), NA (m = 12)
Year of study	852	95%	266	94%	Q = [1996.00, 2008.00, 2012.00, 2015.00, 2019.00], M = 2011.38, SD = 4.58	92	98%	Q = [2000.00, 2008.75, 2012.00, 2015.63, 2019.00], M = 2012.03, SD = 4.71	63	100%	Q = [1992.00, 2004.50, 2008.00, 2014.50, 2019.00], M = 2008.70, SD = 7.01
Face-to-face interview	848	95%	266	94%	No (m = 265), yes (m = 1), NA (m = 16)	83	88%	No (m = 82), yes (m = 1), NA (m = 11)	62	98%	No (m = 60), yes (m = 2), NA (m = 1)
Electronic data collection	786	88%	238	84%	Mixed (m = 4), no (m = 53), yes (m = 181), NA (m = 44)	81	86%	Mixed (m = 5), no (m = 18), yes (m = 58), NA (m = 13)	60	95%	Mixed (m = 1), no (m = 27), yes (m = 32), NA (m = 3)
Group assessment	732	82%	223	79%	Mixed (m = 13), no (m = 180), yes (m = 30), NA (m = 59)	75	80%	Mixed (m = 8), no (m = 63), yes (m = 4), NA (m = 19)	50	79%	No (m = 45), yes (m = 5), NA (m = 13)
Personal contact	829	92%	256	91%	Mixed (m = 8), no (m = 151), yes (m = 97), NA (m = 26)	84	89%	Mixed (m = 8), no (m = 48), yes (m = 28), NA (m = 10)	61	97%	No (m = 34), yes (m = 27), NA (m = 2)

Note. m = absolute number of effect sizes for which the corresponding characteristic could be coded; Compl. = percentage of effect sizes for which the corresponding characteristic could be coded; Distribution = information about the distribution of the coded characteristics. For categorical characteristics, the number of effect sizes per subgroup is reported. For continuous characteristics, Q is quartiles (minimum, 25% quartile, median, 75% quartile, maximum), M is the mean, and SD is the standard deviation. Note that summaries for continuous moderators are computed on the effect size level for this table. In the Results section, some of this information was presented on the level of individual participants (i.e., as summaries weighted by sample size). Some values may therefore differ.

drive manifestations and Table S6 for the indicators of latent sex drive.

Correlation Structure of Outcomes

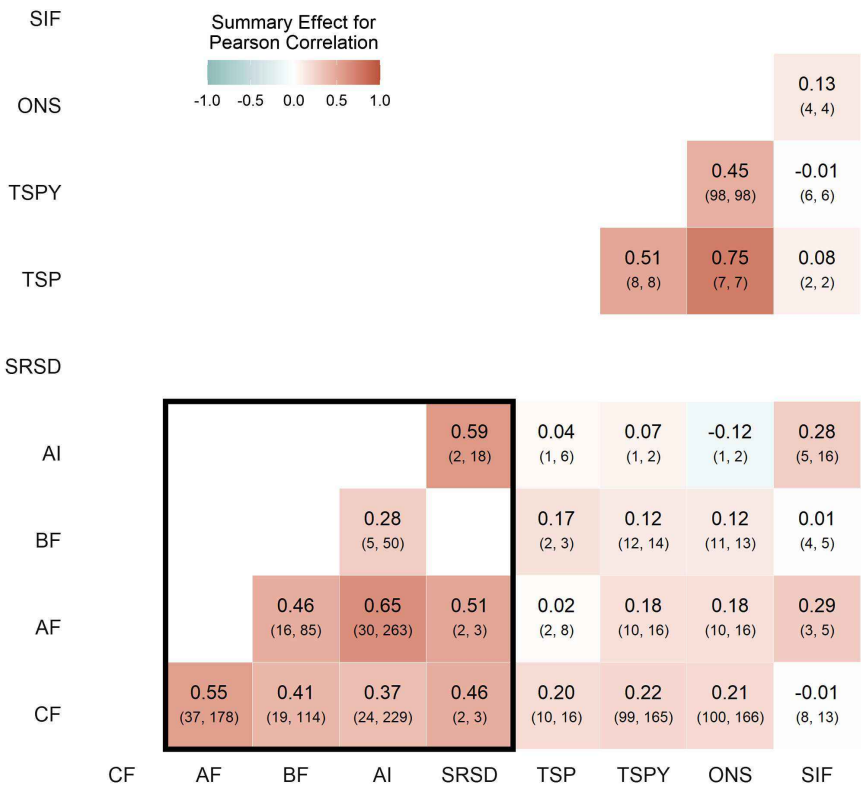
Figure 3 depicts the meta-analytic correlation table. As expected, the sex drive manifestations and indicators of latent sex drive formed a coherent cluster (box with solid line in Figure 3), providing evidence for convergent validity. Summary effects for Pearson correlations ranged from $r = 0.28$ to $r = 0.65$. Correlations between sex drive indicators and bias indicators were lower and less consistent, but still positive.

Outlier Analysis and Treatment

We conducted leave-one-out analyses for the sex drive manifestations to detect outliers with a notable influence on the summary effects. We repeatedly fitted the model for predicting effect sizes from sex drive facet (frequency of sexual cognition, affect, and behavior) in an RVE metaregression while dropping each effect size once. We then examined the change in estimated summary effects and standard errors resulting from dropping the effect size. Results for the leave-one-out analyses are depicted in Figure S2. There were no notable outliers for CF, $(\Delta g)_{min} = -0.0043$, $(\Delta g)_{max} = 0.0038$, nor for behavior frequency (BF), $(\Delta g)_{min} = -0.0194$, $(\Delta g)_{max} = 0.0206$. For AF, however, one study had an outsized influence, $\Delta g = -0.0436$, $\Delta SE = 0.0169$. This study examined older couples (average age = 74.60 years) and found a medium-to-large effect size indicating a higher frequency of sexual affect in women, $g = -0.64$. This outlier is also clearly visible in the corresponding funnel plot (see Figure 4, middle panel, effect farthest left). We removed this outlier from all further analyses. We applied the same procedure to the indicators of latent sex drive and bias indicators, respectively. We removed one effect size for affect intensity and one for sexual intercourse frequency (see Figure S2). Some effect sizes were additionally removed for the moderation analyses based on visual inspection of the scatter plots. These are reported in the Method section.

Figure 3

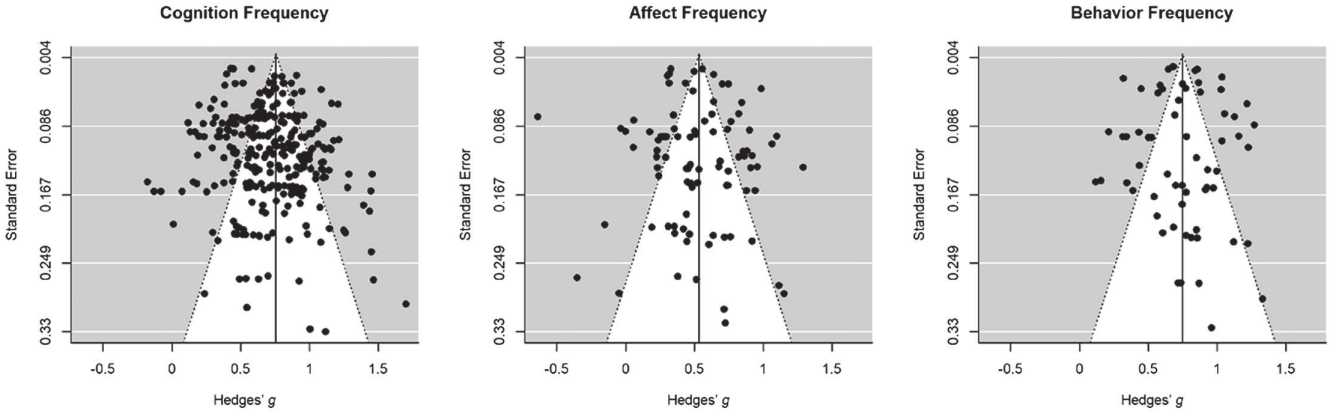
Meta-Analytic Correlation Table



Note. Meta-analytic correlation table displaying convergent validity for sex drive indicators. Values in the table without parentheses are summary effects for Pearson correlations for pairwise complete observations. The first value in parentheses denotes the number of studies that contributed to the summary effect (k) and the second value denotes the number of effect sizes (m). The solid box contains correlations among the sex drive manifestations and indicators of latent sex drive (convergent validity). CF = cognition frequency; AF = affect frequency; BF = behavior frequency; AI = affect intensity; SRSD = self-rated sex drive; TSP = total sexual partners; TSPY = total sexual partners in last year; ONS = total one-night stands; SIF = sexual intercourse frequency. See the online article for the color version of this figure.

Figure 4

Funnel Plots for Sex Drive Manifestations



Note. The solid vertical lines represent the within-subgroup summary effects. *x*-axis: Hedges' *g* effect sizes, positive values indicate larger values in men. *y*-axis: standard error of effect sizes. The dotted lines denote the area in which 95% of effect sizes are expected to fall in the absence of heterogeneity. Leave-one-out analyses identified one outlier in the center plot at $g < -0.5$ and standard error < 0.086. This effect was removed for all other analyses. Summary effects displayed in the figure were computed after removing the outlier.

Gender Differences in Sex Drive

Full results for sex drive manifestations, indicators of latent sex drive, and bias indicators are displayed in Table 3. We first analyzed the sex drive manifestations. We found significant, medium-to-large gender differences in sexual CF, $g = 0.76$, 95% CI [0.71, 0.80], sexual AF, $g = 0.58$, 95% CI [0.49, 0.66], and sexual BF, $g = 0.75$, 95% CI [0.66, 0.84]. The difference between the three facets was significant, $AHZ(68.53) = 7.26$, $p = .001$. The absolute amount of heterogeneity was medium in magnitude, $\tau = 0.21$ (Van Erp et al., 2017). Most of the variation in observed effects was estimated to be due to variation in true effects rather than sampling error, $I^2 = 91.03$.

For the indicators of latent sex drive, there were significant small-to-medium and medium-sized gender differences for affect intensity, $g = 0.40$, 95% CI [0.35, 0.45], and for self-rated sex drive, $g = 0.63$, 95% CI [0.35, 0.92]. The difference between these two indicators was not significant, $AHZ(4.00) = 5.75$, $p = .074$. Again, the absolute amount of heterogeneity was medium sized, $\tau = 0.15$, and overall variation was estimated to be due to variation in true effects rather than sampling error, $I^2 = 90.45$.

Out of the 612 effect sizes relating to sex drive manifestations or indicators of latent sex drive, only 17 (2.8%) showed a descriptively larger sex drive in women (indicated by an effect size of $g < 0$).

Gender Differences in Potentially Biased Responding

Next, we analyzed the bias indicators (see Table 3). There was no significant gender difference for sexual intercourse frequency, $g = 0.04$, 95% CI [−0.09, 0.17]. In contrast, gender differences were significant for total one-night stands, $g = 0.21$, 95% CI [0.18, 0.25], total sexual partners in the last year, $g = 0.15$, 95% CI [0.11, 0.19], and total sex partners, $g = 0.19$, 95% CI [0.02, 0.36]. The difference between these indicators was significant, $AHZ(27.75) = 7.49$, $p < .001$. The heterogeneity was comparable to sex drive manifestations and indicators of latent sex drive, $\tau = 0.16$, $I^2 = 80.41$.

Logic implies that there should be practically no gender differences on any of these indicators, given the premises that (a) the participants included in the primary studies constitute a representative sample of the heterosexual population and (b) all participants responded truthfully. If these premises hold, an empirical gender difference could emerge only if on average men overreported and/or women underreported (or vice versa) due to any motivational and/or cognitive biases that may have influenced responses in an invalid way. However, approximately 11% of the participants in our sample were homosexual. For this subsample (and consequently to a lesser extent for the overall average estimate), valid positive gender differences on the bias indicators in favor of men (e.g., suggesting more one-night stands by men than women) are plausible if homosexual women were less promiscuous and had sex less often than homosexual men (and vice versa for valid negative gender differences).

As a preliminary, post hoc test of this possibility, we conducted metaregression analyses for the bias indicators, regressing each indicator on the percentage of heterosexual participants in the sample. If the gender differences on the bias indicators are driven by differences in sexual behavior between homosexual men and homosexual women, the effect sizes should become larger if there are less heterosexual (and hence more homosexual) participants in the sample. Contrary to the expectation, all slopes were descriptively positive, indicating larger gender differences if the sample included more heterosexual participants. There were insufficient data for the bias indicators of total partners and sexual frequency to conduct significance tests for the slopes ($df < 4$). For a number of one-night stands and number of partners during the previous year, both tests were not significant ($ps > .183$). These tests were not preregistered. We cautiously interpret them as evidence against the possibility that the gender differences we obtained for the bias indicators are driven by gendered same-sex sexuality.

Global Summary Effect, Adjustment for Response Bias, and Natural Language Interpretation

In the previous sections, we reported summary effects separately for each sex drive indicator and bias indicator, respectively. To

Table 3
Main Results

		Summary effect										Test of moderation				
Role	Indicator	*g*	*SE*	*t*	*df*	*p*	95% CI	U_3	*OVL*	*CL*	*k*	*m*	*AHZ*	*df*	*p*	τ	I^2
Sex drive manifestations (global summary effect)																0.09	87.59
	Sex drive manifestations	0.69 (0.55)	0.06	12.10		<.001	[0.58, 0.81]	0.76 (0.71)	0.73 (0.78)	0.69 (0.65)	195	439					
Bias indicators (global summary effect)																0.05	77.64
	Bias indicators	0.15	0.04	4.07		<.001	[0.08, 0.22]	0.56	0.94	0.54	123	244					
Sex drive manifestations													7.26	68.53	.001	0.21	91.03
	Affect frequency	0.58 (0.43)	0.04	13.24	42.09	<.001	[0.49, 0.66]	0.72 (0.67)	0.77 (0.83)	0.66 (0.62)	57	94					
	Behavior frequency	0.75 (0.60)	0.04	17.99	30.17	<.001	[0.66, 0.84]	0.77 (0.73)	0.71 (0.76)	0.70 (0.67)	44	63					
	Cognition frequency	0.76 (0.61)	0.02	35.69	138.72	<.001	[0.71, 0.80]	0.78 (0.73)	0.71 (0.76)	0.70 (0.67)	161	282					
Indicators of latent sex drive													5.75	4.00	.074	0.15	90.45
	Affect intensity	0.40 (0.25)	0.03	15.59	42.34	<.001	[0.35, 0.45]	0.66 (0.60)	0.84 (0.90)	0.61 (0.57)	50	166					
	Self-rated sex drive	0.63 (0.49)	0.09	6.74	3.28	.005	[0.35, 0.92]	0.74 (0.69)	0.75 (0.81)	0.67 (0.63)	7	7					
Bias indicators													7.49	27.75	<.001	0.16	80.41
	Intercourse frequency	0.04	0.06	0.63	14.05	.541	[−0.09, 0.17]	0.52	0.98	0.51	18	20					
	Sex partners in last year	0.15	0.02	8.09	96.82	<.001	[0.11, 0.19]	0.56	0.94	0.54	106	106					
	Total one-night stand partners	0.21	0.02	11.93	94.98	<.001	[0.18, 0.25]	0.58	0.92	0.56	106	106					
	Total sex partners	0.19	0.07	2.61	7.28	.034	[0.02, 0.36]	0.57	0.93	0.55	12	12					

Note. Global and group-wise summary results for gender differences in sex drive manifestations, indicators of latent sex drive, and bias indicators. *g* = Hedges' *g* effect size (positive favors males); *SE* = standard error associated with the *g* value in the same row; *t* = *t* value associated with the *g* value in the same row; *df* = degrees of freedom associated with the *g* value in the same row; *p* = *p* value associated with the *g* value in the same row; 95% CI = 95% confidence interval; U_3 = Cohen's U_3 effect size of nonoverlap; OVL = overlap effect size; CL = common-language effect size, or probability of superiority; *k* = number of studies per subgroup/total; *m* = number of effect sizes per subgroup/total; *AHZ* = Hotelling-*T*-approximated test statistic. *df* = small-sample-corrected degrees of freedom; *p* = *p* value associated with the test statistic and *df* in the same row; I^2 = proportion of the variation in observed effects that is due to the variation in true effects; τ = estimated standard deviation of the true effects. Values in parentheses have been bias-corrected. For the correction, the global summary effect of the bias indicators has been subtracted from the respective summary effect.

estimate a global summary effect of the gender difference in sex drive, we fitted a random-effects meta-analysis model with equal weights to the summary effects of the sex drive manifestations (frequency of sexual cognition, affect, and behavior). The results are displayed in Table 3 and vizualised in Figure 5. The global summary effect was $g = 0.69$, 95% CI [0.58, 0.81].

We also computed a summary effect for all four bias indicators that may indicate a gender difference in (potentially) biased responding. This summary effect was $g = 0.15$, 95% CI [0.08, 0.22]. Assuming that the size of this summary effect is completely driven by men's and/or women's biased responding, then subtracting this bias effect estimate from the summary effect of sex drive differences should establish a global summary effect adjusted for response bias. This bias-adjusted global summary effect was of moderate size: $g = 0.54$. We note, however, that in fact the bias indicators may be more strongly affected by response bias than the sex drive indicators. For example, reporting the number of sexual partners (a bias indicator) may be more prone to self-presentation tendencies than reporting the number of sexual thoughts (a sex drive indicator), and responses to the former may be afflicted with stronger forms of normative pressures. If this reasoning is correct, subtracting the complete bias indicator summary effect constitutes an overcorrection. We, therefore, view the summary effect of $g = 0.54$ as a lower bound for the true bias-adjusted gender difference in sex drive.

Thus, based on the available evidence, we estimate male sex drive to be 0.69 *SD* stronger than female sex drive on average. Out of this difference, up to 0.15 *SD* may be attributable to biased responding, such that the true difference may lie between 0.54 and 0.69 *SD* (not considering the respective confidence intervals around these point estimates).

Standardized effect sizes are well suited to compare effects across different studies, but it can be difficult to comprehend what a standardized effect size actually means in more intuitive terms. To make this summary effect more easily interpretable, we now report natural language interpretations. Corresponding values for the fully adjusted summary effect are presented in parentheses. An effect of $g = 0.69$ (adjusted: $g = 0.54$) means that 76% (adjusted: 71%) of all men will have a stronger sex drive than the average sex drive among women (Cohen's U_3). It also indicates that 73% (adjusted: 78%) of men's and women's sex drive distributions overlap (overlapping coefficient OVL, also see Figure S5). Finally, a $g = 0.69$ (adjusted: $g = 0.54$) indicates that the probability of a randomly picked man having a higher sex drive than a randomly picked woman picked is 69% (adjusted: 65%, common-language effect size CL). For U_3 and CL, switching from the men's to the women's perspective provides a different, yet also worthwhile angle on the statistics: 24% (adjusted: 29%) of women have a larger sex drive than the average man, and the probability of a random woman having a higher sex drive than a random man is 31% (adjusted: 35%). We note that these interpretations remain relative in nature and do not speak to whether the difference is practically relevant in absolute terms.

Publication Bias

Funnel plots for CF, AF, and BF appeared highly symmetric in visual inspection, revealing no indication of bias (Figure 4; see also Figures S3 and S5, for funnel plots of the indicators of latent sex drive and bias indicators, respectively). This impression was confirmed by both Egger's regression tests and the bootstrapped 3PSM tests, which found no indication of publication bias or small-study effects for CF (Egger: $p = .149$; 3PSM: $p = .812$), AF (Egger: $p = .940$; 3PSM: $p = .706$), or BF (Egger: $p = .629$; 3PSM: $p = .271$).

Moderation Analyses

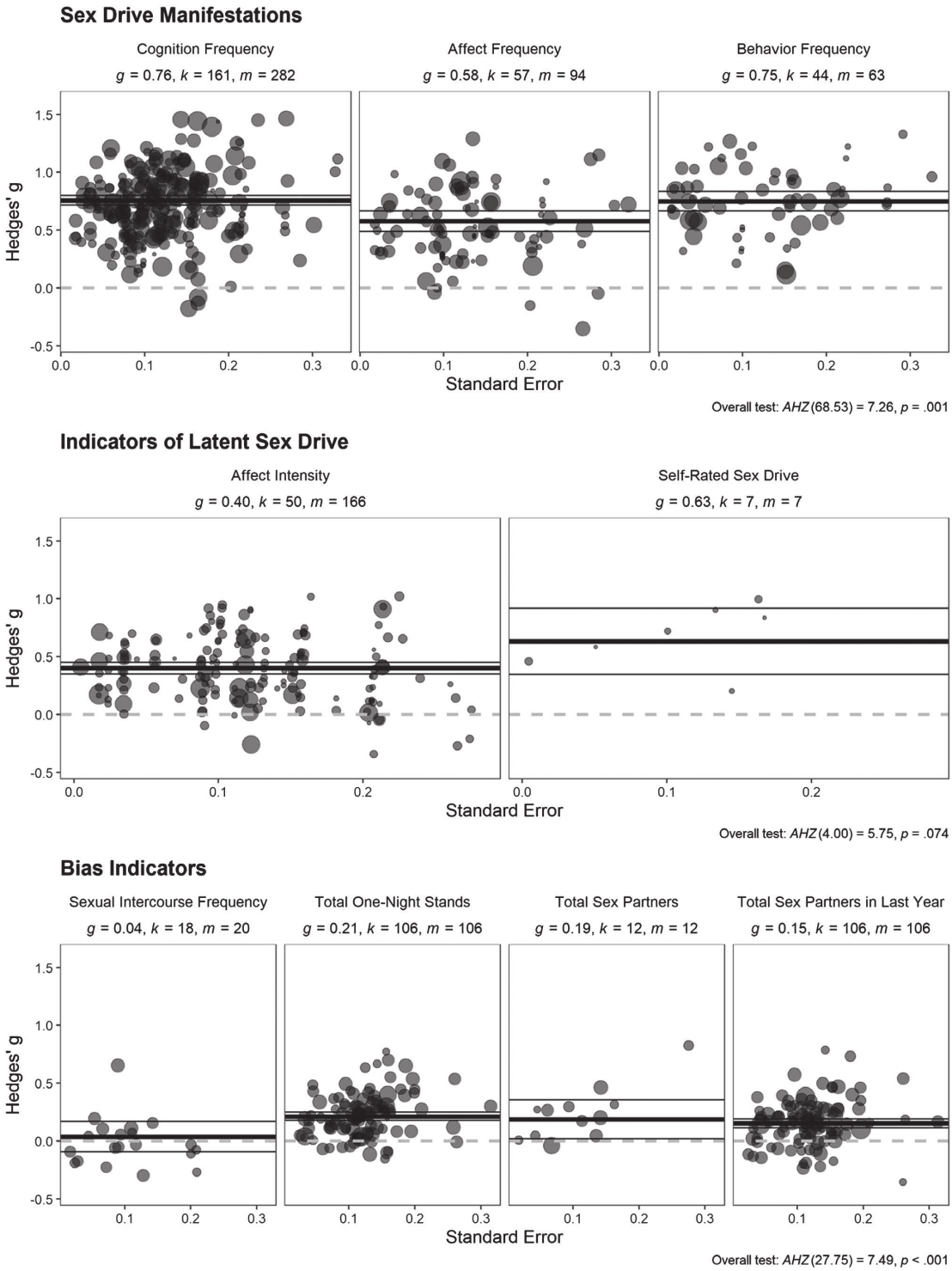
Sex Drive Manifestations

For the sex drive manifestations, tests for moderation by various characteristics of the publication, study, sample, and outcome are summarized in Table 4. Corresponding regression tables are summarized in Table 5. Selected analyses are graphically displayed in Figure 6. Small-sample corrections were employed for all statistical tests (Tipton, 2015; Tipton & Pustejovsky, 2015). These provide reliable results when degrees of freedom are larger than 4. We refrain from reporting p values when this threshold of $df > 4$ is not reached. We did not conduct moderation analyses for contraceptive use and religiosity, as insufficient information was available for these codings.

Cognition Frequency. There was one very strong moderation pattern for the frequency of sexual cognition. Specifically, gender differences were notably larger when the item captured sexual cognitions about extra-pair partners (i.e., others outside of one's current relationship), $g = 0.82$ (item content, e.g., "How often do you have fantasies about having sex with someone you are not in a committed romantic relationship with?"), as opposed to smaller effects for sexual cognitions about a nonspecific partner (e.g., "How often do you think about sex with a partner?"), $g = 0.58$, or nonspecific sexual cognitions without mentioning any partner (e.g., "How often do you think about sex?"), $g = 0.57$, test for difference: $AHZ(49.54) = 21.00$, $p < .001$. Closer examination of the data revealed that this item content coding was correlated with other codings. For example, studies using items about extra-pair partners were more often conducted by male first authors and more often focused on gender differences in sex drive specifically. We consequently repeated all moderation analyses while statistically controlling for this characteristic, collapsing cognitions about a nonspecific partner and nonspecific sexual cognitions into one category to achieve a binary control variable. We report the controlled tests in Tables 2 and 5. The uncontrolled tests are reported in the Supplemental Materials, Tables S4 and S5. This was not anticipated and therefore not preregistered.

After controlling for item content (extra-pair vs. other), there were five significant moderation tests. Gender differences were larger when participants were asked to aggregate the frequency of sexual cognitions across a larger period (e.g., "Over the past month, how often have you fantasized about sex?") compared to smaller periods (e.g., "How often do you think about sex on a typical day?"), $AHZ(5.57) = 8.46$, $p = .029$. Two analyses suggest that not having access to a sexual partner may lead to increases in sexual cognitions for men, decreases for women, or both—in any case, gender differences in sex drive were more pronounced: First, studies that did not restrict sampling to sexually active participants reported larger differences, $AHZ(20.04) = 4.99$, $p = .037$. Second, gender differences were more pronounced when the sample contained a larger percentage of single participants, $AHZ(26.18) = 7.21$, $p = .012$.

Figure 5
Main Results



Note. Main summary effects and confidence intervals for gender differences in sex drive manifestations (i.e., sex drive facets, top panel), indicators of latent sex drive (middle panel), and bias indicators (bottom panel). g = Hedges' g summary effect within the respective subgroup (positive values indicate larger values in men); $AHZ = AHZ$ value for the test of group differences; $p = p$ value for the test of group differences; k = number of studies per subgroup; m = number of effect sizes per subgroup. Black dots represent individual effect sizes. The thick black horizontal lines represent the meta-analytic summary effects within the subgroups. The thin black horizontal lines represent the borders of the 95% confidence interval. The dashed gray horizontal line represents the null effect at $g = 0$. Standard error for each effect is depicted on the x-axis. Circle size represents the weight of the respective effect size in the RVE metaregression model. Darker circles are due to multiple, overlapping effect sizes. AHZ = approximate Hotelling test.

Table 4
Tests for Moderation (Sex Drive Manifestations)

Moderator	Cognition frequency					Affect frequency					Behavior frequency				
	AHZ	*Df*	*p*	I^2	τ	*AHZ*	*df*	*p*	I^2	τ	*AHZ*	*df*	*p*	I^2	τ
Outcome-level moderators															
Aggregation span	8.46	5.57	.029	79.18	0.12	4.50	9.81	.060	93.43	0.25	1.05	14.67	.323	92.63	0.18
Item content						4.24	39.15	.046	90.47	0.21					
Item context											0.18	5.99	.685	94.88	0.20
Type of response scale	2.43	7.03	.163	86.40	0.19	1.70	4.95	.250	92.20	0.22	1.46	31.73	.235	94.90	0.19
Scale range	1.39	50.95	.243	86.57	0.19	1.71	22.19	.204	92.20	0.23	0.51	6.62	.500	95.07	0.17
Item wording	0.84	5.52	.524	86.40	0.20	1.67	49.28	.202	92.23	0.23	2.09	1.38	N/A	94.98	0.19
Publication-level moderators															
Aim to find gender differences in sex drive	1.97	14.82	.181	86.63	0.20	0.17	19.72	.684	93.08	0.24	0.34	6.76	.580	95.57	0.19
Focus on anonymity	0.11	120.36	.739	86.44	0.20	0.31	45.75	.583	92.33	0.24	0.03	31.41	.858	94.68	0.19
Focus on gender differences in sex drive	0.48	27.72	.496	86.03	0.19	0.40	29.13	.531	93.06	0.24	0.19	33.31	.668	95.55	0.20
Focus on gender differences	0.94	101.22	.336	85.74	0.19	1.35	19.65	.259	92.73	0.23	2.21	10.40	.167	95.51	0.19
Gender of first author	1.81	77.35	.182	86.24	0.20	4.36	45.05	.043	91.98	0.23	0.04	32.50	.846	95.03	0.20
Mean author gender	0.04	55.58	.848	86.50	0.20	9.22	23.18	.006	91.36	0.21	0.04	18.08	.845	95.02	0.20
Publication status	2.19	37.81	.147	85.77	0.19	2.05	21.03	.167	92.26	0.23	1.08	13.78	.316	94.46	0.18
Sexuality journal	1.89	68.08	.174	86.49	0.20	0.71	33.56	.405	92.29	0.23	0.04	29.86	.851	95.01	0.20
Sample-level moderators															
Mean age	0.19	31.05	.664	85.07	0.18	0.37	5.80	.566	91.77	0.23	0.43	6.84	.533	93.32	0.21
Percent White	0.10	11.17	.759	88.18	0.21	0.29	4.26	.616	91.78	0.28	0.49	4.89	.515	88.91	0.20
Country-level gender development	2.74	40.36	.105	85.61	0.19	0.25	17.01	.623	92.06	0.24	0.82	19.32	.377	94.74	0.19
Country-level gender inequality	0.64	19.31	.435	85.07	0.19	0.01	35.59	.925	92.02	0.24	1.92	7.46	.206	95.00	0.19
Percent heterosexual	0.13	10.89	.728	88.61	0.19	1.43	12.60	.254	93.97	0.26	0.42	3.45	N/A	95.34	0.26
Average partnership duration in weeks	0.36	7.48	.566	74.72	0.18	4.16	2.03	N/A	83.31	0.26	2.33	1.74	N/A	60.95	0.16
Percent parents	0.76	6.18	.417	83.57	0.24	7.58	2.59	N/A	89.12	0.31	2.27	2.38	N/A	96.29	0.33
Country-level sex ratio	1.58	21.73	.221	85.79	0.20	0.51	12.37	.489	92.05	0.24	0.95	7.13	.362	95.19	0.20
Study restricted to sexually active	4.99	20.04	.037	80.26	0.16	0.15	8.60	.707	93.17	0.25					
Percent single	7.21	26.18	.012	85.80	0.20	5.75	12.72	.033	90.26	0.21	0.91	10.19	.361	94.59	0.24
Percent university students	0.33	11.63	.576	82.54	0.20	0.27	7.53	.616	87.00	0.22	9.54	7.80	.015	81.73	0.18
Study-level moderators															
Anonymity reassurance	0.53	85.80	.468	86.39	0.20	3.18	36.86	.083	92.42	0.24	0.97	33.62	.333	94.99	0.20
Participant compensation	0.63	48.85	.600	86.78	0.22	0.62	8.42	.618	90.77	0.27	1.94	2.98	N/A	93.68	0.26
Electronic data collection	0.10	54.90	.756	87.39	0.20	0.19	4.68	.830	92.10	0.22	0.00	35.56	.949	94.89	0.19
Group assessment	3.86	20.13	.038	87.13	0.19	0.96	3.52	N/A	93.78	0.23	0.48	5.23	.519	95.63	0.19
Personal contact	0.32	11.30	.730	86.08	0.19	0.06	8.22	.947	92.80	0.24	2.06	28.66	.162	94.88	0.19
Sexuality study	4.37	37.96	.043	82.05	0.16	0.00	20.29	.986	93.31	0.27	0.01	17.52	.944	91.77	0.16
Year of study	0.73	46.47	.397	86.44	0.20	0.10	15.91	.755	91.73	0.24	0.16	14.27	.698	94.99	0.20

Note. Tests for moderation of the sex drive manifestations. The tests indicate the significance of the slope for continuous moderators or differences between subgroups for categorical moderators. For cognition frequency, the results are statistically controlled for item content (extra-pair partner vs. any partner/no target). Results for the control variable are not reported. Some models could not be fitted because the number of available codings was insufficient. These are left blank. *AHZ* = Hotelling-*T*-approximated test statistic; *df* = small-sample-corrected degrees of freedom; *p* = *p* value associated with the test statistic and *df* in the same row; I^2 = proportion of the variation in observed effects that is due to the variation in true effects; τ = estimated standard deviation of the true effects. Note that if degrees of freedom fall below 4, significance tests are unreliable. *p* values for unreliable tests are not reported (N/A).

Further, gender differences were larger when studies used either group assessment, $g = 0.63$, or individual assessment, $g = 0.58$, compared to studies that used both types of assessment, $g = 0.38$, test for difference: $AHZ(20.13) = 3.86$, $p = .038$. However, this moderation finding is not straightforward to interpret, as one would expect the results for the "both" coding to fall between the other two if the pattern were meaningful. Finally, gender differences were slightly larger in studies that were not advertised as studies on sexuality, $g = 0.66$, compared to studies that were, $g = 0.55$, test for difference: $AHZ(37.96) = 4.37$, $p = .043$.

Affect Frequency. There were four significant moderation tests. The gender difference was larger when there was no "content" or target of sexual desire specified (e.g., "How often do you feel sexual desire?") compared to items that mentioned an unspecified partner (e.g., "How often do you feel desire for sex with a partner?"), $AHZ(39.15) = 4.24$, $p = .046$. Further, studies by female first authors revealed larger gender differences, $AHZ(45.05) = 4.36$, $p = .043$. In the same vein, research teams with a larger percentage of female authors found larger gender differences in AF, $AHZ(23.18) = 9.22$, $p = .006$. Further, the gender difference decreased when a larger percentage of participants were single, $AHZ(12.72) = 5.75$, $p = .033$. Four tests did not reach the threshold of $df > 4$ due to a low number of studies and effect sizes.

Behavior Frequency. For BF, only the percentage of university students in the sample moderated gender differences significantly, $AHZ(7.80) = 9.54$, $p = .015$, such that the gender difference was more pronounced when the sample included more university students. Six tests did not reach the threshold of $df > 4$.

Indicators of Latent Sex Drive

The results are summarized in Tables S4 and S5. For self-reported sex drive, there were too few studies and effect sizes to conduct meaningful moderation analyses. For sexual affect intensity, three

Table 5

Regression Tables for Moderation Analyses (Sex Drive Manifestations)

Moderator	Cognition frequency							Affect frequency							Behavior frequency						
	g	*SE*	*k*	*m*	*t*	*df*	*p*	*g*	*SE*	*k*	*m*	*t*	*df*	*p*	*g*	*SE*	*k*	*m*	*t*	*df*	*p*
Outcome-level moderators																					
Aggregation span																					
Intercept	0.31	0.08	30	46	3.84	3.96	N/A	0.36	0.09	36	70	4.08	6.56	.005	0.66	0.12	28	42	5.33	8.12	<.001
Slope	0.01	0.00			2.91	5.57	.029	0.01	0.00			2.12	9.81	.060	0.01	0.01			1.02	14.67	.323
Item content																					
Unspecified partner								0.40	0.06	20	20	6.87	18.08	<.001							
No target								0.57	0.06	36	54	10.29	31.59	<.001							
Item context																					
Alone															0.74	0.12	6	6	6.24	4.45	.002
Not specified															0.79	0.04	39	57	18.47	34.63	<.001
Type of response scale																					
No	0.59	0.03	157	268	18.51	47.43	<.001	0.56	0.04	55	82	14.26	48.84	<.001	0.82	0.05	29	32	15.20	24.27	<.001
Yes	0.43	0.10	8	14	4.29	5.89	.005	0.41	0.11	6	12	3.62	4.20	.021	0.72	0.06	19	31	12.45	15.91	<.001
Scale range																					
Intercept	0.51	0.07	155	265	7.02	40.78	<.001	0.80	0.19	54	81	4.17	18.38	<.001	0.67	0.21	28	31	3.23	7.47	.013
Slope	0.01	0.01			1.18	50.95	.243	−0.04	0.03			−1.31	22.19	.204	0.03	0.04			0.71	6.62	.500
Item wording																					
Daydreams	0.39	0.12	6	10	3.14	4.02	.034														
Fantasies	0.66	0.10	120	189	6.42	11.49	<.001														
Other	0.38	0.24	4	5	1.56	1.97	N/A	0.50	0.05	27	49	9.76	23.37	<.001	0.47	0.12	4	7	3.81	2.79	N/A
Thoughts	0.57	0.03	46	74	20.81	38.89	<.001														
Desire								0.59	0.05	35	45	11.23	30.16	<.001							
Masturbation															0.80	0.04	42	49	19.20	36.25	<.001
Self-stimulation															0.71	0.08	2	6	8.45	1.00	N/A
Publication-level moderators																					
Aim to find gender differences in sex drive																					
No	0.60	0.04	127	215	15.68	37.61	<.001	0.58	0.05	35	53	10.82	32.09	<.001	0.79	0.05	32	44	16.38	28.62	<.001
Yes	0.49	0.07	11	28	7.41	9.69	<.001	0.54	0.07	12	25	7.46	10.84	<.001	0.71	0.12	6	13	5.68	4.83	.003
Focus on anonymity																					
No	0.58	0.04	74	136	14.33	47.69	<.001	0.59	0.06	23	32	9.31	21.12	<.001	0.79	0.07	17	25	11.57	14.94	<.001
Yes	0.56	0.04	70	118	15.13	48.88	<.001	0.54	0.05	29	51	10.75	26.14	<.001	0.77	0.05	27	38	15.09	23.52	<.001
Focus on gender differences in sex drive																					
No	0.59	0.04	118	201	15.72	38.04	<.001	0.58	0.06	32	48	9.80	29.36	<.001	0.75	0.06	21	32	11.80	18.10	<.001
Yes	0.54	0.06	20	42	9.16	19.15	<.001	0.53	0.06	15	30	9.50	13.79	<.001	0.79	0.06	17	25	12.72	15.55	<.001
Focus on gender differences																					
No	0.60	0.04	55	93	13.99	45.78	<.001	0.65	0.09	13	21	7.46	11.39	<.001	0.68	0.06	8	11	11.68	6.48	<.001
Yes	0.56	0.04	83	150	15.38	47.88	<.001	0.54	0.05	34	57	11.04	31.35	<.001	0.80	0.05	30	46	14.97	27.01	<.001
Gender of first author																					
Female	0.60	0.03	57	102	17.75	49.38	<.001	0.61	0.05	33	47	11.48	30.39	<.001	0.79	0.06	16	20	13.43	14.50	<.001
Male	0.55	0.04	100	173	13.81	43.76	<.001	0.46	0.05	24	47	10.08	21.04	<.001	0.77	0.06	28	43	13.73	24.36	<.001
Mean author gender																					
Intercept	0.58	0.04	157	275	15.88	46.23	<.001	0.67	0.06	57	94	11.13	25.30	<.001	0.77	0.08	44	63	9.07	12.75	<.001
Slope	−0.01	0.06			−0.19	55.58	.848	−0.26	0.08			−3.04	23.18	.006	0.02	0.12			0.20	18.08	.845
Publication status																					
Published	0.59	0.03	132	220	17.97	52.77	<.001	0.58	0.04	42	61	12.91	38.90	<.001	0.81	0.04	33	42	18.69	29.57	<.001
Unpublished	0.52	0.04	29	62	11.79	29.43	<.001	0.46	0.07	15	33	6.87	12.74	<.001	0.69	0.10	11	21	6.75	8.60	<.001
Sexuality journal																					

(*table continues*)

Table 5 (*continued*)

Moderator	Cognition frequency							Affect frequency							Behavior frequency						
	g	*SE*	*k*	*m*	*t*	*df*	*p*	*g*	*SE*	*k*	*m*	*t*	*df*	*p*	*g*	*SE*	*k*	*m*	*t*	*df*	*p*
No	0.60	0.04	117	196	15.92	48.10	<.001	0.53	0.05	39	65	11.28	35.34	<.001	0.79	0.06	28	43	14.07	25.16	<.001
Yes	0.55	0.03	44	86	15.66	41.51	<.001	0.59	0.07	18	29	8.95	16.31	<.001	0.77	0.06	16	20	13.32	13.68	<.001
Sample-level moderators																					
Mean age																					
Intercept	0.61	0.10	137	234	6.27	41.80	<.001	0.42	0.20	52	83	2.11	8.45	.066	0.90	0.14	31	42	6.18	10.17	<.001
Slope	−0.00	0.00			−0.44	31.05	.664	0.00	0.01			0.61	5.80	.566	−0.00	0.00			−0.66	6.84	.533
Percent White																					
Intercept	0.71	0.12	47	79	6.03	8.17	<.001	0.52	0.10	19	31	5.30	3.08	N/A	0.68	0.18	10	16	3.73	3.61	N/A
Slope	−0.00	0.00			−0.31	11.17	.759	0.00	0.00			0.54	4.26	.616	0.00	0.00			0.70	4.89	.515
Country-level gender development																					
Intercept	−2.26	1.73	143	253	−1.31	39.64	.198	−0.74	2.60	55	91	−0.29	16.96	.778	3.50	3.00	41	60	1.16	19.07	.259
Slope	2.89	1.74			1.66	40.36	.105	1.32	2.63			0.50	17.01	.623	−2.77	3.07			−0.90	19.32	.377
Country-level gender inequality																					
Intercept	0.52	0.08	146	256	6.84	35.87	<.001	0.56	0.10	55	91	5.36	34.77	<.001	0.90	0.09	42	61	10.24	15.13	<.001
Slope	0.30	0.37			0.80	19.31	.435	−0.05	0.55			−0.09	35.59	.925	−0.65	0.47			−1.38	7.46	.206
Percent heterosexual																					
Intercept	0.62	0.14	81	150	4.31	9.31	.002	0.14	0.37	33	63	0.38	10.19	.712	0.61	0.19	24	37	3.19	2.94	N/A
Slope	−0.00	0.00			−0.36	10.89	.728	0.00	0.00			1.19	12.60	.254	0.00	0.00			0.65	3.45	N/A
Average partnership duration in weeks																					
Intercept	0.67	0.09	31	46	7.17	9.85	<.001	0.51	0.10	17	21	5.23	9.31	<.001	0.96	0.10	9	9	9.65	4.94	<.001
Slope	−0.00	0.00			−0.60	7.48	.566	0.00	0.00			2.04	2.03	N/A	−0.00	0.00			−1.53	1.74	N/A
Percent parents																					
Intercept	0.38	0.19	21	37	1.99	4.30	.112	0.36	0.13	12	17	2.75	8.91	.023	0.97	0.14	11	12	7.14	5.59	<.001
Slope	0.00	0.00			0.87	6.18	.417	0.00	0.00			2.75	2.59	N/A	−0.01	0.01			−1.51	2.38	N/A
Country-lvel sex ratio																					
Intercept	−0.56	0.91	146	256	−0.62	21.58	.544	2.04	2.09	55	91	0.97	12.17	.349	−1.44	2.29	42	61	−0.63	7.01	.548
Slope	0.01	0.01			1.26	21.73	.221	−0.01	0.02			−0.71	12.37	.489	0.02	0.02			0.97	7.13	.362
Study restricted to sexually active																					
No	0.62	0.04	97	172	15.70	35.13	<.001	0.56	0.05	36	64	11.24	33.13	<.001							
Yes	0.49	0.06	16	29	8.41	14.93	<.001	0.60	0.09	7	12	6.53	5.84	<.001							
Percent single																					
Intercept	0.51	0.04	88	158	12.93	32.26	<.001	0.62	0.06	42	71	11.00	21.85	<.001	0.72	0.10	33	47	7.05	12.45	<.001
Slope	0.00	0.00			2.69	26.18	.012	−0.00	0.00			−2.40	12.72	.033	0.00	0.00			0.96	10.19	.361
Percent university students																					
Intercept	0.54	0.09	80	132	5.78	9.34	<.001	0.47	0.10	25	39	4.62	4.81	.006	0.62	0.07	20	30	8.71	4.53	<.001
Slope	0.00	0.00			0.58	11.63	.576	0.00	0.00			0.52	7.53	.616	0.00	0.00			3.09	7.80	.015
Study-level moderators																					
Anonymity reassurance																					
No	0.58	0.04	92	164	15.43	46.42	<.001	0.61	0.05	30	43	11.37	28.01	<.001	0.75	0.05	25	33	14.51	22.88	<.001
Yes	0.55	0.04	49	86	12.40	41.64	<.001	0.47	0.06	20	37	8.51	17.54	<.001	0.83	0.07	19	30	12.57	15.92	<.001
Participant compensation																					
Course-credit	0.64	0.06	29	53	10.12	27.41	<.001	0.68	0.14	5	7	5.00	3.74	N/A	1.02	0.12	4	5	8.43	2.74	N/A
Material	0.54	0.05	35	64	10.62	29.14	<.001	0.57	0.10	12	26	5.63	10.66	<.001	0.60	0.09	7	13	6.54	5.88	<.001
Mixed	0.60	0.06	18	29	10.97	21.59	<.001	0.60	0.06	4	8	10.64	2.97	N/A	0.71	0.08	2	6	8.42	1.00	N/A
None	0.62	0.07	18	35	9.29	22.53	<.001	0.46	0.09	6	11	5.05	4.68	.005	0.87	0.08	7	10	10.30	5.81	<.001
Electronic data collection																					
No	0.59	0.05	36	53	10.93	35.42	<.001	0.57	0.07	13	18	7.99	10.89	<.001	0.79	0.06	20	27	13.32	17.38	<.001
Yes	0.57	0.03	96	181	17.12	41.31	<.001	0.55	0.05	34	58	11.06	31.43	<.001	0.80	0.06	21	32	13.46	18.36	<.001

(*table continues*)

Table 5 (*continued*)

Moderator	Cognition frequency							Affect frequency							Behavior frequency						
	g	*SE*	*k*	*m*	*t*	*df*	*p*	*g*	*SE*	*k*	*m*	*t*	*df*	*p*	*g*	*SE*	*k*	*m*	*t*	*df*	*p*
Mixed								0.68	0.18	3	5	3.73	1.85	N/A							
Group assessment																					
Mixed	0.38	0.07	10	13	5.51	9.70	<.001	0.52	0.05	4	8	10.67	2.84	N/A							
No	0.58	0.03	103	180	16.61	41.61	<.001	0.58	0.05	39	63	12.29	35.81	<.001	0.78	0.04	33	45	18.11	29.25	<.001
Yes	0.63	0.08	19	30	7.86	22.64	<.001								0.90	0.16	5	5	5.46	3.91	N/A
Personal contact																					
Mixed	0.56	0.06	5	8	8.99	3.82	N/A	0.58	0.07	4	8	7.82	2.87	N/A							
No	0.56	0.03	84	151	16.55	44.14	<.001	0.57	0.06	29	48	10.00	26.82	<.001	0.75	0.05	26	34	15.18	23.42	<.001
Yes	0.60	0.04	57	97	13.53	48.20	<.001	0.55	0.06	19	28	8.67	16.78	<.001	0.87	0.06	17	27	13.75	14.36	<.001
Sexuality study																					
No	0.66	0.05	25	42	12.44	21.37	<.001	0.61	0.05	12	18	11.46	10.30	<.001	0.79	0.09	13	20	9.11	10.01	<.001
Yes	0.55	0.04	44	87	14.96	26.67	<.001	0.61	0.07	21	39	8.60	19.45	<.001	0.79	0.05	23	31	16.59	20.62	<.001
Year of study																					
Intercept	9.65	10.64	152	266	0.91	46.36	.369	−5.25	18.27	56	92	−0.29	15.90	.778	−4.04	12.16	44	63	−0.33	14.24	.745
Slope	−0.00	0.01			−0.85	46.47	.397	0.00	0.01			0.32	15.91	.755	0.00	0.01			0.40	14.27	.698

Note. Metaregression tables for moderation of the sex drive manifestations. For categorical moderators, point estimates for subgroups and corresponding significance tests are presented. For continuous moderators, values are presented for the intercept and slope. For cognition frequency, results are statistically controlled for item content (extra-pair partner vs. any partner/no target). Results for the control variable are not reported. Some models could not be fitted because the number of available codings was insufficient. These are left blank. g = Hedges' g effect size (positive favors males); SE = standard error for Hedges' g effect size; k = number of studies per subgroup; m = number of effect sizes per subgroup. t value from t test testing the parameter against zero; df = small-sample-corrected degrees of freedom; p = p value associated with the t value and df in the same row. Note that if degrees of freedom fall below 4, significance tests are unreliable. p values for unreliable tests are not reported (N/A).

moderation patterns emerged. Gender differences were larger when the aggregation span for the response scale was larger (e.g., 2 weeks vs. 2 days), $AHZ(11.37) = 7.07$, $p = .022$. Item content also had a significant influence, $AHZ(23.05) = 15.76$, $p < .001$, such that gender differences were larger for desire for sex when no target was mentioned, (content = "no target": $g = 0.45$), and desire for masturbation (content = "masturbation": $g = 0.49$), and smaller for desire for sex with an unspecified partner (content = "unspecified partner": $g = 0.27$), or specifically one's own partner (content = "own partner": $g = 0.27$).

The context in which desire occurred was also relevant, $AHZ(14.16) = 21.41$, $p < .001$: Gender differences were very small for sexual desire in romantic situations, $g = 0.09$, small for desire while having sexual thoughts, $g = 0.23$, small to medium for nonspecified contexts, $g = 0.43$, medium sized for while spending time with an attractive person, $g = 0.50$, and medium to large for when first seeing an attractive person, $g = 0.67$.

Interim Summary

The comparably small number of significant moderation analyses despite the multitude of theory-driven and methodological moderator candidates coded (see Table 4) suggests that the gender differences in sex drive facets are remarkably robust. This view is further corroborated by a different perspective on the moderator analyses. Up to this point, we have discussed the moderator analyses as a function of sex drive facet (cognition, affect, behavior). To examine the robustness of a moderator, it is also informative to inspect whether a significant moderator in one facet also moderates gender differences in one of the other facets. The only moderator for which this was the case was the percentage of participants who were single. As this percentage increased, gender differences increased for CF and decreased for AF. All other moderators were significant for only one facet despite the facets being substantially positively correlated (Figure 3). No moderator was significant for all three sex drive facets. This further suggests that there are few substantial moderating factors of gender differences in sex drive.

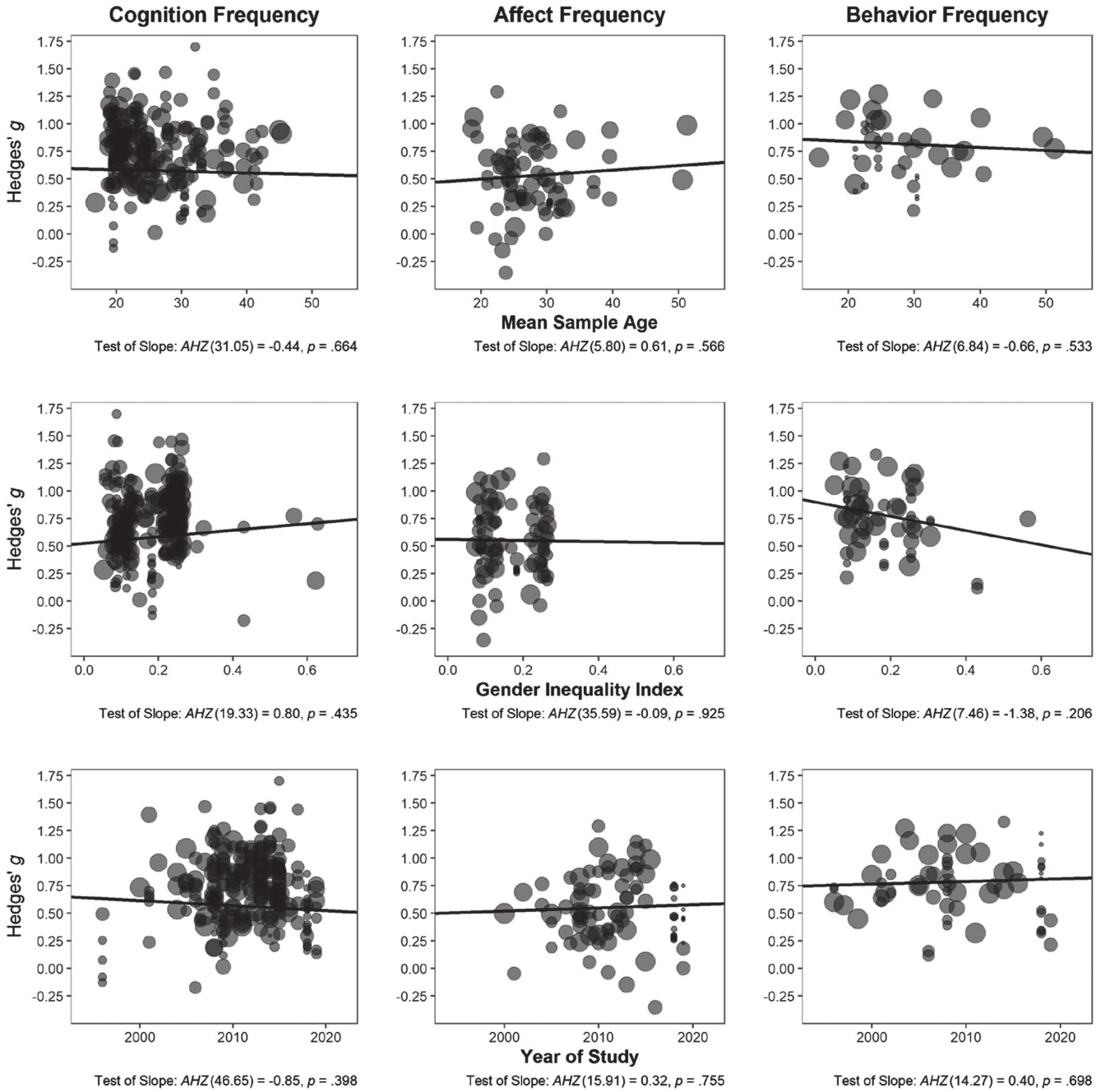
Discussion

Sex drive and particularly the notion of gender differences in sex drive have sparked considerable debate. This debate has been afflicted by underdeveloped conceptualizations and heterogenous measurements of sex drive, making it difficult to structure and compare the diverse findings. The present article seeks to make two substantial contributions—first, a theory-driven coherent conceptualization of sex drive, and second, a comprehensive meta-analysis of gender differences in sex drive that adheres to current best-practice standards for quality, reproducibility, and transparency (Lakens et al., 2016; Moher et al., 2009).

We understand sex drive as an individual's intrinsic motivation to obtain sexual experiences and pleasure. This latent motivation is expected to manifest in the psychological triad of sexual cognition, affect, and behavior, and to vary both within and between individuals. Building upon modern and integrative concepts of personality (Fleeson, 2001; Fleeson & Jayawickreme, 2015; Johnson, 1997; Roberts, 2009), we propose that individuals differ in their typical (trait) level of sex drive, without questioning intraindividual (state)

Figure 6

Moderation Results for Mean Sample Age, Gender Inequality, and Year of Study



Note. Depicted are scatterplots for metaregression analyses. g = Hedges' g effect size (positive favors males); $AHZ = AHZ$ value for the test of the slope; $p = p$ value for the test of the slope. The solid black lines represent the slopes of the metaregressions. Circle size represents the weight of the respective effect size in the RVE metaregression model. Darker circles are due to multiple, overlapping effect sizes. Higher values for the Gender Inequality Index denote higher inequality. AHZ = approximate Hotelling test.

variability. This conceptualization is not only rooted in seminal understandings of the nature of personality traits (McCrae & Costa, 2003; Roberts, 2009), but it also provides a clear rationale for deriving suitable indicators of sex drive: the frequency of sexual cognitions, affect, and behaviors.

The meta-analysis includes a total of 621,463 persons from 211 studies and 856 effect sizes. Overall, we found a stronger sex drive in men compared to women with a moderate-to-large effect size, g = 0.69, 95% CI [0.58, 0.81], confirming previous findings (Baumeister et al., 2001). Summary effects varied across sex drive

facets—that is, the three sex drive manifestations—from moderate for affect ($g = 0.58$) to moderate to large for cognition ($g = 0.76$) and behavior frequency ($g = 0.75$). A meta-analysis of within-study correlations between sex drive manifestations and indicators of latent sex drive provided evidence for our conceptualization's convergent validity. We also examined variables that should logically not reveal any substantive gender differences (e.g., total sex partners or one-night stands), and thus may be indicative of biased responding. Across multiple of these response-bias indicators, we found small gender differences on average ($g = 0.15$). We then subtracted the effect size for potential bias ($g = 0.15$) from the meta-analytic gender difference in sex drive ($g = 0.69$) to arrive at an estimate of the lower bound of bias-adjusted gender differences in sex drive: $g = 0.54$, a medium-sized effect. Since this may or may not constitute an overcorrection, we argue that a range of point estimates of $g = 0.54$ to $g = 0.69$ best represents our main finding (see the next section, for a discussion of possible biased responding).

To put this finding into perspective, we relied on natural language interpretations for this effect size range: overlap, nonoverlap, and probability of superiority. These interpretations indicated that, assuming normality, the distributions of male and female sex drive greatly overlapped (73%–78%), that the average man has a lower sex drive than 24%–29% of women, and that the probability of a random woman having a higher sex drive than a random man is 31%–35%. Particularly, the latter interpretation is quite intuitive: When a woman with an unknown sexual motivation walks down the street, she will on average exceed every third man she encounters in her drive to pursue sexual gratification.

We also applied the bias correction procedure to the summary effects within the subcategories to attain lower bound estimates for each indicator. After correction, gender differences were medium to large for cognition frequency ($g = 0.61$) and behavior frequency ($g = 0.60$), medium sized for affect frequency ($g = 0.43$) and self-rated sex drive ($g = 0.49$), and small for affect intensity ($g = 0.25$).

Analyses of effect size heterogeneity (I^2) showed consistently that 80% or more of the observed variation in effect sizes was not due to sampling error, but rather variation in the true effects. This is not surprising given that our analyses included very large studies, some with thousands of participants. There should be little sampling error in such large studies, so any excess variability will be attributed to true effects. When considering the absolute variation in true effects (τ) rather than the proportion of variation due to true effects (I^2), heterogeneity was average compared to other meta-analyses in psychology (Van Erp et al., 2017).

Apart from natural language interpretations of the summary effect, it can also be informative to compare empirical effects with benchmarks to put them in perspective (Funder & Ozer, 2019). In terms of common statistical effect sizes (g, Hedges, 1981), the obtained gender differences are considerably larger than many other gender differences in the domain of sexuality (Petersen & Hyde, 2010) and gender differences from a broad variety of other domains (Hyde, 2014), but of similar magnitude as some domains known to exhibit reliable gender differences, such as spatial cognition and physical aggression (Hyde, 2014). Even after conservatively correcting for potential gender-specific response bias, the effect sizes are also larger than most effect sizes in social psychology and research on individual differences (Gignac & Szodorai, 2016; Richard et al., 2003). Broadening the perspective to domains other than psychology, the effects are in a similar range as the gender difference in weight for U.S. adults ($d = 0.54$), but less than half the size of the gender difference in height for U.S. adults ($d = 1.81$; Meyer et al., 2001).

Although these comparisons of statistical effect sizes help to situate the present effects in the context of other bodies of literature, they leave the substantial question unanswered of what effect sizes of this magnitude really mean in everyday life. For example, it is unclear how these observed gender differences influence heterosexual dating behavior or the dynamics of heterosexual long-term relationships in the context of various other influences—such as socially learned behavioral patterns and expectations, the partners' impression management considerations, or the distribution of gender differences in sex drive across heterosexual couples. After all, we analyzed facets of sex drive that are usually not readily observable to others (cognitions, affect, masturbation behavior). Does sex drive manifest in observable behaviors in everyday life? And if so, how? How accurate are women's and men's perceptions of other's sex drive? These questions are pivotal, but they cannot be answered based on the current data. It is up to future research to answer these questions and to disentangle the actual effects of gender differences in sex drive from perceived gender differences in order to reveal the real-world implications of the present findings.

One key feature of the present meta-analysis is that it revealed gender differences in relative rather than absolute terms. On any absolute scale, it may be that both men and women have a high sex drive, and that men's is merely a little higher. Similarly, both men and women could be regarded as relatively low in sex drive on an absolute scale, women just somewhat lower than men. The key insight behind this observation is that the present findings by no means imply that women generally have a low sex drive or that men generally have a high sex drive. It is impossible to come to an absolute conclusion based on the present analysis (e.g., that men's sex drive is X times higher than women's sex drive).

Biased Responding

Sexuality is a sensitive topic, which begs the question as to what extent reporting biases may have influenced our results. Some evidence suggests that women tend to underreport and men tend to overreport permissive sexual attitudes and behaviors (e.g., Alexander & Fisher, 2003; Jonason, 2008; Mitchell et al., 2019), possibly due to different social punishments and rewards for these behaviors (Endendijk et al., 2020). In the case of the behavioral facet of our sex drive conceptualization, some evidence suggests that masturbation can be associated with shame and guilt for women (Kılıç Onar et al., 2020). To the extent that this is the case, this may bias reports about gender differences in masturbation. In light of these considerations, one may wonder: How likely are biased response tendencies to drive the gender differences found in the present analysis?

First, we argue that some of the sex drive facets derived from our conceptualization are less prone to biased reporting than other constructs for which bias has been previously documented (e.g., sexual attitudes or number of sex partners). For instance, for a woman who is concerned with not appearing too sexually permissive, it may be easier to report frequent sexual thoughts than to report having had many different sex partners. In addition, men may stand to gain little social status by reporting that they think about sex frequently and masturbate a lot. With respect to masturbation

specifically, a meta-analysis revealed no significant gender differences in attitudes toward masturbation ($d = 0.02$, Petersen & Hyde, 2010), suggesting that attitudes toward masturbation will not affect the genders differently. For men, masturbating a lot may be seen as nothing to brag about because it may indicate that a man cannot fulfill his sexual needs with actual sexual intercourse, but has to resort to masturbation. This would argue against a strong bias (or any for that matter) toward larger gender differences in masturbation that originated from biased responding. Consistent with the notion that masturbating a lot is not necessarily a desirable characteristic for men, a recent experimental study demonstrated a reversed sexual double standard for masturbation, such that men received social punishment for masturbating; they were seen as lower quality partners than women who masturbated (Haus & Thompson, 2020). In a similar vein, one reviewer suggested that in recent decades the public discussions have tended to encourage female sexuality, while (strong) male sex drive has frequently been viewed more critically, pointing to the possibility that sexual double standards may be shifting, at least in western societies. This could even lead females to overreport and males to underreport sexual thoughts, desires, and behaviors. Despite these preliminary findings, the question of if and to what extent the sex drive indicators used in the context of the present conceptualization are prone to bias measured gender differences toward larger or smaller values than warranted on the construct level is an important question for further research.

Second, our moderator analyses found no evidence for moderation by characteristics of the primary studies that should affect perceived privacy, such as group assessment or personal contact with the research team. Similarly, there was no evidence that gender differences have decreased over time. Had these differences been driven by biased responding, a decrease would have been plausible considering societal changes toward less restrictive social norms and attitudes toward sexuality.

Finally, we examined gender differences in several bias indicators that should theoretically exhibit little to no substantive gender differences in heterosexual populations (e.g., number of total sex partners or one-night stands). These analyses suggest that biased responding may have indeed played a role, but that this effect was small ($g = 0.15$ at most). The effect may be driven by social norms through unconscious or subconscious influences, such as memory errors, different estimation strategies, or differential accounting for "edge cases" of having had sex, but they may also at least partly be driven by self-presentation tendencies for men to overreport and/or women to underreport their sexual experiences. There are arguments to be made that subtracting this estimate of response bias from the gender difference in sex drive could be an overcorrection. Due to their characteristics (i.e., all behavioral; all but intercourse frequency typically found in the literature on sexual double standards, Endendijk et al., 2020), these bias indicators may be even more prone to (gender-specific) biased responding than the sex drive manifestation items. Also, it could be that these measures do not indicate pure bias, but that they partly reflect true differences due to undersampled subpopulations such as sex workers (vs. consumers of sex work) or gender-specific responses among homosexual persons (i.e., homosexual men may have sex more frequently and may have more sexual partners compared to homosexual women). We found no association between the percentage of homosexual participants in the sample and gender differences on the bias indicators. This speaks against the possibility that some of the gender difference we obtained for the bias indicators is valid, rather than pure bias, but does not rule it out. Yet, even when taking the full mean gender difference of these bias indicators as a proxy for the extent of motivated response bias and correcting for bias in the main analyses—a quite conservative approach—a substantial gender difference of approximately medium size (Cohen, 1988) remains. This indicates that the identified gender difference in sex drive is unlikely to solely be the result of biased responding.

Publication Bias

The academic incentive structure of recent decades has strongly favored the file-drawering of findings that did not reveal the hoped-for outcome (Nosek et al., 2012). As a result, publication bias is widespread in the social sciences (Fanelli, 2010, 2012). This is concerning, given that severe publication bias can strongly distort meta-analytic effect size estimates (Friese & Frankenbach, 2020). For several reasons, publication bias was unlikely to affect present meta-analysis. First, for maximum fit with our theoretical conceptualization, we extracted individual items from a diverse array of larger inventories. Thus, we analyzed a different subset of data than the primary researchers. Second, gender differences in sex drive were not focal to many of the original studies. This means that whether (and to what extent) gender differences in sex drive emerged was likely not relevant for many authors when deciding how to proceed with their projects once the data were analyzed. Third, we included unpublished data, which counteract publication bias.

Implications for Theory

In the introduction, we reviewed a set of psychological theories that either make predictions about a gender difference in sex drive or rely on its existence as a theoretical prerequisite. In this section, we discuss the implications of our findings for these theories.

Sexual strategies theory (Buss & Schmitt, 1993) posits that women have evolved to show more sexual restraint and selectivity than men because, for them, the evolutionary stakes are much higher in sexual encounters (i.e., women bear the biological risks and opportunity costs of pregnancy, childbirth, and infant care). The theory does not speak directly to gender differences in sex drive, but a stronger generalized motivation to pursue sex among men seems more plausible under the assumptions of sexual strategies theory than vice versa. Our results are consistent with this perspective.

Social role theory (Eagly & Wood, 1999) and social learning theory (Bussey & Bandura, 1999) state that men and women experience different social role expectations and social reward patterns, respectively. Empirical observation suggests that such differences in social context do indeed exist, such that the expression of sexuality tends to be encouraged for men but sanctioned for women (e.g., sexual double standard hypothesis, Crawford & Popp, 2003). Notably, both theories predict gender differences both on the construct level and on the measurement level. In other words, men and women may actually think, feel, and act in ways consistent with gender-specific roles and reward patterns (i.e., they may truly have different sex drives), but they may also "just" self-present in different ways in order to conform with their social context (i.e., they exhibit response bias). Our results provide support for both of

these possibilities: Men and/or women may not answer fully truthfully to questions regarding their sex drive, but we also found a substantial true gender difference in sex drive above and beyond biased responding.

SET (Baumeister & Vohs, 2004) is rooted in the assumption that men are more interested in sex than women and posits that, as a result, the negotiation and exchange of heterosexual sexuality follow the pattern of an economic marketplace, in which men offer resources to obtain sex from women. An empirical refutation of this assumption would have rendered the theory void of its first and most central tenet. Despite providing support for this particular tenet, we note that our findings on sex drive neither prove nor disprove SET itself. Our results confirmed a prediction that can be derived from the theory, namely that there should be gender differences in biased responding regarding the tallies of past sexual partners and sexual engagements due to the differential signaling implications for men and women (low tallies for women signal higher value of sex deserving greater male investment of resources; high tallies for men signal the ability to obtain sex through other resources).

Finally, the gender similarity hypothesis (Hyde, 2005) states that similarity between men and women is the norm and dissimilarity the exception. Our results suggest that, in addition to previously documented exceptions like physical aggression, mental rotation, or spatial perception, sex drive is another notable exception where robust gender differences exist.

Some Evidence for Moderation

Uncorrected gender differences were large for frequency of sexual cognition ($g = 0.76$) and behavior ($g = 0.75$), moderate for frequency of sexual affect ($g = 0.58$) and self-rated sex drive ($g = 0.63$), and yet somewhat smaller for intensity of sexual affect ($g = 0.40$). These differences could be rooted in the underlying temporal sequence of psychological processes that might mediate the emergence of sexual events: A sexual episode may start with some fleeting sexual affect or impulse, triggered by internal or external stimuli. This impulse may lead to more developed cognitions about sex, a sexual fantasy perhaps, which is then later enacted in solitary or partnered sexual behavior. Along this process, men and women may differ in their ability and/or motivation to inhibit sexual experience and behavior. It could be that processes further downstream are easier to regulate, that is, sexual cognitions and behavior are easier to regulate than affect. Accordingly, gender differences may be exacerbated for cognition and behavior compared to affect if, on average, women are more motivated or men less able (and motivated) to inhibit sexuality. Note, however, that this is speculative at this point. The temporal sequence of events could also typically start with a fleeting thought or fantasy (cognition) that sometimes develops into a sexual desire (affect). This would be in line with a recent experience-sampling study that found more frequent sexual cognitions than sexual affect, and more frequent sexual affect than sexual behavior (Weber et al., 2022a). Addressing such process-related questions would require more fine-grained data that allow examining the temporal sequence of the occurrence of events in everyday life.

Apart from the differences between the sex drive indicators, there was relatively little reliable evidence for any of the many theoretically derived and methodological moderator variables. The overall gender differences were remarkably stable. Nevertheless, despite this general impression of remarkable effect size consistency, a noteworthy pattern emerged for sexual cognitions: Gender differences were considerably more pronounced when sexual cognitions pertained to an extra-pair partner (i.e., a person the respondent is not in a relationship with; large gender difference) compared to an unspecified partner (medium-sized gender difference). This result is in line with previous findings on gender differences in sociosexuality (Lippa, 2009; Simpson & Gangestad, 1991). Other moderation findings were smaller in magnitude, and in several cases, tests barely crossed the significance threshold of $p = .05$. Type 1 error rates may be inflated due to multiple testing of moderators, so caution should be exercised in interpreting these findings.

Moderation patterns arose relating to the phrasing of questions. Items with a larger aggregation span (e.g., daily frequency of sexual fantasies over 30 days vs. 3 days) yielded larger gender differences for cognition frequency and affect intensity. A natural explanation for this effect is that more aggregation leads to more precise estimates and hence larger effect sizes. Alternatively, this pattern may also point to a previously undiscussed source of response bias. When participants retrospectively report how often or how intensively psychological states occurred over a period of time, longer time periods may involve more uncertainty, guesswork, and ultimately more response bias due to reliance on stereotypes: In the face of uncertainty, people may draw more heavily on perceived societal norms, which may reward disclosure of sexuality for men and punish it for women (Crawford & Popp, 2003). Larger observed gender differences for longer aggregation spans may thus either be closer to the true difference due to more accurate measurement or, instead, farther away from the true difference due to more response bias in line with societal norms. This is left for future primary research to find out.

There was an inconsistent association between gender differences in sex drive and the percentage of singles in the sample. A larger percentage of singles in the sample was associated with larger gender differences in sexual cognitions. For sexual affect, though, the pattern was reversed: A larger share of singles in the sample was associated with a smaller gender difference. This inconsistent pattern may result from some or all the following processes: Being single may (a) increase sexual cognitions in men, (b) decrease sexual affect in men, (c) decrease sexual cognitions in women, or (d) increase sexual affect in women. However, while theoretically interesting, the underlying processes remain speculative, and this potential three-way interaction pattern should be replicated and further illuminated in dedicated primary research.

Gender differences in sexual affect intensity were strongly dependent on the content and context of desire evoked by the questionnaire item. The difference was comparatively smaller in romantic situations ($g = 0.09$), and when a partner ($g = 0.27$) or the participant's long-term partner ($g = 0.27$) were referenced. The largest difference was obtained for items that gauged sexual desire "when first seeing an attractive person" ($g = 0.67$). Taken together, these observations point to the possibility that gender differences in sexual motivation may be larger when intimate relationships are not yet established and may decrease after the relationship has been initiated. However, future research needs to consolidate these possibilities with recent longitudinal evidence showing that gender differences in sex drive increase over the course of a long-term relationship (McNulty et al., 2019, see the discussion of partnership duration as a moderator below).

Apart from these isolated findings, no consistent moderation patterns were found. Some of the nonemergent moderator effects can be cautiously interpreted as evidence for the robustness of the results. For example, similar gender differences in sex drive emerged whether or not the study focused on gender issues and whether or not it focused on gender differences in sex drive (with the exception of a slightly less pronounced gender difference for CF when studies focused on gender differences in sex drive or aimed to find them). Likewise, similar effects emerged whether a study was advertised as a sexuality study to participants or as a study primarily concerned with a different domain (again apart from a slightly smaller gender difference for CF found in "sexuality" studies). As mentioned in the previous section, a range of other characteristics relating to perceived privacy in the study context (e.g., group assessment, personal contact with the research team) did not emerge as moderators.

For other factors, it was more surprising that moderation effects did not emerge. Previous longitudinal evidence suggested that gender differences in sex drive increase over the course of a relationship (McNulty et al., 2019), yet partnership duration did not emerge as a moderator in the present meta-analysis. We are hesitant to overinterpret this null-finding as meta-analytic analyses on the sample level have much lower resolution than dedicated participant-level work. We note, however, that the finding is consistent with a recent machine learning study showing that it is difficult to predict differential changes over the course of a relationship from baseline variables like participant sex (Joel et al., 2020). Neither age, year of study, nor gender inequality exerted a significant moderating effect: Gender differences in sex drive remain relatively stable across the life span, across countries with different gender inequalities, as well as across time, indicating that previous findings on gender differences in sex drive continue to hold true today (Baumeister et al., 2001). It is worth noting that there seem to be no changes in effect size over time during the covered period. This could be tentatively interpreted as supporting an evolutionary perspective on gender differences. If, in contrast, the gender difference was a primarily cultural product, the effect size should have changed (and become smaller) with a changing culture. Then again, progress toward gender equality (in terms of educational and economic attainment) has slowed down since the turn of the millennium (England et al., 2020). The lack of change in gender differences over time may reflect this development.

It would also have been plausible to expect that gender differences in sex drive vary across the life span. Moreover, both social learning theory (Bussey & Bandura, 1999) and social role theory (Wood & Eagly, 2012) predict that gender differences have decreased over time as gender stereotypes and gender inequality decreased. These theories also predict that gender differences are less pronounced in countries with less gender inequality. Our results are consistent with a meta-analysis of gender differences in sexual behaviors and attitudes, which found no moderation by age and year of study for masturbation (Petersen & Hyde, 2010). The findings are inconsistent with evidence from the same meta-analysis indicating a larger gender difference in masturbation in countries with greater gender inequality. Corroborating the present findings, a large-scale study across 53 nations also found no moderation by gender inequality for self-rated sex drive (Lippa, 2009).

Although the nonemergence of these moderating effects is theoretically surprising, they may have a mundane methodological explanation: range restriction. Year of the study only ranged from 1992 to 2019, which may not have been long enough to capture long-term cultural changes. Similarly, most data stemmed from countries with relatively high levels of gender equality, rendering tests for moderation by country-level GDI and GII less informative than desired.

One variable may potentially impact gender differences in sex drive but can unfortunately not be tested in our data: women's menstrual cycle phase. Women may be less likely to experience sexual cognitions, affect, and desire for masturbation during certain phases of their cycle. If this were to be the case, frequency-based indicators of sex drive may be biased toward lower estimates for women compared to men. By contrast, men's sexuality does not fluctuate along a stable monthly cycle. Relevant to this concern, a recent large-scale diary study based on more than 26,000 self-reports by more than 1,000 women found hardly any changes in both in-pair and extra-pair desire across 40 days for women using hormonal contraceptives. The study also found generally stronger in-pair desire among hormonal contraception users, but more pronounced in-pair and extra-pair desire around ovulation in naturally cycling women (Arslan et al., 2021; cf. Huang et al., 2020). These results thus suggest an increase in desire around ovulation rather than a decrease around menstruation. In the present meta-analysis, we coded the percentage of women using hormonal contraceptives, but this information was unfortunately not available for most studies, which precluded a formal moderation analysis. In any case, the fluctuations documented by Arslan et al. (2021) were rather small (around 0.2 on a 6-point scale for extra-pair desire and less than 0.2 on a 5-point scale for in-pair desire). Thus, at this point, the available evidence suggests that the gender differences found in the present analysis are unlikely to result from changes in the menstrual cycle. However, we do deem it important to examine the role of the menstrual cycle further for all three sex drive facets.

Future Directions for Conceptualizing Sex Drive

Our theoretical rationale for defining sex drive had two central pillars. The first pillar was that traits are relatively enduring patterns of thoughts, feelings, and behavior (McCrae & Costa, 2003; Roberts, 2009). The second pillar was the understanding of traits as intraindividual density distributions of psychological states (Fleeson, 2001, 2004; see Figure 1). One pathway to further develop this conceptualization would be to broaden the perspective beyond frequencies and consider further dimensions such as the intensity or even duration of sexual events. Such a perspective would call for a more fine-grained theoretical position on what characterizes a person with a stronger versus weaker sex drive. For example, some researchers may argue that a person with a stronger sex drive should not only experience sexual events more often but also more intensely and more enduringly. Other researchers may argue that a stronger sex drive will manifest itself in the more frequent experience of sexual events, but when a sexual event is experienced, there is no reason to believe that this should be more intense compared to a person with a lower sex drive. We leave it to future theoretical work to develop coherent and specific positions on these and similar questions regarding intensity and duration.

Such advancements, in theory, can also improve psychometric practices in sexuality research when the measurement is guided by theoretical work. In the present meta-analysis, almost no study

provided definitions of sex (or sex drive), so there was little control over what participants had in mind when responding. This can be a validity concern. For example, it could be that most people think of sex primarily in terms of penile–vaginal intercourse. One could argue that due to physical differences this experience is not equivalent for women and men, implying that the genders may have partly different experiences in mind when responding to questions indicative of sex drive.

Another intriguing issue pertains to the relative weight of each sex drive facet for the overall construct. The relative importance of cognition, affect, and behavior may differ between traits (Pytlik Zillig et al., 2002). Historically, traits have been defined primarily in terms of overt behavior (Pervin, 1994), suggesting a particularly strong weight for behavior. More recent definitions of traits have emphasized cognition and affect as additional central facets (Johnson, 1997; McCrae & Costa, 2003; Roberts, 2009).

In the current meta-analysis, we weighed each facet equally to compute the overall gender difference in sex drive. Arguments for other weights could be made: Sexual cognition (e.g., thoughts) is the most frequently used indicator of sex drive (Conley et al., 2011), suggesting a stronger weight for this facet. Conversely, some people may see an inherent association between sexual affect (e.g., desire) and sex drive, and indeed, sexual desire is often in the center of academic discussions around the concept (Dawson & Chivers, 2014). Nevertheless, behavior is also pivotal, since a person who thinks about or desires to feel sexual pleasure but never engages in solitary or partnered sexual behavior would hardly qualify as someone high in sex drive (although it should be noted that some people freely abstain from sexual behavior, e.g., for religious reasons). Finally, one could argue that there is a funnel-shaped hierarchy inherent to the conceptualization of sex drive proposed here: A sexual episode may often start with a cognition, perhaps only a fleeting thought. If time and circumstances allow, this thought may develop into a sexual desire. Again, only a subset of desires will eventually lead to actual behavior because a variety of reasons preclude individuals from putting every sexual desire into practice. One implication of this view could be to regard sexual cognition as the purest indicator of sex drive and consequently assign the largest weight to this facet, followed by affect and behavior.

As a final note, we did not connect the present conceptualization to extant research on sex drive in clinical contexts, and clinical studies were explicitly excluded from the analysis. It stands to reason that clinical phenomena such as hypoactive or hyperactive sexuality (Kafka, 2010; Kaplan, 1977) can be placed at the extremes of the sex drive continuum suggested by our conceptualization, but explicating this link is left to future research.

Limitations

In this section, we discuss questions that could be raised about the conclusiveness and implications of the finding that men's sex drive is on average stronger than women's. Some common methodological concerns have been already discussed in previous sections (for publication bias, see the Discussion section; for effect size dependency, see the Method section).

Response Bias

The present analysis employed various means to address the possibility of response bias due to gendered stigma regarding sexuality, including a correction procedure based on additional meta-analytic estimates. These considerations suggest that it is unlikely that the documented gender difference in sex drive is solely due to response bias, yet some uncertainty regarding the presence of biased responding remains and should be addressed in future dedicated primary research.

Limitations of Moderator Analyses

Despite a large number of participants and studies included in the review, some moderation analyses suffered from methodological limitations. For some analyses, unavailable codings reduced statistical power. Some moderators were subject to range restriction (e.g., most studies stemmed from countries with relatively low gender inequality), which can compromise regression analyses. Finally, the sample-level analyses we employed for some hypotheses (e.g., the association of sex drive and mean sample age) can have lower resolution than participant-level analyses.

Rate of Responses to Data Requests

Our method relied on data for individual questionnaire items, which was not directly available for most publications eligible for inclusion in the analyses. Missing data were requested from the original authors but could only be obtained for 39% of eligible publications. It is possible that mean gender differences in sex drive in the unavailable data are systematically different from the differences we observed in the available data.

Specificity of Sexual Cognitions

We found more frequent sexual cognitions in men compared to women. But how specific are these gender differences? One previous study found that men reported not only more sexual cognitions than women but also more other need-based cognitions referring to sleep and food (T. D. Fisher et al., 2012). Is it possible that the observed gender difference in CF is general and *not* specific to men's and women's sex drive? We deem this possibility unlikely. First, gender differences in CF were particularly pronounced for extradyadic cognitions, consistent with the ample evidence for gender differences in sociosexuality (Lippa, 2009). Second, a recent experience-sampling study in more than 200 young adults in committed relationships also found gender differences in sexual cognition, but not for other needs, including sleep and food (Weber et al., 2022a).

Generalizability

Psychological studies often examine people who are not representative of the world population, such as undergraduate students in Western countries (Henrich et al., 2010). This was also reflected in the present work. Most (but not all) studies were conducted in Western countries. University students, young adults, and White/Caucasian people were overrepresented. Future research on gender differences in sex drive should focus more specifically on older adults, as well as people of non-White ethnicities and people from non-Western cultures. Additionally, restricting the search to articles written in English or German may have introduced cultural or language-based biases. We also note that the present analysis did

not specifically address the sex drive of gender-nonbinary and transgender people. This should also be addressed in future work.

Conclusions

The key promise of meta-analyses is theoretical and empirical integration. The present work puts forth a coherent conceptualization of sex drive, grounded in trait theory, that directly translates into clear-cut indicators of the three postulated construct facets. Our meta-analysis documents that men's sex drive is stronger than women's, with a medium-to-large effect size ($g = 0.69$). Men think and fantasize about sex more often, experience sexual affect such as desire more often, and more often engage in solitary sexual behavior (masturbation). Biased responding may have inflated these differences, but is unlikely to fully account for the effect. The conservative, response-bias-corrected effect estimate is still of moderate size ($g = 0.54$). Natural language interpretations highlight that, despite the evidence for stronger sex drive in men on average, individual women exceeding individual men in sex drive is far from unusual.

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