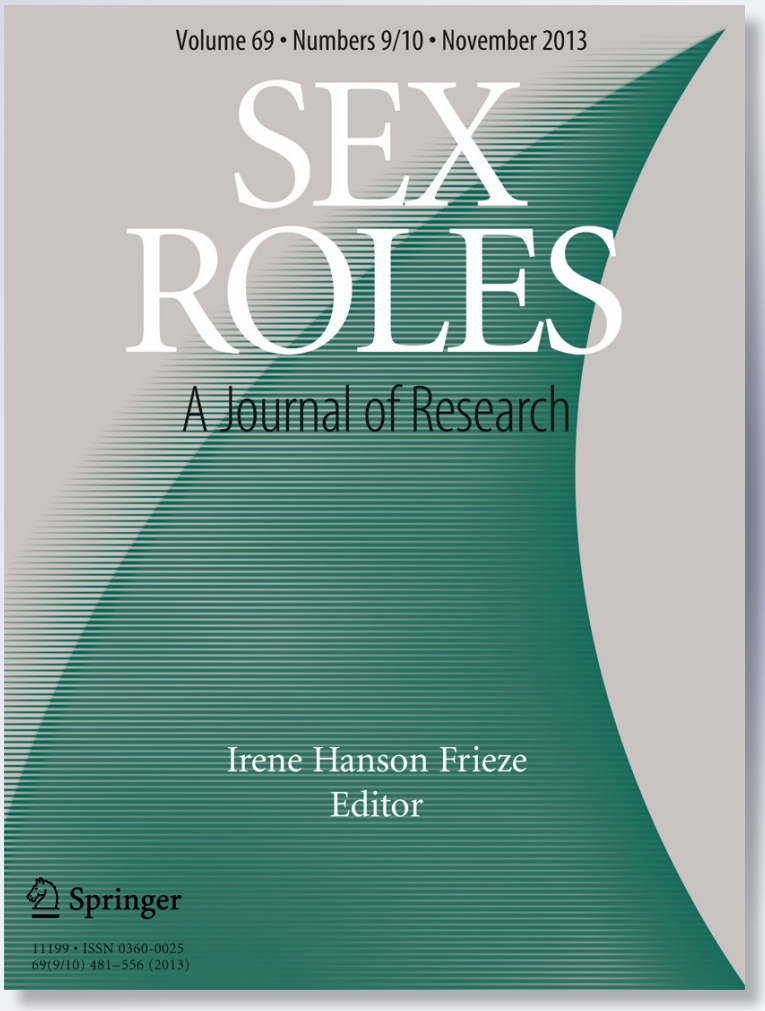
Addressing Conceptual Confusions About Evolutionary Theorizing: How and Why Evolutionary Psychology and Feminism Do Not Oppose Each Other

Charlotte Chuck Tate

Sex Roles
A Journal of Research

ISSN 0360-0025
Volume 69
Combined 9-10

Sex Roles (2013) 69:491-502
DOI 10.1007/s11199-012-0226-3



Springer

Sex Roles (2013) 69:491–502
DOI 10.1007/s11199-012-0226-3

ORIGINAL ARTICLE

Addressing Conceptual Confusions About Evolutionary Theorizing: How and Why Evolutionary Psychology and Feminism Do Not Oppose Each Other

Charlotte Chuck Tate

Published online: 9 October 2012

Abstract This commentary is a rejoinder to the Buss and Schmitt (2011) and Eagly and Wood (2011) commentaries concerning how evolutionary psychology and feminism might fit together. This rejoinder provides one path toward uniting these perspectives in psychological literature by accomplishing three tasks. First, this rejoinder addresses and removes conceptual confusions offered by Buss and Schmitt (2011) concerning the philosophy of science and psychology research. Second, this rejoinder fits evolutionary psychology perspectives into a correctly understood philosophy of science for psychology. Third, enacting Eagly and Wood's (2011) suggestions, this rejoinder connects evolutionary psychology and feminism by expanding Tate and Ledbetter's (2010) stepwise model for understanding the simultaneous contributions of evolutionary and developmental factors to psychological phenomena. Thus, by removing conceptual roadblocks, this rejoinder provides a scaffold for fruitful discussions concerning the interplay of evolutionary psychology and feminist perspectives when characterizing gender differences and similarities.

Keywords Evolutionary psychology · Feminism · Meta-theories · Philosophy of science · Stepwise contributions · Sexual strategies theory

Introduction

The Special Issue of *Sex Roles*: "Feminist Reappraisals of Evolutionary Psychology" *(Volume 64, Numbers 9–10 / May 2011)* contributed greatly to the discussion regarding the connection between evolutionary and feminist theorizing for psychological science. The empirical assessments of evolutionary positions in particular were a welcome contribution to the literature. It should also be noted that Eagly and Wood's (2011) commentary on the Special Issue was a well-developed and insightful attempt to advance the discussion beyond the often oppositional stances that feminist and evolutionary psychology perspectives take with respect to each other. However, as I argued in this rejoinder, certain conceptual confusions in the Buss and Schmitt (2011) commentary may inhibit the coming together of feminist and evolutionary approaches. Once the conceptual confusions are removed, I introduce new theorizing concerning (a) proper critiques and avenues of advancement for sexual strategies theory (SST; Buss and Schmitt 1993) and (b) an expansion of an alternative position that integrate both evolutionary psychology and feminism—namely, a stepwise understanding of the contributions of evolutionary and experiential influences on explaining psychological phenomena (see Tate and Ledbetter 2010), into which the Tate (2011, Special Issue) investigation fits. With these goals, this commentary may be viewed as a complementary position to the one developed by Eagly and Wood (2011).

C. C. Tate (✉)
Department of Psychology, San Francisco State University,
1600 Holloway Avenue,
San Francisco, CA 94132, USA
e-mail: ctate2@sfsu.edu

Philosophy of Science Distinctions and their Proper Roles

The Buss and Schmitt (2011) response to the Special Issue was importantly flawed in its depiction of the philosophy of science underlying the feminist and evolutionary psychology meta-theories. As I developed below, a major flaw was that Buss and Schmitt confused the meanings of *meta-theory* and *research theories* by casting SST as meta-theory when it is, in fact, a research theory by their own definitions of these terms. The confusion of these two concepts further affected the presented understanding of *research hypotheses*, and how they relate to empirical predictions. It is necessary to enumerate and

remove the many fallacies that followed from this flawed perspective because continued argumentation in that vein can only lead to further misinterpretations and misunderstandings of both evolutionary psychology and feminist psychology.

The Proper Role of Meta-Theories

Buss and Schmitt (2011) introduced the term "meta-theory" (p. 769) to the discussion of the connection between evolutionary psychology and feminism, and this term (and the concepts to which it points) need to be clearly delineated so that readers can truly appreciate the philosophy of science concerns at play. In the briefest statement, one might also consider the meta-theory the broadest top-down approach, or an umbrella under which many different perspectives can be collected—something akin to the largest conceptual theme that could collect any number of approaches. With this understanding, Buss and Schmitt (2011) are correct in asserting that there are many different evolutionary psychology (research) theories that exist within the meta-theory (see also Buss 2011). More particularly, one can view a meta-theory as the foundational conceptual apparatus that directs researchers' attention to the explanatory variables or ways to organize information that are internally consistent to the framework in which the meta-theory exists. Evolutionary psychology as a meta-theory directs researchers' attention to genetic factors that are ultimately influencing the areas of psychology that researchers find worthy to study (see also Eagly and Wood 2011). Importantly, the fact that any meta-theory directs attention to genetic factors as meaningful levels of analysis does not mean that the meta-theory itself is in principle falsifiable using the logic of falsifiability introduced by Popper (1959) and set-up earlier by philosopher-scientists such as Peirce (1934/1994). Instead, meta-theories are *more or less useful* given the efficacy of the levels of analysis to which they direct attention. In a sense, meta-theories are evaluated on how well they can continue to advance knowledge. To illustrate this point more clearly, one can consider the historical example of behaviorism as a meta-theory in psychological science. Behaviorism meets the definition of a meta-theory because it directed research attention to overt or manifest behavior only, not mental states (see Watson 1913). Several perspectives within this meta-theory existed, such as classical conditioning and operant conditioning (see Brennan 1998). Classical conditioning and operant condition can be viewed as *research theories*—or particular approaches within the larger behaviorism meta-theory. Stating the relationships in these ways illustrates what Buss and Schmitt (2011) mean by research theories—they are more specific approaches within the meta-theory framework. Though researchers differed in their use of any research theory within the behaviorism meta-theory, behaviorism dominated psychological research from the 1920s until the early 1960s (Brennan 1998). Yet, the behaviorist meta-theory gave way to a new meta-theory—cognition—a shift often referred to as "the cognitive revolution" (see Brennan 1998; Rychlak 1991). Importantly, behaviorism as a meta-theory was not disproven or falsified by the so-called "cognitive revolution" in psychology. Instead, behaviorism was relegated to a less useful status than cognition because it could not account for certain empirical results. Specifically, Tolman (1948) and Garcia (e.g., Revusky and Garcia 1969) conducted studies that could be best interpreted only if one allowed for the measurable existence of mental representations in rats and other animals that appeared to influence manifest behavior. However, the behaviorism meta-theory and all its research theories argued that no such focus on mental states was needed to conduct psychological science well (see Watson 1913; Skinner 1984). By demonstrating that one could measure something like a mental state, Tolman and Garcia argued for a new meta-theory to be added to the conduct of psychological science—namely, a cognition meta-theory (cf. Brennan 1998). Notice that Tolman and Garcia did not falsify the meta-theory of behaviorism; instead, they argued that the field of psychology needed another, more inclusive meta-theory to help describe the results they demonstrated regarding mental representations in animals, including humans.

One feature of meta-theories is that they help provide explanatory mechanisms in the form of building block constructs that serve as a trellis on which research theories and then operational definitions can be developed. For the behaviorism meta-theory, two major building block constructs were *stimulus* and *response*. Examining prominent works developed within the behaviorist meta-theory, one can see how these building block constructs were used to fashion the inner workings of classical conditioning (Watson 1913) and operant conditioning (Skinner 1984). In brief, stimuli always preceded and led to responses—which allowed for elaborated theories of learning based on reinforcement (e.g., Pavlov's dog, Skinner's box; Skinner 1984). It is worth noting that the building block constructs of any meta-theory are not tied exclusively to that meta-theory. For instance, psychology still uses stimulus-response reasoning—which was part of the behaviorist meta-theory—to describe many phenomena from the interplay of mental representations (e.g., Morsella 2005; Morsella and Bargh 2010) to the structure of automatic cognitive processing (Bargh and Chartrand 1999; Tate 2000), among other phenomena that fit into a cognitive meta-theory. Thus, behaviorism as a meta-theory was never falsified; rather, there was a shift away from its exclusive focus on manifest behavior even while many of its building block constructs (e.g., stimulus-response reasoning) remained and became part of new meta-theories.

The brief description and illustration of meta-theories above reveals two salient points that are important for interpreting the Buss and Schmitt (2011) commentary and advancing the discussion concerning evolutionary psychology and feminism.

First, when *meta-theory* is correctly understood, an evolutionary approach to psychology may be viewed as more or less useful depending on whether the levels of analysis to which it directs research attention help (or hinder) researchers when answering questions regarding psychological phenomena. Second, the direction of research attention to genetics as an important level of analysis does not by itself specify whether similarities or differences should result from any particular genetic patterns. Instead, research theories argue for such specificity.

To the first point, behaviorism can be seen as "falling out of favor" as the dominant meta-theory because it could not appropriately account for mental states and internal experiences as other, newer meta-theories could (see Brennan 1998). Similarly, evolutionary psychology is able to meet the same fate—as all meta-theories are. Recall, however, that even while the meta-theory itself can "fall out of favor," building block constructs may remain and be repurposed for use in other meta-theories. Thus, a focus on genetics as influencing psychological phenomena is not the exclusive purview of evolutionary psychology. To the second point, behaviorism focused researchers on overt behavior as the meaningful level of analysis. Within this focus, two very different *research theories*—classical conditioning and operant conditioning—were able to develop and flourish (for a time) and create competing research hypotheses and predictions (see Brennan 1998, for a description of where each conditioning theory diverged). Likewise, within the evolutionary psychology meta-theory, at least two different *research theories* can be developed and each can create different research hypotheses and predictions, such as sexual strategies theory (SST; Buss and Schmitt 1993) and dynamic interactionist theory (Kenrick et al. 2003; see Tate and Ledbetter 2010). Thus, evolutionary psychology is not monolithic, and there may be competing understandings of psychological phenomena *within* an evolutionary meta-theory—as Buss and Schmitt (2011) also note.

The Roles of Research Theories, Hypotheses and Predictions

Table 1 represents the different parts of the conceptual apparatus that can be described as the philosophy of science in psychological science, as well as the functions of these parts. Table 1 is my adaptation of what is listed in many undergraduate research methods textbooks in psychological science (e.g., Cozby 2007)—illustrating its fundamental place within research training. Table 1 uses the language of Buss and Schmitt (2011) so that it fits into the present discourse, and, being an adaptation of research methods discussions, Table 1 is generally consistent with Kuhn's (1962) arguments about the structure of scientific paradigms and sets up falsification criteria as outlined by Popper (1959). From Table 1, one can understand that the meta-theory is the foundation for a research theory, which is in turn the foundation for a specific research hypothesis, which in turn is the basis for empirical predictions. An empirical prediction is usually arrived at by a deductive reasoning process about the data arrangement under certain measured conditions (but see Tate 2011, Introduction, for a discussion of an *abductive reasoning* approach to empirical predictions that is consistent with Peirce 1934/1994). In any case, empirical predictions can be either falsified or supported. If the empirical prediction is falsified, one tends to lose confidence in the research hypothesis from which the prediction was derived. Since the hypothesis is part of a larger research theory, one can revise the research theory or discard it, depending on the discovery of a further phenomenon (revise) or the presence of an alternative research theory that can explain the same data pattern when the favored research theory cannot (discard). If the empirical prediction is supported, one tends to extend the research hypotheses to other questions and gain confidence in the research theory from which the prediction was derived. The research theory is updated to include the confirmatory or supportive cases, and new research hypotheses are generated and tested. This process is generally the one outlined in research methods textbooks used in the social and natural sciences (e.g., Popper 1959). Importantly, there is no mention of the meta-theory in the logic of falsification. This is sensible because the meta-theory is separable from the research theory and further separable from the hypotheses and predictions. As foundations, one can only ever view meta-theories as more or less useful given the outcomes of the research process at the levels of research theories, empirical predictions, and falsification.

Table 1 Simplified table of conceptual terms, functions for theory building and testing in psychological science

Term	Function
Meta-theory	On which variables does one focus explanatory attention?
Research theory	How does one organize the variables of focus provided in the meta-theory?
Research hypotheses	What should one expect as a general data pattern from the research theory in this particular case?
Empirical predictions	Given the constraints of methodology and measurement (viz. operationalization), what pattern of results should be expected to support or falsify the research hypothesis?
Statistical modeling/testing	Was the research hypothesis supported or falsified by the observed data arrangement?

Feminist Psychology Meta-Theory and Philosophy of Science

Feminism is also a meta-theory in the manner described above. Thus, feminism can be used to derive research theories, research hypotheses and empirical predictions within psychological science. A feminist psychology meta-theory focuses researchers' attention on the bundle of constructs that are described as "gender" (including any biological influences on these constructs) in order to gain information about psychological phenomena. Within a feminist psychology meta-theory there are also different research theories. Eagly and Wood (2011) describe two such research theories as difference feminism and similarity feminism (see their paper for details). Just as above, each feminist psychology research theory would generate research hypotheses that would further lead to empirical predictions, and only the latter is the subject of falsification (see Table 1). Even when falsification has occurred, it can only reverberate to the level of one of the research theories; it does not directly affect the meta-theory. The meta-theory of feminist psychology can only be judged to more or less useful given the efficacy of focusing on gender as aiding scientific understanding of psychological phenomena.

Evaluating the Two Meta-Theories and the Beginnings of Confusion

From this construal of meta-theory, both the feminist psychology and evolutionary psychology meta-theories seem useful at present. Eagly and Wood (2011) noted many of the information gains that have occurred from a feminist psychology meta-theory. Similarly, Buss and Schmitt (2011) noted many of the information gains that have occurred from an evolutionary meta-theory. Yet, these commentaries diverge in important ways after this enumeration. As I will develop in the next section, the Buss and Schmitt commentary conflated the concepts that are separated in Table 1—even though they introduced these distinctions themselves. Rather than focusing on the appropriate level of analysis concerning the support for or falsification of empirical predictions from SST presented in the Special Issue, as I will develop in the next section, Buss and Schmitt (2011) created an inaccurate and misleading argument about how meta-theories are influenced by the aggregation of knowledge. Moreover, it is my contention that such inaccuracies contribute to the perceived fundamental rift between evolutionary psychology meta-theories and feminist psychology meta-theories, even though, as I develop below, there is no such rift at the level of meta-theory. In fact, the difficulties may only exist at the level of certain research theories and certain research hypotheses.

Roadblocks to True Integration of Evolution and Feminism: The Buss and Schmitt (2011) Logical Fallacies Enumerated

Fallacy #1: Research Theories are Not Meta-Theories

Buss and Schmitt's (2011) main contention in response to the Special Issue articles is a persistent confusion between the meta-theory of evolutionary psychology and the particular research theory of SST. Buss, in particular, has made this confusion before (see Buss 1995, 1998; Confer et al. 2010) and its ubiquity continues to hinder useful philosophy of science discourse. This confusion may in fact be a major source of the distrust of evolutionary psychology in general among students and researchers. The irony is that while Buss and Schmitt (2011) are correct in asserting that much misunderstanding surrounds the evolutionary approach to psychology, Buss, in particular, appears to be a purveyor of this misunderstanding via faulty logic and inappropriate criticisms of those who would offer different interpretations of evolutionary psychology at the level of research theories—not meta-theory.

One specific example of inappropriate criticism and faulty logic is revealed through Buss and Schmitt's (2011) reaction to the falsification of specific empirical predictions derived from SST by Special Issue authors Harris (2011), Pedersen et al. (2011), and Tate (2011). The faulty logic lay in the juxtaposition of two claims—one that is true and another that is not. The true claim that Buss and Schmitt make is that the falsification of specific empirical predictions derived from SST by Harris, Pedersen et al., and Tate does not affect the meta-theory of evolutionary psychology. As I have developed above, this statement is formally correct because lack of support for predictions only reverberates up to research theories. However, Buss and Schmitt (2011) continue their argument by invoking a faulty claim. Buss and Schmitt erroneously argue that because SST (a research theory) has support on many other predictions, it cannot be importantly impacted by evidence mounting against specific empirical predictions. Buss and Schmitt (2011) go so far as to write: "the burden of proof must fall on those who contend that these gender differences do not exist, and those who do so must deal with the entire corpus of empirical evidence rather than cherry-picked fragments of that body of evidence" (p. 782). In this way, Buss and Schmitt allow a reader to believe that SST is somehow like a meta-theory. At this point, one can see their faulty logic is actually two specific errors combined: (a) a misrepresentation of the distinctions they introduced, and (b) introducing irrelevant information for the argument at issue. The first error (misrepresenting distinctions) is that gender differences are predicted by many different theories, including feminist research theories (see Eagly and Wood 2011). Thus, claiming gender differences as unambiguous support for evolutionary psychology in general or SST in particular is a

rhetorical, rather than substantive, move. Moreover, as their quote demonstrates, Buss and Schmitt invite readers to believe that SST is a truly meta-theory because it is apparently immune to the falsification that might happen on its empirical predictions. Otherwise, why state that researchers "must deal with the entire corpus of empirical evidence rather than cherry-picked fragments of that body of evidence" (Buss and Schmitt 2011, p. 782)? Yet, SST is not a meta-theory; it is a research theory. Research theories must be amenable to falsification processes, which is the same as saying they must have hypotheses and empirical predictions that follow from them that can be tested using research methods and statistical analyses. The Special Issue authors Harris (2011), Pedersen et al. (2011), and Tate (2011) did test some of the empirical predictions generated by SST. Thus, these SST predictions are subject to falsification—as any research theory's empirical predictions are. It is not "cherry-pick[ing]" (to paraphrase Buss and Schmitt 2011, p. 782) to conduct falsification incrementally—or one empirical prediction at a time. In fact, this incremental procedure appears to be described by Popper (1959) in the logic of falsification itself.

The second error (introducing irrelevant information for the argument at issue) is that Buss and Schmitt's (2011) exposition allows a reader to believe that other research hypotheses generated from SST are related to each other when considering the falsification process. Yet, Popper's (1959) arguments imply that different research hypotheses and their empirical predictions do not affect each other at the level of falsification. That is, falsification works on specific empirical predictions, not on other predictions that have not been tested. For example, the desired number of sexual partners has no logical dependency on desired qualities in sexual partners or any other research hypotheses that lead to empirical predictions from SST. Furthermore, predictions are falsifiable because they only refer to particular data patterns under a subset of conditions (see also Pierce 1934/1994; Rychlak 1994). Thus, to argue that other predictions about entirely separate data patterns with entirely separate subsets of conditions have any bearing on the results of a specific test of an empirical prediction is a spurious argument. By virtue of this spurious reasoning, the Buss and Schmitt (2011) commentary only served to derail the discussion in a manner that allows proponents of SST to believe that they may simply deny any evidence against specific predictions using the "weight of the large body of empirical evidence" across all SST research hypotheses and empirical predictions (presented as Buss and Schmitt 2011, Table 1, p. 774). The larger irony of this situation is that Buss and Schmitt praised Harris (2011), Pedersen et al. (2011) and Tate (2011) for conducting empirical tests of SST only to deny the outcomes of these tests. In effect, Buss and Schmitt insinuate that only confirmatory tests of SST are meaningful. The most unfortunate consequence of this reasoning is that Buss and Schmitt perpetuate confusion about whether research hypotheses in evolutionary psychology can be falsified empirically. Ironically, Buss (1987, 1995) has assiduously tried to state that SST is empirically testable (and therefore falsifiable) since the early days of proposing this research theory.

Fallacy #2: Theory Testing is Not Time-Dependent

Buss and Schmitt's (2011) commentary introduced another spurious argument that can derail useful discussion on the intersection of evolution and feminism. This spurious argument can be viewed as one of anachronism or, colloquially stated, "we got there first." Buss and Schmitt (2011) encourage readers to view SST with special status (above other research theories) because SST proponents made certain empirical predictions before other theorists did. Buss and Schmitt (2011) note that "Sexual Strategies Theory provided the first psychological theory of mating to hypothesize an array of diverse mating strategies within the human evolved arsenal" (p. 772) and then go on to state that there has been "a tremendous volume of empirical evidence has accrued that support various elements of it central tenets" (p. 772). Yet, this is another rhetorical, rather than substantive, move. Such a statement is tantamount to suggesting that because Newton proposed gravity as attractional force before Einstein proposed gravity as the curvature of space-time that we should give Newton's theory special status and view the Newtonian theory as more correct. (Also recall that Einstein was born 152 years after Newton died.) Research theories, hypotheses, and empirical predictions need to be evaluated in a manner that does not allow time-of-proposal to be an important consideration for whether one accepts or rejects them. No important scientific advancements could be made if researchers viewed the first propositions as the most correct in the absence of evaluating and re-evaluating them empirically over time (cf. Kuhn 1962).

Fallacy #3: No Alternative

Among the most serious logical fallacies that Buss and Schmitt (2011) commit in their defense of SST is never once enumerating an alternative explanation to SST—even in principle. Since Buss and Schmitt do not separate meta-theory from research theory, these authors may see no need to entertain alternative explanations, since, when understood correctly, meta-theories do not have alternative explanations as such. Thus, Buss and Schmitt (2011) allow an interested reader of the discussion to believe that SST's empirical predictions are immune to alternative explanations, when, in fact, no empirical prediction is immune to alternative explanations (see Rychlak 1994; cf. Popper 1959; cf. Pierce 1934/1994). Related to this point, Buss and Schmitt's (2011) Table 1 is a list of *empirical predictions*—all of which are subject to alternative

explanations. In point of fact, the findings presented in Buss and Schmitt's (2011) Table 1 illustrate that SST is plausible, but provide no "weight of evidence" to the extent that rarely have any of those studies listed in that Table tested an alternative account to these findings. One might consider Buss and Schmitt's cross-cultural research (e.g., Buss et al. 1990; Schmitt 2003, 2005) to be an attempt to eliminate alternative explanations. Nevertheless, because the evolutionary psychology meta-theory also necessitates evolutionary processes operate more or less evenly on the species when it first branched from a common ancestor, then humans are very similar to each other across cultures in their evolved psychology (see Buss 1995; Buss et al. 1990). Thus, the cross-cultural research is not really testing an alternative explanation; instead, it is testing the efficacy of the generic evolutionary approach to psychology. Yet, Kasser and Sharma (1999) did offer an alternative explanation to the specific findings from Buss et al.'s (1990) 37 cultures data. Kasser and Sharma showed that empirical predictions derived from SST were only supported when there were large social, economic, educational and other disparities between gender groups (as measured by the World Health Organization indicators). If one attempted to minimize the Kasser and Sharma findings as merely showing the flexibility of manifest behavior even while genetic differences from "deep evolutionary time" (Buss & Schmitt, p. 769) are presumed to be operating behind the scenes, this response would be a weak deflection at best because it does not address the possibility that the original 37 cultures data set findings were the result of a statistical aggregation bias—capitalizing on asymmetrical variability across the countries in that data set (as Kasser and Sharma 1999 appear to demonstrate). Merely considering the possibility that some findings presented as support for SST might actually reflect research methods or statistical biases would benefit theory-building and theory-testing in psychological science.

Fallacy #4: Physiology is Not Genetics (and the Two are Separable)

Buss and Schmitt (2011) attempt to critique several Special Issue authors by arguing that "[l]earning and culture are almost never true alternatives to evolutionary psychology" (p. 775)—a paraphrasing of Tooby and Cosmides (2005). Yet, the Buss and Schmitt statement is oversimplified. The subtler point that Tooby and Cosmides (2005) are making—the one that Buss and Schmitt did not convey—is that culture cannot be an alternative explanation that is separate from biology when "biology" is understood as a combination of physiology and genetics. A bevy of investigations has demonstrated that much of psychological functioning has clear and regular physiological correlates (Cacioppo et al. 2007; Decety and Cacioppo 2011). For example, the whole field of *cultural neuroscience* is devoted to enumerating the connections among brain physiology, mental states, and behavior for questions that intersect with culture (see, e.g., Kitayama and Park 2010). The subtle point is that behavior is only sometimes modeled as having a direct genetic link through physiology because physiology can sometimes be influenced by environmental input only—and this environmental input is what researchers call learning or culture. Take the example of expert birdwatchers who use the fusiform gyrus—an area of the brain located in the occipito-temporal cortex—to visually process different types of birds (Gauthier et al. 2000). The fusiform gyrus is also where humans process facial information about other humans—thus it was at one time referred to as the fusiform face area as it was believed to be specialized only for human face processing (cf. Gauthier and Curby 2005; Ruffman and Perner 2005). Seeing the activation of the fusiform gyrus for expert birdwatchers when identifying birds provides no convincing argument that their birdwatching expertise is genetic—even if one believes the brain area has specialized functions. To infer the primacy of genetic programs in this case, one would have to demonstrate that expert birdwatchers are (a) genetically different from non-experts and (b) that this genetic difference causally predisposes them to watch birds more often or differently than non-experts, and then (c) the genetic difference affects the fusiform gyrus such that it aids these experts selectively, and not those who lack the "birdwatching gene." This argument, however, seems untenable—not because of the number of steps involved for it to be demonstrated—but because a salient alternative interpretation of these findings exists. The alternative is that for people who began birdwatching (for some psychosocial, cultural or experiential reasons), practice effects at the skill are evident in one's physiology and anatomy—in this case increased activation in the fusiform gyrus as observed by Gauthier et al. (2000). Thus, for Buss and Schmitt (2011) to allow readers to believe that learning or culture is tantamount to the unfolding of specialized genetic programs for specific tasks without substantial, independent modification of physiology or anatomy from experience is inconsistent with brain research that shows that some physiological differences between people are based on expertise with the environmental inputs (e.g., Gauthier et al. 2000; see also Gauthier and Curby 2005). Of course, genes may set-up flexible programs on which individual learning supervenes (see Ruffman and Perner 2005); yet, because of this, physiology may not provide a one-to-one link to specialized genetic processes. It appears that physiology conveys some amount of information from genetic and environmental sources simultaneously. The relative proportions of these sources likely vary across psychological phenomena.

Furthermore, invoking the idea that some behavior is adaptive as a response to particular selective pressures—as SST theorizing does to identify which behaviors are the focus of research (see Buss and Schmitt 2011; Confer

et al. 2010)—does not circumvent the larger issue that physiology contains simultaneous genetic and environmental information. Admittedly, birdwatching is likely not a source of selective pressure from an SST perspective because it does not implicate survival or reproduction. Thus, SST would actually remain moot about the origin of expert birdwatching. Yet, the birdwatching example can be used to elucidate core elements in the reasoning style invoked by SST. For example, how does one determine the relationship of a behavior to survival and reproduction? The answer is not obvious insofar as I could argue that people who could expertly identify birds could also kill them more easily—thus promoting survival and reproduction in the ancestral past. On this argument, this is why expert birdwatchers still exist today. However, my survival and reproduction story regarding birdwatchers is probably less useful than an alternative explanation which argues that people who watched birds repeatedly (for fun or social bonding), and thereby became experts, have this expertise reflected in their functional brain anatomy. Again, the alternative explanation is more viable not because of the amount of steps in the explanation, but because there is no evidence suggesting that expert birdwatchers genetically differ in systematic ways from non-experts even though the groups differ physiologically in their use of the fusiform gyrus when perceiving birds. The fact that one can create a coherent account of physiological differences across people that does not invoke genetic predispositions (as demonstrated here with birdwatching) makes plausible the idea that even some phenomena that might be described as being relevant to reproduction or survival (i.e., presumed adaptations), in principle, at least, have alternative physiological explanations. Taking this reasoning one step further, even when groups might genetically differ from each other, physiological differences that might correlate with some psychological phenomenon may or may not reflect the operation of genetic programs; they could, in principle, reflect experiential differences between the groups. Crucially for proponents of SST, the level of analysis is genetic (as Eagly and Wood (2011) correctly argue), but, as one can see from this discussion, physiology is separable from genetic influences.

To be fair, it is formally correct to argue that genetic predilections posited by SST are in fact responsive to environmental inputs, and Buss and Schmitt (2011) state this in their commentary. Yet, this also means that any researcher should be able to disentangle the two different signals (genes and inputs) in any research study. However, by imprecisely citing Tooby and Cosmides (2005), Buss and Schmitt (2011) seem to contradict their stated position. Even if one allows culture to never be an alternative explanation to biology, there is still a need to identify distinct sources of variability and track how each contributes to the phenomenon under investigation. Nevertheless, Buss and Schmitt (2011) provide no useful manner by which to do this, nor do they appear to accept that such a separation has happened unless the results fit into the SST research hypotheses. To summarize, SST appears to have a three-part problem. One, SST argues for gene contribution to certain psychological phenomena with no assessments of genes (see Tate 2011, Introduction). Two, SST rarely assesses physiology or functional brain anatomy (each of which is demonstrably influenced by experiential factors in addition to genetic factors) to disentangle genetic and other influences on psychological functioning. Three, SST only has a well-repeated story about how gender differences could have been advantageous for survival and reproduction without providing an alternative explanation for the gender differences—even in principle.

Advancing the Discussion About Evolutionary Psychology and Feminism: Separately and Together

Taking the balance of the points above, it appears that a real discussion of the issues was not facilitated—nor can it easily be facilitated—by the Buss and Schmitt (2011) commentary on the Special Issue. Accordingly, in this section, I enumerate (a) a more useful way to argue for SST given an accurate portrayal of it as a research theory, not a meta-theory; (b) the ways in which the meta-theory of evolutionary psychology is commensurate with feminism as a meta-theory; and (c) how scholars can move forward given points (a) and (b).

The Features of SST as a Research Theory

Buss and Schmitt (2011) are correct to point out that focusing on manifest behavior is only part of the evidence for or against SST because manifest behavior is multiply determined. For SST as a research theory, the main issue is that sometimes the "signal" from the genetic differences posited can be dampened or masked by focusing on a level of analysis too far away from the gene-level (e.g., manifest behavior) (cf. Buss 1995). It is for this reason that in the Special Issue Tate (2011) focused her level of analysis on desires and wants, which may lead to *behavioral intentions* that may or may not come to pass based on other constraints. This is also consistent with the original Buss and Schmitt (1993) formulation. Consequently, if scholars critique SST on the grounds that actual behavior is not enacted as SST predicts that it could be, such critics miss the point of SST: It refers to the desire to act, not the trappings of actual behavior.

SST is a gene-centered theory, and to appropriately test its empirical predictions, the methods of assessing genetic differences should become more advanced. Yet, SST studies still rely on self-reported gender as a proxy for genetic differences between women and men. As Tate (2011) argued, assessing

self-reported gender is actually a report about physiology and how genital anatomy was influenced in utero (see also Hines 2004). Since the genes are the locus of the presumed desires for SST, the best-case scenario would be to conduct research that groups people based on their actual chromosomal arrangement (e.g., XX, XY). This task is admittedly expensive at present. Yet, short of being able to accomplish the direct gene-level assessment, one could at least separate birth-assigned sex category (a statement about anatomy) and current gender identity (a statement about one's self-categorization) when asking participants to self-report information that will be used to group data for analysis. As Tate (2011) argued, people are likely reporting one or both of these separable pieces of information, but the current, widely used demographic question *What is your sex?* Or *What is your gender?* cannot disentangle these referents. It is for this reason that Tate et al. (2012) recently developed a two-question method of asking respondents to self-report gender that explicitly asks about current gender identity and birth-assigned sex category. To better evaluate SST, analyses could be conducted using birth-assigned sex category answers within the Tate et al. (2012) two-question method. This procedure would not avoid the fact that birth-assignment is influenced by prenatal hormones—which are separable from genes (e.g., Hines 2004; Hines et al. 2003). However, one would be able to argue more consistently about the physiology relevant to the psychological phenomenon since researchers know that birth-assignment of female and male are based on specific anatomical structures that result from specific hormonal influences.

Likewise, statistical analysis strategies need to respect the fact that the genetic level is the meaningful focus for SST. In the Special Issue, Tate (2011) respected this point with her multiple regression analysis as a way to understand the relative contributions of the (proxy for) genetic and the psychosocial and experiential variables. Importantly, the signal from genetics should still be detectable to the extent that even the single-question self-report of gender can carry some genetic information about sex chromosomes. Thus, given the methods available at the time of the study, Tate's (2011) investigation was a fair test of SST as a research theory because it did not bias a researcher against the possibility of finding any number of data arrangements that could be interpreted consistent with a genetic contribution to the findings. The same logic applies to Pedersen et al. (2011) and Harris (2011) investigations in the Special Issue.

To further evaluate SST, researchers are encouraged to use extensions of regression analysis, such as sequential modeling, multilevel modeling, path modeling and associated techniques to showcase the relative contributions of genes to the psychological phenomena of interest, and allow for more complex understandings, such as the nesting of genetics within physiological patterns. Using extensions of multiple regression analysis allows researchers to detect the genetic signal relative to other signals in the data sets, irrespective of whether the other variables included in the model are considered alternative explanations or simply additional sources of variance in the measured response. Using more sophisticated techniques, researchers could also begin to model differences between chromosomal sex groups in a mediated moderation analysis (Preacher and Hayes 2008, Figure 2.3, p. 34, for examples) with different types of "environmental inputs" as mediators or moderators (depending on the research question). Such analysis plans and research methods precision should be the future of SST because researchers need to establish the reliable link between chromosomal sex groups and some psychological phenomenon (e.g., jealousy, desired number of sexual partners) before they can then explore how the contours of environmental inputs (generally speaking) affect this same phenomenon. At this point, however, SST is still at the stage of establishing such a link using the proxy of the single-question self-report of gender for certain dependent measures, such as desired number of sexual partners (see Pedersen et al. 2002, 2011; and Tate 2011, for the inconsistency of this specific link). As developed above, scholars should remember that self-reporting gender using a single-question (with only two responses [*female*, *male*]) also appears to feature experiential and psychosocial variables that contribute to the response (Tate 2011; Tate et al. 2012). It may be exactly these non-genetic variables that account for far more variability than the unique variance attributable to the imprecisely measured proxy for chromosomal sex (see Tate 2011).

The Ways in Which the Meta-Theories of Evolution and Feminism are Commensurate for Psychological Science

To lay a foundation for commensurability between the evolutionary psychology meta-theory and other meta-theories in psychology, Tate and Ledbetter (2010) briefly developed an argument that evolutionary processes can be modeled as stepwise contributions to psychological phenomena in combination with experiential and psychosocial factors. "Stepwise contributions" refers to conceptualizing different factors as influencing psychological phenomena in formalized steps or parts. The simplest version of a stepwise contribution would be to consider genes contributing at the first "step" of a measured response, then adding the contribution of experiential variables at the second "step." Similar to statistical analysis that are described as stepwise (e.g., hierarchical linear modeling), one can determine how much variance in the measured response is attributable to each factor at each step.

The Tate and Ledbetter (2010) commentary was a direct response to a similar confusion between meta-theory and research theory presented by Confer et al. (2010). The main difficulty that Tate and Ledbetter found with the SST-style

argumentation presented by Confer et al. was the attribution of observable differences between men and women to the unfolding of different genetic plans—with the caveat that these observable differences only show up in the domains of selective pressures (Buss and Schmitt 2011). Thus, to be fair to SST, there can be domains of observable similarity between women and men, and the explicit statement for why these similarities occur is because there is no selective pressure driving differences between the groups (see Buss and Schmitt 2011). As Buss and Schmitt (2011) state when they introduce evolutionary psychology as a meta-theory: "Women and men are expected to differ in domains in which they have faced recurrently different adaptive problems over human evolutionary history. They are expected to be similar in all domains in which they have faced similar adaptive problems over human evolutionary history" (p. 769). Thus, it would seem that SST has some explanatory flexibility to account for similarities and differences between gender groups. However, as developed below, the flexibility is only apparent. Notice that an important implication of this version of evolutionary psychology is that it cannot easily account for two types of outcomes: (a) observed similarities between men and women when there are different underlying genetic influences within each group or (b) observed differences between men and women when there are similar underlying genetic influences across the groups.

In contrast, the Tate and Ledbetter (2010) stepwise position is sophisticated enough to model similarities and differences between gender groups, but unlike SST, the Tate and Ledbetter position contains the ability for the variance in responses to be tracked by psychosocial and experiential variables in addition to or over and above genetic processes. Importantly for the Tate and Ledbetter position, it is well known that physiological processes (e.g., hormone release) and some anatomical processes (e.g., brain wiring) are experientially dependent. Thus, examining the physiological differences within and across people is no good argument for distinctions specific to genes (also see Fallacy #4 above). To demonstrate the theoretical advantage of a stepwise understanding of evolutionary processes over an SST-style formulation, reconsider the expert birdwatching example (see Fallacy #4 above). A stepwise approach could invoke a generic or multi-purpose categorization process that may in fact have a genetic contribution to any visual processing at step 1. The observable variability between expert birdwatchers and non-experts in their ability to visually identify birds and the differences in activation of the fusiform gyrus result from experiential influences (in this case, practice) that affect physiology and brain area activation. In this way, one does not have to assume that differences between groups are located at a genetic level. In fact, the opposite may be true—similarities may be located at the genetic level and differences are accounted for by experiential contributions over and above the genetic contribution. (Recall that expert birdwatching is not likely an adaptation resulting from some selective pressure, and thus SST would be moot as to why it occurs; see Fallacy #4 above).

When SST reasoning is applied to presumed locations of selective pressure, such as reproduction (e.g., sexual orientation) or survival (e.g., suicidality) (see Confer et al. 2010, for exactly these examples), the conceptual move is toward locating the differences across groups at the genetic level rather than some other level of analysis (e.g., physiology). The Tate and Ledbetter (2010) thesis is that even for those arenas of presumed selective pressure—such as mate preference differences, which Buss and Schmitt argue for as being driven by genetic differences between groups—it is possible to develop arguments that advance genetic similarity across groups and propose that observable differences are accounted for better by a complex collection of experiential sources—including the part of physiology that is separable from genetics.

In this way, the stepwise reasoning can be used to understand the Tate (2011) findings (as Tate and Ledbetter 2010, suggest). Tate's (2011) finding that self-reported gender did not importantly contribute to the desired number of sexual partners indicates that evolutionary processes may code for similar desired numbers of sexual partners across people. This conclusion is plausible given that self-reported gender category is separable from gender role stereotype endorsement (as argued in Tate's (2011) Study 2 and Study 3). Tate's (2011) main finding was that respondents' desired number of sexual partners is better tracked by their endorsement of communal (or femininity) stereotypes—a psychosocial variable—than it is by their self-reported gender—the presumed genetic influence. As noted above, self-reported gender had a near-zero contribution to this effect. Buss and Schmitt (1993) would have predicted (a) the opposite pattern or (b) that self-reported gender still predicted desired numbers of sexual partners even when considering gender role stereotypes. Nonetheless, Buss and Schmitt (2011) downplayed the contribution of the Tate findings by stating "…it is important when evaluating mediation that when variables overlap, they are not in fact measuring the exact same concept" (p. 775)—attempting to provide a statistical critique against the Tate (2011) analysis by implying that communal stereotype endorsement and identifying as a man or woman are measuring the same concept. Yet, when one considers the actual statistical analysis provided by Tate, the Buss and Schmitt comment only reveals deep confusion about the principles of statistics. This confusion represents another error that needs to be addressed and removed from the discussion so that useful dialogue may continue. Tate (2011) reported nearly identical semipartial correlations for degree of communal stereotype endorsement predicting desired number of sexual partners *within each gender group* (Tate 2011, Table 3)—indicating

that the relationship is equally strong for both men and women, and predicts in the same direction for both groups. Consequently, it is clear that the endorsement of communal stereotypes is not the same as identifying as a woman. For Buss and Schmitt's (2011) statement to be true, Tate's result should have shown up for women only—not men as well—or shown up in opposite directions across the groups. Furthermore, Tate (2011) reported a mean difference between men and women in their endorsement of communal traits (p. 653), and still found that the same relationship between communal traits and desired number of sexual partner occurred for each group *despite* the mean difference. For these reasons, Tate (2011) made a spurious correlation argument (see pp. 653–654), not a mediation argument. Mediation would require a clear link between self-reported gender (X) and the desired number of sexual partners (Y) before any mediator (Z) could intervene (cf. Baron and Kenny 1986). In effect, one must establish X→Y before X→Z→Y (see Fiedler et al. 2011, p. 1231). However, both Pedersen et al. (2002) and Tate (2011) have empirically shown that the link between self-reported gender and desired number of sexual partners is tenuous. Specifically, the link only happens when researchers use means but not medians—even when the latter are more appropriate given data dispersion patterns (see Pedersen et al. 2002)—and one-tailed significance tests (Tate 2011). Presuming that mediation would be an appropriate way to test SST in this case is too strong a statement from Buss and Schmitt (2011) and begins to derail a useful conversation about how researchers can appropriately evaluate research hypotheses using statistics as a part of the falsification process. Thus, despite Buss and Schmitt's confusion of fundamental concepts in statistical analyses, Tate (2011) provided some evidence to suggest that differences on a psychosocial dimension account for individual differences—within and between self-reported gender groups. This reasoning fits into the evolution meta-theory overall because the argument from Tate's (2011) results is that evolutionary processes coded for similarity, and that socio-cultural experience and physiological differences help determine the patterns of variability.

The argument that genetics may code for similarity across the human species is central to arguments such as universal emotion expression (e.g. Matsumoto and Willingham 2006), and these theories also allow for experiential modulation of aspects of facial expression phenomena. For instance, Matsumoto and Willingham (2006) found that congenitally blind and congenitally sighted judo competitors showed the same facial expressions under the same conditions, yet the length of showing the expression seemed to be experientially dependent—less for sighted individuals who learned the social consequences of these expressions. Thus, the position that evolutionary processes may code for similarities across persons that are modifiable by experience is a tenet held by existing evolutionary theories that do not invoke SST.

Moving Forward with Evolution and Feminism Together

The Tate (2011) empirical study and the Tate and Ledbetter (2010) outline of the stepwise research theory of evolutionary contribution to individual differences are ways in which one can develop positions that are consistent with the evolution meta-theory and the feminism meta-theory simultaneously. The Pedersen et al. (2011) investigation also has this feature, as do the implications from the Harris (2011) investigation. The evolutionary meta-theory focuses on genes as a necessary contributor to psychological phenomena, and the feminist meta-theory focuses on gender as a necessary contributor to our understanding of the contours (similarity or difference) of psychological phenomena. Thus, the stepwise understanding outlined by Tate and Ledbetter (2010) seems to exist at the intersection of the two meta-theories to the extent that it models genetic contribution to psychological phenomena as step 1, then determines whether gender information seems to create similarities or differences on those phenomena as further steps. Furthering this point, while it is true that Tate and Ledbetter (2010) argue that evolutionary processes largely code for similarity across genders and sex groups, the position itself is not inconsistent with positing or finding differences. The Tate and Ledbetter (2010) stepwise understanding could allow for genetic differences between chromosomal groups (e.g., XX and XY) that are part of Step 1 that are then attenuated at Step 2 by experiential variables. Similarly, genetic differences at Step 1 could be exacerbated at Step 2 by experiential variables. Also, all the other mathematical possibilities (e.g., mutual main effects, direct effect of Step 1 with no contribution of Step 2) exist and are possible to recover for research hypotheses and empirical predictions. In any case, one should notice that unlike the imprecise SST reasoning, Tate and Ledbetter's position would also argue for the detectability of the genetic differences at some level (e.g., semi-partial correlations or partial correlations) even while the experiential and psychosocial variables might have a larger effect size, satisfying part of Eagly and Wood's (2011) call for standardized comparisons. Unfortunately, the SST reasoning as presented by Buss and Schmitt (2011) provides no way for researchers to see the relative contributions of any of these influences, even while they explicitly argue that these different influences exist. Thus, while SST-style reasoning has internal difficulties, there is no difficulty in demonstrating that evolution and feminism are neither mutually opposing nor incommensurate meta-theories.

Summary and Conclusions

In this commentary, I have proposed that the arguments espoused by Buss and Schmitt (2011) are largely unhelpful to furthering the discourse about evolutionary processes and

feminist approaches. This unhelpfulness is located in the persistent conceptual confusions presented by Buss and Schmitt (2011) even while they attempt to make useful distinctions and points. Furthermore, within my rejoinder, I tried to provide a theoretically workable avenue by which to advance the Eagly and Wood (2011) position that there is still much work to be done in creating explicitly feminist positions concerning evolutionary processes. As Eagly and Wood (2011) correctly state, Pedersen et al. (2011) have begun this work, but I would add that Tate (2011) has also started this work as well (see Tate and Ledbetter 2010). It is my hope that future research on the intersection of feminist approaches and evolutionary processes can benefit from the delineation of the philosophy of science issues at play in empirical tests of psychological phenomena outlined in Table 1. Summarizing this commentary, one can adhere to a feminist meta-theory and an evolutionary meta-theory simultaneously. Yet, researchers should be clear on how each meta-theory is actually related to research theories, to research hypotheses, and then to empirical predictions. Until now, based largely on the conceptual confusions offered by Buss and colleagues, the field has not been able to progress past trying to argue with evolutionary perspectives as if these perspectives oppose feminist perspectives. Yet, for reasons that Buss and Schmitt (2011) did not provide, the two are not at all opposing. Researchers may now advance the literatures hitherto explored under the separate umbrellas of feminism and evolution as really being under the same umbrella with diverse and divergent research theories and research hypotheses. Like any science, psychology can only provide support or falsification for empirical predictions, but everyone should be clear about what such falsification or support actually means for the foundational perspectives (viz., meta-theories) from which these empirical predictions ultimately emanate.

Acknowledgement I would like to thank Diego Aste and the Social Perception, Attitudes and Mental Simulation (SPAMS) Lab for helpful comments on the ideas presented in this article.

References

Bargh, J., & Chartrand, T. L. (1999). The unbearable automaticity of being. *American Psychologist, 54*, 462–479. doi:10.1037/0003-066X.54.7.462.

Baron, R. M., & Kenny, D. A. (1986). The moderator-mediator variable distinction in social psychological research: Conceptual, strategic, and statistical considerations. *Journal of Personality and Social Psychology, 51*, 1173–1182. doi:10.1037/0022-3514.51.6.1173.

Brennan, J. F. (1998). *History and systems of psychology* (5th ed.). Upper Saddle River, NJ: Prentice Hall.

Buss, D. M. (1987). Sex differences in human mate selection criteria: An evolutionary perspective. In C. Crawford, M. Smith, & D. Krebs (Eds.), *Sociobiology and psychology: Issues, ideas, and findings* (pp. 335–351). Hillsdale: Erlbaum.

Buss, D. M. (1995). Psychological sex differences: Origins through sexual selection. *American Psychologist, 50*, 164–168. doi:10.1037/0003-066X.50.3.164.

Buss, D. M. (1998). Sexual strategies theory: Historical origins and current status. *Journal of Sex Research, 35*, 19–31. doi:10.1080/00224499809551914.

Buss, D. M. (2011). *Evolutionary psychology: The new science of the mind* (4th ed.). Needham Heights: Allyn & Bacon.

Buss, D. M., & Schmitt, D. P. (1993). Sexual strategies theory: An evolutionary perspective on human mating. *Psychological Review, 100*, 204–232. doi:10.1037/0033-295X.100.2.204.

Buss, D. M., & Schmitt, D. P. (2011). Evolutionary psychology and feminism. *Sex Roles, 64*, 768–787. doi:10.1007/s11199-011-9987-3.

Buss, D. M., Abbott, M., Angleitner, A., Asherian, A., Biaggio, A., Blanco-Villasenor, A., et al. (1990). International preferences in selecting mates: A study of 37 cultures. *Journal of Cross-Cultural Psychology, 21*, 5–47. doi:10.1177/0022022190211001.

Cacioppo, J. T., Tassinary, L. G., & Berntson, G. G. (2007). *Handbook of psychophysiology* (3rd ed.). New York: Cambridge University Press.

Confer, J. C., Easton, J. A., Fleischman, D. S., Goetz, C. D., Lewis, D. M. G., Perilloux, C., & Buss, D. M. (2010). Evolutionary psychology: Controversies, questions, prospects, and limitations. *American Psychologist, 65*, 110–126. doi:10.1037/a0018413.

Cozby, P. C. (2007). *Methods in behavioral research* (9th ed.). Boston, MA: McGraw-Hill.

Decety, J., & Cacioppo, J. T. (2011). *Handbook of social neuroscience*. New York: Oxford University Press.

Eagly, A. H., & Wood, W. (2011). Feminism and the evolution of sex differences and similarities. *Sex Roles, 64*, 758–767. doi:10.1007/s11199-011-9949-9.

Fiedler, K., Schott, M., & Meiser, T. (2011). What mediation analysis can(not) do. *Journal of Experimental Social Psychology, 47*, 1231–1236. doi:10.1016/j.jesp. 2011.05.007.

Gauthier, I., & Curby, K. M. (2005). A perceptual traffic jam on highway N170. *Current Directions in Psychological Science, 14*, 30–33. doi:10.1111/j.0963-7214.2005.00329.x.

Gauthier, I., Skudlarski, P., Gore, J. C., & Anderson, A. W. (2000). Expertise for cars and birds recruits brain areas involved in face recognition. *Nature Neuroscience, 3*, 191–197.

Harris, C. R. (2011). Menstrual cycle and facial preferences reconsidered. *Sex Roles, 64*, 669–681. doi:10.1007/s11199-010-9772-8.

Hines, M. (2004). *Brain gender*. Oxford: New York.

Hines, M., Ahmed, S., & Hughes, I. (2003). Psychological outcomes and gender-related development in complete androgen insensitivity syndrome. *Archives of Sexual Behavior, 32*, 93–101. doi:10.1023/A:1022492106974.

Kasser, T., & Sharma, Y. S. (1999). Reproductive freedom, educational equality, and females' preference for resource-acquisition characteristics in mates. *Psychological Science, 10*, 374–377. doi:10.1111/1467-9280.00171.

Kenrick, D., Li, N., & Butner, J. (2003). Dynamical evolutionary psychology: Individual decision rules and emergent social norms. *Psychological Review, 110*, 3–28. doi:10.1037/0033-295X.110.1.3.

Kitayama, S., & Park, J. (2010). Cultural neuroscience of the self: Understanding the social grounding of the brain. *Social Cognitive and Affective Neuroscience, 5*, 111–129. doi:10.1093/scan/nsq052.

Kuhn, T. S. (1962). *The structure of scientific revolutions*. Chicago: University of Chicago Press.

Matsumoto, D., & Willingham, B. (2006). The thrill of victory and the agony of defeat: Spontaneous expressions of medal winners of the 2004 Athens olympic games. *Journal of Personality and Social Psychology, 91*, 568–581. doi:10.1037/0022-3514.91.3.568.

Morsella, E. (2005). The function of phenomenal states: Supramodular interaction theory. *Psychological Review, 112*, 1000–1021. doi:10.1037/0033-295X.112.4.1000.

Morsella, E., & Bargh, J. A. (2010). What is an output? *Psychological Inquiry, 21*, 354–370. doi:10.1080/1047840X.2010.524597.

Pedersen, W. C., Miller, L. C., Putcha-Bhagavatula, A. D., & Yang, Y. (2002). Evolved sex differences in the number of partners desired? The long and the short of it. *Psychological Science, 13*, 157–161. doi:10.1111/1467-9280.00428.

Pedersen, W. C., Putch-Bhagavatula, A., & Miller, L. C. (2011). Are men and women really that different? Examining some of Sexual Strategies theory (SST)'s key assumptions about sex-distinct mating mechanisms. *Sex Roles, 64*, 629–643. doi:10.1007/s11199-010-9811-5.

Peirce, C. S. (1934/1994). What pragmatism is. In P. Moser & D. Mulder (Eds.), *Contemporary Approaches to Philosophy*, (pp. 19–33). New York: MacMillan.

Popper, K. (1959). *The logic of scientific discovery*. New York: Routledge.

Preacher, K. J., & Hayes, A. F. (2008). Contemporary approaches to assessing mediation in communication research. In A. F. Hayes, M. D. Slater, & L. B. Snyder (Eds.), *Advanced data analysis methods for communication research* (pp. 13–54). Thousand Oaks, CA: Sage.

Revusky, S., & Garcia, J. (1969). Learned associations over long delays. In G. H. Bower & J. T. Spence (Eds.), *The psychology of learning and motivation*. New York: Academic.

Ruffman, T., & Perner, J. (2005). Do infants really understand false belief? *Trends in Cognitive Sciences, 10*, 462–643. doi:10.1016/j.tics.2005.08.002.

Rychlak, J. F. (1991). *Artificial intelligence and human reason: A teleological critique*. New York: Columbia University Press.

Rychlak, J. F. (1994). *Logical learning theory: A human teology and its empircal support*. Lincoln: University of Nebraska Press.

Schmitt, D. P. (2003). Universal sex differences in the desire for sexual variety: Tests from 52 nations, 6 continents, and 13 islands. *Journal of Personality and Social Psychology, 85*, 85–104. doi:10.1037/0022-3514.85.1.85.

Schmitt, D. P. (2005). Sociosexuality from Argentina to Zimbabwe: A 48-nation study of sex, culture, and strategies of human mating. *The Behavioral and Brain Sciences, 28*, 247–275. doi:10.1017/S0140525X05000051.

Skinner, B. F. (1984). Behaviorism at fifty. *The Behavioral and Brain Sciences, 7*, 615–667. doi:10.1017/S0140525X00027618.

Tate, C. (2000). Automatic assumptions of automaticity. *American Psychologist, 55*, 766–767. doi:10.1037/0003-066X.55.7.766.

Tate, C. (2011). The "problem of number" revisited: The relative contributions of psychosocial, experiential, and evolutionary factors to the desired number of sexual partners. *Sex Roles, 64*, 644–657. doi:10.1007/s11199-010-9774-6.

Tate, C., & Ledbetter, J. N. (2010). Oversimplifying evolutionary psychology leads to explanatory gaps. *American Psychologist, 65*, 929–930. doi:10.1037/a0021024.

Tate, C. C., Ledbetter, J. N., & Youssef, C. P. (2012). A two-question method for assessing gender categories in the social and medical sciences. *Journal of Sex Research. Advance online publication*. doi:10.1080/00224499.2012.690110.

Tolman, E. C. (1948). Cognitive maps in rats and men. *Psychological Review, 55*, 189–208. doi:10.1037/h0061626.

Tooby, J., & Cosmides, L. (2005). Conceptual foundations of evolutionary psychology. In D. M. Buss (Ed.), *The handbook of evolutionary psychology* (pp. 5–67). Hoboken: Wiley.

Watson, J. B. (1913). Psychology as the behaviorist views it. *Psychological Review, 20*, 158–177. doi:10.1037/h0074428.