

## REPLY TO COMMENTARIES

I am grateful for the interest in my paper. I will respond to these two commentaries in the order I received them.

### ALAN SUGARMAN

In the introductory section of his commentary, Alan Sugarman expresses disappointment that I did not outline a general psychology in my paper. Having come to the view that it is “a manifesto designed to demonstrate the broader psychoanalytic relevance of neuroscience” (p. 1139), he says that it does not properly address such fundamental topics as development, pathogenesis, and mutative action; it ignores much of developmental and cognitive psychology; and it “goes from neurobiology and chemistry directly to mental experience” (p. 1141). These are unexpected criticisms of a paper focused on *drive theory*, and which makes no claim to present a general psychology. In the circumstances, I can only suggest that Sugarman reads what I have written—within this same theoretical framework—on those other topics that interest him (e.g., Solms 2017a,b, 2018a,b,c, 2019a,b). The same applies to what I have written on the topic of dreams, which might prompt Sugarman to reconsider his view that neurophysiology “does not provide any understanding into the meaning of dreams” (p. 1143). I think it fair to say that my work on this topic has reestablished the fact, long denied by other neuroscientists, that dreams are meaningful and motivated (see, e.g., Solms 1997, 2000a,b, 2001a,b, 2004, 2009, 2012, 2014; Solms and Hobson 2006; Solms and Turnbull 2006; Malcolm-Smith et al. 2012).

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In the second section of his commentary, Sugarman argues that my paper is reductionistic in that it ignores sociological and psychological levels of analysis in favor of the neurobiological. He is right to say that “each of these levels is important to understanding some area of human functioning” (p. 1142), but he seems to have overlooked the opening paragraphs of my paper, which explain why the neurobiological level is particularly important *for drive theory*. Anyone who shares Sugarman’s view that “neuroscientific concepts are relevant only in explaining brain functioning, not mental functioning” (p. 1142), should carefully reread the quotations from Freud that I provided in those opening paragraphs. Freud (1920) spoke of “the necessity for borrowing from the science of biology” (p. 60). This view, held by the founder of psychoanalysis, to the effect that borrowing from biology is *necessary* when it comes to drive theory, hopefully puts an end to Sugarman’s further argument that I use neurobiological concepts “to make [the revision of drive] theory sound scientific without acknowledging that they are relevant only to phenomena other than those dealt with by psychoanalysis” (p. 1142). Freud, at least, accepted that the concept of drive belongs to neurobiology no less than it does to psychoanalysis.

Sugarman goes on to proclaim that “no superordinate model of human functioning has yet been discovered that can encompass and integrate the neurological and psychological spheres of human experience” (p. 1142). He adds, with reference to the philosophical “hard problem,” that the superordinate mathematical and physical models I use “can be understood only as metaphors” (p. 1142). I assume from this that Sugarman has not read my book (2021b) on the hard problem, which shows that mathematical and physical concepts like entropy and Markov blankets are anything but metaphors. Simply put: they *explain* the workings of the mind and the brain, both. In this regard, Sugarman might also find the commentaries on my book by the philosophers Thomas Nagel and Daniel Dennett—and my replies to them—enlightening (Nagel 2021; Dennett 2021; Solms 2021c).

The assertion, in the third section of Sugarman’s commentary, that “Solms writes as though representations are not part of the psychological amalgam that makes each individual’s cognitive/emotional mind unique” (p. 1143) suggests that he does not fully understand the concept of a “predictive model” (which is mentioned repeatedly in my paper). Predictive models perform precisely that individualizing and representational function; they are conceptually identical with what Freud called “memory systems.”

Next, in his fourth section, Sugarman cites psychoanalytic authorities from the 1960s and 1970s to make the point that “the concept of psychic energy . . . is based on Freud’s outdated understanding of physics” (p. 1144). The absolute starting point of my paper is the fact that *subsequent* developments in physics enable us to *replace* Freud’s thermodynamic conception of psychic energy with an informational one.

The same applies to Sugarman’s further claim—in the fifth section of his commentary—that my conception of the ego is “anthropomorphic” (p. 1145). This misconstrues the concept of “active inference,” which is derived from modern statistical physics (see Friston 2013). Complex dynamic self-organizing systems that use active inference are indeed “self-activating and self-regulating, autonomous . . . , independent, symbol-utilizing minds that can make themselves up” (Schafer 1973, p. 166, quoted by Sugarman, p. 1145).

The sixth section of Sugarman’s commentary—“The Absence of Subjective Experience and Meaning in a Neuropsychanalytic Model”—rehearses Blass and Carmeli’s critique of neuropsychanalysis as a whole (2007). I have twice engaged with Rachel Blass directly on these issues, in Oxford-rules debates before psychoanalytic audiences (Solms and Blass 2008, 2017). On both occasions, the voting went decidedly against her. Unfortunately, she declined to allow a video recording to be posted on the internet; interested readers can read my reply to Barnaby Barrett’s very similar critique of neuropsychanalysis as a whole (Solms 2015b). Here I have space to respond only briefly to Sugarman’s main two concerns, namely, subjective experience and meaning.

Concerning the former, Sugarman says that “Solms writes as though mind is a biological entity” (p. 1146). That is true: I consider psychology to be part of the life sciences. Like Freud, I think the mind is embodied and that it evolved to perform specific biological functions. It does not follow, however, that I “minimize the importance of subjectivity both in theory and in clinical psychoanalytic practice” (p. 1146). It is precisely because I was so frustrated by the neglect of subjectivity in neuroscience that I decided to train as a psychoanalyst (see Solms 2021b). Neuropsychanalysis was, and remains, a concerted effort to persuade my neuroscientific colleagues that we will never understand how the brain works if we neglect its most unique property: that it *feels* like something to be a brain. Surely the capacity for subjectivity *does* something (see Solms 2015b; Solms and Turnbull 2002). This is not a reductionist

materialist view. Like most neuropsychanalysts, I am a dual-aspect monist, which means I take the view that the functional dynamics of what Freud called the “mental apparatus” can be studied from two different *observational* perspectives: the outer, objective one (seeing a brain) and the inner, subjective one (being a brain). These two perspectives are complementary; neither is ontologically more “real” than the other (see Solms 1995). It was precisely this mistake—and a neglect of the subjective perspective—that led my colleagues to erroneously reduce dreaming to REM sleep (see the references above).

Concerning meaning making, I must agree with Sugarman (quoting Pulver 2003, p. 762) that neuroscience may “say something about the general functioning of . . . motivations, but by its very nature it can say little about the meaning they have for an individual” (p. 1146). This is roughly equivalent to what I said about “meaning making” on pp. 1067–1068 of my paper. I agree completely that neuroscientific methods are not suited to investigating what is idiosyncratically meaningful for each individual; that is why I have always argued that we need to use both neuroscientific and psychoanalytic methods. However, neuroscience certainly can reveal important things about the nature of human motivation and meaning making *in general*, and that is what my paper is about. As regards the clinical utility of such new knowledge, I would encourage Sugarman to reread what I wrote about this on p. 1080 of my paper, and also to read Solms (2018b) and Smith and Solms (2018). These two articles—which are all that I have published on the topic to date—should reassure him that it is certainly not my view that “analytically oriented treatment” consists of “helping the patient know her historical reality using logic and manifest content” and that I show “no attempt to understand or address the unconscious logic and meaning” behind my patients’ beliefs (p. 1147).

The seventh and last section of Sugarman’s commentary concerns my revision of drive theory itself. He writes that in the United States “theorists rarely use the actual concept of drive these days; instead, we use the term *wish*, a psychological concept that involves a mental representation. Wish is not an abstract, biological, or mechanistic concept based on dubious ideas about erogenous zones and psychic energy” (p. 1148). The opening sections of my paper, already cited above, make clear what is at stake if psychoanalysis renounces the concept of drive. A rereading of my paper will also make clear to unbiased colleagues that it is by no means a defense of “dubious ideas about erogenous zones.” Rather, I am of the view that

Freud's oral, anal, phallic, and genital "components" of the libidinal drive are in fact independent entities, only one of which deserves to be described as sexual. I believe that these so-called component libidinal drives are better conceptualized under the separate headings of PANIC/GRIEF, RAGE, PLAY, and LUST, respectively. (See my commentary on Kernberg [Solms 2021d].)

Next Sugarman writes: "To restrict the concept [of drive] to phenomena rising from the body (brain) leaves out psychological constructs like object relations, safety, holding, fantasy" (p. 1148). In my paper, I make the point very clearly that the sources of drive should *not* be restricted to the body. Moreover, my revised conception of drive (in stark contrast to Freud's) is intrinsically object-relational. How can we conceive of a PANIC/GRIEF drive without reference to a care-giving object, a RAGE drive without reference to a frustrating object, a PLAY drive without reference to friendly objects, a FEAR drive without reference to threatening objects, a CARE drive without reference to dependent objects, and so on?

Even more surprisingly, Sugarman decries my neglect of "the importance of internal conflict" (p. 1148). My paper states clearly that seven emotional drives provide more scope for internal conflict, not less, than Freud's two.

Sugarman ends the final section of his commentary by saying that "there is nothing in the paper that demonstrates any conceptual advantage to defining drives as he does" (p. 1149). Why? Because my conceptualization of drive as free energy (i.e., as the average error between predicted and actual outcomes) boils down to "adaptation," which is "traditionally seen as involving primarily the ego, not the drives" (p. 1149). The fact is, prediction-error values *just are* a measure of the demand made upon the ego for work (which is, lest we forget, Freud's definition of drive). The free energy principle is a unifying principle stating that homeostatic demands (drives) are nothing other than demands for *predictive* (i.e., representational, cognitive) work. Which reminds me: the concept of prediction occupies much the same place in computational neuroscience as does the concept of wish in Freud's metapsychology. (On this score, for a detailed treatment, see Solms 2020).

### PAUL SCHWARTZ

In the introductory section of his commentary, Paul Schwartz says that Freud's notions of free and bound energy are based only loosely on

Helmholtzian physics, “which as near as can be determined, Freud himself never formally acknowledged” (p. 1158). But it hardly needed acknowledgment; everybody knew, in Freud’s day, that these terms were introduced by Helmholtz (1882). Schwartz goes on to say that “it should be noted, however, that Freud’s free and bound neuronal cathectic energies were chiefly distinguished not by their thermodynamic properties, but by whether their hypothesized electrical neuronal cathexes were freely and flexibly deployable by the mental apparatus” (p. 1159). This elides the starting points of my paper, that the nineteenth-century thermodynamic understanding of entropy was deepened in the twentieth century when Jaynes (1957) showed that thermodynamic entropy is merely an application of *information* entropy—which links directly with the way Freud used the concept. As I have said in my response to Sugarman, recent developments in physics enable us to *replace* Freud’s thermodynamic conception of free energy with an informational one.

Schwartz says also that Helmholtz contributed to the formulation of “the Second Law of Thermodynamics (not the First, as Solms states)” (p. 1159). I have never before heard anyone question Helmholtz’s fundamental contribution (1847) to the First Law!

In the second section of his commentary, Schwartz quotes Panksepp and Biven’s metaphorical use of the term “ancestral memories” (2012) to support his claim that “phylogenetic memories exist in our unconscious” (p. 1160). I have discussed this issue fully elsewhere (Solms 2022), so here I will just say that everything pivots on the word “memory.” Speaking neuroscientifically, as opposed to metaphorically, there are no such things as inherited memories, let alone episodic memories, which is what Freud posited. (Instincts are not memories.)

Next, Schwartz questions my assertion that Freud considered the phrase “unconscious affect” an oxymoron. He quotes Freud at length, in an attempt to show that he was equivocal on this score. I can reply (as the editor and translator of Freud’s complete works) that, if one studies his writings on this issue in their totality, one is left in no doubt that he rejected the notion of unconscious affect, utterly, from first to last. Whenever he says that we *speak* of “unconscious affects,” he immediately clarifies that we speak loosely, because the notion is metapsychologically incorrect. Actually, Freud’s position is well summarized in one of the sentences that Schwartz quotes: “Strictly speaking, then, and although no fault can be found with the linguistic usage, there are no

unconscious affects” (Freud 1915b, p. 178). Quoting *The Ego and the Id*, where Freud speaks of an “unconscious sense of guilt,” Schwartz seems to think that his earlier position was part of “his [abandoned] discharge theory of affects” (p. 1161). I must therefore draw attention to what Freud (1924c) wrote *after* 1923: “we [should] give up the term ‘unconscious sense of guilt’, which is in any case psychologically incorrect, and speak instead of a ‘need for punishment’” (p. 166). (For a detailed discussion of this matter, see Solms in press.)

In his third section, Schwartz says: “Solms reasons as follows: drives are by my definition conscious; therefore, I conclude that drives must be conscious” (p. 1161). That is not my reasoning. Freud (1915a) defines drive as “a measure of the demand made upon the mind for work” (p. 121). Autonomic forms of homeostasis, such as blood-pressure regulation and the like, do not make demands *upon the mind*; that is why I use the word “needs” for them, rather than “drives.” In my paper, I present detailed arguments in support of my claim that needs become drives only when we feel them. However, Schwartz claims: “In support of his contention, Solms argues that often we engage in sex while trying *not* to reproduce, and he suggests that sexual behavior is determined predominantly, if not entirely, by our voluntary conscious choice” (p. 1162). I say nothing of the sort. I say only that although the evolved mechanisms that underpin our felt urges (sexual and otherwise) are unconscious, the urges themselves are felt. This doesn’t mean that such urges are voluntary; it means only that they *drive* voluntary behavior.

Next Schwartz questions my statement that “the energy that is not currently employed in effective work is ‘free’” (p. 1163; emphasis removed); “here again,” he says, “Solms is not correct. In fact, the amount of energy within a system that is *available* to do work is the free energy, whereas the amount of energy within a system that is unavailable to do work is the bound energy” (p. 1163; emphasis added). This is a misreading; I wrote that “the energy that is *not currently employed* in effective work is ‘free’” (emphasis added). “Not currently employed in work” therefore means “currently available for work.” In other words, Schwartz and I say the same thing: the energy that is not currently employed is *available*; that is, it is *free* to do work.

Schwartz goes on to say that I mistakenly equate Friston’s variational free energy with thermodynamic free energy. “At the outset,” he writes, “Friston explicitly stated that his informational free-energy principle

should be distinguished from the statistical-thermodynamic free-energy” (p. 1163). He then quotes Friston as saying, “the only link between these two uses of the term ‘free energy’ is mathematical; i.e., both appeal to the same probabilistic fundamentals” (p. 1163). Friston’s point here is a version of the one I made above, with reference to Jaynes. Thermodynamic free energy is rooted in the same mathematical formalism as variational free energy, where there is an *information* exchange between a system and its environment, as opposed to a *heat and temperature* exchange.

Still on the subject of Friston, Schwartz continues:

Friston and colleagues have already formulated several interesting preliminary attempts to extend the free-energy principle to several aspects of Freudian meta-psychology . . . , though none of these papers directly address the issue of Freudian drives. Thus, . . . we must for now temper our enthusiasm for Solms’s claim that “Friston’s free energy principle achieves Freud’s greatest ambition, namely, ‘to furnish a psychology that shall be a natural science: that is, to represent psychical processes as *quantitatively determinate* states of specifiable material particles, thus making those processes perspicuous and free from contradiction” (Freud 1895, p. 295) [p. 1164].

Actually, Friston and I wrote a paper (Solms and Friston 2018) in which we make explicit reference to Freudian drives in the way I conceptualize them in the paper under discussion here; we even use Freud’s terms (1895) to formalize the mechanics. Moreover, in three subsequent publications (Solms 2019a, 2020, 2021b), I have elaborated our quantitative arguments and spelled out in considerable detail their roots in Freudian drive theory. The second of these papers was accompanied by a commentary written by Friston, strongly supporting my approach to this aspect of Freudian theory.

Schwartz misunderstands also my remarks about the confluence of drives. Of course, I agree that there are hybrid forms of emotion. Thus, for example, I explain (on p. 1074 of my paper) that the primary drives of PANIC/GRIEF and RAGE interact to yield the secondary emotion of guilt. My point about confluence is simply that drives are *categorical variables* in the sense that each one of them must be satisfied in its own right (otherwise we die); and that this is why they are *qualitatively* distinct.

With respect to my arguments for a conscious id, Schwartz writes: “Solms’s theoretical reasoning is once again flawed. . . . the most parsimonious explanation . . . is to simply relocate Freud’s mostly conscious



ego to the upper brainstem and relocate Freud's unconscious id to the cerebral cortex; that is, the ego and the id should simply trade their anatomical homes" (p. 1165). Anybody who knows anything about the upper brainstem will surely agree that it cannot possibly support the functions that Freud assigned to the ego; likewise, anyone who knows anything about the cerebral cortex will agree that it cannot perform the functions he assigned to the id.

Schwartz closes his third section with the statement that, since "defenses are unconscious, what exactly is being defended against if not *unconscious* affects?" (p. 1165). The following two extracts from Solms (in press) clarify this issue.<sup>1</sup>

The unconscious ego, with its defensive functions, strives to gain control of affective consciousness [i.e., the id]. This goal of the ego can be achieved in various ways. At one theoretical extreme, the ego can convert affective consciousness entirely into cognitive consciousness. This coincides with the fictitious case of Mr. Spock, where the (affective) drive—or the "demand made upon the mind for work"—exerted by the id upon the ego yields perfectly efficient (cognitive) work. At the opposite extreme, the ego can also fail entirely to achieve this goal, in which case it will be overwhelmed by affect, and cognitively incapacitated. At yet another extreme, the ego can exclude affective consciousness from its (cognitive) realm entirely. This results in the id generating negative affects of which the ego has no *knowledge*; a common enough situation, as every psychoanalyst will attest. . . .<sup>2</sup> All the common-and-garden varieties of defence can be located between the theoretical extremes I have just described.

Here is the second extract:

Freud (1920) writes: "The problem of the *quality* of drive impulses and of its persistence throughout their various vicissitudes is still very obscure and has hardly been attacked up to the present" (p. 44; emphasis added). One of these vicissitudes, he continues, is a desexualization of the libidinal drive, which then becomes a "displaceable and *neutral* energy" (p. 44; emphasis added). This can only mean that it has *lost* its erotic quality. He continues: "If this displaceable energy is desexualized libido, it may also be described as *sublimated* energy; for it would still retain the main purpose of Eros. . . . If thought-processes in the wider sense are to be included among these displacements, then the activity of

<sup>1</sup>In the Freud quotations in these extracts, I have translated *Trieb* as "drive" rather than Strachey's "instinct," and *Triebkraft* as "driving forces" rather than Strachey's "motive forces."

<sup>2</sup>Here we recognize one variety of the oxymoronic "unconscious feelings."

thinking is also supplied from the sublimation of erotic driving forces” (p. 45). This is a model example of the process I formulated above concerning how the unconscious ego, via its defensive function, gains control over affective consciousness. This is how the drive *demand* for work exerted by the id yields efficient *work* by the ego. Sublimation, I submit, is the means by which the primary, affective form of consciousness is converted by the ego into the secondary, cognitive form.

In his closing section, Schwartz concludes: “while Solms has proposed a number of quite radical revisions to Freudian drive theory, these revisions do not hold up to close scientific scrutiny” (p. 1165). I don’t agree with this assessment. It is Freud’s drive theory that does not hold up to modern scientific scrutiny; the proposed revisions, by contrast, are sound and should serve us well.

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