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Response to commentaries

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My target article was a response to what I saw to be an ongoing problem in the neuroscientific dream debate: empirical findings were being used to either defend or attack Freud’s theory of dreams, while the necessary conceptual work (i.e., providing a coherent and clear conceptualization of the theory *before* submitting to empirical scrutiny) was being neglected. In this respect I completely agree with Claudio Colace’s statement that “the attempts to clarify and systematize the Freudian concepts are a preliminary task for every neuroscientific attempt to find the neural correlates of these.” I welcome the fact that many of the commentaries have seized upon various shortcomings in the target article and have added clarification and amplification of some important issues. I shall not, however, attempt to address the wide variety of thoughtful comments made but, instead, focus on what I consider to be some outstanding issues in this discussion, with a particular emphasis on shortcomings of alternative accounts.

The superego as censor

Several commentaries raise the important matter of the role of metaphor in science and discuss the importance of “cashing” these out into literal terms. As Agnes Petocz’s commentary points out, however, metaphors are found in all branches of natural science, and the problem in scientific explanation is not with metaphors *per se*, but with determining precisely how far such

metaphors map to the phenomenon in question. According to Cheshire and Thomä (1991), this involves “marking off the area of negative analogy,” which requires spelling out the relevant characteristics of both the phenomenon and metaphor and identifying the necessary points of contrast. Part of the problem with Freud’s account, however, is that his metaphors are often conflated with anthropomorphism, and, as the literature reveals, there is dispute as to what is to be taken literally and what is to be taken metaphorically (and hence, to some extent, there is an argument that “alternative” accounts exist in Freud’s work). However, in my view anthropomorphic metaphors are not necessarily problematic, such as when reflecting the influence of object-relations in the development of the psyche (e.g., the development of “imagos” or proto-types of the parents—Hopkins, 1995), but there are conceptual issues that need addressing before populating the mind with homunculi or “little persons,” which are said to decide and dictate our mental lives. The commentaries by Agnes Petocz, Nigel Mackay, and Doris McIlwain make contributions here, and I would just add to my earlier discussion that a general problem with explaining behavior in terms of an internal agent or “person” is that such explanations tend to defer actual explanation, because without some breakdown of “persons,” a vicious regress of explanations to (sub)persons emerges, or the position succumbs to the notion that these agents “freely choose” to act, making behavior literally inexplicable (Maze, 1983, 1987).

Colace, Calvin Yu, and McIlwain all raise the important issue of the role and conceptualization of the superego, both in repression and in dreaming, and undoubtedly the superego is one of the most important contributions Freud made to an understanding of personality. However, I also believe that the superego is another area of psychoanalytic theory that requires careful thinking through. While it is true that psychoanalysis discusses the development and character of the superego (as Colace notes), its *precise* character is not entirely clear, because, for the most part, discussion of the superego is in terms of what it does (its activities or “functions”) rather than what it *is*, and care is needed not to reify a set of behaviors into a substantive (i.e., committing the *fallacy of constitutive relations*: see Anderson, 1927; Boag, 2005; Maze, 1983). In Freud’s theory, “[the] super-ego is the representative for us of every moral restriction” (Freud, 1933, pp. 66–67), acting as an internal aggressor mimicking external authority:

First comes renunciation of instinct owing to fear of aggression by the *external* authority. (This is, of course, what fear of loss of love amounts to, for love is a protection against this punitive aggression.) After that comes the erection of an *internal* authority, and renunciation of instinct owing to fear of it—owing to fear of conscience. In this second situation bad intentions are equated with bad actions, and hence come a sense of guilt and need for punishment. The aggressiveness of conscience keeps up the aggressiveness of the authority. [Freud, 1930, p. 128, emphasis in original; cf. Freud, 1926, p. 128]

However, what this “internal authority” actually is requires careful stipulation (“authority” is not a quality or property of anything, and it would be more accurate to say that *something* is in authority—i.e., something stands in a particular relationship to other things). Similarly, the “aggressiveness of conscience” needs to go further to specify precisely what “conscience” is (and terms such as “moral function” are similarly ontologically obscure). Thus the question remains concerning precisely what is doing the activities associated with the superego. Freud addresses this, to an extent, when he writes that the superego develops through “identification”: “The basis of the process [of superego development] is what is called ‘identification’—that is to say, the assimilation of one ego to another one, as a result of which the first ego behaves like the second in certain respects, imitates it and in a sense takes it up into itself” (Freud, 1933, p. 63).

Keeping in mind that “identify” requires *something* identifying with something else, Freud proposes that the “ego” (which is conceptualized here in terms of

the dominating drives) identifies with aspects of the parents. So what are we to make of the superego? Here I follow the view that the superego is not an agent that acts but, rather, the socialized moral beliefs that guide (and limit) behavior, and whose basis is in unconscious fear of punishment and hope of reward (Brenner, 1994; Maze, 1983, 1987). What *acts* on the moral beliefs are the drives themselves. That is, there is no special superego agency *per se* but, rather, one “part” of the personality (a subset of drives) “attacking” another part deemed to bring frustration and loss of love (the aspects of personality deemed “immoral”). A similar view is proposed by Arlow and Brenner (1964) when they write, “[t]he superego is not an entity. It is made up of innumerable demands some of which are precisely related to specific situations” (p. 81). The advantage of the account proposed here is that it clarifies the ontological status of such prohibitions as *beliefs*, standing as barriers constraining the drives in their search for gratification and avoiding frustration. This is more parsimonious than postulating a “higher entity” within the psyche (the object of criticism within the target article) and retains Freud’s point that the superego’s influence consists “essentially, of what is taken over by other people” (i.e., the “internalization” of authority) (Freud, 1940, p. 147) and is in keeping with what I consider to be the psychoanalytic aim of uncovering the “primitive” processes underlying seemingly “higher,” more complex behaviors (as well as keeping humans firmly entrenched in the animal kingdom—Freud, 1917, pp. 140–141). Consequently, when Yu says early in his commentary that he believes that morality has some basis in fear of loss of love (following Kohlberg and Piaget), there is no disagreement here, but I do not then see the need to suggest a “moral centre” in the brain rather than postulating areas of the brain associated with simpler processes such as fear conditioning or perhaps even “emotional learning” (Doherty, Kringelback, Rolls, Hornak, & Andrews, 2001—McIlwain’s commentary provides a similar account). Furthermore, while I am not entirely clear what Yu’s position is concerning the superego as agent and the motivation attributed to it, if the superego is motivated by its own anxiety at the prospect of “immoral desires,” then really it is protecting itself, not the “ego,” and so the whole superego/ego relationship implicated by the anthropomorphic metaphors requires further consideration anyway. Having said all of this, the similarity between the account presented here and accounts presented in the commentaries (e.g., Colace) should not be underestimated. In both cases motivational processes are paramount, threat evaluation occurs (i.e., there is *something* that knows that a particular desire or wish will lead

to danger or is “immoral”), and as a result repression (or perhaps more generally “defense”), instigated by anxiety, occurs and prevents direct expression of the behavioral aim. The difference, however, is that in the target-article account, rather than postulating a separate “censor” that has a transcendental, omnipotent status, it is the drives that know and act on morality, and morality itself is simply an extension of fear of actual danger “internalized” broadly as beliefs.

Following on from this there are still conceptual issues that need addressing in providing a coherent account of repression, and Petocz and Mackay note that I sidestep the complex issue of the logical paradox of “knowing in order not to know” (Maze & Henry, 1996). There are a few reasons for not having addressed this. As Petocz notes, this raises issues that are likely to be contentious (e.g., “consciousness”), which although not problematic in themselves, would likely detract from the general flow of argument being made. It is also an issue that I have chosen to discuss in depth elsewhere (Boag, submitted-a), although I would add the following comment. One problem of Freud’s censor account, which Maze and Henry (1996) note and which I draw attention to in the target article, is explaining the motivation of the censor and the supposed relation to the ego. How does the censor know what to repress? It cannot be informed by the ego, because the ego does not know the repressed, so the censor must have its own agenda. However, understanding the mind in terms of multiple knowers is not a problem (Boag, 2005; Petocz, 1999), nor is saying that one part of the personality prevents another part from knowing aspects of mental life based on anxiety. In fact, a dynamic account of repression, where the repressed is re-known prompting anxiety, is possible, and the resolution of the paradox hinges on the recognition that repression (mediated by neural mechanisms) inhibits knowledge of knowing the repressed (see also McIlwain’s commentary). Additionally, Petocz is correct in saying that a discussion of “consciousness”—specifically the *relational* view that she refers to—“would be an opportunity to address the underlying confusion about the mind–brain relationship” (cf. McIlwain’s commentary), and perhaps such a discussion would have obviated certain comments concerning “higher” and “lower functions,” the “ego,” and their relation to consciousness. As McIlwain points out, there are also important questions concerning precisely what is meant by knowing one’s own mental processes (“self-observation”). However, I cannot attempt to do justice to these issues in such little space and will simply direct the interested reader to Mitchell’s (1988) paper, which I believe provides the most coherent account of the above-mentioned issues.

Determinism, motivation, and the instinctual drives

Although Mark Blechner begrudges the focus on Freudian psychoanalysis, Freud’s general commitment to determinism (even if not always consistently applied) and his theory of instinctual drives helps conceptualize a coherent account of motivation that makes his theory particularly suitable for neuroscientific research and the study of dreams. Since to make sense of the world is to understand its workings, which involves understanding its causal relations, “[t]hose who are interested in mind’s workings will naturally take up a determinist position” (Anderson, 1936, p. 125). Here any explanation of behavior will be in terms of causal antecedents rather than attributing a “free,” self-changing, or self-directed variable or positing future events or “purposes” as causes. If every event is caused by antecedent conditions, then the outcome or purpose is irrelevant to explanation, and Blechner is certainly correct when he says that my position “is a long way from Freud’s view that . . . the reason we dream is to disguise and contain that drive so that we can stay asleep.” What is identified as a “purpose” or “reason” of dreaming is really an effect (that may or may not occur), and if we are attempting to understand *causes* of effects, any supposed “purpose” is unnecessary for explaining why something occurs (see Maze, 1983, p. 19). This is not to say that beliefs about the future are thereby redundant in explanations of behavior (since they may be causal antecedents), but beliefs alone cannot explain how the organism acts; some account of what drives (impels) and directs behavior (i.e., the policy to act on any given belief), consistent with a deterministic psychology, still needs to be provided (Mackay, 1994, 1996; Maze, 1983, 1987). This is where a theory of motivation is necessitated for any account of behavior, and here it is Freud’s theory that proves to be substantive. As Maze notes: “Freud’s metapsychology, though unfinished, was the one great systematic attempt in modern psychology to outline a deterministic, physiologically based theory of motivation and extend it to embrace all of human behaviour, bodily and mental” (Maze, 1983, pp. 142–143).

Although drive or instinct theories are open to explanatory abuse (e.g., positing any instinct or drive to explain any given behavior), as Maze (1983) points out, Freud’s (1915) stipulation of identifying drives by their bodily source prevents positing drives *ad hoc* and *ad libitum*. Here Maze (1983) develops Freud’s notion of the instinctual drive “source” in terms of what he calls “biological engines” or physiological driving

mechanisms, “not to be confused with the notion of disembodied forces or energies” (p. 136), and such an account is entirely compatible with neuroscientific conceptualizations and “indispensable as an explanatory basis for a science of behavior and mental life” (p. 176). If we reject the notion of freely chosen mental activity, then any serious inquiry as to the causes of why one person thinks of x at one time and y at another, or finds one situation gratifying or frustrating, etc., will require a comprehensive account of motivation, which Freud’s general model provides. As McIlwain’s commentary points out, “drives, for Freud, are somatically anchored sources of policy with regard to aspects of reality relevant to their satisfaction.” That we may *think* we have “free will” simply reflects our ignorance of the actual causal mechanics involved. Thus, as Maze (1993) has cogently demonstrated, although developments in object-relations theory may provide important insights into aspects of personality, such theories still require a platform along the lines provided by Freud (an appeal to basic drives) to explain why any object is satisfying or frustrating in the first place.

Such biological drives are not mere theoretical fictions, because there is ample “hard” evidence of their existence (Panksepp, 1999, 2003; Pfaff, 1999; Sowards & Sowards, 2003), even if their complete workings are to be fully understood. Furthermore, the evidence of *multiple* motivational systems provides a neuroscientific basis for appreciating that: (a) any behavior may reflect the confluence of several drives, and (b) that behavior may result from motivational conflict, especially considering that the socialization process, for the most part, attempts to extinguish certain behaviors through punishment (drive frustration) while instilling ideals (substitute gratifications) and moral prohibitions (Maze, 1983). I should also emphasize the role of affective processes here, which McIlwain draws attention to, especially with respect to “*affective inhibition*,” because this is a central feature of Freud’s deterministic account of motivated distortion based on anxiety (“Affective inhibition promotes the displacement of interest along associative chains until the drive stands in relation to an imagined state of affairs that comes in sufficiently below the anxiety bar set by morality”—McIlwain). Furthermore, in terms of neurological underpinnings, it appears that “affect and drive are two sides of the same coin” (Lezak, 1995, p. 94; cf. Panksepp, 1999, 2001), and so any account postulating affective processes contributing to dreams should be committed to some discussion of motivation as well. Similarly, Freud’s (1900) example of the “hungry baby” (p. 565) provides a coherent framework for

how “wishes” and “desires” arise in the first place (see also Mackay’s 1994 discussion).

While not pretending to have done justice in such brief space to the Freudian position, Freud’s deterministic, mechanistic position is entirely compatible with a neuroscientific approach to understanding personality and provides a concrete approach to understanding the driving, affective mechanisms and the conflict arising due to socialization, which contribute to behavior, feeling, and thought. This Freudian neuropsychanalytic approach penetrates beneath the level of “person” to reveal the underlying dynamics of behavior, dynamics that reveal multiple neural/physiological structures driving us into relations with the world and others (Maze, 1993; Peskin, 1997; Mills, 2004). Thus “motivation,” conceived of in terms of drives determining behavior, is not the “phlogiston of psychology” (Morgane & Mokler, 2000, p. 976); given both the empirical evidence and conceptual arguments, I do not see how any serious attempt at a complete theory of behavior and mental life (including dreams) can neglect a theory of drives, affects, and conflict. With this in mind, I will address some issues raised by the commentaries of Petocz, Gottesmann, and Cartwright.

Primary- and secondary-process mentation and the sleep-wake cycle

In reference to my target article, Agnes Petocz points to an “uncritical appeal” to Freud’s discussion of primary-process mentation as “the default explanation of dream disguise and bizarreness.” Petocz’s criticism of the primary/secondary-process distinction is with respect to both the supposed temporal order and in terms of positing *qualitatively* different processes and systems of the mind. Simply put, the primary process must actually follow the secondary process “since one needs to have perceived the real object in order to be able to hallucinate it” (Petocz, 1999, p. 169), and the supposed peculiarities said to be exclusive to one process or system can be found in the other (and so there is not a case for qualitative differences at all—see also McIlwain’s commentary). Although Petocz calls for abandoning the terms primary and secondary process altogether (p. 170), she acknowledges a certain, albeit limited, descriptive value; for instance, the infant may be especially prone to wishful, hallucinatory thinking given an inability to tolerate frustration. I am in agreement with Petocz’s general argument here (the rejection of Freud’s “systematic” account of mentality), but I believe that we can possibly salvage something

resembling the primary process (other than Petocz's example above), without necessarily being committed to the identified problems.

I am particularly grateful for the inclusion of the commentaries from both Claude Gottesmann and Rosalind Cartwright since to an extent their research represents two broad directions within dream research. Gottesmann compares dreams to psychotic episodes, based on brain-state changes (a position, for example, similar to that of Hobson, 2005, and Kahn & Hobson, 2003), while Cartwright believes that dreams are generally nonbizarre and reflect ordinary concerns. I believe both positions contain an element of truth, although both require further thinking through, which when achieved demonstrates that the insights of either position are best accommodated within a psychodynamic framework.

Beginning with Gottesmann's account, it is not important for psychoanalytic theory to deny the effect of differences in brain states upon dreaming. As Colace's commentary notes, Freud's overdeterminism view of causality is a theoretical strength, if for nothing more than that it provides a realistic perspective of the complexity of the world. Thus it is not the case, as Blechner seems to attribute to the Freudian position (or perhaps to me), of seeking "a unitary cause of dreaming." As Freud (1913a) writes in connection with religion (but which, I think, applies equally well to dreams): "If psycho-analysis is compelled . . . to lay all the emphasis upon one particular source, that does not mean it is claiming either that that source is the only one or that it occupies first place among the numerous contributory factors" (p. 100). Moreover, Freud was well aware that organic states needed to be taken into account before jumping to psychogenic conclusions, even if psychoanalysis dealt exclusively with the latter (Freud, 1913b, p. 175). Turning, then, to Gottesmann's account, we find: "The electrophysiological, tomographic, . . . pharmacological and neurochemical characteristics of REM sleep show highly significant modifications when compared to normal waking. It would be surprising if such disturbances . . . had no influence on the higher integrated brain processes, thus inducing the bizarreness of dreaming not necessarily related to disguised content."

Leaving aside the issue of the relationship between REM sleep and dreaming (although it appears that the former is not a necessary condition for the latter: Bischof & Bassetti, 2004; Solms, 2000), it may well be that such brain-state characteristics contribute to dream-bizarreness. However, as is well recognized within psychoanalytic thinking, the problem with any

account that attempts to explain dreams purely in terms of general patterns of brain activity is that the apparent simplicity conceals an explanatory failure. As Terry McMullen (University of Sydney) recently reminded me, such accounts do not explain the *specific content* of dreams. In fact, Kahn and Hobson (2003, p. 58) explicitly state that their approach is an "attempt to articulate the distinguishing universal characteristics of dreaming as against the individual content of dreams" (though whether you can divorce universal characteristics from an account of specific content requires further consideration). Nevertheless, I appreciate Gottesmann's commentary because it explains in exceptional technical detail that brain activity in dreaming resembles that found in psychotic states, which appears to be an interesting and incontrovertible point. As is well known, Freud, for his part, also recognized the "indisputable analogy between dreaming and insanity" (Freud, 1900, p. 92). However, Freud went a step further in recognizing that "there is, of course, no such thing as arbitrary determination in the mind" (Freud, 1901, p. 680) and that a *comprehensive* theory of dreams must, at least in principle, account in causal terms for why a person dreams of x rather than of y or z , in the same way as we might try to understand why some people develop paranoid schizophrenia instead of delusions of grandeur. Any suggestion that dream content is "random" (e.g., Hobson, 1999, p. 172) merely postpones the explanatory question, for what is literally meant by "random" here? Chaotic? If so, it is difficult to imagine how repeated and coherent themes in dreams could be possible when they clearly are. Moreover, from a determinist position, "random" means that we are simply not aware of all the relevant causal antecedents, in much the same way as the results of a coin toss may *appear* random although we know that either result is purely due to explicable causal factors (i.e., "chance" is not a variable in itself). Alternatively, appeal to "greater excitability" of memories to explain dream content also requires careful consideration. For instance, Gottesmann writes: ". . . dual electrophysiological and neurochemical cortical disinhibition during REM sleep could also help explain the recurrent dreams, mainly composed of negative affects, that sometime occur even long after traumatic life events. The pressure of memory-'traces' content with higher excitability to enter the dreamer's consciousness could be stepped up by the decrease or loss of cortical control."

This raises questions as to what is precisely meant by "higher excitability" and whether there would be a reason to expect that it precludes motivational

processes (Freud, of course, realized that memory and motivation are intimately bound, for which there exists neurological evidence: e.g., Morris & Dolan, 2001). In any case, a causal account of “higher excitability” requires explication, and although there may be alternative explanations here (Gottesmann raises the issue of “abreaction,” but I am not sure, myself, how best to coherently conceptualize such a process), I believe that a psychodynamic model postulating neural bases for affective and motivational processes (and taking into account learnt associations and conflict) addresses this issue. In fact, if you accept a deterministic model of the mind, I do not see how motivational factors can be omitted from a theory of dreams, and to give substance to the notion of motivational factors one needs to postulate drives of the type discussed earlier.

Having said this, however, I believe that accounts such as those proposed by Gottesmann do make an important contribution to our understanding of dreams. Specifically, such accounts may be stipulating states of brain activity that are particularly prone to the condition Freud referred to as primary-process mentation (displacement, condensation, illogical and irrational thinking styles). In much the same manner as a fever or certain drugs may influence coherency, style of thinking, or tendency to hallucinate, the state of sleep (and perhaps especially more so REM sleep) may similarly be a state that gives rise, for example, to a tendency to confuse x with y and not to recognize the difference (“tendency,” though, is a question-begging term, and more is needed to provide a coherent mechanism of what precisely is going on). However, although sleep, like the effects of certain drugs, may influence our style of thinking, if you accept a deterministic account of motivation it is still our motivational states, responsible for our waking cognition and behavior, that are carried over into sleep, providing the impulsion and direction of what is thought and dreamt about.

Motivational processes may, of course, be affected by interactions with substances or states that intensify or reduce levels of drive (Cardinal & Everitt, 2004; Colace, 2004), but while psychoanalytic thinking may too often jump to attributing psychogenic causes to pathological conditions (rather than considering organic ones), so too do proponents of purely biological accounts forget that when dealing with humans we are dealing with motivated, dynamic beings. As the clinical studies reported by Kaplan-Solms and Solms (2000) clearly demonstrate, although organic damage may affect particular functioning, the psychodynamic factors are not thereby null and void but, instead, play themselves out to the extent that their “vehicle” permits (unless, of course, the necessary areas for motivational

processes are disrupted, which then reveals deficits in activity: see Lezak, 1995). Thus, although organic pathology may possibly contribute to conditions such as attention-deficit/hyperactivity disorder (Aron & Poldrack, 2005) or obsessive-compulsive disorder (Chamberlain, Blackwell, Fineberg, Robbins, & Sahakian, 2005) (I will leave it as an empirical question), motivational processes still contribute to explaining the specific psychological and behavioral content within these conditions. Similarly, just as frontal-lobe damage may lead to problems of “self-regulation” (Grattan & Eslinger, 1991) and “control problems” (Lezak, 1995, p. 95), this still implicates motivational processes requiring regulation and control. In fact, it could even be pointed out that a psychodynamic framework is already to some extent implicit in the nonpsychoanalytic neuroscientific literature. Consider the statements that the prefrontal cortex “permits a flexible transition between competing behavioural patterns” (Konishi, Jimura, Asari, & Miyashita, 2003, p. 7776), or that the basal ganglia mediate “the competition between incompatible inputs” (Redgrave, Prescott, & Gurney, 1999, p. 1016). What else is being talked about here if not motivational conflict?

A theory of drives fleshes out what these behavioral patterns or inputs might be and, in turn, informs and can be informed by neuroscientific studies of “aversive learning” (e.g., Fanselow & Poulos, 2005; Maren, 2003; Maren & Quirk, 2004; Ressler, 2004; Tinsley, Quinn, & Fanselow, 2004) and “response inhibition” (e.g., Kok, 1999; Thayer & Friedman, 2002). The primacy of a psychodynamic model, however, based on one similar to Freud’s, is that it provides an integrative framework for situating all human activity (and before the Popperian objection is raised, it should be remembered that neuroscience is one method of falsifying such a project). A psychodynamic framework readily accommodates concepts such as fear conditioning and response inhibition (as above) and provides a welcome relief from much of mainstream academic research, which attempts to study isolated “faculties” of the mind (e.g., the study of “intelligence” or “memory”) without any appreciation of how these might fit into the “whole.” There is, however, some truth to the statement that “[n]euroscience is still having a ‘resistance crisis’ to integrative concepts that are needed to understand the global abilities of the brain” (Panksepp, 2001, p. 78). Despite this, there are hints that some nonpsychoanalytic neuroscientific perspectives are emerging that view humans as motivated dynamic systems (e.g., Redgrave, Prescott, & Gurney, 1999; Thayer & Friedman, 2002), which dispose of “choice” and instead talk of mechanical, causal processes when discussing

“behaviour selection” (which does not require expelling the importance of psychological processes) and which are at times consistent with Freud’s theory of motivational conflict.

While for the most part the views being presented here are not new and simply restate what many have already noted (e.g., Johnson, Colace), the point I am emphasizing is that although biological brain states (like those posited by Gottesmann) may influence our thinking *style*, such accounts do not negate the role of motivational factors contributing to thinking and behavior (and, accordingly, to dreams). In fact, even if REM sleep were a necessary condition for dreams, it would not mean that it was the only one; there can be many necessary causal conditions for the occurrence of any situation. In fact, I would suggest that motivational factors are just as causally necessary; I would go so far as to say that if motivation (i.e., specifically drive) is taken out of the equation, then you will not have dreams (or any thinking activity whatsoever).

At this juncture I would like to turn to Cartwright’s position where she notes “a continuity of emotional/cognitive concerns across the wake–sleep cycle.” I think that this is a fair observation, but one that requires further thinking through, for what precisely is meant by “emotional/cognitive concerns”? As with Gottesmann’s account, I believe Cartwright’s position is eventually going to have to face the issue of motivation, and, as the previous discussion has attempted to demonstrate, some account of drives will prove to be the most substantive prospect here. As an aside, Cartwright’s comment concerning intense learning and REM sleep (“how invested the subject is in the task to be learned is related to the increase in number of REMs and in REM-density measures”) is intriguing (see also Cartwright, 2000; Smith et al., 2004), because if we give substance to the term “investment” here in relation to the motivational drives, then we are basically saying that our motivational states contribute causally to REM occurrences.

In any case, I do not think that any neuropsychanalytic account disputes the finding that specific changes in brain activity may influence the style of thinking characteristic of the dreaming state (Gottesmann). But I would also suggest that our “ordinary concerns,” as products of the drives (our motivational and affective processes), do carry over into sleep (Cartwright). The state of sleep, in a similar fashion to the effect of drugs, may allow our motivational states to be subjected to hallucinatory experience and hyper-associative types of thinking. Whether this should be considered “regression” from secondary- to primary-process mentation is debatable (and perhaps better

conceived in terms of the ebb and flow of our biological lives). Nevertheless, attempting to give a universal account of dreams without taking motivational factors into account will always be incomplete. Consequently, a neuropsychanalytic model that appreciates motivational sources, affective processes, conflict, inhibition (repression), and also takes into account the effects of changes of nervous system conditions on subjective experience seems to me a plausible biological account for appreciating the nature of dreams and, indeed, all of human behavior.

Dream bizarreness and wish-fulfillment

I would like to respond to Blechner’s interesting commentary to clarify another issue related to wish-fulfillment and dream-bizarreness. Blechner reports the case of a patient who had once earlier raped his cousin and later dreamt that he forced his daughter to have sex with him. Blechner writes that “[i]f there were a dream censor, it was not working very well; it did not mask the incestuous and coercive nature of the memory that was troubling him. Neither Freud’s censorship model nor the model . . . elaborated . . . [in the target article] adequately account for my patient’s dream.” Although this last statement is immediately contradicted by Blechner’s next paragraph (where he provides an example of how a “compromise-formation” model might explain it—before appealing to nontestability), I would first like to clarify an important issue that I should have made explicit in the target article. I did not mean to imply that dreams are never simply examples of transparent, unconflicted wish-fulfillment (although, regrettably, the phrase “ubiquity of conflict,” taken literally, does not admit to degrees of “everywhere”). The position that all dreams involve conflict and “disguise” is also at times attributed to the Freudian position, and then evidence of open wish-fulfillment is cited as evidence against Freudian dream theory (e.g., McCarley, 1998, p. 131), despite Freud himself providing clear examples of openly wish-fulfilling dreams (e.g., Freud’s own dream of drinking water after eating salty anchovies—1900, pp. 123–124—or his report of children’s dreams—pp. 127–131; see also commentaries by Colace and Mackay). Consequently (returning to Blechner’s criticism), for someone who raped a cousin with impunity, dreams of sexual coercion might not be sufficiently anxiety-provoking to lead to repression and might simply be demonstrating open wish-fulfillment (despite a case of guilty conscience). How are we to know who is correct? There is no simple answer, but I would like

to briefly address Blechner's own interpretation of the dream, because it appears to be held up to demonstrate the truth of his theory. We are told that at some time prior to the dream of coercing his daughter into sex, the patient, when confronted by the cousin that he had raped, denied it:

The patient, having denied the abuse of his cousin, felt guilty. The latent dream thoughts were something like: "I disowned the gravity of what I did to my cousin, and the way my adult denial to her aggravated the situation. In my dream, I consider: what if it were my own daughter? Would I be more concerned about the consequences of an incestuous rape of my own daughter?" . . . The dream allows my patient to confront the troubling nature of what he did to his cousin by portraying it in a situation that would be even more troubling than the reality. The problem is solved in this case by a manifest dream whose content makes the dreamer more anxious than the related dream thoughts (Blechner).

It is not clear how the correctness of Blechner's interpretation here is any more testable than a "compromise-formation" position, except perhaps that the patient accepted the interpretation as correct. Unfortunately, such an interpretation could be accepted by the patient simply because it provides a satisfying rationalization for someone "feeling guilty" about the past (allowing the patient to see himself as altruistic and caring) and/or providing a convenient screen for actual sexual desires. Thus, if the patient's acceptance of the interpretation, or even "symptom reduction" (e.g., mitigating guilt), is used to demonstrate that the theory behind the interpretation is correct, then the difficulty of relying on such indicators is that humans seem particularly prone to accepting gratifying fantasies at the expense of faithfulness to reality (consider, for instance, the "opiate" of religious beliefs), as reflected in the statement: "*We believe what eases our minds*, whether it is true or false" (Anderson, 1934, p. 72, emphasis in original). So how can we progress toward evaluating which theory is correct? I would suggest that a theory that provides a coherent deterministic conceptualization of the motivational components behind the dream, including the physiological dynamics of personality (i.e., the neural substrates of affective/motivational processes), affords a more promising avenue for evaluating the plausibility of any such theory or interpretation. While I will not attempt to definitively refute Blechner's theory here, I would suggest that he thinks further about his view of the motivational processes underlying such a dream and whether such a "problem-solving" account can be made consistent with a deterministic psychology.

Final comments

I opened this response to commentaries noting the importance of theoretical clarification before attempting to discuss empirical evidence, and I would like to briefly address another area of contention that I believe needs sorting out so that the dream debate can move forward. When Hobson (2005) writes that dreams "do reflect an individual's personality, concern, feeling, and conflicts . . ." (p. 28), it is unclear why there is so much resistance on his part to accepting "Freudian" motivation and conflict contributing to dream plots. However, I think that part of the problem here is that Hobson is attacking something other than what is being proposed in accounts such as that provided by Solms (2000). For instance, Hobson (2000) appears to expect "Freudian wishes" to be somehow different from any other motivated desire, because he writes, in response to Solms's theory: "how we get from motivation and reward to Freud's unconscious wishes is not at all clear" (p. 952). However, if we look at what Solms has to say, it is clear he is discussing Freudian wishes plainly in terms of "appetitive cravings" and takes Freudian theory, in part, to propose that "[d]reams are *motivated* (i.e., dream plots are shaped by basic emotions and drives . . .)" (Solms, 2004, p. 20, emphasis in original). Similarly, Colace (2004), following Freud, discusses "infantile wishes" simply in terms of "imperative needs" (p. 173). If this is the theory "on the table," so to speak, it is unclear to me, given what Hobson writes above, why there is any reason for him to reject this particular aspect of psychoanalytic dream theory (although it raises the question of whether Hobson is engaging Solms's [et al.'s] account for what it actually is). Unfortunately (and without specific reference to Hobson's research here) attacks on stereotyped positions attributed to Freud are so common and unamenable to correction that they appear to take on a pathological dimension in terms discussed by Mitchell (2000; cf. Boag, in press). Nevertheless, any serious scientific exposition and evaluation of a theory must address such basic conceptual issues.

While I do not believe that neuroscience is the only method for psychoanalysis here to scientifically develop (I would, in fact, argue, that much more conceptual work needs to be conducted as well), I do believe that neuroscience promises a tangible measure that will help provide greater demonstration and understanding of psychoanalytic concepts. In particular, I believe that neuroscience is one important step toward demystifying psychoanalysis and making it intelligible to the interested nonclinical researcher. Furthermore, tying concepts such as repression and inhibition to neural

mechanisms further helps avoid the logical problem of circular explanation that Macmillan (1991, 1996) has drawn attention to (Boag, submitted-b). Additionally, while there is no theoretical objection to openly wish-fulfilling dreams, I believe that the amount of conflict in mental life is easily underappreciated, given that for the most part we have no knowledge of the biological factors (which are undoubtedly the source of our behaviors), nor of the effects of early socialization, that shape our mental life. The present directions in neuroscience that break the “person” down into a collection of motivational systems, each “jostling” within a singular organismic apparatus, and the psychoanalytic finding that socialization directly targets our biological imperatives and gives rise to “internal” constraints (the superego), make drive conflict and competition seem inevitable. Consequently, although dreams may at times be transparent, unconflicted forms of wish-fulfillment, I do not see how motivational conflict could not be a major contributing factor shaping our dreams and consequently giving rise to what appears to be their bizarre characteristics.

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