From Dynamic Lesions to Brain Imaging of Behavioral Lesions: Alloying the Gold of Psychoanalysis with the Copper of Suggestion

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Contemporary studies in the cognitive neuroscience of attention and suggestion shed new light on psychoanalytic concepts of yore. Findings from neuroimaging studies, for example, seem to revive the notion of dynamic lesions—focal brain changes undetectable by anatomical scrutiny. With technologies such as brain imaging and reversible brain lesion, some findings from modern biological psychiatry seem to converge with nineteenth-century psychiatry, reminiscent of the old masters. In particular, suggestion has been shown to modulate specific neural activity in the human brain. Here we show that "behavioral lesions"—the influence that words exert on focal brain activity—may constitute the twenty-first-century appellation of "dynamic lesions." While recent research results involving suggestion seem to partially support Freudian notions, correlating psychoanalysis with its brain substrates remains difficult. We elucidate the incipient role of cognitive neuroscience, including the relative merits and inherent limitations of imaging the living human brain, in explaining psychoanalytic concepts.

Keywords: fMRI; genetics; hypnosis; neuroscience; ontology; suggestion

> "It is very probable, too, that the large-scale application of our therapy will compel us to alloy the pure gold of analysis freely with the copper of direct suggestion . . ."
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> Sigmund Freud, Fifth International Psycho-Analytical Congress, Budapest (Freud, 1919 [1918], pp. 167–168)

Alongside the established science of attention, the unfolding science of suggestion is gradually reframing core psychodynamic ideas. Suggestion and attention are pivotal themes in cognitive science (Raz & Buhle, 2006). They reify the links between brain and behavior and bind psychology to the techniques of neuroscience (Posner & Rothbart, 2007a). Experimental findings show that suggestion and attention influence cognition, affect, thought, and action (Posner & Rothbart, 2005, 2007b). In addition, studies involving imaging of the living human brain and genetics begin to unlock the neural underpinnings of and the mechanisms underlying the influence of suggestion on behavior (Posner, Rothbart, & Sheese, 2007; Raz, 2008a). The bulk of the evidence supports the idea that attention and suggestion form overlapping organ systems (Raz, 2005a). Indeed, these psychological constructs draw on overlapping brain circuitry, functional neuroanatomy, neuromodulators, and cellular structure (Fernandez-Duque & Posner, 2001; Posner & Fan, 2004; Raz, 2006; Raz, Lamar, Buhle, Kane, & Peterson, 2007). Thus, the relationship between attention and suggestion has been established theoretically as well as experimentally (Raz, 2004, 2005a, 2005b, 2006, 2007, 2008a; Raz & Buhle, 2006; Raz, Lamar, et al., 2007).

Multiple accounts corroborate the involvement of psychological parameters, including suggestion and expectation, in the modulation of biological processes (Harrington, 2008). The literature is fraught, however, with uncritical accounts of suggestion. In one cancer patient, for example, suggestion seemed to trigger dramatic tumor shrinkage and miraculous remission, while a subsequent suggestion led to abrupt death (Klopfer, 1957). Beyond anecdotes, many compelling assays demonstrate the power of suggestion (Brown, 1985; Gauld, 1992). Believing that they were ingesting alcohol, for example, participants in psychology experiments displayed the symptoms of alcohol intoxication

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even when drinking nonalcoholic beverages (Marlatt & Rohsenow, 1981). Treated as if they were hypervigilant pilots, Harvard undergraduates outperformed "regular" Harvard control students on visual acuity tasks, including a "routine eye check-up" (Langer, 1989). Although follow-up research demonstrated that visual acuity hardly improved, students in the experimental condition were better than controls at identifying small targets on a screen (Raz, Marinoff, Landzberg, & Guyton, 2004; Raz, Marinoff, Zephrani, Schweizer, & Posner, 2004; Raz, Zephrani, Schweizer, & Marinoff, 2004). In another study, researchers found that by repeatedly suggesting to adults that they had become ill from eating particular foods in childhood, participants consequently avoided those foods (Bernstein, Laney, Morris, & Loftus, 2005). Taken together, such studies provide a sampling of the diverse influence that suggestion can wield on physical and mental experience.

Recent findings elucidate early psychodynamic ideas regarding the power of suggestion. The French neurologist Jean-Martin Charcot coined the term "dynamic lesions"—anatomically unobservable neurophysiologic alterations that produce a marked change in behavior—to explain what was occurring in hysteria (Charcot, 1889). Thereafter, Freud theorized that certain behaviors are the result of bodily changes that take place in response to language (Freud, 1893). Most scholars agree that by "language" Freud was likely referring to abstractions that go beyond mere words (Makari, 2008). We propose that one central formulation—suggestion—carries a powerful psychological influence that affects individuals via their subjective beliefs and experiences.

Recontextualizing psychoanalysis through the lens of cognitive neuroscience, we sketch herein how modern findings rekindle the old flame of dynamic lesions. We demonstrate how recent research findings from studies of suggestion are congruent with at least some psychodynamic concepts. The conceptual reduction of psychoanalysis to brain mechanisms is appealing, albeit unlikely. We outline the shortcomings and relative virtues of such a reductionist account. Finally, we submit that the field of neuropsychoanalysis may benefit from adopting an abstemious outlook regarding the prospects of brain imaging, while we argue that Charcot's and especially Freud's grasp of suggestion may benefit from the modern designation of behavioral lesions.

From dynamic to behavioral lesions

The concept of dynamic lesions has been helpful in explaining how psychological stressors may propel organic brain changes (Chertok, 1977). Observing hysteria, Charcot postulated that temporary neural changes correspond with patient symptomatology. According to Charcot, for example, a portion of the right hemisphere was responsible for hysteric paralysis of the left arm (Koehler, 2003). Although unable to find a focal brain abnormality to account for his patients' symptoms (Goetz & Bonduelle, 1995), Charcot considered dynamic or "functional" lesions to be the underlying cause. Thus, the emerging dynamic lesion model linked organic disorders with unexplainable somatic symptoms (i.e., today's psychogenic disorders).

Reminiscent of dynamic lesions, the recent technology of transcranial magnetic stimulation (TMS) produces transient changes in brain function. TMS delivers a short burst of a powerful magnetic field to a specific brain area, inducing a temporary brain "lesion" that is reversible in nature and leaves no anatomical traces (Bohning et al., 1998). Brain researchers uncover more complex lesion-like behaviors using variations of this methodology including repetitive TMS, which induces longer-lasting, yet temporary, changes (George, 2003). As we show throughout this piece, despite differences, intriguing overlaps bind TMS-induced dysfunction to Charcot's age-old concept of dynamic lesions.

Increasingly ubiquitous, functional magnetic resonance imaging (fMRI) is a noninvasive brain-measurement technology, which opens a window into the neurological underpinnings of behavior. By examining the influence of words on the workings of the mind, fMRI can disentangle the behavioral lesions that specific suggestions can invoke (Raz & Shapiro, 2002; Shapiro, 2004). As a case in point, using hypnotic suggestion as an experimental intervention, one of us has conducted multiple imaging studies showing that specific suggestions correlate with focal brain changes (Raz, 2004; Raz & Buhle, 2006; Raz, Fan, & Posner, 2005). For example, suggestion has been shown to influence neural processing in the domains of color vision (Kosslyn, Thompson, Costantini-Ferrando, Alpert, & Spiegel, 2000), audition (Szechtman, Woody, Bowers, & Nahmias, 1998), pain (Kong, Kaptchuk, Polich, Kirsch, & Gollub, 2007; Rainville, et al., 1999), and word-reading (Raz, Moreno-Iniguez, Martin, & Zhu, 2007; Raz, Shapiro, Fan, & Posner, 2002; Raz et al., 2005).

Suggestion reduces conflict

The classic Stroop task serves as a clear example of the top-down effect of words on brain functioning. In this paradigm, participants identify the ink color of printed letters. Individuals are usually slower and less accurate indicating the ink color of an incompatible color word (e.g., responding "blue" when the word "RED" is displayed in blue ink) than identifying the ink color of a congruent color word (e.g., responding "red" when the word "RED" is inked in red). This difference in performance constitutes the Stroop conflict and is one of the most robust and well-studied phenomena in attentional research (MacLeod, 1991). The dominant view of Stroop researchers regards reading as a largely automatic process whereby skilled readers cannot withhold activating a word's underlying meaning despite explicit instructions to attend only to its ink color (MacLeod & MacDonald, 2000).

Findings from people who performed a classic Stroop task under the influence of a posthypnotic suggestion to obviate the Stroop conflict challenge the automaticity of the Stroop effect. Suggesting that the stimuli (i.e., English Stroop words) would be meaningless scribbles written in an unfamiliar foreign language either removed or reduced Stroop interference and facilitation in highly hypnotizable participants (MacLeod & Sheehan, 2003; Raz, 2004, 2006; Raz, Moreno-Iniguez, et al., 2007; Raz et al., 2002, 2003). Investigation of the neural correlates of this phenomenon uncovers a complex and compelling story (Raz, Fan, & Posner, 2005). If suggestion can override what most cognitive scientists consider an automatic process such as reading, the notion of automaticity may require a revision. In line with Charcotian ideas, the top-down influence of suggestion could have important therapeutic potential in reversing other ingrained, seemingly automatic behaviors and in elucidating the neural substrates of placebo responses. For example, our pilot data from children diagnosed with Tourette's syndrome show that hypnotic suggestion can transiently ameliorate tic symptoms (Raz, Keller, Norman, & Senechal, 2007). The idea of testing individuals while changing their attentional efficiency with suggestion, rather than altering the experimental task, is in line with recent reports about the effects of attention training and meditation (Kerr et al., 2008; Lutz, Slagter, Dunne, & Davidson, 2008; Moore & Malinowski, 2009). Highly suggestible individuals—that is, the vast majority of children and about 15% of adults—could well be candidates for investigation in this new field (Raz & Buhle, 2006).

These Stroop experiments, as well as the "behavioral-lesion studies," outline the neural correlates underlying how experimental suggestions can dramatically impact behavior. Therefore, fMRI may support the link between dynamic and TMS-induced lesions.

Early ideas surrounding dynamic lesions seem relevant in our technology-laden era. While TMS can emulate dynamic lesions, fMRI affords a look into the behaving brain. Despite substantial technological challenges, concurrent TMS-fMRI measurements are beginning to occur (Denslow, Lomarev, George, & Bohning, 2005). When properly yoked, these disparate techniques seem to complement one another and elucidate the spectrum of dynamic-behavioral lesions.

A brief history of suggestion

Individuals under the influence of a charismatic authority can have bodily experiences that many professionals consider to be "all in the head." The King's Touch (KT), for example, refers to the historic belief that illness could be cured by the touch of a divinely inspired leader (Jacob, 1974). Whereas KT is more difficult to trace down in other cultures, Western European history identifies Edward the Confessor as the first ruler who touched to cure (Alexander & Selesnick, 1966). Following his rule, kings in both France and England cured diseases by means of touch, with specific maladies (e.g., tuberculosis of the neck) seeming especially amenable to the hand of the king. By the fifteenth century, KT had extended beyond the throne. Consequently, Irish stroker Valentine Greatrakes, who initially had few clients, eventually amassed thousands: "his barns and outhouses crammed with innumerable specimens of suffering humanity" (Laurence, 1910). While Greatrakes probably practiced a layman's form of psychotherapy that had previously belonged to members of the ruling class (Bromberg, 1954), modern science speciously dismisses KT as preposterous. One Nobel Laureate, for example, claimed that chicken soup might be a more credible source of healing than KT, reasoning that ingesting soup might have chemical effects on the body, whereas the symbolic act of KT seems impossible to influence physiological change (Weinberg, 1992). Suggestion, however, entails more than words and can bring about veridical physiological change (Harrington, 2008; Kosslyn et al.; Rainville et al., 1999; Raz & Buhle, 2006; Raz, Fan, & Posner, 2005; Szechtman et al., 1998).

From parlor magic and hysteria-inspired psychiatry all the way to contemporary brain research, suggestion has made its way into empirical science (Harrington, 2008; McHugh, 2006). Freud was responsible for much of the conceptualization of this transition, although his original ideas may gall contemporary behavioral scientists (e.g., see peer commentaries by Crews, by Kihlstrom, by McNally, and by Wegner on Erdelyi, 2006). Because and in spite of Charcot, Bernheim, Breuer, and Janet, Freud was able to carve out a distinct variation of French psychopathology. By tracing Freud's interaction with the concept of suggestion, we can begin to see how our modern conception of it matured. Following Théodule Ribot's model, Charcot employed associational psychology alongside hereditary explanations to conclude that hypnotic suggestions permit ideas to enter the mind. At the same time, intellectuals such as William Carpenter in England and William James in America speculated that humans are actually automata reigned over by unconscious physiology. Instead of focusing on physiology, however, Charcot's novel approach relied on psychology. His growing psychological theory was compelling: if one idea could cause paralysis, then another idea may cure it. Together with Freud, physicians from around Europe flocked to Paris to witness both Charcot's dazzling performances of hypnotic suggestion and the dramatic patient behaviors that followed. These doctors were eager to learn the scientific method of the *psychologie nouvelle*—a method that would soon after disintegrate.

Leaving Vienna and fleeing mounting criticisms of his advocacy of cocaine, Freud arrived in Paris to learn from Charcot that the days of great discovery in pathological anatomy were over. Charcot was adamant to go beyond anatomical lesions. Although some of Freud's mentors (e.g., Brentano) argued that the science of the mind was too undeveloped to marry physiology with psychology, Charcot and his impressive cadre (e.g., Babinski and Gilles de la Tourette) wowed Freud. Returning to skeptical Vienna, Freud was certain that the altered consciousness phenomena of hypnosis were genuine. He was becoming a prominent Viennese representative of French ideas about hysteria, hypnosis, psychology, and psychopathology, even as incipient omens heralded Charcot's demise.

In 1886, Hippolyte Bernheim of Nancy published his own landmark study, *On Suggestion and Its Therapeutic Applications*, in which he challenged two major tenets of Charcot's Salpêtrière group. Bernheim claimed that hypnosis was *incongruent* with psychopathology (i.e., healthy people could be hypnotizable) and that trances were easy to elicit in the majority of both women *and* men (i.e., not just in women). Furthermore, Bernheim claimed that hypnosis was not even necessary for suggestions to take effect (cf. Raz, Kirsch, Pollard, & Nitkin-Kaner, 2006). Trying to preempt a demoralizing blow, Freud decided to translate Bernheim's book into German. By 1888, readers of the German text encountered an aggressive translator who contended with the author. The debates between the Nancy and Salpêtrière schools generated considerable research and findings that largely countered many of Charcot's dogmas. Consequently, post-1888 Freud began to distance himself from his colleagues in Paris and sidestepped defending Charcot's positions by mentioning that it was incumbent upon advocates of the Salpêtrière to prove their theories.

Rebuking Bernheim and Charcot, Freud outlined what would become his own distinct theoretical ground. By 1892, Freud began to distinguish himself in a crowded field of psychopathologists and suggestive therapists. Through a deep engagement with French medicine, he proposed a model that had the potential to redefine the study of psychopathology. In Paris, however, his ideas won him lifelong enemies, making French clinicians and academics largely hostile to Freudians over the ensuing decades. Sigmund Freud and Pierre Janet, for example, became harsh rivals: while Janet discounted Freud's work as derivative and disparaged its critical innovations as flawed, Freud dismissed Janet for his insistence on an inherited feeble-mindedness in hysterics. Leaving behind French psychopathology, Freud tried to secure his new discoveries by finding a place for them in a scientifically tenable model of the mind. Suggestion was central to Freud's theme.

Freud's view of suggestion as "a conscious idea, which has been introduced into the brain of the hypnotized person by an external influence and has been accepted by him as though it had arisen spontaneously" (Freud, 1888, p. 77) is congruent with today's understanding of the phenomenon (i.e., that under the influence of suggestion, people may undergo temporary physical changes that are solely the product of their own mental states; Raz & Shapiro, 2002). Freud noticed that his patients' symptoms were incompatible with symptoms of organic lesions—patients with hysterical paralyses, for example, did not show the degree of muscle atrophy present in those with biological paralyses (Koehler, 2003), and he reasoned that no fundamental brain damage had occurred. His resulting theory that "slight and transitory" (Freud, 1893) dynamic lesions in certain brain regions affect respective body parts is similar to at least some modern models of short-lived behavioral lesions (Raz, 2008a).

Freud's approach was taken up by psychoanalytically trained psychiatrists, who have demonstrated that suggestion can elicit real changes. One study, for example, investigated the effects of hypnotic suggestion on urine output in patients who had been deprived of fluids for about 15 hours (Hulet, Smith, Schwarcz, & Shapiro, 1963). Findings showed that after suggesting to patients that they had drunk six glasses of water, they increased their urine flow as much as fivefold relative to a prehypnotic baseline condition in which their bladders had

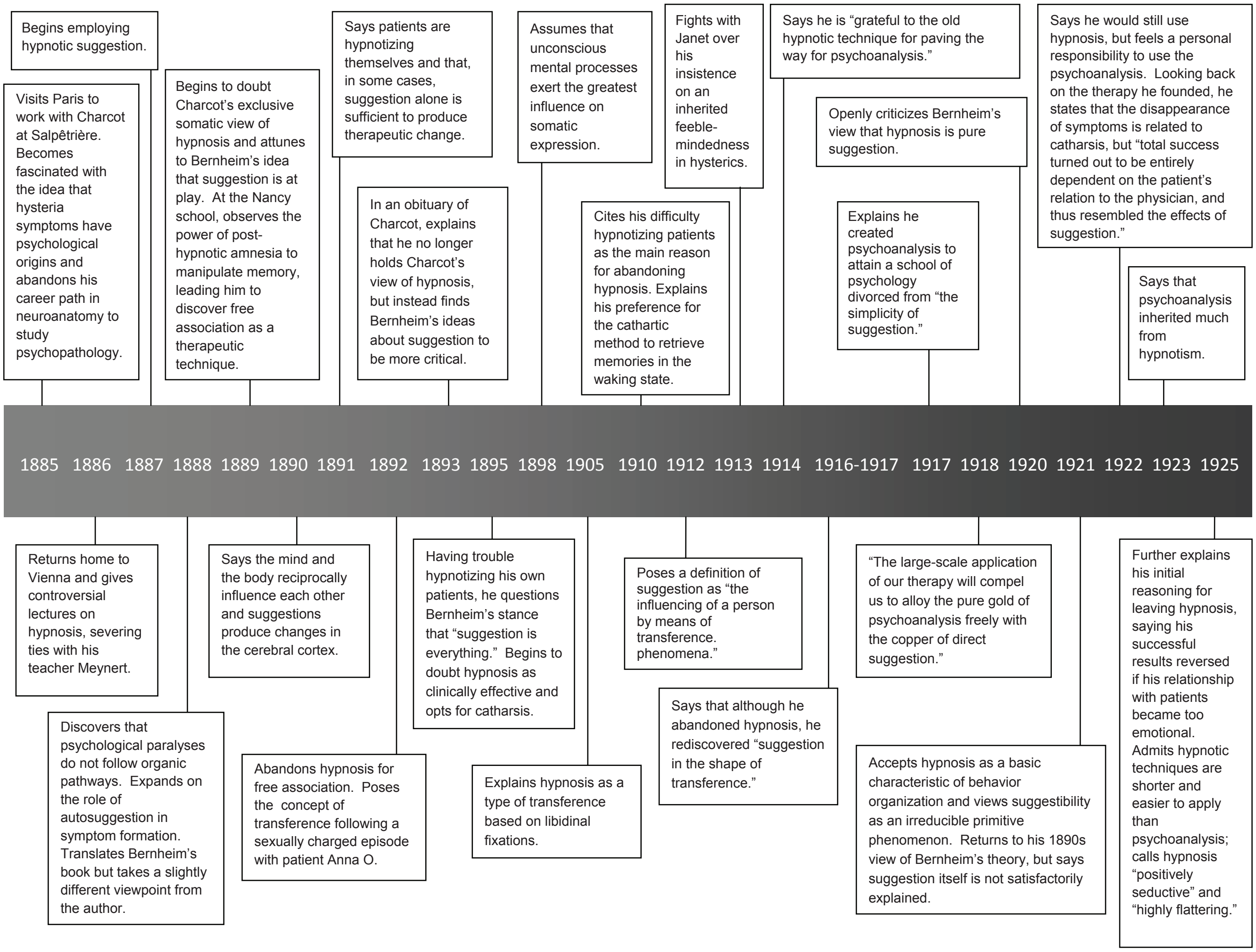

Figure 1. A timeline of Freud's deepening appreciation for suggestion.

been emptied. Findings from comparable studies using neuroimaging illustrate significant signal changes in brain areas associated with sensation and perception (Raz, Fan, & Posner, 2005). For example, we used a posthypnotic suggestion to demonstrate how highly suggestible individuals, who are otherwise proficient readers, appear to turn into neuropsychological patients incapable of reading or processing word stimuli at the semantic, phonologic, or orthographic levels (Raz, Fan, & Posner, 2005; Raz, Moreno-Iniguez, et al., 2007; Raz et al., 2002, 2003). Thus, indirect neurological indices form the backdrop for the resurfacing of Freud's "mysterious leap from mind to body" (Shapiro, 2004). In this regard, selective findings from empirical neuroscience support, at least in part, psychoanalytic ideas.

Modern formulations of suggestion overlap with the increasing cachet of psychosocial parameters in medicine, including demand characteristics (Laurence, Beaulieu-Prevost, & Chene, 2008), expectations (Benham, Woody, Wilson, & Nash, 2006), and placebos (Kirsch, 1985; Lynn, Kirsch, & Hallquist, 2008). These effects have to do with medical changes arising from knowledge that therapy is occurring, rather than from actual effects of a drug or treatment (Benedetti et al., 2003). Previously vilified as products of mere suggestion, such psychosocial parameters are slowly gaining a respectable place in modern medicine (Raz & Guindi, 2008). Norman Cousins, for example, made his way back to health from a serious collagen illness by checking himself out of a hospital and into a hotel, ingesting inordinate amounts of vitamin C, and laughing at comedies (Cousins, 1976). Whether his recovery was the product of autosuggestion or an elaborate venture of self-administered placebos, recent experimental findings show that context—including doctors' words, attitudes, and behaviors—may affect pathological conditions through the modulation of specific neurochemical mechanisms (Benedetti, 2002, 2008) Thus, the contribution of psychosocial factors to medical change elucidates the modern-day conceptualization of suggestion.

The culture of suggestion

People today seem to experience specific diseases differently from how people experienced the same diseases years ago (Harrington, 2008). In the same way that universities or political systems have histories, diseases too go through a process of cultural evolution. Compelling evidence demonstrates that the way we experience stress today is probably a product of the post-World War II era. Before that time people responded to the vicissitudes of life with different symptomatology—such as fatigue, exhaustion, and photophobia—which typically called for them to retire early to bed. Since then, however, we have replaced "bad nerves" with "stress" and have adopted a new way to experience it, and consequently to treat it. For example, Heidi—Johanna Spyri's heroine of Swiss literature whose trip back to nature reverses her failing health and bouts of sleepwalking—and young Teddy Roosevelt—whose hunting and fishing excursions brought him from bad nerves to strength—show that past remedies were different from those we use today. Indeed, a transformation in the way we construe a disease often implies a transformation in the options we may consider for a remedy.

Scholarly illustrations of cultural suggestions abound. For example, menopausal women in Japan rarely experience the hot flashes and night sweats that are widespread among the older women of North America. One potential explanation for this transcultural difference has to do with the way societies view mature women. In contrast to North America, Japanese society seldom construes female aging as a sign of diminished worth. Accordingly, some scholars argue that vasomotor symptoms differ according to the suggestions of these separate cultures (Locke, 1993, 1998). Lactase, the enzyme necessary to digest milk, provides another example that culture is a coconstruct of biology. While the majority of adult humans rarely produce lactase, descendants of populations that domesticated cattle and used milk as a central food source (e.g., Europeans) are more likely to carry a genetic variation allowing lactase to persist into adulthood (Beja-Pereira et al., 2003; Durham, 1991). Clinical examples also include culturally specific instances, such as forms of panic among Vietnamese and Cambodian patients in the United States (Hinton, Um, & Ba, 2001; Hinton et al., 2007). Thus, disparate beliefs and expectations are likely to evoke particular behaviors and experiences. The effect that culture has on behavior is consistent with our modern concept of suggestion.

Perhaps the apotheosis of this type of cultural suggestion comes from the history of hypnosis. The mental and physiological experience that comprises hypnosis has morphed in ways that reflect changing social expectations and mores (Gauld, 1992). Eighteenth-century patients of Anton Mesmer, for example, felt animal magnetism racing through their bodies. Patients of Amand-Marie-Jacques de Chastenet (Marquis de Puységur), on the other hand, replaced these symptoms by providing evidence of having access to heightened, even supernatural, mental abilities. Furthermore, by the second half of the nineteenth century, these occult-like characteristics disappeared, and, instead, hypnosis became a quasi-pathological phenomenon, with specific physiological profiles such as catalepsy, lethargy, and somnambulism. Thus, the collective construction of our mental processes seems to have a history (Harrington, 2008).

Joining psychoanalysis with (cognitive) neuroscience

Reconciling psychoanalytic and neuroscientific perspectives is an honorable cause. In the beginning, Freud sought a description of behavioral and mental functioning that was consonant with contemporary neuroscience (Freud, 1950 [1895]). Thus, it seems reasonable that some members of the psychoanalytic community—as well as of this Journal—dedicate their mission to this goal. Although neuroscience is the generic term, the common allusion is to the subfield of cognitive neuroscience—the study of how the brain enables the mind. Melding (cognitive) neuroscience with psychoanalysis is a potential oxymoron, however, because psychoanalysis and brain science each represent a radically different standpoint (Makari, 2008). Neuroscience suggests an ideal—a fantasy in which biological disease entities avoid the messiness of psychology, meaning, or culture (Kirsch, 1985, 2008; Raz & Guindi, 2008). Psychiatry, for example, has been largely engulfed by this biological model. Although psychiatry has been mostly interested in the social sciences for much of the twentieth century, a biological paradigm has come to dominate it over the past 25 years (Luhrmann, 2000; Shorter, 1998). This approach becomes explicit when leaders, such as the scientific directors of the Canadian and U.S. national institutes that fund mental health, submit to a reductionist biological model in the editorial pages of the *Journal of the American Medical Association* (Insel & Quirion, 2005).

The language and conceptualizations of neuroscience are vastly different from those of psychoanalysis. Even for fundamental "terms of art" that seemingly look and sound the same, neuroscientists and psychoanalysts are often worlds apart (Westen & Gabbard, 2002). According to Charles Brenner (1982), for example, psychoanalysis applies to the study of the mind in conflict; however, appellations such as "conflict" and "conflict resolution" signify radically different meanings in psychoanalysis and in neurocognition. Freud anchored his notion of conflict in instinct theory while neuroscientists typically focus on perceptual—rather than drive—conflict (e.g., variants of the Stroop effect; MacLeod & MacDonald, 2000). Indeed, with a few notable exceptions, neuroscience avoids motivational concepts almost entirely. Cognitive neuroscientists therefore rarely construe conflict as a fundamental rivalry between sex and survival or sex and aggression, and psychoanalysts seldom view conflict through the lens of Stroop-like tasks (Raz & Buhle, 2006). On the one hand, cognitive neuroscience emerged largely from academic psychology, drawing on experimental behavioral science and using paradigms that often had neither ecological validity nor clinical merit. Consequently, the cognitive sciences retained a rather circumscribed discourse regarding conscious mental operations, self-awareness, and subliminal information processing. On the other hand, psychoanalysts drew on the immense world of the unconscious and its chthonic undercurrents of emotion and motivation.

Psychoanalysis' original concern with the "ego" has gradually morphed into an interest in the much more psychological notion of the "self" (Hartmann, 1956). This shift is critical because the self has become a central notion, arguably as fundamental as ego was in the days of Freud. While the gap between psychological science and neuroscience (i.e., between ego and the brain) may be getting smaller, the gap between the experiential (i.e., self- or subjective awareness) and the nonexperiential (i.e., ego or conceptual theories) seems as wide as ever (Michels, 2008). Whereas psychological constructs, such as attention, strengthen the connection between brain and behavior and submit psychology to the techniques of neuroscience (Raz & Buhle, 2006), psychoanalytic concepts, such as the ego, remain largely unbridgeable.

Psychoanalysis and the perils of neuroimaging

Hardly any advance in neuroscience has garnered as much public interest as neuroimaging. The crisp images of the human living brain in action seem to mesmerize the masses, including many a psychoanalyst. For example, because researchers proposed that they can operationalize transference experimentally (Berk & Andersen, 2000), psychiatrists have proposed an fMRI study to capture transference phenomena (Gerber & Peterson, 2006). Furthermore, it is our understanding that psychiatrists are currently organizing research efforts, including neuroimaging, for an outcome study of psychoanalysis in comparison with alternatives such as supportive expressive psychotherapy and cognitive behavioral therapy. Before examining results from any imaging excursion into one of the cornerstones of the psychoanalytic process, however, the psychoanalytic community may want to ruminate about what will likely transpire. After all, it takes a great deal of computer processing and human judgment to get from blood oxygen levels to a snapshot of transference in the brain.

Technologies such as fMRI entice researchers to submit higher brain functions, including morality (Greene, Nystrom, Engell, Darley, & Cohen, 2004), to scientific scrutiny. The images harbored by such efforts, however, may enthrall more than explain (McCabe & Castel, 2008). This type of "neurorealism" speciously leads individuals to believe that images of brain activity make a behavioral observation more scientific (Racine, Bar-Ilan, & Illes, 2006a). Consequently, media coverage frequently oversimplifies research findings and marginalizes caveats (Racine, Bar-Ilan, & Illes, 2006b). In November 2007, for example, the *New York Times* (*NYT*) published an op-ed column describing fMRI findings from undecided voters who viewed photographs and videos of the major candidates in the 2008 U.S. presidential election (Iacoboni et al., 2007). According to the study's authors, the findings revealed "some voter impressions on which this election may well turn." A later editorial in *Nature* lambasted studies that simply place individuals in fMRI scanners and then come up with elaborate stories describing the results (*Nature*, 2007). Hence, as the use of fMRI becomes more ubiquitous, consumers of neuroimaging may benefit from a measure of rigor (Kriegeskorte, Simmons, Bellgowan, & Baker, 2009).

Functioal MRI signals are weak and occur amid much "noise" in the form of false signals. Moreover, the real signals are often so weak that researchers have to stimulate a person's brain time and again to discern an incipient pattern. To study the brain areas that respond to faces, for example, researchers typically present many faces in order to detect an increase in neural activity in a specific brain location. Thereafter, they repeat the experiment on a dozen or more additional individuals to ascertain that the same brain areas consistently light up across people. In many cases, this outcome is unwarranted even though face recognition is a relatively robust process compared with, for example, transference. Thus, psychoanalysts should be careful to embrace fMRI findings identifying higher brain functions that appear to index psychoanalytic constructs.

Functional MRI studies frequently produce billions of data points—most of them sheer noise—wherein one can find coincidental patterns (Kriegeskorte et al., 2009). Additionally, many fMRI studies dip into the same data twice: first to pick out which parts of the brain are responding and then to measure the response strength. This practice of double dipping is statistically problematic and results in findings that appear stronger than they actually are (Vul, Harris, Winkielman, & Pashler, 2009). Thus, onlookers must exercise great caution when beholding the casting of messy data into attractive images.

Rendering psychoanalytic concepts amenable to neuroscience research calls for an even keener appreciation of the limitations of neuroimaging. As 17 prominent cognitive neuroscientists pointed out in a collective reply to the *NYT* op-ed piece, one of the core shortcomings of a naïve fMRI approach hinges on reverse inferences—inferring a specific mental state from the activation of a particular brain region (Aron et al., 2007). For example, anxiety involves fMRI signal changes in the amygdala, but so do many other things, including intense smells and sexually explicit images. The blunder of "reverse inference" is widespread, and many neuroimagers—including signatories to the *NYT* rebuking response—have sinned by reverse-inferencing in an attempt to understand how brain mechanisms subserve mental processes (Poldrack & Wagner, 2004). Because cognitive neuroscience is a relatively new field of scientific inquiry, however, some of the same researchers who have initially advocated the idea of reverse inferences have grown considerably more skeptical of it in recent times (Poldrack, 2006). Although reverse inferences may nevertheless be useful in specific situations, cumulative analyses over the past few years have resulted in marked disillusionment regarding many of the reverse inferences presented in the literature. Thus, past support for reverse inferences has taken a turn against it.

Reverse inferences are particularly common in newer fields such as social cognitive neuroscience and neuroeconomics, not to mention neuropsychoanalysis—fields in which researchers are still trying to identify the cognitive processes underlying the behaviors they investigate. One study, for example, used fMRI to explore the neural underpinnings of individuals who were mulling over moral dilemmas (Greene, Sommerville, Nystrom, Darley, & Cohen, 2001). Brain areas with fMRI signal changes included regions that had been linked to "emotional" and "rational" cognitive processes in previous studies. Researchers thus concluded that these two types of processes are active, to different degrees, in different types of moral judgments. The rigor of such arguments, however, depends on the evidence that a focal brain area instigates a particular mental process. However, at least some of the emotional brain regions in the morality study have also been associated with memory and with language. It is curious that such caveats typically escape mention (Miller, 2008).

Using results from brain imaging as probabilistic markers of brain states may represent a viable approach, but we must scrutinize the probabilities. Testing these odds on real data revealed that while engagement of an individual region did provide some statistical information regarding the engagement of a mental process, the added information was relatively weak (Poldrack, 2006). Cognitive neuroscience may ultimately find ways to predict mental states using brain imaging data. Even then, rather than surfacing from localized activity in a focal brain region, such predictions will likely result from both subtle activation patterns and the coordinated activity across many brain regions.

Using specific reverse inferences (e.g., the association of fMRI signal change in the amygdala with anxiety) is a function of previous publications. The distribution of terms in the literature, however, is a function of past theories that have driven publications in particular directions, and which may hardly reflect current perspectives. For example, the scientific literature contains many more citations for "amygdala and anxiety" than for "amygdala and happiness." This difference, however, is a reflection of roughly thirty years of research investigating the association between anxiety and amygdala activity, whereas only recently have researchers begun to examine the role of the amygdala in positive emotional responses. Thus, to deduce that fMRI signal changes localized to the amygdala are a strong prognostic of negative emotion may be misleading.

Functional MRI has transformed neuroscience in fewer than two decades. Many studies, however, including some of those that garner the most attention in the popular and trade press, shed little light on the neural mechanisms of human cognition, affect, thought, and action. Researchers attempt to confront the limitations of fMRI by conducting experiments that match human fMRI data with analogous fMRI and electrophysiological recordings of neural activity in nonhuman primates. The general idea is to follow up on the human findings by identifying equivalent regions of the monkey brain using fMRI, and then recording the activity of individual neurons in those locations using microelectrodes. In some cases, single-neuron recordings in monkeys have confirmed fMRI findings in humans (Tsao, Freiwald, Tootell, & Livingstone, 2006). Whereas the parallel human–monkey approach represents an admirable albeit time-intensive paradigm, one of its main drawbacks is the difficulty in applying it to study many types of human cognition and social interaction, including psychoanalysis.

Comely fMRI-generated images may seduce the general public, but even neuroscientists seem to fall for them and overlook the limitations of neuroimaging. One constraint is the narrow sliver of the human experience that researchers can capture when a person has to keep still inside a scanner. Another limitation pertains to resolution: using fMRI to measure nuanced neural activity is akin to observing ocean currents to learn about the properties of water droplets. Functional MRI can only detect large-scale activities: generalizations to subtle local effects is speculative, and tenuous at best. In addition, with standard fMRI equipment, even the atomic volume-pixel unit of imaging (i.e., the voxel) typically comprises millions of neurons. Neurons can fire hundreds of impulses per second, however, and the fMRI signal—triggered by an increase in oxygenated blood—builds incrementally and peaks after several seconds, not instantaneously. Thus, fMRI is an indirect and crude tool for investigating how neuronal ensembles "compute" cognition and behavior. Functional MRI can be helpful in guiding where something is happening in the brain, but it is considerably more difficult to use this neuroimaging technique to elucidate mechanisms.

The promise of neuroimaging

A very different approach to overcoming some of fMRI's constraints comes from new analysis tools borrowed from machine-learning research. In a standard fMRI study, neuroscientists average together the fMRI activation from neighboring voxels. While averaging makes it easier to detect differences between experimental conditions, this technique follows the assumption that neurons from different voxels all behave the same way. This assumption, however, is extremely unlikely. Instead, it is possible to use statistical tools—multivariate pattern classifiers—to take a finer-grained look at brain activity and consider patterns of activation across many individual voxels without averaging. These methods shift the focus from trying to identify the specific brain regions activated during a particular task to trying to identify how the brain processes germane information.

An early demonstration of this statistical approach came from a neuroimaging study that presented participants with hundreds of images of faces, cats, houses, and scissors (Haxby et al., 2001). The investigators identified statistically distinct brain-activity patterns elicited by each type of object. Functional MRI activation in the primary visual cortex made it possible to determine the orientation of lines a participant was viewing, a feat previously thought impossible because neurons that share a preference for lines of a particular orientation pack into columns narrower than a voxel (Op de Beeck, Haushofer, & Kanwisher, 2008; Tong, 2003). A recent session in the Cognitive Neuroscience Society annual conference presented a variety of new findings illustrating how this new analysis of fMRI data can reveal information processing in the brain that would be overlooked by conventional analyses (Raizada, 2008). Hence, rather than looking at whether a specific brain region is active, researchers are beginning to focus on whether the activity pattern in many different voxels can predict what people are experiencing. In other words, instead of inferring that a spider induces anxiety, researchers could collect patterns of brain activity evoked by known anxiety inducers (e.g., images of snakes, accidents about to happen, and presurgical situations) and see whether the spider pattern forms a statistical match. Although it may well be that such classifiers will help rescue fMRI research from the logical perils of reverse inference, even with the promise of these new tools fMRI remains limited to revealing correlations between cognitive processes and activity in the brain.

Fuctional MRI may be most effective when people view it as one tool in a toolbox (i.e., by employing converging techniques and evidence). Increasingly, neuroscientists are using fMRI and related methods to investigate the connectivity between different brain regions involved in cognitive functions such as language and memory. One fMRI approach is to identify brain regions showing synchronized activity when subjects perform a given task. In some cases, researchers use diffusion tensor imaging (DTI) to further determine whether physical connections link those areas that fire together. A relatively new MRI method, DTI provides a way to visualize the axon tracts that connect regions. Some researchers are trying to establish causal links between brain and behavior. Having linked a brain region to a particular behavior using fMRI, for example, researchers are following up with TMS experiments. If the behavior then changes, the brain region probably plays a role in controlling it.

Psychoanalysis is about the patient–therapist interaction, and neuroimaging can offer a glimpse into the brain changes that occur during such interactions. Budding labors have taken a preliminary look at these scantily explored aspects. One line of such effort, for example, took the form of hyperscanning—a method by which multiple subjects, each in a separate MRI scanner, can interact with one another while their brains are simultaneously scanned (Babiloni et al., 2006, 2007a, 2007b; Montague et al., 2002). Psychoanalysis may benefit from careful assays that will marry the clinical interaction with the potential of concurrent brain imaging of both analyst and patient. Such findings will likely elucidate the neural correlates of the core exchange.

The science of suggestion provides a good way to bring together neuroscience and psychoanalysis. Specifically, neuroimaging findings fuel research interest in the use of hypnosis and suggestion to further examine our subjective experience and to glean insight into both healthy and pathological cognitive functioning (Oakley & Halligan, 2009). Studies drawing on brain imaging and suggestion offer new ways to understand psychoanalytic conundrums (Oakley, 1999; Tallabs, 2005). As a case in point, neuroimaging studies on hypnotic paralysis may elucidate lines of difference behind subjective and intentional mechanisms (Halligan, Athwal, Oakley, & Frackowiak, 2000; Ward, Oakley, Frackowiak, & Halligan, 2003), as such lines putatively exist (Cojan et al., 2009).

Conclusion

Charcot asserted that suggestion could affect somatic response through dynamic lesions. Freud further propelled the idea that "words" have the power to change brain function and influence physiology. Modern imaging tools carry dynamic lesions into a new "behavioral-lesion" model. While TMS demonstrates how bursts of high magnetic fields produce short-lived behavioral alterations, fMRI illuminates how words can influence minds. In concert with other experimental tools, these converging approaches elucidate how suggestion can unravel deeply ingrained processes (Raz, Lamar, et al., 2007). In this regard, current investigation into the neurological correlates of suggestion seems to hark back to century-old ideas (Cojan et al., 2009; Gauld, 1992; Harrington, 2008; Makari, 2008; McHugh, 2006; Oakley & Halligan, 2009). Ongoing psychodynamic notions regarding mind–brain interactions (e.g., Brenner) as well as recent experimental support for analytic concepts and theories (Bateman & Fonagy, 2008; Crits-Christoph et al., 2008; Milrod et al., 2007) have barely nibbled at fundamental questions. On the one hand, psychological constructs such as attention bridge the lacuna between brain and behavior and unite psychology with the brain sciences (Raz & Buhle, 2006). On the other hand, psychoanalytic concepts such as the ego continue to resist experimental probes. As a result, it is difficult—indeed, it may be conceptually impossible—to build tangible connections across the gap between the self and the ego. In developing psychoanalysis, Freud envisaged it as supporting a conduit to a nascent field of neuroscience. Ironically, now that neuroscience has matured, psychoanalysis has largely moved further away in its interests and perhaps even passed into another realm (Michels, 2008).

Cognitive neuroscience may implicitly have a place for the ego as a conceptual construct within its view of multiple "executive" functions. For Hartman (1956), as for Freud, the ego represents a number of executive-like functions, which seem to coincide with the current view of neural control networks. Unlike representations, such as the self, these higher brain functions appear to be psychological "transactions." Cognitive neuroscience, however, has bowdlerized the meaning of central psychoanalytic concepts, rendering certain issues orthogonal. For example, neuroscientists have taken the term conflict and transformed it from one of opposing forces, usually involving emotional or motivational undercurrents, into a mild inhibition of a prepotent response in the context of a cognitive paradigm (Egner, 2009). In the Stroop task, for example, little opposition exists in any strictly psychological sense. It may be difficult, therefore, to generalize from Stroop data to the true emotional conflict of potent opposing forces.

Many analysts have fallen into the trap of comparing modern brain imaging to nineteenth-century phrenology (Uttal, 2001). This is a detrimental position, however, because it demonstrates a profound misconception of neuroimaging technologies and estranges the few scientists who might best promote the credibility of psychoanalysis (Posner, 2003). In addition, the psychoanalytic community should heed at least two caveats. The first is that research on suggestion can only partially support Freudian notions lest it confound confirmatory investigation with exploratory ones (Raz, 2008b). The second caveat is that psychoanalysis is unlikely to be reducible to brain mechanisms. Hence, the marriage of psychoanalysis with neuroscience must rely on a judicious, rather than callow, grasp of the relative merits as well as shortcomings of brain imaging technology. While psychologists and psychiatrists have leveraged neuroscience to provide interesting popular concoctions (Brizendine, 2006; Peled, 2008; Westen, 2007), such general accounts are unlikely to explain the interface of psychodynamics and neuroscience. "The devil is in the details" and depending on the interpretation of the output from a multimillion-dollar brain scanner, the result may be objective and scientific, or of little more value than tea leaves in the bottom of a cup—ambiguous and susceptible to a large number of possible outcomes.

Karl Popper's "falsifiability criterion" posits that a theory is truly scientific if it retains the possibility of showing itself false. The history of science reveals, however, many theories that were initially unfalsifiable, which we can group into two separate types. Theories of the first type lacked falsifiability because they were insufficiently operationalized in terms of measurable variables (e.g., psychoanalysis), whereas theories of the second type were unfalsifiable because they were underdeveloped. The latter theories, nonetheless, served a valuable heuristic purpose in generating a large body of useful research from which new theories and empirical findings could evolve. Revisions to psychoanalytic theory will likely permit its transition from the first to the second type of theory—for example, studies led by Milrod in the United States (Milrod et al., 2007), by Fonagy and Target in the United Kingdom (Fonagy & Target, 2007), and by others in Germany and Sweden. This shift will afford more testable predictions as additional research increasingly draws on new methodologies, including behavioral lesions, TMS, and fMRI. As neuroimaging studies begin to elucidate the neural correlates of culture (Han & Northoff, 2008), the influence of suggestion and motivation on cognitive control is likely to pave the road to a more scientific understanding of psychosocial factors (Kouneiher, Charron, & Koechlin, 2009; Oakley & Halligan, 2009). Drawing on research domains including placebo effects, response expectancies, psychotherapy, and psychoanalysis unravels the neural underpinnings of suggestion and forges a potential model for research demonstrating the convergence between psychoanalytic theory and neuroscience investigations.

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Alloying the Gold of Psychoanalysis with the Copper of Suggestion: From Suggestion to Transference Interpretation

Commentary by Dianne Casoni & Louis Brunet (Montreal)

The first part of this commentary discusses from a psychoanalytic perspective some of the arguments presented in Raz & Wolfson's Target Article. The second part of the commentary focuses more precisely on a paradox associated with the notion of suggestion as used in the article when seen from a psychoanalytic perspective—in particular, in relation to the shift in Freud's interest from suggestion to unconscious psychic processes. Ideas for further research are then presented.

Keywords: Freud's *neurotica*; interpretation; psychic content; suggestion; transference; unconscious

Amir Raz and Joanna Wolfson's article is assuredly among the most balanced and well-informed target papers that *Neuropsychoanalysis* has published. Not only is the presentation of research findings explicit and clear, but the critical discussion of each result's relative scientific value, significance, and relevance to psychoanalytic conceptualizations respects the highest of standards. Furthermore, the authors' arguments constitute sure companions for all those who are not familiar with cognitive neuropsychological methods and research, or who are in need of sure guidelines to interpret its results. The first part of this commentary outlines and discusses a few of Raz & Wolfson' s arguments from a psychoanalytic perspective. The second part of the commentary focuses more precisely on the paradox associated with the very notion of suggestion in psychoanalysis, in an attempt to link it to Raz & Wolfson's work and, finally, to offer ideas for further research.

The authors operationalize suggestion as one central formulation that "carries a powerful psychological influence that affects individuals via their subjective beliefs and experiences." This definition, although complete, nonetheless falls short of the dynamic vision psychoanalysis holds of the unconscious mind. In that sense, when Raz & Wolfson refer to one of a long line of psychological studies of suggestion in the formation of personal beliefs (Bernstein, Laney, Morris, & Loftus, 2005)—research that has been paramount in understanding the role played by suggestibility in both children and adults in the witness stand (Ceci & Bruck, 1995; Ceci, Papierno, & Kulkofsky, 2007; London, Bruck, Ceci, & Shuman, 2005)—they point to a psychological definition of suggestion that, if taken within a psychoanalytic paradigm, would take on a different meaning. In that sense, although suggestibility can indeed be linked to unconscious processes, it would be premature, as the authors themselves argue, to attribute more psychoanalytic meaning to such studies that hint to, rather than support, a strong conceptual congruence with psychoanalytic thought.

Nonetheless, some cognitive neuropsychological research methods and results do hold promise. For instance, using functional magnetic resonance imaging (fMRI), Raz and colleagues conducted studies showing that specific suggestions correlate with focal brain changes. In one such series, they used posthypnotic suggestions with proficient readers who appeared to become incapable of reading or of processing word stimuli at the semantic, phonologic, or orthographic levels, as if they were psychoneurologically affected. For the authors, such neurological indices, although indirect, "form the backdrop for the resurfacing of Freud's 'mysterious leap from mind to body' [1919, p. 15]," lending partial support to psychoanalytic ideas. Although there is indeed a leap from mind to body that is evidenced in such research results, what is specific to psychoanalysis is not—notably, unconscious motivations or conflicts, as the authors noted. Neuroimagery cannot take a picture of the ego, Raz & Wolfson remind readers. Indeed, the ego is not a concrete entity, but a coherent set of conscious and unconscious processes that is not directly researchable with neuroscientific tools. In that sense, as Raz & Wolfson argue, psychoanalysis is not reducible to brain mechanisms—nor is the unconscious reducible to its processes, be they unconscious ones, might we add.

From a metapsychological standpoint, a certain paradox stands out in Raz & Wolfson's otherwise well-thought-through article. As the authors have outlined, although Freud, on the one hand, did not discover suggestion, it nonetheless constitutes an intrinsic part of the conceptual history of psychoanalysis; more precisely, suggestion played a fundamental role in the

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first psychoanalytic treatment of hysteria, the founding stone of psychoanalysis. On the other hand, a displacement, as it were, is observable in Freud's work wherein he slowly abandoned his interest in suggestion as a means of accessing psychic content in favor of an interest in psychic processes per se. It is in this vein that concepts such as the structural model of the mind, the second drive theory, repetition compulsion, the death instinct, and so forth became essential parts of metapsychology. Important personal insights led Freud to this movement away from suggestion as the sole means of recovering unconscious material, or, put differently, as the only way of accessing the mind. In a letter to Fleiss in September 1897, Freud—stating that he was in rather a good mood considering the realization he had come to—wrote that he no longer believed in his *neurotica.* He gave four main reasons explaining why he was abandoning this line of theorizing, stating notably that there are "no indications of reality in the unconscious, so that one cannot distinguish between truth and fiction that has been cathected with affect" (1897, p. 264).

Indeed, during the months preceding his letter to Fleiss, Freud discovered dimensions to the unconscious mind that he had not suspected before. Thereafter, suggestion, as a relevant conceptual tool, started to lose its place in favor of unconscious fantasy. Suggestion does not exist in a void, Freud discovered: fantasies or associations are bound to link themselves, unconsciously, to the content, affects, or emotional context of any suggestion. Furthermore, unconscious content acts as a psychological magnet to the very process of suggestibility, according to a more specifically psychoanalytic view of the phenomenon. Therein lies the paradox in Raz & Wolfson's attempt to link neuropsychological research results on suggestion to their psychoanalytic counterparts, in that they are forced to use a psychological definition of suggestion instead of a more psychoanalytically modern one, which would include unconscious motivations and processes in the study of suggestion and hence help explain corresponding research results.

From the beginning of the 1900s, psychoanalysis abandoned suggestion as a therapeutic tool because of the very nature of the unconscious and, as Freud discovered, the fact that in every treatment situation, transference phenomena occur that not only are a much more powerful source of change, but permit the closer monitoring of the inevitable, yet sometimes subtle, effects of suggestion. Transference analysis does indeed give access to an observational scene that is much more specific to the person concerned by its experience than the one offered by suggestion. The authors have suggested, to that effect, that the neuropsychological study of transference, through simultaneous hyperscanning technologies, is a project for the future. It could indeed constitute a means of distinguishing the effect of suggestion, as seen through a psychoanalytic perspective, from that of transference analysis. Are both processes similar, and/or, if they are dissimilar, what aspects make them so? Indeed, although Freud attempted to move away from suggestion in the course of the development of psychoanalysis, notably through the introduction of the analysis of the transference (and later through countertransference self-analysis), there remains a fundamental question that neuropsychological research can keep in mind for future research: does the brain register a suggestion in the same manner as it would a transference interpretation—is the latter but a sophisticated version of the former?

If current or future technological means permit the differentiation between linking and "delinking" processes in the brain, studies could be conducted on psychoanalytic hypotheses that postulate that transference interpretation should have a delinking effect (deconstructing unconscious links between representations), with the aim of helping the individual free him/herself from compulsively associating past experiences to new relationships. In spite of the authors' call for prudence in interpreting neuropsychological results, it is tempting to imagine the possibility not only of identifying mechanisms of suggestion in the mind, but of differentiating them from other psychic processes like learning or even processes of identification with the other. There could also be a way of researching a difficult psychoanalytic problem: when is a transference interpretation fruitful, and when does the patient simply understand and use interpretations as suggestions?

Finally, the authors propose an interesting discussion as they suggest two reasons explaining why theories like psychoanalysis may be deemed unfalsifiable. A third way of understanding the issue could follow from Edgar Morin's (2004) distinction between sciences that operate within a paradigm wherein they seek to identify specific variables and sciences that operate within a paradigm of hypercomplexity. Psychoanalysis, which falls into the latter category, does not seek prediction but, rather, strives to understand complexity. Raz & Wolfson have, for their part, succeeded in their Target Article in highlighting the margins of such complexity with a view to freeing emerging neuropsychological science from a simplified view of the brain. Such an endeavor is worthy of psychoanalytic communities, and of our support and commendation.

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Understanding the Evolution of Medical Traditions: Brain/Behavior Influences, Enculturation, and the Study of Sickness and Healing

Commentary by Horacio Fabrega Jr. (Pittsburgh, PA)

My commentary seeks to expand the relevance of the discussion that Raz & Wolfson so cogently articulate and formulate in two directions: (1) cross-cultural anthropology and (2) evolutionary anthropology and psychology. The "interface problem" is relevant to (1) understanding diverse systems of sickness and healing across human history and culture (i.e., from small-scale societies on through great civilizations of medicine including biomedicine) and (2) differences between mental experience and social behavior including sickness and healing across human biological evolution, a long stretch of time that involved the emergence of conceptual understanding of self, other, and situation and executive functions and working-memory capacity in members of genus *Homo* and eventually *Homo sapiens*.

Keywords: enculturation; ethnomedicine; evolution of mentalization; healers; healing; sickness

Amir Raz and Joanna Wolfson's Target Article discusses scientific and clinical efforts that seek to understand relations between mental phenomena and somatic and visceral-somatic muscular activity, change, and processes. The article affirms a philosophical truism. In a strict sense, dualism is problematic. On the one hand, dualism signals an ontological gap hard to cross, and, on the other, clinical and experimental efforts designed to grab hold of and measure phenomena that exemplify the gap—that mediate mind–body interactions—have to be content with at best latching on to small parts of it. Time-honored concepts and efforts of psychoanalysts and neuroscientists to successfully engage with the brain/behavior interface tend to simplify and gloss over the subtlety and reach of each other's vision as well as essential aspects that comprise the interface in the first place. Talk through words and terms may be shared and thus heard, but substantive semantic, epistemologically relevant informational exchange is not. The article supports and encourages scholars to continue to nibble at and not bowdlerize the interface problem but also advises and counsels that they may fail (and ultimately might be unable) to fully satisfy each other's expectations and needs. Essentially, the article appears to set up a new interface: mentalization disciplines compared to neuroscience.

The Target Article, which is a masterful analytic odyssey, provides a sobering analysis of the *strengths and limitations* of what cognitive neuroscience and mind-engaging therapies can offer clinical endeavors. Their Target Article articulates what I will formulate as a *culture-specific resource* (CSR) *of ideas and methods* for understanding and handling diverse and important medical conditions through psychological influences. I will not address the nuts and bolts of their hardware

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or practical consequences of using it prudently but, instead, venture freely into territories about which they urge restraint.

Raz & Wolfson's point of reference—the "distinct variation of French psychopathology" of the late nineteenth century and its modern spawns—represents but a culturally and historically specific formulation of a *preselected* class of health problems. Implicit in Raz & Wolfson's article is a way of comprehending phenomena of medical interest far removed from Western clinics and controlled laboratories and not limited to just classic "functional lesions." Furthermore, when properly contextualized, Raz & Wolfson's CSR enables appreciation of the biases and conceits of conventional understandings and approaches to health problems. What the mechanisms of their hardware epitomize gain significance if situated in a framework that comprehends evolutionary social sciences (i.e., behavioral ecology, evolutionary psychology, and dual-inheritance theory), and comparative medicine.

Questions begged and answers sought

Raz & Wolfson raise tantalizing questions and exhibit restraint in formulating answers to them. Here is a free-associated sample.

What are the basic features and effects of a hypnotic state per se, often termed "neutral hypnosis" and absorption? To what extent does the power of CSR (e.g., hypnosis/suggestion) derive from primary brain/behavior entrainment, which they seem to favor, compared to other theoretical approaches—for example, social learning and construction? Is the method of emplacement or are the consequences of entrained brain/behavior influences (adumbrated via CSR) more pronounced when conscious or nonconscious? In what shades and colors do brain/behavior influences as per CSR entrainment come in? What power do Raz & Wolfson ascribe to social/cultural suggestion compared to individual suggestion? Do they privilege the benefits of suggestion (i.e., enhancement of health) compared to its costs (e.g., *folie à deux*, ritual and charismatic indoctrination, harmful health consequences of seductive advertising)? How long does a posthypnotic suggestion last, and what are its determinants? To what extent can brain/behavior entrainment based on CSR principles be realized through nonlinguistic procedures—for example, via an inducer's gesture, demeanor, self-presentation, touching and comforting, and modes of parent–infant attachment? How effective are CSR procedures (attention/suggestion/mentalization) with individuals who are deaf, mute, or otherwise significantly compromised by neurocognitive developmental impairments?

The "attention is an organ system" view is grounded in the alerting, orienting, and executive problem-solving capacities of higher apes. At what point in biological evolution, involving emergence of language, cognition, and culture, as these are customarily construed, did a capacity for suggestion and hypnotic-like influences emerge? Alternatively: What do Raz & Wolfson believe are evolutionary foundations of brain/behavior programs that CSR principles can harness? Are concepts of absorption and automaticity, often construed as correlates and consequences of entrainment via suggestion–hypnosis–mentalization (and of "dissociation"), relevant to how higher apes behave in different circumstances?

Raz & Wolfson are not explicit enough about what they construe as salient features of psychoanalysis—for example, the old and the new of theory, method, process, and explanations about correlates and outcome. Implicitly, the references that they cite approvingly suggest openness to mentalization therapies and procedures (Fonagy & Target, 2007). Raz & Wolfson are not clear about what psychoanalysis can accomplish, and whether they see any useful correspondence between what psychoanalysis does compared to neuropsychological entrainment through hypnosis/suggestion. Can a protracted working alliance between analyst and analysand be likened to a training experience producing positive, consequential, and long-standing changes involving the neuropsychology of self-organization and control, changes not easily captured by simple attitudinal, pencil-and-paper queries about positive benefits of treatment?

Raz & Wolfson exclude concepts of dissociation, repression, and altered states of consciousness. Such concepts are used in studies involving medical anthropology and history of medicine and encompass phenomena that certainly map onto the terrain delineated by Raz & Wolfson. Unfortunately, leaving aside the influence of "secret observers" who report on pain experience during hypnotically induced analgesia experiments, dissociative phenomena ordinarily take place outside the experimental setting, are not always satisfactorily described by anthropologists, involve complex interactions of numerous variables, and their authenticity as examples of "dissociated states" is open to question. Do Raz & Wolfson eschew "dissociation" and its bedfellow, "repression"?

The authors' sober analyses suggest why such questions cannot now and may never be answered satisfactorily, but how they are important is taken up in later sections.

Reconfiguring CSR's compass as a guide for comparative medical research

Raz & Wolfson's article provides a conceptual and methodological framework of enormous scope and underscores biases and fallacies inherent in biomedical reductionism applied to health problems. Appreciation of this claim is facilitated through concepts italicized and briefly explained in what follows.

Culture encompasses experiential (e.g., ideas, concepts, beliefs, values, motivational and emotional proclivities) and procedural (e.g., social practices, methods, habits of behavior) material. A cultures preexist individuals, and they learn it. *Enculturation* is the process in terms of which an individual internalizes its culture. It involves shaping of culturally distinctive information-handling systems or brain networks and has wide-ranging medical implications (Fabrega, 1979, 1981; Han & Northoff, 2008; LeVine, 1990; LeVine & Norman, 2001). The cognitive-neuroscience and mentalization influences that CSR harnesses explain diverse elements of enculturation. Presuming identity-theory contradualism, one can define a person's or people's *cultural psychology* (e.g., their rendition of feelings, ideas, bodily perceptions) as behavioral dispositions and resulting actions that devolve from and are correlative with results of enculturation.

A subtext of Raz & Wolfson's article is *sickness and healing* writ large. Why and how CSR works, and what it does, represent human universals of medical significance. Across human history, disease has entered social and cultural spaces through the small portal of sickness: its physiological, psychological, and behavioral (i.e., biopsychosocial) manifestations are how individuals have seen and coped with "on the ground" occurrences of disease (Engel, 1960, 1977; Fabrega, 1974, 1975, 1976a, 1997). In "real life," enculturation and cultural psychology shape sickness and healing ensembles. *Ethnomedicine*—the cultural embedment of ideas and practices involving sickness and healing—is also a human universal. It is framed in terms of a society's prevailing conventions about meaning. Unique accomplishments of *Western* ethnomedicine (i.e., biomedicine) involve its objective, impersonal conception of mechanisms underlying disease/treatment and its big picture of the influence of genetics of populations on distribution and prevalence of disease (i.e., evolutionary medicine) (Nesse & Williams, 1994; Stearns & Koella, 2008; Trevathan, Smith, & McKenna, 2008).

What Raz & Wolfson concentrate on is culturally determined. A subset of health problems that were initially given a philosophical and theological reading during Western antiquity and the medieval era (e.g., involving melancholia, hysteria, phrenitis, sin) evolved into notions of madness, insanity, and mental illness during the early-modern and modern era and came to be formulated in terms of evolving ideas and practices of late-nineteenth-century Anglo-European protopsychiatrists (Fabrega, 1989). Such syndromes, of focal concern to Raz & Wolfson and CSR, can be termed *conditions of psychiatric interest* (CPI). CPI is also a human universal and, like sickness (with which it may or may not be equated in a group's ethnomedicine), is a holistic (biopsychosocial) thing. Besides their intrinsic general anthropological relevance, CPI have been apportioned to and then worked to disciplinary and political advantage by psychiatry (Scull, 1993).

The biases that Raz & Wolfson's CSR unmask are made clear when one appreciates that Western ethnomedicine (i.e., biomedicine) developments involving, for example, microbiology, physiology, and anatomy have created partitions and boundaries within and around sickness and CPI. Ideas about causality, locus of pathology, and pathophysiology of health problems have narrowed their ontology and epistemology. (This is perhaps more evident in North America compared to Anglo-Europe.) The result is that conventions resulting from the culture and history of Western ethnomedicine have exemplified biomedical reductionism: much of the "somatic" biology (i.e., general pathophysiology) of CPI has been marginalized (e.g., beyond the psyche) and its "psychosocial" correlates extolled.

CSR's formulation and evolutionary understanding of health problems

Raz & Wolfson's discussion of CSR is nothing if not quintessentially modern and practical. However, it adumbrates a way of formulating answers to Darwin's basic question of *origins*. It represents an indispensable tool for a *general anthropology* of behavior and experience—including, in particular, sickness, healing, and CPI (Fabrega, 2002a). For present purposes, general anthropology encompasses diverse behavioral sciences and principles of biological and cultural evolution, the study of behavior and adaptation of humans following the pongid–hominin split of six or so million years ago (Tooby & DeVore, 1987).

Genetic and epidemiological data about the high level and distribution of medical diseases and CPI and their susceptibility genes in contemporary human populations imply they were prominent in earlier populations as well (Keller & Miller, 2006; Stearns & Koella, 2008). Consequently, it is reasonable to formulate an answer to Darwin's question of origins, and pushing this lands one in the territory of Raz & Wolfson's CSR. What might have been the shape, form, and consequences of sickness including CPI in ancestral and ancient environments? We are obviously denied direct entry into social-behavior spaces and health-relevant circumstances of ancestral and ancient human populations. However, one can safely assume that health problems of such peoples reflected universal brain/behavior relationships and that these, inherent in Raz & Wolfson's CSR, influenced their *form, content, and* consequences (Fabrega, 2005, 2007).

The effects stemming from CSR principles do not just comprehend brain/behavior influences on form and content of sickness and CPI in small-scale societies. They encompass broader issues in such societies—for example, motivations underlying the organization and execution of social and subsistence activities, trance and altered states of consciousness, and forms of shamanistic healing. Furthermore, such principles also provide a pathway for understanding medically relevant phenomena of complex ancient civilizations. They include earlier "Great Traditions" of medicine, Central and Far Eastern forms of spirituality and religious healing, Near Eastern and Greek practices of religious communities, and the influence of rituals (essentially mental-tuning exercises conducted in highly emotionally arousing group circumstances)—all of which had profound health implications.

An important caveat can be deduced about the quintessential modernity of CSR. Studies in evolutionary medicine, evolutionary social sciences, cultural anthropology, and population genetics suggest commonalities (genetic signatures) of diseases and psychiatric disorders in ancestral human communities. Their susceptibility genes provide a record of their appearance and hazards in earlier times. It is tempting to extrapolate from this and see modern diseases and psychiatric disorders played out in similar attires in ancestral populations (i.e., as CPI and sickness). However, one needs to keep in mind that such environments did not produce modern cultural psychologies and social identities (nor perhaps, modern bodies, discussed later), the instrumentalities that have shaped our view of how medical phenomena are presented to us in behavior and experience.

There is much consensus in social sciences that modernity stamps self-identity and experience in distinctive ways, and Raz & Wolfson's CSR exemplifies this. The autonomous, independent, individualistic, private, and cognitively centered self (and concept of person) constitutes a special modern construction that differs from selves who situate in different ecological, cultural, and historical contexts. The cultural psychology of peoples not crushed by secular, dualist modernism exemplified world views that melded family, kin, and village community (e.g., social *contra* individualistic selves), understandings about spiritual beings and their influence in everyday life, connections between self/body/mind and workings of society, the cosmos, seasons, climate, and the like (Fabrega, 2000). A CSR-sensitive brain/behavior formulation of past medical phenomena should take fuller account of holistic, micro–macro relationships. It was in terms of prevailing cultural psychologies of past societies and peoples that sickness and CPI were fathomed. Selves and persons of long-gone other worlds owned brain/behavior programs designed by natural selection and hence were in some way universal (Brown, 1991; Tooby & Cosmides, 1992), but their effects as per sickness and CPI were subject to distinctive forms of enculturation, as indicated earlier.

Principles of Raz & Wolfson's CSR as a framework for ethnomedical studies

Raz & Wolfson's formulation of brain/behavior influences provides a window for the study of perennial problems in the general anthropology of systems of medicine. A vexing problem involves explanation of the therapeutic efficacy of ethnomedicines of small-scale societies and the longevity of scholarly, academic ("Great") ancient traditions of medicine (e.g., Indian, Chinese, Graeco/Roman, Mesopotamian), all of which have persisted and seemingly flourished as social institutions without biomedical insights (Fabrega, 1997, 2002b, 2009). Ancestral and ancient populations were obviously plagued by intractable conditions of disease, especially infectious ones, for which they lacked medical bullets. Nevertheless, Raz & Wolfson's discussion of attention, hypnosis/suggestion, mentalization, and placebos makes apparent the diverse influences that words, cultural expectations, psychological influences, and culturally orchestrated alterations of consciousness have on phenomena pertinent to sickness and healing.

However, a charismatic shaman's use of medical herbs and his command over brain/behavior influences (compatible with principles of CSR) are not the sole ways in which one can explain the longevity and flourishing of ancient medical theories and practices (Fabrega, 1976b, 2002b). Diverse and complex factors involving brain/behavior influences (i.e., enculturation) are responsible for shaping medically relevant ideas, beliefs, values, habit patterns, responses to sickness, and social practices affecting a population's health. They range from diet, food preferences, and the social biology and social physiology of behavior, on the one hand, to ideas about and patterns and influences involving cognitive/emotional control and self-organization, on the other. More to the point, enculturation writ large also shapes how sicknesses unfold, get insinuated into the body, are expressed and played out, and then are dealt with.

Since enculturation involves not just surface phenomena of sickness (e.g., beliefs) but its underlying pathophysiology, one can ask: To what extent are processes that lead to sickness in ancestral populations "culturally automated" in analogy to the way some behaviors are said to be automated (e.g., the Stroop effect)? To what extent are they responsive (e.g., moderated, neutralized, and/or rescinded) *only to culturally programmed* attentional processes that engage deliberative and social emotional brain circuits harnessed by religious spiritual engagement of healers and healing systems (i.e., prevailing nonmodern world views)? These are questions that clash with standard conventions for measurement of medical efficacy (i.e., placebo-controlled). This logic also implies that configurations of sickness and CPI that were prevalent in earlier periods of human history may no longer figure importantly in modern varieties of sickness and CPI.

Our data and assumptions about the natural history and prognosis of modern diseases and psychiatric disorders cannot unproblematically be transferred across historical and cultural spaces. What we record as "a" (singular) placebo or therapeutic effect in a controlled treatment trial is compelling for sure, but its cogency depends on the fact that it rests on profound pruning of cultural variation across subjects in diverse, holistic (i.e., biopsychosocial) aspects of self-identity and experience. Clearly, placebo trials have been successfully applied cross-culturally (although mainly across modern nationalities). Are conditions of placebo-controlled studies applicable across individuals not enculturated with modern assumptions of self, experience, and enculturated brain/behavior programs?

The relationship between hypnosis/suggestion and mentalization on the one hand, and aspects of cultural experience and healing on the other, are not elaborated in the Target Article. What form of brain/behavior tuning do Raz & Wolfson envision resulting from enculturation in a society heavily saturated with ideas about the omniscience, omnipotence, and omnipresence of diverse spiritual beings, gods, or godlings? The modern view of self and body and social reality is the product of centuries-long processes of *cultural and social indoctrination and suggestion*. Raz & Wolfson's CSR suggests it has affected all aspects of body and cognitive/motivational experience and responsiveness.

Does CSR advert to a unified perspective for medicine?

Innate conditioned response patterns and sensitivity to environmental stimuli are basic mechanisms that shaped the health biology of higher primates and early hominins. In *Homo sapiens,* natural selection in ancestral environments integrated such response patterns with changes in anatomy, physiology, metabolism, cognition, and brain/behavior programs in conformance to prevailing ecology (e.g., altitude, temperature, climate, food resources, microbial agents).

Genus *Homo* evolved a *capacity for enculturation* through patterns of mentalization and mind–body embedment exemplified in concepts articulated by Raz & Wolfson (as well as in personality and temperament proclivities). These were molded by distinctive modes of attachment to caretakers, emotional milieus in groups, and behavioral ecologic influences prevailing in ancestral environments. Features of environment insinuated in diverse biopsychosocial systems and functions of *Homo sapiens* influenced its health problems. Biopsychosocial pathologies (abnormal deviations) were determined by and yoked to ancestral environments (Fabrega, 1975). However, in changing the social and physical environment in which the individual lived (Tomasello, 1999), culture caused disequilibria among the systems of biopsychosocial functions, creating a different mix of health problems (i.e., the environmental mismatch principle).

Evolution of language, cognition, and culture as we understand them enabled an enhanced capacity for volition/conation based on acquired knowledge about the world and self, including theory of mind, metacognition, mental time travel, and autonoeisis (Tulving, 2005). Behavior came to be more self-consciously influential through expanded forms of deliberative thought and executive memory.

In *Homo sapiens*, volition-conation or self-guidance is determined by *neurocognitive and mentalization programs* that influence biopsychosocial response patterns in conformance to principles adumbrated by Raz & Wolfson. Evolved capacities for self-governance have gained privileged access to such programs at a potential cost: individual vulnerability to have behavior "captured" by psychologically resonant influences ("suggestions" from others, seductive foods, drugs of abuse), resulting in diverse health consequences.

Neurocognitive/mentalization programs answer to naturally selected evolutionary imperatives and are ultimate determinants of an individual's general medical well-being. A natural way of construing health status is in terms of functions and malfunctions encoded in those inherited programs. But this point of view is not regnant in the contemporary philosophical tradition of medicine.

Political economic transformations of modernity shaped ideas and brain/behavior dispositions involving the self, the anatomy and physiology of the body, and the spiritual and natural world. They splintered old categories and created new ones through which experience, behavior, and medical well-being were construed. The transformations gave birth to psychiatry and general medicine as separate disciplines. Modern psychiatry evolved and was nourished in a medical culture that excluded general medical conditions, and vice versa for general medicine. For psychiatry, this special bias facilitated and made "natural" a focus on the pathology of the psyche and its historiography (Berrios, 1996; Berrios & Porter, 1995) and, for general medicine, an emphasis on somatic pathology. The former feeds logically into a concern with emotion, motivation, cognition, and social behavior, whereas the latter minimizes aspects of psyche, opting instead for anatomy and physiology.

The scope of Raz & Wolfson's discussion and implications of their CSR underscore the fact that the territory of psychiatry compared to that of general medicine exemplifies a *cultural, conventional, and not natural demarcation of human health problems* (i.e., one that would conform to evolutionary biology). In the modern philosophy of medicine, general medical/psychiatric comorbidities are theoretically unproblematic: they are construed as inevitable consequences devolving from the presumed differences between the "natural" ontology and epistemology of diseases of a "mental" organ of experience and behavior compared to other "somatic" organs involved in general physiological and metabolic functions that traditionally are construed as peripheral to "psychiatric" concerns.

On the other hand, through the prism of evolutionary biology, which Raz & Wolfson's principles embody, the concept of general medical/psychiatric "comorbidities" can be viewed as theoretically suspect, for it reflects and conforms to cultural presuppositions about medicine that happen to have evolved in modern societies in response to distinctive sociological and political economic factors (as well as scientific ones, of course). In a biopsychosocial perspective that incorporates insights involving enculturation, cognitive neuroscience, and mentalization, all forms of comorbidities merely represent diverse facets of universal human vulnerabilities to sickness.

If the basic dictum of evolutionary social sciences gains sway—its emphasis on inclusive fitness maximization and on brain/behavior programs as common denominators of adaptation—then it is reasonable to reexamine conventional divisions and classifications within medicine, their different ways of parceling and explaining signs and symptoms of sickness and CPI, and the implications of all of this. The pivotal importance of brain/behavior relations in general health, not just as per functional psychiatric conditions but in a much more pervasive sense through its modulation of basic processes involving general metabolism, endocrinology, and physiology as well as habitual risk behaviors, raises the question of whether the basic dicta and methods summarized by Raz & Wolfson provide a basis for a unified language of medicine and approach to healing writ large.

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Suggestion as Clinical Tool—More Than Just a Suggestion?

Commentary by Ilan Goldberg (Tel-Aviv)

In their article entitled "From Dynamic Lesions to Brain Imaging of Behavioral Lesions: Alloying the Gold of Psychoanalysis with the Copper of Suggestion," Raz & Wolfson provide an updated review of the biological basis of suggestion, from a historical perspective of its origin to more modern conceptual aspects, as well as the most recent work elucidating neuronal correlates. In an attempt to reconcile psychology with cognition, a complete explanation in terms of brain mechanisms remains to be fully uncovered. Indeed, beyond the conceptual issue, how words may influence the brain via suggestion is debated. The challenge is to better characterize a unified picture of brain function and its intrinsic relation to the mind. The clinical aspects of suggestion including placebo effects are discussed in this commentary. A relatively large body of evidence investigating placebo effects and suggestion points to a robust and consistent effect in a large spectrum of brain disorders. Further study of suggestion as a potential tool in clinical practice is warranted.

Keywords: conversion; placebo effect; nonorganic symptom; suggestion

In a general neurological clinic, up to one-third of patients may be reported as having "nonorganic" symptoms. In other words, neurological deficits are observed without any evidence of structural lesions, and routine clinical workup fails to find any "physical" abnormality (Carson et al., 2003; Snijders, de Leeuw, Klumpers, Kappelle, & van Gijn, 2004). Many of these patients are given a diagnosis of conversion or dissociative syndrome. The development of the disciplines of neuroscience has, however, contributed to a shift in the biological understanding of functional symptoms. Indeed, disorders that were once considered "nonorganic" and the result of a disturbed mind may be more accurately explained in terms of a biological brain disorder as characterized by neurophysiological correlates. For example, development of the field of electroencephalography (EEG), which allowed identification of epileptiform activity, was of prime importance to support the concept and diagnosis of epilepsy as a biological disease.

In contrast to evidence-based medicine that has evolved according to the application of objective scientific criteria, it is still Freud's psychodynamic conceptualization that remains the central reference to account for dissociative disorder. According to the *DSM* (APA, 1994), conversion is clinically defined as a deficit of sensory or motor function that cannot be explained by a medical condition, and where psychological factors are judged to be associated with the deficit because symptoms are preceded by conflicts or other stressors. In the *ICD-10* (WHO, 1992), the dissociative disorders are classified using similar diagnostic criteria.

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For instance, how words exert their influence on neuronal processes is at best explained "metaphysically," by esoteric concepts. Metaphors like "behavioral lesions" or "dynamic lesions," as used by Amir Raz and Joanna Wolfson to account for conversion syndromes, may indeed provide interesting modern empirical findings that strengthen the psychoanalytic concepts. While encouraging attempts are made to shed light on mental mechanism, methodological issues discussed elsewhere, as well as inconsistencies in findings, hamper the development of a unified picture of higher brain functions implemented in these mental processes (Harvey, Stanton, & David, 2006). A real understanding of neural processes underpinning psychological mechanisms needed to bridge the gap between cognitive neuroscience and psychoanalytic theories is lacking.

In spite of increasing attention in recent years, placebo effects, though often robust, still remain difficult to characterize cognitively. Ethical, historical, social, and cultural factors—in conjunction with a bad reputation among the scientific population—are likely to be involved in maintaining this status (Oken, 2008).

The use of suggestion in seizure populations has been studied clinically. A nonepileptic seizure (NES, also called hysterical seizure) may clinically mimic an epileptic seizure except that no epileptiform abnormality can be found upon use of video–EEG monitoring, which is used to correlate clinical seizures with epileptic events in the brain and is considered the most reliable clinical method to diagnose seizures. Even if the prevalence of NES is low in the general population (4%), it is highly significant (up to 30%) in patients referred to epilepsy centers (Bodde et al., 2009). Video–EEG monitoring, albeit noninvasive, is an expensive and relatively inconvenient procedure. Also, both types of seizures potentially coexist in a given patient. Therefore, recognition and discrimination of NES is not always easy and represents a challenge in the management of seizure patients. Saline-provocation test during video–EEG monitoring has been investigated in patients with suspicion of NES (Bazil et al., 1994) as well as in patients with poorly controlled or atypical epilepsy (Cohen & Suter, 1982). The suggestion of seizure provocation associated with injection of saline induced NES in this patient population. Furthermore, the suggestion implemented by the antidote treatment (identical saline injection) terminated NES. These results show a high rate of positive provocation tests—typical seizure without epileptiform activity on EEG, as high as 37% in the general seizure population patients and 48% in a group of 57 patients with poorly controlled seizures. The authors conclude that suggestion is a useful and inexpensive tool in the diagnosis of NES. Interestingly, in a recent review of literature on placebo effects, Oken (2008) precludes the use of suggestion in the diagnosis of NES.

Despite the goals of evidence-based medicine, not all common practices are evidence-based. Plenty of examples exist in a wide spectrum of neurological and nonneurological conditions—from the use of anticholinesterase inhibitors in cognitively impaired patients to the efficacy of surgery in osteoarthritis. In the former, despite no evidence for efficacy from a randomized, placebo-controlled trial (recommendation level B), the clinical use of anticholinesterase agents in mild cognitive impairment patients is widespread (Doody et al., 2009). In the latter, the results of a well-designed study indicate that the placebo effect of performing knee arthroscopy for osteoarthritis accounted for the main therapeutic benefit observed at follow-up (Kirkley et al., 2008).

Though the use of suggestion in clinical routine revisits critical ethical issues, one cannot put aside the mandatory need for correct recognition of pathological processes in order to provide the best possible management at both individual and population levels, including adequate treatment and limitation of associated morbidity and cost. Therefore, neuroimaging and neurophysiological and neuropsychological studies bear a prominent role in the understanding of placebo effects. The growing empirical approach, which has to be further investigated, will allow, in return, a more rational and efficient clinical use of suggestion. Thus, in order to improve both diagnostic and therapeutic strategies, the role of suggestion has to be taken into consideration when dealing with patients in a clinical environment.

Given the lack of a detailed understanding of placebo effects in terms of brain mechanisms, suggestion remains to be further underpinned in order to release its potential as a clinical tool in the management of the vast spectrum of pathologies affecting the mind. Since the price of the gold of psychoanalysis may seem "expensive" to grasp clinically, one can imagine the copper of suggestion as a more "cost-effective" cognitive tool for use in a routine clinical setting.

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The Mind–Brain Barrier in 2010

Commentary by Robert Michels (New York)

The history of psychoanalysis includes recurring attempts to formulate the relationship between neuroscience and psychoanalysis. However, neither of these disciplines has been static. The result is an old question that is also a new question: How do we think of the neuroscience of 2010 in relation to the psychoanalysis of 2010? The question for neuropsychoanalysis is not whether the brain is involved in behavior but, rather, what significance, if any, does knowing more about that involvement have for psychoanalysis. In neuroscience it would probably be more valuable to explore simpler elements of memory, affect, cognition, etc. than to pursue such complex constructions as transference or therapeutic response. At the same time, psychoanalysis has been far more enriched by studies of developmental psychology, infancy and childhood, and family interactions, as well as by studies of language and communication, than by studies of neurosciences. Research at the interface of cognitive neuroscience and psychoanalysis requires decisions regarding resource allocation. These must consider not only the scientific opportunities but also the value of probable results to each of the related disciplines.

Keywords: cognitive neuroscience; neuroimaging; neuropsychoanalysis; psychoanalysis; psychoanalytic research; suggestion

The history of psychoanalysis includes recurring attempts to formulate the relationship between what neuroscience tells us about the brain and what psychoanalysis tells us about the mind, starting with Freud's "Project for a Scientific Psychology" (1950 [1895]) and continuing to the current enthusiasm for "neuropsychoanalysis." At first glance it seems clear that there should be some relationship—but not clear what it should be. Are these two distinct and untranslatable realms of discourse, or are they two perspectives on the same subject? Will we eventually be able to translate one to the other? Will we be able to explain—that is, reduce—mind phenomena to the language and concepts of the brain sciences?

Of course, neither of these disciplines has been static. The recent explosive growth of knowledge in neuroscience is well known: new developments in genetics, biochemistry, and cellular biology, together with new methods of brain imaging, have led to a neuroscience that Freud could not have imagined. Less widely recognized are the changes and developments in psychoanalysis—the emphasis on the therapeutic relationship, enactment, the transference–countertransference constellation, hermeneutic approaches, and two-person psychologies have led to a psychoanalysis that Freud could not have imagined. The result is an old question that is also a new question: how do we think of the neuroscience of 2010 in relation to the psychoanalysis of 2010?

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Raz & Wolfson assess the question from the perspectives of contemporary cognitive neuroscience and contemporary psychoanalysis. They invoke Charcot's concept of "dynamic lesions"—that is, "focal brain changes undetectable by anatomical scrutiny"—as a model for understanding the role of the brain in shaping behavior, and they review research on suggestion, one of the oldest concepts in psychoanalysis. It has long been known that suggestion influences behavior; they add intriguing examples of the scope and power of that influence. They go on to review the evidence of specific known brain areas that mediate the effects of suggestion, demonstrating how new methods of imaging allow us to detect brain changes that were previously undetectable.

Of course, our a-priori assumption that any mental or behavioral phenomenon must be correlated with a brain phenomenon has been so powerful that, in a sense, their demonstration is unnecessary. Before the work they describe, we might not have known exactly which brain area was involved, but it is hard to imagine a twenty-first-century scholar—neuroscientific or psychoanalytic—who did not assume that some area was involved.

The question, then, for neuropsychoanalysis is not whether the brain is involved in behavior but, rather, what significance, if any, does knowing more about that involvement have for psychoanalysis. Here the problem becomes more difficult.

Raz & Wolfson warn us of the difficulties in analyzing and interpreting neuroimaging data, and the errors that occur when naïve enthusiasts attempt to do so. However, what if we assume that the studies are done carefully and interpreted cautiously? Presume, for example, that one of the studies that they describe which is now under way is able to demonstrate that resistant transferences are associated with a specific pattern of neural activity, and that the interpretation and resolution of the resistance leads to a change in the neural pattern. Certainly this would be exciting, but would it make any difference to psychoanalysis? I would argue that we already assume that resistant transference is correlated with some brain state, and that its resolution must mean that that state has changed. We simply don't know what that state is. Discovering it would be an important step in neuroscience, but we would have to discover new psychoanalytic interpretative strategies that affect those transferences in order to have exciting progress in psychoanalysis. Certainly systematic research might lead to such a discovery, but it would be research in the clinical psychoanalytic process, not research in neuroscience.

I would argue that the critical question today is not whether such a pessimistic view is inevitable but, rather, in view of where the fields are at present, and what is possible in the near future, how should our resources and efforts be directed, and what are the risks to be avoided?

Probably the most important risk is that in the attempt to build bridges between psychoanalysis and neuroscience, the intellectual and research agendas of both disciplines will be distracted from more fruitful and generative strategies.

In neuroscience, the cost of specific projects can be very high, and at this point in the development of our knowledge of neurocognition, it would probably be more valuable to explore simpler elements of memory, affect, cognition, etc. than to pursue such complex constructions as transference or therapeutic response.

At the same time, psychoanalysis has been far more enriched by studies of developmental psychology, infancy and childhood, and family interactions, as well as by studies of language and communication, than by studies of neurosciences. A shift of emphasis from the former to the latter could lead to loss of opportunity.

The question remains of how we might explain the immense popularity of neuropsychoanalysis within the psychoanalytic community. I believe that the answer has more to do with the sociology of the profession than with the expanding knowledge base of the field. Psychoanalysis has been battered by often justifiable attacks on its lack of scientific status, its weak basis in empirical research, and the lack of interest or even disdain of its practitioners in remedying these deficits. The research that would be required to address these challenges is research on treatment, process, and outcome and clinical trials—not neuropsychoanalysis.

However, this kind of research is boring to most psychoanalysts, employing rating scales and statistical methodology to test and validate hypotheses that most clinical analysts believe need no testing. Analysts support the work because of societal pressure, particularly economic pressure such as the threat to insurance reimbursement. In contrast, neuropsychoanalysis is intellectually engaging, much more interesting, and links psychoanalysis to one of the most exciting areas of modern science. Its popularity is understandable, but, as of now, unfortunately, it has little to do with the practice of psychoanalysis.

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A Few Suggestions about Suggestion, Psychoanalysis, and Neuroscience

Commentary by Lionel Naccache (Paris)

Suggestion is a crucial phenomenon to test the relevance of cross-studies between psychoanalysis and cognitive neuroscience. In their target paper, Raz & Wolfson present substantive arguments to defend the idea that functional brain imaging and contemporary electrophysiological tools can shed new light on the mechanisms at work in suggestion. However, they also emphasize the existence of serious potential pitfalls and of both methodological and theoretical limitations in this project of convergence. I develop here some reasons to be optimistic on the fecundity of this project, and, more largely, I advocate a hetero-phenomenology approach to elaborate a neuroscientific theory of subjectivity. Within this project, psychoanalysis is not considered as a science but as a first-person psychology endowed with a potentially rich source of knowledge. However, this richness is not to be found in the theoretical claims of psychoanalysis—which are open to the same errors as other purely introspective thoughts—but, rather, in the substantial empirical evidence experienced by the patients and by the therapists during the cure.

Keywords: hypnosis; suggestion; functional neuroimaging; heterophenomenology; psychoanalysis; fictionalization

In an open-minded and exploratory article, Amir Raz and Joanna Wolfson draw information from various sources of psychoanalysis and cognitive neuroscience from the perspective of revisiting such historical concepts as "conflict," "attention," "suggestion," and "ego" in the light of recent empirical findings. Using a pragmatic approach close to the context of experimentation, they propose several promising tracks to readdress these concepts. In particular, they suggest interesting links between the transient and reproducible modifications of brain function induced by transcranial magnetic stimulation (TMS) and the concept of "dynamic lesions" (Charcot) central to the fields of hypnosis and early psychoanalysis. In the same vein, Raz & Wolfson also use the functional magnetic resonance imaging (fMRI) literature to emphasize—and to communicate to a nonexpert audience—the existence of a subtle brain anatomophysiology taking into account the dynamics of multiple functional large-scale brain networks, miles away from a caricatured phrenology. It is, of course, very ironic that since Mesmer, magnetism is never far away when mind issues are under scrutiny!

In their effort to compare psychoanalytic concepts with recent neuroscience results, the authors are conscious of some of the various pitfalls that are threatening their approach, such as the potentially massive errors secondary to the blindness to polysemy: when the very same words point to different concepts or phenomena (see their comments on "conflict," for instance, or the common confusion between conscious and unconscious repressions). Raz & Wolfson also mention some of the current limitations of fMRI tools that one should keep in mind when trying to "bridge the gap"—the contemporary neuroscientific bridge might not be fully achieved yet! In my opinion, this last point is a fundamental issue that goes far beyond the neuroscientific elucidation of suggestion or psychoanalysis. Indeed, the construction of a neuroscientific theory of subjectivity is dependent on the solutions we will be able (or not) to imagine that go beyond these limitations. I would therefore reinforce this idea by adding the following six major methodological points:

1. There is a need to explore mind–brain relations outside the "stimulus–response" (SR) paradigm that has dominated our scientific activity for decades. One can easily understand the rationale of this domination, since the use of the SR paradigm offers a unique situation where *input* and *output* subjective states (e.g., perception) are rather well controlled. However, many (most?) aspects of the dynamics of our psyche are not well captured by this rigid experimental paradigm.
2. The possibility exists to explore spontaneously an ongoing stream of consciousness, with recent encouraging results from the "resting-state" patterns of brain activity related to introspection and mind-wandering (Christoff, Gordon, Smallwood, Smith, & Schooler, 2009; Goldberg, Harel, & Malach, 2006; Raichle et al., 2001).
3. There is a necessity to develop alternative methods of analyzing fMRI data, enabling us to escape from signal-averaging across successive trials. Indeed, since "repetition" does not exist in terms of subjective experience, one should be aware that averaging suppresses not only "noise" in the data, but

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also some correlates participating in the subjective uniqueness of each conscious state. In fact, core mental architecture of consciousness can probably be captured by averaging-based methods (e.g., empirical and theoretical results on conscious access, in the light of the global workspace model of consciousness: Dehaene & Naccache, 2001), but just as Raz & Wolfson wrote, in another context, "the devil is in the details," so is the singularity of each conscious experience. Note that current pioneering works with single-trial methods (Delorme & Makeig, 2004; Esposito, Mulert, & Goebel, 2009) allow us to carry some optimism.

4. Raz & Wolfson focused on fMRI, with its ~1-s time-resolution, but given the ~100-ms timescale of chaining of discrete conscious contents within the "stream of consciousness," one should note that some of most promising results in that field have originated (and probably will originate) from electrophysiological tools (e.g., EEG, MEG, intracranial recordings).
5. Raz & Wolfson also emphasized that phenomena relevant to psychoanalysis—such as suggestion—are systematically intersubjective. Therefore, we need to be able to record patterns of brain activity within a real or simulated intersubjective relation. For instance, in a very elegant subjective simulation of social exclusion in fMRI, Eisenberg, Lieberman, and Williams (2003) revealed the activation of the pain matrix in this form of social pain. Such work paves the way for the emergence of a rich experimental literature on the neuroscience of intersubjectivity in real or simulated contexts.
6. Finally, the rise of reverse neuroimaging might offer us a unique way to infer part of the current mental content of an individual. At present, we interfere massively with the spontaneous stream of conscious thoughts of an individual when probing his/her current conscious content: we ask the subject for an explicit behavioural report (e.g., verbal act). The recent developments of various machine-learning classifiers of fMRI data suggest that we can go way beyond this "interference principle" or "observer paradox," by accessing part of the current conscious content of an individual on the sole basis of signal-processing methods (Mitchell et al., 2008; Pereira, Mitchell, & Botvinick, 2009). Reverse neuroimaging methods and neural-activity decoding tools may thus offer us the unique opportunity to observe the stream of consciousness of a subject from a third-person perspective.

With this methodological agenda in mind, I think that we may be more optimistic than Raz & Wolfson about what they call the "perils of neuroimaging." Functional brain imaging is a very fast-growing field, and one should not look at its current limitations as definitive. If this calendar meets with our expectations, within a few years we may reasonably hope for a better understanding of how suggestion works. Suggestion is indeed at the crossroads of the processes at work in intersubjective cognition and, therefore, also in psychoanalysis: theory of mind, empathy, impact of beliefs, remote executive control on other minds, conscious influences on nonconscious processes (Naccache, 2008).

In their article, Raz & Wolfson wisely consider that recent results of cognitive neuroscience only "partially support Freudian notions." I would go a step beyond and defend the notion that psychoanalysis is not comparable to neuroscience in terms of scientificity: psychoanalysis is not a science, but a unique source of knowledge on how intersubjective conscious fictionalization processes work, and on how this mode of relation can convey a therapeutic value in some psychological disorders. In a recent essay, I promoted this view through a comparison of the Freudian theoretical model of the unconscious with current neuroscientific knowledge about unconscious cognitive processes (Naccache, 2006). The denouement of my enquiry was rather skeptical about the scientific validity of core concepts of Freudian theory(ies) such as active unconscious repression and a lifetime of unconscious representations. However, in the same way, I recognized in the Freudian posture the gist of a first-person psychology—a psychology taking into account very seriously the subjective representations and beliefs of an individual, irrespective of the actual veracity of these. Psychoanalysis begins with subjective interpretation, not with the objective description of a phenomenon (e.g., dream interpretation). In a word, Freud might not be the discoverer of the concept of unconscious, which was formulated before him (see, for instance, Ellenberger, 1970), but he was an insightful explorer of the nature of our consciousness: a world full of fictions and beliefs, endowed with a form of mental causality. In this view, contradictions between Freudian theories and contemporary neuroscience should not be considered as too problematic. The richness of psychoanalysis might not to be found in its theoretical proposals but, rather, in the importance of the conscious phenomena at work in the cure. Psychoanalytic theoretical claims are themselves conscious fictions, because psychoanalysis is a first-person perspective. This can be illustrated in particular by the various contradictory psychoanalytic schools and theories. If correct, this approach would allow us to elaborate a theoretical construct free of a-priori psychoanalytic concepts, but rich in the substantial empirical evidence experienced by the therapists and by the patients on the couch.

The project of heterophenomenology (Dennett, 1992), which I am advocating here, aims at building a genuine "neuroscience-fiction"—that is to say, a neuroscience taking conscious fictions very seriously but without crediting these subjective constructs with what they are certainly not: necessarily valid scientific theories or intuitions. Several other fields of experimental psychology have demonstrated that in many cases our own introspection of how our mind works can be misleading (Naccache & Dehaene, 2008). Beyond these introspective errors, the process of introspection is in itself a great subject of knowledge and science: how do we interpret our mental life and credit these interpretations with belief? Even if the interpretations and beliefs elaborated by the subject during conscious fictionalization are incorrect, they capture our neuroscientific interest. This also applies to psychoanalysis conceived as a specific form of conscious fictionalization.

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The Bicameral Brain and the Conflicted Mind and Their Relation to Suggestion and Hypnosis

Commentary by Theodore Shapiro (New York)

Three issues suggested by Raz & Wolfson are illuminated and discussed: (1) Differences between cognitive and dynamic unconscious and their relationship to the history of Freud's original neurological frame of reference; his abandonment of the "Project" as a basis for clinical work and the later post-Freudian attempts to link conflict-based ego psychology to the conflict-free sphere of thought. (2). Discreteness of levels of inquiry from the vantage of instrumentation and technique, as well as the need to keep separate the language of the varied domains that are studied. (3) Suggestion, transference, and dialogue are intertwined, but the historic sequences of their emergence are important to understand Freud's need to disavow suggestion. The independence of free association from suggestion and the significance of repression in Freud's theory would be untenable if all he was doing was infusing the clinical field with preformed suggestions. The new method demanded discovery of what had been repressed for the demonstration Freud intended.

Keywords: levels; compromise; conflict; bicameral brain; structural theory; Stroop effect

Before I address Amir Raz and Joanna Wolfson's thoughtful and also cautionary contribution to our integrative literature, I must explain that I was a coauthor with Raz of an initial programmatic paper, "Hypnosis and Neuroscience" (Raz & Shapiro, 2002), concerning the trajectory we set to study their interplay, as well as of papers cited in their article concerning hypnosis and the Stroop effect (Raz, Shapiro, Fan, & Posner, 2002; Raz et al., 2003). I also am a practicing psychoanalyst and an academic investigator interested in the impact of empirical findings on psychoanalytic thought and theory. My work with Raz has significantly affected my psychoanalytic thinking.

Two examples from prior publications will amplify my point of view. A line from my Plenary address to the American Psychoanalytic Association states that our work together on hypnosis convinced me that verbal interventions and interpretations penetrate top-down to the substrate and are processed in the brain as well as the mind (Shapiro, 2004). In an earlier critique of a paper by Solms (1996) concerning a somewhat reckless model of brain localization, I suggested that the construct, ego, is a hypothetical segment of mind. As such, it is unlikely to be located in an anatomically discrete region (Shapiro, 1996). Raz and Wolfson also take this up in their article by properly indicating that the idea of executive functions as used in the literature of cognitive neuroscience could be considered to be rough ego equivalents. However, data from brain imaging studies indicate that the many ego functions of psychoanalysis are more likely to be represented anatomically at many sites. Moreover, their connectivity is uncertain. As stated in many prior discussions between psychoanalysts and representatives of border disciplines, the neuroscientist's cognitive unconscious is not the dynamic unconscious of the psychoanalyst.

Even earlier in my career, writing about the biological bases of latency (Shapiro & Perry, 1976), I cautioned against seeking strict isomorphism between mind and brain, and also that the data from the various domains of inquiry should not contradict the data from any other area of inquiry. Humans can be studied at many levels—behaviorally, cognitively, neurologically, biochemically, and genetically—but the data are registered at each level by different scientists in different languages and codes. When discrepant data emerge between levels, reconciliation should be fostered by further work. The linguist in me reaches to reiterate that just as the ego is not congruent with executive functions, drives are not instincts, etc. Moreover, cross-talk between levels of inquiry is possible only by continuous retranslation. Raz & Wolfson's careful discussion of the technical aspects of interpreting (f)MRI output in achieving a voxel, temporal resolution, and probe salience should sober any former devotee of simple mind–body reductionism.

I now turn directly to the Raz & Wolfson article. They ask whether psychoanalytic understanding is enhanced by recent findings of neuroscience concerning hypnotic suggestion. The issues of hypnosis and symptom formation are closely linked to Charcot's conundrum concerning dynamic lesions. I interpret Charcot's query as an extension of the nineteenth-century scientist's quest for new explanations for the "mysterious leap from mind to body" when no anatomical lesion could be found. I rephrase the issues in the following manner: Do extra-clinical investigations

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of mind and brain enrich our psychoanalytic practice and/or our theories? When rephrased this way, we find an extensive literature on this matter that precedes the Raz & Wolfson article (see summary arguments in Shapiro & Emde, 1995).

I initially focus my discussion on the pivotal idea of conflict in psychoanalysis as seen by Freud and now studied by neuroscientists. I then try to extend the discussion of Freud's apparently desperate disavowal of hypnotic suggestion and suggestion itself within psychoanalysis.

First, Freud's models, including the topographic and structural theories, were both bipartite in order to accommodate the role of repression as a mediating mechanism between levels of awareness (each model also included a derived third agency, *Pcs.—preconscious*—and *superego,* not relevant to this discussion). Although Freud abandoned his first attempt to construct a neuronally based psychology (the "Project": 1950 [1895]), he could not escape his neurological training, which crept into his model of psychology and ideas of mental organization. Furthermore, he explicitly paid tribute to the influence on his work of the English neurologist, Hughlings Jackson, who described a bicameral brain to account for release phenomena viewed as the loss of inhibition and deficient modulation in the behavior of stroke victims because of cortical lesions. It is not a stretch of the imagination to see the parallel to Freud's two layers of mind in both early and later psychoanalytic theories. Just as release phenomena were viewed as a lesion in behavioral constraint or a breakthrough of impulse, a breakthrough of id derivatives to consciousness was permitted by deficient or compromised egos. Thus, willy nilly, Freud built a neurologically based model of mind while disavowing that neural and psychological levels were necessarily interrelated as a congruent conceptual unity.

Dynamic lesions, or functional lesions described by Charcot, were in the air across the biological sciences because physicians of the late nineteenth century did not yet have the technology to view the anatomical correlates during life that they had so recently discovered by autopsy. Medicines' guiding spirits, Rudolf Virchow and Karl Rokitansky, had put their stamp on late-nineteenth-century medicine by demonstrating postmortem anatomical concomitants to clinical deficits, but they had no explanation for more transient or intermittent states. Thus, there were functional states from unknown chemical lesions, etc., that were not available to clinical observation or palpation or derived from body fluids. Too many illnesses did not have clear anatomical correlates.

In discussing the possibility of "dynamic lesions," I am reminded of the description of a dialogue between "phthisiologists" in Thomas Mann's *Magic Mountain* regarding what they heard on auscultation and percussion in contrast to what they saw on the newly discovered X-rays. This to me is a commentary on how new technology may be initially suspect when it enters medicine. On the other hand, psychoanalysts of our time may also be too zealous in welcoming new data from neuroscience as a panacea and "explain all" for their problems without fully grasping the artifactual possibilities of these new methods (Shapiro, 2004).

Raz & Wolfson agree that cognitive conflicts are different from dynamic conflicts. They also indicate that the preemptive intrusive power of reading that interferes with the Stroop task instructions for color naming is based on cognitive prepotency. It is not a motivational intrusion on a process because of a sensed danger and need for defense, as in dynamic conflict. Nonetheless, the fact that we wiped out the reaction-time delay by hypnotic suggestion does tempt me to look further for some functional parallelism in the cingulate activation as a site for compromise formation in psychoanalytic conflict too. Raz & Wolfson are cautious in suggesting this, but it may be that in the future we will be able to devise a cognitive task or probe that could tell us more about which brain mechanisms are used to ward off psychologically dangerous mental intrusions—that is, id derivatives.

Currently there are several variations of the Stroop test that include emotionally charged experimental paradigms, but they are not yet specific enough to allow us to designate the functional anatomy of depth psychological constructs such as repression and defense. In addition, an evolutionary perspective might prompt suspicion that single structures like the cingulate may subsume related functions like cognitive conflict and psychoanalytic compromise. An example suggesting this surmise has been demonstrated in the multiple functions of what was once called the fusiform face area. Humans are expert in face recognition, but they also attain discriminant expertise in other domains, such as car connoisseurship. During such judgments about autos, the same fusiform gyrus is activated, demonstrating its modular role in expertise of many kinds.

Such findings in neuroscience remind me of the initial excitement about the prospects of the new technology and the promise of expanded applications to answer more complex questions that touch on linkages between emotion and cognition. My first responses to (f)MRI images of word localization in the brain led me to expect that future investigations might include more spicey and affect-charged words to determine if they would have similar BOLD (blood oxygen-level dependent) effects and locales. Emotionally salient words can lose their sting by repetition and suffer extinction. Such phenomena based on laboratory interventions allow Raz & Wolfson to alert us to Posner's work (e.g., Posner & Rothbart, 2007) on attention as a possible new direction for studying the interplay of psychoanalytic theory and neuroscience. If such new directions could be operationalized, new routes to understanding psychoanalytic propositions could be developed. However, my reading indicates that each advance in technology may have a limit to its application and yield, and new avenues must be sequentially introduced. The idea of using attentional disposition as a new probe seems sound insofar as Freud's early attempts to understand free association forced him to consider the role of attention in his work. He analogized the fleeting centrality of what arises consecutively in consciousness as a demonstration of the vast repository of latent unconscious thoughts that are with us only fleetingly.

At this juncture, I return to the proposal that data collected at each level of inquiry are not automatically translatable to other levels: genetic codes have their own coherence. The next level of protein synthesis is similarly organized by its enzymes and base peptides, and the next level of physiological signal and neuronal transmission cannot be easily translated to thought except in the most generic sense. The BOLD effect, too, is merely a measure of blood oxygenation and is roughly related to the neuronal activation registered in voxels that have been boosted above a threshold in a region of interest in the brain to create a color-coded image of difference from the less activated surround. All of these are a far cry from the specifics of thought. Nonetheless, experiments of the sort discussed in this essay indicate that we may have been breeching some of the barriers between and among the levels in our search for the brain correlates of Charcot's dynamic lesion. Freud's later repression of an unconscious fantasy and compromise formation also were believed to be the immediate causes of hysterical symptoms that had no anatomically clear correlates.

The levels argument can be further illuminated by returning to the two experiments in which I participated. In the research designed to induce diuresis in hydropenic subjects by hypnosis (Hulet, Shapiro, Schwarcz, & Smith, 1963), as well as in the hypnotic suggestion to do away with the Stroop interference effect, each research team had to decide which *hypnotic suggestion* to use. What *verbal entity* received by the mind, carrying what meaning, would be specific enough to elicit the response that was posited in our hypothesis that the body could be affected by the mind (top-down processes)? If it were just a matter of getting done what we said on our authority as physicians or professionals alone, "Abracadabra" might have worked.[1]

If we could "talk directly to the physiology" we might have shouted orders at the hypothalamus to delay the antidiuretic hormone flow or to the renal tubule to not reabsorb the serum. In the case of the Stroop, why didn't we talk directly to the cingulate to suspend action? These all seem ridiculous, because they represent talking across levels. The effect, then, must be elicited within the psychological cognitive system by verbal linguistic shapes that have illocutionary force before it takes hold in the substrate. So we suggested "drinking lots of water" in one experiment and "inability to read" as the most psychologically salient linguistic forms, for the design of each experiment. Each addresses the level of mind via the agency of words and meanings—our dependent variables were physiologic measures of change in urine flow (Hulet et al., 1963) and delay in response and changes in activation of the cingulate (Raz, Fan, & Posner, 2005).

The next area touched on by Raz & Wolfson that piques my interest as a clinician concerns Freud's high anxiety regarding suggestion and his insistence that his interpretations were more than directions to a hypnotic subject. He hoped his patients would not lose their symptoms because of the authority or magical words of the hypnotist, but because the interpretations unearthed and described in words a hidden unconscious fantasy that was sequestered in the mind as a foreign body and effected behavior in the form of hysterical symptoms via compromise formation. He also was trying to move away from suggestion because it would place in jeopardy the independence and creativity of his model of hysteria. How would he be distinguished from Charcot, Bernheim, and Janet? More importantly, when he came to understand the powerful effect of positive transference on the receptiveness, or even gullability, of his patients, he further had to shun suggestion because the autonomy of his theory of cure depended on making the unconscious conscious and eliciting an abreaction.

In our modern psychoanalytic arena, Freud's wish to strip suggestion from interpretation is again problematic. Some theorists believe that all that goes on in an analysis (or for that matter in conversation) is *sugges-*

[1] I learned from Amir Raz that abracadabra is likely a bastardized Aramaic phrase, "It will be as I say!"

tion and that all the positive effects of treatment come from the *relationship* and what has come to be known as the "coconstruction" of new narratives. In this model the analyst and patient are writing the therapeutic "script for cure" together. The functional dynamic lesions might as well be disintegrated by hypnosis.

Both the discovery of the power of positive transference and the suspicion of suggestion can be explored linguistically. Indeed, the idea of speech acts, or pragmatics, includes not only the semantic message but the illocutionary force or social intent. As such, all communications include an aim to influence: a message to be transferred from mind to mind—every communication uses rhetorical strategies as well as semantic reference. Some philosophers even suggest that every utterance is an interpretation (some turn out to be more conventional, others less). If hypnosis increases receptivity, Freud found that familiarity, trust, and positive transference did so as well. Most modern therapeutic hypnotists depend on some authority and dispense with the "magic" of the trance.

In summary, Raz and I, in our programmatic paper on hypnosis and cognitive neuroscience (Raz & Shapiro, 2002), asked whether modern techniques in each area can illuminate the operative aspects of the other. Raz & Wolfson now ask whether neuroscience may soon be able to illuminate the workings of psychoanalysis. I believe it will, but never as a naïve reduction or translation of one level to another. Psychophysical parallelism seems to be a better bet. An action potential or ERP (event-relation potential) blip may correspond to a thought, but studying it cannot alone tell us the nature of the thought or its meaning.

A small proviso in my argument has been raised by recent work in which, within narrow domains of choice, scientists could predict what the cognitive probe or sound bite was by reading the activation patterns alone (Haxby et al., 2001). Raz and other critics note that this new development in "reverse inference" has limited application. New hope for this approach, which includes the enhanced visualization of tract connectivity and network activation by DTI (diffusion tensor imaging), are in the air already. In a new summary article in *Science*, a staff writer, Greg Miller (2008), further describes the promise and pitfalls of such reverse inferences.

All these new developments point to an ever-increasing technological promise. I believe with Raz & Wolfson in the future possibilities of science, but we must read their article carefully because it describes the dangers of too easy a translation and poor understanding of the significance of the artifacts created by instrumentation. At the same time, I do not yield easily to the claim that psychoanalysis is a hermeneutic enterprise. They have given up on further illumination of our work by extra-clinical means. I concur with Raz & Wolfson that studies of the process of treatment, outcome, and new theoretical constructs can possibly enrich our clinical theory. Also studying what we do and say in a laboratory context by employing whatever amazing new instruments are on the horizon may shed better light on what happens in the consulting room.

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The Predictive Power of a Comprehensive Psychoanalytic Theory

Commentary by Howard Shevrin (Ann Arbor, MI)

The growth of neuroscience from its beginnings as an organically based psychiatry to its current ascendance as the main avenue for understanding the link between behavior and the brain is well presented by Raz and Wolfson. Their analysis of potential relationships between psychoanalysis and neuroscience is limited by only taking into consideration the clinical application of psychoanalysis and ignoring psychoanalysis as a comprehensive theory of the mind.

Keywords: ego psychology; free associations; fMRI; hypnosis; neuroscience; psychoanalytic theory; suggestion; unconscious

Amir Raz and Joanna Wolfson have crafted a difficult and enlightening blend of history and critique. Their historical examination of the place of suggestion in psychiatry, and, most interesting, in Freud's development as a psychoanalyst, is revealing and informative. I wish they had not confined their insights to a graphic timeline (see Figure 1 in the Target Article), but had discussed the critical turning points in the text. For example, in one box (the third at the top, linked to 1889) they refer to how Freud's observation of post-hypnotic amnesia and related memory changes led him to free association, a highly important turning point in the emergence of psychoanalysis from a hypnotic treatment based on suggestion to a treatment based on spontaneous patient communications. One wishes they had spelled out how he arrived at free association from these earlier observations. Also illuminating were the authors' brief excursion into the influence of culture on the form of psychiatric disorders. Of perhaps greater interest, and at the heart of their article, was the way they traced the emergence of a functional view of mental disorders from a narrowly organic perspective. Freud was the beneficiary of considerable prior exploration that he then capped with his own revolutionary contributions. Perhaps this historic development can be epitomized in the authors' comment that, as Charcot demonstrated, if an idea can cause paralysis, perhaps an idea can cure it. Or, to amplify, if an unconscious conflict can cause paralysis, perhaps an interpretation can cure it.

Paralleling this paradigmatic shift from organic to dynamic or functional lesions, and then to what the authors' prefer to call behavioral lesions, has been a gradual loosening up of a localization view of the brain in favor of a modular view linked more to behavioral than strict brain criteria. The authors argue convincingly that neuroscience should move beyond the modular approach to a more complex systems view in which dynamic patterns of synchronized neural activity more closely parallel the flow of mental activity. In this regard, their critique of too-literal modular interpretations of fMRI findings should be quite welcome to psychoanalysts. Shulman and Reiser (2004), the former a physical chemist and contributor to the MRI revolution in medical diagnosis, and the latter a noted psychoanalyst, presented an earlier examination of these same issues. Further on in this commentary I will have occasion to refer to the Shulman and Reiser critique as it bears on fMRI research and unconscious processes, a topic that surprisingly Raz & Wolfson do not mention. This is especially puzzling since hypnosis and, in particular, post-hypnotic amnesia have been closely linked to the concept of an unconscious.

The remainder of this commentary is devoted to Raz & Wolfson's examination of the relationship of psychoanalysis to neuroscience and, in particular, brain imaging. At one point they "submit that the field of neuropsychoanalysis may benefit from adopting an abstemious outlook regarding the prospects of brain imaging." More generally, they caution both neuroscientists and psychoanalysts to beware of equating identical terms that refer to very different phenomena. They correctly point out that conflict in the Stroop effect is a long way from conflict on the couch. They are skeptical that terms such as ego can easily carry over to brain functioning, although at a later point they acknowledge that the ego psychological definition of ego as a repository of controlling functions could be aligned with the cognitive science concept of executive functions located in the cortex.

Aside from these helpful and wise admonitions, their analysis suffers from being too closely tied to psychoanalysis as exclusively a clinical practice, whereas from Freud onward to figures like Hartman (1959), Rapaport (1959), Rubenstein (1972), and Rangell (2007) the reach has been toward a comprehensive theory of the mind, the very virtue Eric Kandel admired about psychoanalysis and identified as the most comprehensive theory available to psychiatry (Kandel, 1998)—

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and perhaps to psychology and cognitive neuroscience, the latter two being largely theory averse and finding infatuated. By comprehensive, I mean that the theory incorporates all that constitutes the mind (and thus the dynamically functioning brain): conscious and unconscious, perception, memory, affect, motivation (in all its forms), development, normal and pathological conditions, object relations, and the centrality of individual differences. Viewed from this perspective, the clinical enterprise emerges as a special case. In short, there is a basic science of psychoanalysis first approached by Freud in the "Project" (1950 [1895]), present but obscured in *The Interpretation of Dreams* (1900), and blossoming later in his three metapsychological papers (1915). In these papers Freud shines as a cognitive psychologist, acknowledged insightfully by Erdelyi (1985) and admired by Kitcher (1992) as the first interdisciplinary cognitive psychologist while criticizing him for his failure to keep up with the many fields he drew upon and thus rendering some of the theory outdated. Kitcher makes it plain she was using Freud's success and failure as a cautionary tale aimed at her cognitive colleagues who were becoming enamored with fMRI, event-related potentials, and other brain measures.

I bring this theoretical aspect of psychoanalysis to the fore as contrasted with the clinical situation in order to call attention to the fact that psychoanalysis in the hands of the people mentioned earlier was a general psychology of the mind and not solely a psychiatry of the conflicted mind. As primarily a clinician and a clinical theorist, Charles Brenner (1982) was correct in defining psychoanalysis as dealing with the mind in conflict; he was incorrect when he included all of psychoanalysis in his definition. In fact, Brenner himself ventured to theorize beyond the couch when he reasoned that defenses used the basic cognitive processes of the mind for their motivational purposes, as in repression, which can be defined as unconscious motivated forgetting and thus is subject to the basic laws of forgetting discovered by cognitive psychologists and just plain psychologists going back to Hermann Ebbinghaus.

I would call Raz & Wolfson's attention to the possibility that psychoanalysis as a comprehensive theory based on certain general suppositions may be the only psychological theory capable of testable postdiction and prediction across a broad spectrum of phenomena. I will start with the two fundamental suppositions of psychoanalytic theory as a general theory of mind: (1) the assumption of an unconscious mental life; (2) the existence of two principles of mental functioning, the primary and secondary process. Together these constituted what Freud referred to as the two pillars of psychoanalysis. It has been almost a hundred years since Freud published his paper simply called "The Unconscious" (1915). When psychology at first, and cognitive neuroscience later, turned its attention to the possibility of an unconscious mental life, it was not within any comprehensive theory—that did not exist—but within the narrow confines of perception. In 1980 I published a paper in which I argued, as Freud had seventy years earlier, that the unconscious was a necessary assumption of all psychology, bringing together a range of findings from subliminal research (Shevrin & Dickman, 1980). In the intervening years there has been a veritable avalanche of research across many fields—perception, memory, emotion, motivation, prejudice, addiction, mood and anxiety disorders, amnesia, Alzheimer's, Parkinsonism, amnesia, autism, neglect syndromes—in which unconscious factors have been identified. These findings have emerged from the use of a range of psychological and neuroscience methods (including fMRI). If cognitive psychology and neuroscience were, like physics and biology, given to thinking in comprehensive theoretical terms (e.g., relativitity theory, theory of evolution) this astounding turnabout from rejecting the concept of an unconscious as a behaviorist pariah to being a universal presence would have been appreciated as providing powerful support for the one theory that predicted that this would in fact be the case once the appropriate methods were available.

In view of the authors' focus on fMRI, it is notable that a neuroscientist, Marcus Raichle, has called attention to what he called the "dark energy" of the brain, which is not taken into account by fMRI investigators, who only study a small fraction of brain activity responding to external stimuli:

> The adult human brain represents about 2% of the body weight, yet accounts for about 20% of the body's total energy consumption, 10 times that predicted by its weight alone. What fraction of this energy is directly related to brain function? Depending on the approach used, it is estimated that 60 to 80% of the energy budget of the brain supports communication among neurons and their supporting cells. The additional energy burden associated with momentary demands of the environment may be as little as 0.5 to 1.0% of the total energy budget. *This cost-based analysis implies that intrinsic activity may be far more significant than evoked activity in terms of overall brain function*. [Raichle, 2006, p.1249; emphasis added]

Note the final point: "intrinsic activity may be far more significant than evoked activity in terms of overall brain function." We might suppose that "intrinsic activity" would refer to processes like expectancy, motivation, feeling states, and certainly the full range of unconscious activity. Indeed, Shulman and Reiser (2004) take Raichle's "intrinsic activity" a step further. They suggest that this enormous amount of activity is reflected in the high baseline that is "subtracted" away in the usual fMRI practice and that the remaining small increment of stimulus-related activity becomes the focus of interest. They further suggest that much of this baseline (intrinsic) activity is composed of conscious and unconscious processes:

> Can we relate the high level of neural activity in the resting brain to the mental process and content of conscious and unconscious imagery, thinking, and fantasy in awake human subjects, when the brain is considered to be "at rest"—that is, lacking external stimulation? There is no dearth of internal stimuli carrying information from within the body to the cerebral cortex under this condition. Internally generated stimuli carry information about body systems via neural, including autonomic pathways that ascend via brainstem (periaqueductal grey, ascending reticular activating and mesolimbic [seeking] systems) and various other subcortical structures and systems, including thalamus, limbic system (including hippocampus and amygdala), and the hypothalamus—all of them activating, along the way, interrelated neuroendocrine, emotional, and memory circuits. We hypothesize that the high baseline brain activity supports processes of mentation that can profitably be fitted within the framework of Freud's psychology. [Shulman & Reiser, 2004, pp. 139–140]

These two quotations describe a very different brain from what is investigated in most fMRI studies, and they support Raz & Wolfson's desire to see future research move away toward more systemic approaches.

A similar development is beginning to happen with respect to the second pillar of psychoanalytic theory—the two principles of mental functioning, the primary and secondary process. The counterpart in cognitive psychology are dual-process theories in which two kinds of thinking are described, many characteristics of which parallel Freud's distinction (Brakel & Shevrin, 2003; Stanovich & West, 2000).

We may, in fact, be in a position to predict the findings that will emerge and the scope of these findings. They should be as wide ranging as the findings dealing with unconscious processes. Dual-process theories are making an appearance in the new field of behavioral economics and decision making. The long unchallenged assumption of classical economics that economic decisions are always rational is falling before the evidence accumulated over the years in support of Kahneman and Tversky's (1979) prospect theory, leading ultimately to a Nobel Prize for Kahneman, the surviving author. Decisions are often based, and in systematic ways, on thinking that in common-sense language is irrational, especially when the prospect of loss enters the picture This is precisely what Freud had in mind when he formulated his two principles of mental functioning (Freud, 1911). Thoughts governed by anxiety and by conflict, often unconscious, appear on the face of it as irrational, but follow their own rules, and hence are a-rational (Brakel & Shevrin, 2003). In particular, as Freud developed initially in his monograph *On Aphasia* (1891) and carried forward in his later clinical and theoretical contributions, language functions differently under conditions of anxiety and conflict and when it is unconscious, leaning more heavily on the concrete phonemic character of words than on their meaning, resulting in seemingly irrational transitions in thought such as are found in schizophrenic and manic thought disorders, neurotic symptom formation, and dreams. In a paper published in this Journal, we have demonstrated that the sound of a word is more likely to prime a similar-sounding word when it is subliminally rather than supraliminally presented; furthermore, degree of anxiety affects this outcome (Villa, Shevrin, Snodgrass, Bazan, & Brakel, 2006).[1]

Thus, the two pillars of Freud's theory of the mind appear to be gathering support from diverse sources, supporting the generality of the theory as a comprehensive theory of the mind.

There are also important hypotheses that can be derived from within the general theory that have always been bones of contention. I refer to the hypothesized existence of powerful forms of motivation that can assume both conscious and unconscious form, as well as to the hypothesized existence of defenses such as repression that operate unconsciously and implicate various uses of attention, a particular interest of the authors. With respect to the motivational hypothesis, Berridge (1996) has demonstrated that powerful "wantings" exist unconsciously in addicts and can neuroanatomically be distinguished from emotion, thus undermining the tendency to conflate motivation with emotion. With respect to defenses, we have shown that patterns of time-frequency components of event-related potentials related to unconscious conflict (in the psychoanalytic sense) reveal defensive activity operating unconscious-

[1]The prediction that interest in Freud's "second pillar" dealing with the primary and secondary processes would soon enter the nonpsychoanalytic literature was confirmed by the appearance in *Brain*, the leading neuroscience journal, of an article demonstrating that Freud's concept of the ego and his distinctions between two different principles of thought were supported by neuroscience evidence (Carhart-Harris & Friston, 2010).

ly (Shevrin et al., 1992). Of further interest is that these time-frequency patterns take us into the realm of synchronized brain activity, advocated by the authors as an important next step in investigating how the behavioral brain behaves. In a more recent study, we have found that alpha synchronization functions as an inhibitor of unconscious attention to a phobic stimulus presented subliminally (Shevrin et al., submitted).

These are but the beginnings of exciting convergences between the neuroscience of the mind and the psychoanalytic theory of the mind, once we untether psychoanalytic theory from the clinical situation to achieve a more comprehensive view of the mind, and once we untether neuroscience from a modular, purely cognitive view of the brain. Raz & Wolfson seem themselves to be heading in this direction.

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The Central Role of Suggestion in All Clinical Encounters Including Psychoanalysis

Commentary by Moshe S. Torem (Akron, OH)

The title of the article by Raz & Wolfson implies that the role of suggestion in psychotherapeutic change is described as "copper" while change through psychoanalysis is described as "gold," implying that suggestion has an inferior role compared to changes achieved by psychoanalysis. Some have stated that Freud himself considered his method of psychoanalysis to be a superior tool for therapeutic change compared to the supposedly unsophisticated method of suggestion. Recent studies of brain imaging have given suggestion a new importance since it has now been validated as a real and powerful phenomenon that has a biological foundation in the human brain. This has not yet happened for psychoanalytic concepts such as ego, id, transference, countertransference, etc. This commentary argues the point that Freud never truly divorced himself from recognizing the important role of suggestion in psychoanalysis. Moreover, I believe that suggestion has a central role in all clinical encounters including the practice of psychoanalysis, as is shown in greater detail in the body of this commentary. It is my opinion that accepting this point of view will benefit practitioners of psychoanalysis as well as their patients.

Keywords: brain imaging; Freud; placebo; psychoanalysis; suggestion; therapeutic change

In their article "From Dynamic Lesions to Brain Imaging of Behavioral Lesions: Alloying the Gold of Psychoanalysis with the Copper of Suggestion," Amir Raz and Joanna Wolfson attempt to integrate the conceptual phenomena of suggestion as now recorded on brain imaging with some basic psychoanalytic concepts such as ego and self. This article has great value to the practice of therapeutic interventions in the clinical setting. The authors provide a succinct review of the historical aspects of suggestion and of Freud's efforts to separate psychoanalysis from suggestion based on therapeutic modalities including hypnosis. In my commentary, I respond primarily from a clinical perspective. I will *not* devote much space to the brain imaging aspect of the article other than to state that I deeply respect the authors' critical perspective on overenthusiastic media reports on brain imaging and the unrealistic hopes and goals of some psychoanalysts to brain-image the ego or of others wishing to brain-image the id, the superego, or such phenomena as transference and countertransference, seeking verification of these concepts by the litmus test of brain imaging.

Is suggestion inferior to the "pure gold" of analysis?

Differentiating the "pure gold" of psychoanalysis from the supposedly inferior methods of the practitioners of suggestion was a lifelong aim of Freud (Wachtel, 1993). This may explain the prevailing negative attitude of many psychoanalysts toward suggestion and its use in psychoanalysis—even though, as Raz & Wolfson point out, psychoanalysis was born out of suggestion. Wachtel (1993) points out that "Freud was struggling with two quite different implications of suggestion, one therapeutic and one essentially epistemological. Freud was too honest and perceptive an observer to deny the highly significant role of suggestion in psychotherapeutic change" (pp. 180–181). By creating this new sophisticated method he called psychoanalysis, Freud harnessed the power of suggestion to move the psychoanalytic process forward in gradually promoting the uncovering of the hidden portions of the patient's psyche and also overcoming the resistances to the psychoanalytic process (Wachtel, 1993, p. 181). Schmideberg (1939) pointed out that suggestion operates in all interpretation and intervention regardless of whether or not the therapist intended it to be. This is so because suggestion operates through unconscious mechanisms. This point of view was supported by Young (1931), who went further to elaborate that the mere presence, attitudes, and behavior of a therapist have a suggestive influence on the patient, regardless of the therapist's conscious intentions. So, since this cannot be avoided, clinicians should learn to effectively employ it to benefit their patients therapeutically. Milton Erikson (Erikson & Rossi, 1980) recognized the power of suggestion to bring about therapeutic change and utilized hypnosis to augment the power of suggestion and activate therapeutic change, bypassing a patient's resistances, as described by Haley (1973).

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Defining suggestion

From a clinician's perspective, what is needed is a clear and acceptable definition of suggestion. The lack of clarity about what suggestions really are has been a challenge for clinicians and scientists for many years. In a book entitled *The Psychology of Suggestion,* authored in 1898 by Boris Sidis and endorsed by William James (the father of modern American psychology), the issue of defining suggestion is addressed. In the first chapter, Sidis wrote:

> Psychological investigators employ the term suggestion in such a careless and loose fashion that the reader is often puzzled as to its actual meaning. Suggestion is sometimes used for an idea bringing in its train another idea, and is thus identified with association. Some extend the province of suggestion and make it so broad as to coincide with any influence man exerts on his fellow beings. Others narrow down suggestion and suggestibility to mere symptoms of hysterical neurosis. This is done by the adherents of the Salpêtrière School. Suggestion, again, is used by the Nancy School to indicate the cause which produces that peculiar state of mind in which the phenomena of suggestibility become especially prominent. [Sidis, 1898, p. 5]

Almost a century later, Weitzenhoffer (1989) devotes a whole chapter of his book on hypnotism to defining and clarifying the concept of suggestion in nonhypnotic and also hypnotic states. Weitzenhoffer defines *suggestion* as "a communication which evokes a nonvoluntary response which reflects the ideational content of the communication." He then proceeds to define the term *suggestion effect*, which is "the conversion of an idea into an action that satisfies the two defining criteria of a traditional suggestion, i.e. (1) it evokes the automatism in the person receiving the suggestion that (2) reflects the realization of the ideational content of the communicated suggestion." In other words, according to Weitzenhoffer a communication (verbal or nonverbal) qualifies for the definition of suggestion only if it produces the intended response of the communication in the other person and that response is experienced as involuntary.

In their article, Raz & Wolfson provide an excellent and detailed historical review of the concept of suggestion dating back to the times of Charcot and Bernheim in nineteenth-century Europe and its evolution through Freud's psychoanalysis and present-day cognitive neuroscience, hypnosis, and psychiatry. However, they do not clearly define what they mean when they use the term *suggestion*. Do they accept Weitzenhoffer's definition? Does the person have to experience the response to the communication as involuntary? Does the person have to remember that a communication was given and they are responding to it in the form of a suggestion? What if such persons say that they remember and they know and they fully own the response as voluntary? Does this still qualify for the definition of suggestion and suggestion effect?

In previous publications, Raz (2007a, 2007b) acknowledged the power of suggestion to change people's memory and behavior even if these are nonhypnotic suggestions, However, in those publications there is no clear definition of what constitutes a suggestion. I believe that Raz & Wolfson's article, as well as any others dealing with this concept, would greatly benefit by defining suggestion and clearly communicating what it does and does not entail. It is also important to clarify the concept of suggestion as it relates to hypnosis. Does the person demonstrating the effect of an internalized suggestion have to be in a hypnotic trance (regardless of whether a formal induction of hypnosis was or was not done)? Does the person have to be in a hypnotic trance when the suggestion is given? Do the authors believe that suggestions can be powerful and effective on another person even if that individual is not in a state of hypnotic trance?

Suggestion and the placebo effect

Anne Harrington (2008) from Harvard University recognizes the historical background of suggestion and describes in detail how it helped soldiers deal with and therapeutically resolve traumatic memories (p. 62). She then says:

> If suggestion was openly deemed a good treatment for traumatized soldiers used to taking orders, its usefulness was also recognized, though much less openly, for ordinary civilian patients also long accustomed to doing whatever the doctor told them. Given the concern at the time about the ethical propriety of hypnosis, however, doctors working in a civilian context preferred to work their suggestive effects by indirect means—using fake pills and potions—and referred to the results by a new name; the placebo effect. [p. 62]
>
> What if it were possible to think about the placebo less as a form of quackery and more as a form of suggestive psychotherapy? [p. 63]

In a recent comprehensive review article by Thompson, Ritenbaugh, and Nichter (2009) and devoted to the subject of the placebo response from a broad anthropological perspective, the authors provide an extensive investigation into the origin of the concept of placebo, the placebo effect, and their relevance to the practice of medicine in the past, present, and future. In a section reviewing the underlying mechanisms for placebo and the placebo effect, they mention: conditioning, expectancy, the therapeutic relationship, and meaning (pp. 122–128). The authors recognize the importance of classical conditioning in explaining placebo and recognize the important contribution of Irving Kirsch (1985, 1997, 2004, 2008) to our understanding of placebo. However, they point out the special place of expectancy and its underlying dynamic of suggestion in understanding placebo phenomena:

> perhaps the most thoroughly researched model of the placebo effect has become expectancy. Simply put, expectancy is the patient's level of expectation that he or she will be helped by the treatment. Expectancy is a fairly robust and comprehensive model, in that it recognized the importance of the therapeutic context in supporting expectancy and triggering a placebo effect. One of the most important factors in triggering an expectancy-induced placebo response appears to be verbal instruction or suggestion. [p. 123]

They cite a study by Vase, Robinson, Verne, and Price (2003) focused on a group of patients diagnosed with irritable bowel syndrome, showing that "adding a verbal suggestion for pain relief can increase the magnitude of placebo analgesia to that of an active agent" (p. 124). In other words, a very important element in understanding placebo phenomena is clearly tied to the concept of suggestion. In recent years, the study of placebo phenomena and their importance to understanding the nonspecific elements involved in any treatment, including surgery, has been given high priority, as evidenced by the numerous research studies and publications on this subject (Benedetti, 2008; Bootzin & Bailey, 2005; Frenkel, 2008; Price, Finniss, & Benedetti, 2008; Raz, 2007a, 2007b; Raz & Guindi, 2008; Raz, Raikhel, & Anbar, 2008).

In conclusion, Raz & Wolfson's article is a very important contribution to the literature of suggestion and psychoanalysis. It recognizes the central role of suggestion in the development of psychoanalysis and the background for why many psychoanalysts have not used suggestion as part of their therapeutic practices. The authors seem to imply that some practitioners in the field of psychoanalysis view the new status of suggestion with some envy, since it has now attained validation as a real phenomenon by the brain imaging technologies. So it would not be surprising that psychoanalysts would like to bring about a similar status to their own concepts such as ego, transference, countertransference, etc. However, what if this is not achievable? Does it mean now that any behavioral or psychological phenomenon that cannot be demonstrated on brain imaging loses its validity and becomes less real? A clue to answering this question may be found in a statement attributed to Albert Einstein: "*In life some things that count cannot be counted and some of the things that can be counted do not count for much.*" I hope that this article will inspire more creative responses from others in the field and provide fresh questions and ideas for new avenues of research.

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From Dynamic Lesions to Brain Imaging of Behavioral Lesions: Response to Commentaries

Amir Raz

Keywords: fMRI; genetics; hypnosis; neuroscience; ontology; suggestion

RESPONSE TO CASONI & BRUNET
A neighborly approach to neuropsychoanalysis

Dianne Casoni and Louis Brunet are my across-the-road neighbors and upstanding members of the Montreal psychoanalytic community. "A good neighbor—a found treasure" says one adage; "No man tells the truth about himself, only his neighbors do" says another. Thus, beyond full disclosure, this neighborly collaboration may bring us closer to the truth.

In the first part of their commentary, Casoni & Brunet take a psychoanalytic view of suggestion, which draws on the unconscious mind. Suggestions, however, can be conscious too; individuals complying with suggestions are usually cognizant and may be well aware that they are responding in line with a suggestion. Teasing apart the influence of conscious from unconscious suggestions may be difficult (Raz, 2007b). After all, beliefs and experiences need not be overt declarations by the subject. Rather, they can be ingrained without awareness. A good example of unconscious mechanisms playing into one's beliefs involves the Implicit Associations Test (IAT; Greenwald, McGhee, & Schwartz, 1998)—a supraliminal experimental task presumed to measure people's unconscious feelings about different groups of objects (for a demo, see https://implicit.harvard.edu/implicit/demo). Evidence shows that manipulation of context could rapidly change the outcome of people's unconscious beliefs (Foroni & Mayr, 2005). Specifically, when subjects read about an uncommon scenario in which flowers were bad and insects were good for the environment, they elicited shorter reaction times for pairing flowers with negative words than insects with negative words, indicating that the suggestion instigated immediate effects. On the other hand, when a matched sample of subjects received only a simple instruction to view insects as good and flowers as bad, their natural tendency to see flowers as good did not change, further illustrating that suggestion can influence these associations in a rapid, unconscious manner. In this regard, the idea that people may unknowingly alter their beliefs following suggestion resonates with Freud's notion that unconscious processes guide mental and physical responses.

Neuroimaging studies allow us to view aspects of the brain under the influence of suggestion and see how suggestion, in turn, affects bodily response (Raz, Fan, & Posner, 2005). Functional MRI, however, is a measure of mass action: it hardly permits us to explain what is happening specifically, but it does provide a nice framework—a first-order approximation, which can translate into a meaningful starting point. As Casoni & Brunet point out, brain scans cannot

Acknowledgement: Amir Raz would like to thank Joanna Wolfson for help in preparing these responses. The proverbial "we" throughout the text refers to Raz and Wolfson.

show us the unconscious; few neuroimagers, if any, would claim otherwise. Instead, having suggestible individuals achieve specific, often remarkable, cognitive feats while performing in the scanner may elucidate how these operations are possible (Oakley & Halligan, 2009). People who participate in such experiments often report that they are not the authors of their own actions and that things are happening *to* them (rather than *by* them), although it is easy to verify that they are undeniably the sole instigators of these actions. This line of investigation is important to psychoanalysts as well as to social scientists (e.g., Aarts, Custers, & Wegner, 2005; Aarts, Wegner, & Dijksterhuis, 2006).

Psychoanalysis is about understanding human agency and subjectivity and how people think about human feelings. Many psychoanalysts are of the opinion that imaging of the living human brain flattens psychoanalysis by taking the rich and complex ways of understanding human beings and collapsing them into a brain scan. This notion, however, is untoward. Research reports over the past 15 years describe unprecedented convergence between the social sciences and the neurosciences, resulting in several new areas of study including social cognitive neuroscience, social neuroscience, affective neuroscience, and neuroeconomics. In each of these areas, researchers examine the social and emotional aspects of the human mind by applying the techniques from the neurosciences. Specialty journals (e.g., *Social Cognitive and Affective Neuroscience*) provide venues for human and animal research that uses neuroscience techniques to understand the social and emotional aspects of the human mind and human behavior. In this regard, "the future is already here," even if many criticisms apply (see the Overarching Argument at the end of the responses).

Although certain scholars seem to have very clear ideas about the answer (Chertok & Stengers, 1992), it is difficult to conclude whether individuals who follow therapists' interpretations are responding to suggestion or to something specific to psychoanalysis. Neuroimaging is unlikely to answer such questions. Aside from concerns about the technical limitations of neuroimaging to uncover the basis of human behavior, we must appreciate the weakness of "neurological determinism"—the danger of making sweeping social generalizations on the basis of niche neurological research findings. Even if neuroscience can deliver a descriptive account of our mental lives, including our moral and social views, it may still be a long shot to suppose that it should be relevant to the prescriptive or normative concerns of fields such as moral and political philosophy. In this regard, neuroscience might help policymakers set and achieve more realistic goals, but science does not—and probably ought not—say what those goals should be.

RESPONSE TO FABREGA
From the genetic basis of heritable traits toward ontological cognitive neuroscience

Although only obliquely related to the Target Article, Horacio Fabrega's perspective raises interesting issues concerning how molecular and evolutionary forces can create changes at the level of DNA, and how these changes lead to observable differences among individuals. Beyond the mechanics of science, this approach brings into focus ontological issues in the cognitive neurosciences.

When scientists mapped out the human genome about a decade ago, they expected to find the genes underlying many diseases and traits such as height and weight, but this outcome has hardly transpired. Instead, it turned out that most diseases and traits are much more complex, probably resulting from interactions among dozens, if not hundreds, of genes. Genome-wide association studies enable us to identify a small fraction of these genes, but most remain elusive. Because there are so many factors, the effect of one individual factor is small and any sort of statistical test often lacks power to identify it (see Response to Goldberg).

Using yeast as a vehicle, scientists have recently been able to examine the genetics of complex traits (Ehrenreich et al., 2010). Compared to humans, yeast affords enormous sample sizes—it is easy to grow billions of microscopic single-celled yeast organisms in a small container. Because we start with a known pair of parent strains, we can treat these organisms as children from one family and track what regions of the genome contribute to trait variation. This approach examines one single, large family, and it may be difficult to generalize yeast to humans. Furthermore, the inheritance of height within one family may rely on one set of factors, whereas in a different family it may rely on a different set of factors. Thus, in terms of being able to explain the genetics of common traits and common diseases, we remain far away from having anything close to a full picture. Obviously, unraveling genetic codes associated with higher order traits—for example, relevant to suggestibility, transference, and psychoanalysis—is even a longer way off. Nevertheless, leads to such trajectories are already in place (e.g., Lichtenberg, Bachner-Melman, Gritsenko, & Ebstein, 2000; Raz, Fossella, McGuiness, Zephrani, & Posner, 2004; Raz, Hines, Fossella, & Castro, 2008).

Genetics represents a real scientific revolution. Neuroimaging constitutes another, and the imaging of genetics yet another, but what we are missing is a function matching brain material to mental material. This ontological correspondence is sorely lacking and creates a situation where information is abundant but knowledge is scant. We have a pretty good idea of what a brain atlas should include, but how about a mind atlas? (cf. Uttal, 2001, chap. 3). Cognitive psychology hardly provides a map of the mind. Instead, it informs us what some parts of the mind are, and even teaches us how we can measure those experimentally—it does not outline a big map of "the structure of the mind."

Since the days of William James, cognitive scientists have been talking about the building blocks of the mind—modules such as attention, memory, and perception. But people do not always mean the same thing when they say "conflict" and "inhibition." Thus, what we really need is a formal specification of the things that exist in each domain—an ontology. In genetics this ontology is in place (see www.geneontology.org); alas, in the cognitive sciences it remains tenuous. Yoking such an ontology with the databases mentioned in the Response to Goldberg, we may enter a golden age where the necessary conceptual and technological ingredients combine to permit explosive discovery.

RESPONSE TO GOLDBERG
The (statistical) power of neuroimaging

Ilan Goldberg provides an interesting clinical lens on the use of suggestion in psychogenic nonepileptic seizures (PNES), wherein video and electroencephalography (EEG) monitoring serve as the definitive investigation. His oblique nod toward placebos joins a growing appreciation for a burgeoning field of clinical science (e.g., Raz & Guindi, 2008; Raz, Raikhel, & Anbar, 2008). Studies reporting provocative tests (e.g., Benbadis et al., 2000; Cohen & Suter, 1982; Ribaï, Tugendhaft, & Legros, 2006; Wassmer, Wassmer, & Donati, 2003) often receive an ethical scolding (e.g., Gates, 2001; Wilner, Keezer, & Andermann, 2010), yet a survey reporting responses from 426 members of the American Epilepsy Society reported that 93% of respondents routinely use provocation tests (mostly with intravenous saline but also with alcohol pads, tuning forks, and hyperventilation) and that 87% indicated that their patients did not have difficulty in accepting the use of provocation tests (Schachter, Brown, & Rowan, 1996).

Suggestions have been used in clinical neuroscience, including in concert with fMRI scans; however, many cognitive neuroscience studies report inflated correlations in whole-brain fMRI analyses. Some researchers attribute these overstated correlations to a ubiquitous tendency to use nonindependent analyses (see the Overarching Argument at the end of the Responses), whereas others attribute them to the dangerous combination of small sample sizes (e.g., many fMRI studies can hit the printers with fewer than one or two dozen participants) and stringent alpha-correction levels (Yarkoni, 2009). Furthermore, systematic exclusion of participants to ensure sample homogeneity (e.g., matching parameters such as age, gender, and intelligence), titration of task difficulty to ensure that performance is within a statistically viable limit, and little regard to baseline individual differences (e.g., regarding cognitive, affect, or personality) are all noteworthy methodological caveats. Primarily, however, lack of statistical power—in other words, the probability of detecting a significant effect in a sample given that the effect actually exists in that population—is a tenuous issue in many neuroimaging studies. Consequently, interpretation of results from low-power fMRI is meeting mounting skepticism.

As a matter of good practice, members of the scientific community tend to be skeptical. Science thrives on a zetetic approach, and scientists are typically conservative in what they consider a "generally accepted view." Two types of errors, however, stand in the way of any gatekeeper of science. One pertains to how nonexistent phenomena may pass as real or generally accepted. The other pertains to how real phenomena, which should be generally accepted, may pass as nonexistent. Scientists typically pay more attention to the former trap, and some consequently tend to be overzealous or dogmatically skeptical; members of this staunch group can be skeptical of their own belly buttons. The second trap, however, is usually less explored.

A recent review of a number of negative consequences associated with the use of low-powered correlation tests in fMRI studies reports Type II error (Yarkoni & Braver, 2010). False negatives—errors of failing to reject null hypotheses when they are, in fact, not true—comprise errors of failing to detect a veridical difference, thus resulting in tests of poor sensitivity or excessive skepticism. It is bad form to miss real effects; to make things even worse, however, an additional ill consequence of insufficient power is an increase of significant effect sizes. Thus, the combination of low power and effect-size inflation can lead to misinterpretation of fMRI results. In this regard, a meta-analytic approach to neuroimaging is likely to be more revealing than any individual study (cf. Cognitive Neuroscience Society, 2010).

The brain imaging literature is getting difficult to navigate and master because the number of publications is increasing exponentially (see Figure 1). Accordingly, we must move toward a synthesis: advocate for papers that integrate the literature and distil the most important findings (cf. Raz & Buhle, 2006). Indeed, low power is one of the main reasons propelling people to favor an integrative research approach. It is essential, for example, that we look across different studies and see whether we can conclude that many of the same brain regions show up in specific, circumscribed tasks. Unfortunately, neuroimaging does not have a “consensus language” for describing such areas.

Jacob Cohen, whose contributions to statistical analysis in the behavioral sciences have earned him a place of fame, showed that in behavioral science—at least in experiments of social and abnormal psychology—the average study had about 50% power for medium effect sizes (Cohen, 1962). With more than 30,000 citations to this contribution alone, his development of rough norms for small, medium, and large effect sizes, not to mention his easily used methods for estimating statistical power for a planned study, made his 1969 book the classic in its field, with widely used subsequent editions and eventually computer programs (Cohen, 1969). It is embarrassing to admit that in many neuroimaging studies we would be lucky to have even the 50% power that Cohen first pointed out nearly five decades ago (cf. Cohen, 1992; Maxwell, 2004; Sedlmeier & Gigerenzer, 1989; Yarkoni, 2009).

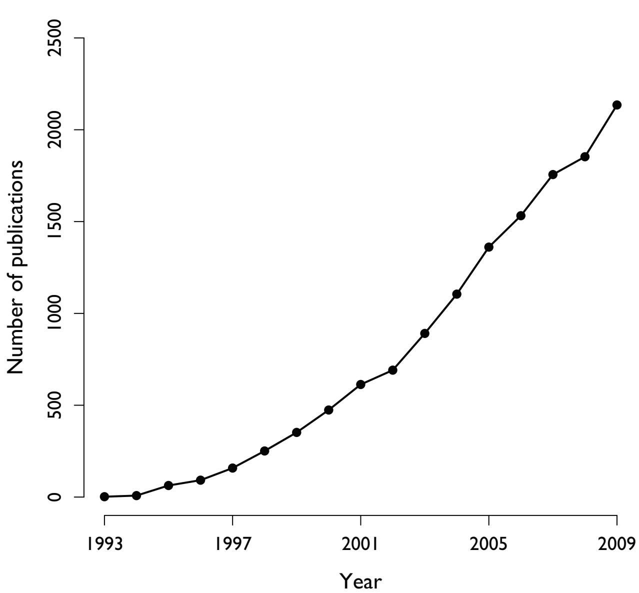

Figure 1. Timeline of number of publications indexed by PubMed and involving fMRI. (Variation on an unpublished slide by, and courtesy of, Tal Yarkoni.)

Power is not always a problem in neuroimaging. In some cases, the effects are towering—big enough to detect without a snag. Alas, it is difficult to know in advance the contexts wherein the effects will be conspicuous and those wherein running 24 bright-eyed participants will be insufficient to detect an effect. We must therefore inject a measure of rigor and advocate for judicious interpretations. The evidence shows that we have learned a tremendous amount over the past two decades even without running $N = 100$ participants in individual fMRI studies (cf. Raz et al., 2009), but we must be mindful of a more sustained and statistically robust stratagem. Such a cumulative approach can help us achieve multiple goals such as wade through an enormous literature; separate reliable from spurious findings; identify the latent structure that underlies all these disparate findings; and develop a better way to integrate and frame studies. In addition, it can help us gain better estimates, when power limitations are absolutely inevitable.

Other fields within science have dealt with similar issues with a measure of success. For example, in the domain of genetics (see Response to Fabrega), despite a massive multiple-comparisons problem similar to that of neuroimaging, we hardly encounter the problems that plague fMRI research. One of the reasons for this difference centers on the fact that in genetics we can define a polymorphism or a haplotype and proceed to test it with the consensus of the scientific community. On the other hand, in the realm of neuroimaging we seldom agree on even the most basic things such as what location in the anterior cingulate cortex—which involves thousands of voxels—we should use for an a-priori Region-of-Interest analysis. That is a major shortcoming of current neuroimaging. One possible solution is to have consensus regions based on a meta-analytic approach. Such cumulative tools are now budding through specialty repositories of brain-mapping data (i.e., surfaces and volumes, structural and functional data) from many laboratories across the globe.[1] These datasets are reconfigurable so that individuals can tailor them to pursue any desired meta-analysis.

We can extend this approach, as the Target Article suggests, to networks (i.e., we can identify patterns,

[1] For example, the Surface Management System Database (http://sumsdb.wustl.edu/sums) spans about 15% of the fMRI activation foci reported in more than 10,000 experimental studies that have been published to date.

not just regions, of interest). By creating a map of the areas that are consistently activated across laboratories on specific tasks, we can also find overarching neural patterns and parcel dynamics. Defining small parcels with respect to neuroanatomical boundaries and applying nonmetric, multidimensional scaling (to use one example), we can reduce the global neural complexity to a small number of networks. Such distilled information is helpful to learn about connectivity and the functional groupings/relationships among disparate neural areas. More importantly, it provides a way to tease apart and hone in on a-priori networks of interest.

RESPONSE TO MICHELS
Toward a personalized neuroscience

Robert Michels, a psychoanalyst of pedigree and a paragon of clear thinking, acknowledges that the a-priori assumption about mental or behavioral changes is that they have to do with brain activity, but he throws a typical Michels wrench: What can neuroscience actually do for psychoanalysis? More specifically, even if we did everything right and imaging technology showed us the brain areas involved in psychoanalytic activity, how would these findings matter for the psychoanalyst in the trenches?

I agree with Michels that knowledge of the specific brain areas implicated in psychoanalytic defenses, drives, etc. would be unlikely to affect the extent to which these naturally occur. I also concur that such discoveries would probably bring more immediate excitement to neuroscientists than they would to psychoanalysts. The operative words, however, are "naturally" and "immediate." Let me sketch what I mean.

Neuroscience could probably benefit from a measure of modesty. Claims that neuroscientists can interpret and understand consciousness or locate free will make social scientists and psychologists, let alone psychoanalysts, turn to them with a mixture of "shock and awe." As is the case with other situations (e.g., that of Donald Rumsfeld), the shock is right; the awe is probably overrated. Dialogue among different disciplines is crucial if we are to understand ourselves. In more ways than one we need the biologist Edward Osborne Wilson's notion of "consilience," the bringing together of ideas from different disciplines—however, not necessarily the sort that collapses everything into the language of neuroscience. For example, some scholars of psychoanalysis explore what neuroscience can say about the broader social and political world (Westen, 2007). Others make claims of evolutionary psychology for innate and evolved moral properties and decision-making mechanisms, or use neuroimaging to demonstrate a biological propensity within our brains to make moral decisions (Greene & Paxton, 2009; Greene et al., 2009; Paharia, Kassam, Greene, & Bazerman, 2009). Cultural, social, and historical parameters, however, clearly influence our moral judgments; these judgments change—sometimes radically—across generations and cultures. More importantly, neurobiological, evolutionary, and psychological claims about the existence of these fundamental moral propensities are irrelevant to political life, where decisions must be operationalized as a function of politics, economics, and society—scarcely brain science.

Francis Sellers Collins—a leading geneticist, former director of the U.S. National Human Genome Research Institute, and the newly appointed director of the 27 institutes that comprise the National Institutes of Health (NIH)—is hardly bashful about one of his guiding visions: using genetics to create individualized treatments. Noted for his landmark discoveries of disease genes and his leadership of the Human Genome Project, Collins claims that, perhaps sooner than we envisage, DNA sequencing will be ubiquitous, and complete genetic information will be a part of one's medical record. Such an addition would herald a new era of "personalized medicine" in contrast to our current "one-size-fits-all" medicine. Because we are all different based on our heredity and our environmental exposures, it makes good intuitive sense that practitioners should incorporate this important information into any clinical intervention. Individuals with a higher-than-average risk for diabetes, for example, should be motivated to take better charge of their health (e.g., through exercise and nutritional programs). In a similar vein, if people know that they are at a higher-than-average risk for breast or prostate cancer, they will have a chance to make personal decisions about the kind of screening processes they want to follow. This approach would likely aid in avoiding some negative outcomes. In addition, when a person falls ill and needs to take a specific drug, DNA tests would afford expressly tailored dosage information based on the person's particular metabolism. Whereas most clinicians seem ready to embrace the prospect of individualized medicine, fewer practitioners appreciate what this approach entails for behavioral science and psychotherapy.

With a rising awareness of the role that an individual's genetic makeup plays in health, the ability to sequence the entire human genome represents a tangible scientific revolution that is just beginning to impact our lives. On the one hand, it seems natural that Collins had his entire genome sequenced. On the other hand, scientists have overwhelmingly focused their genetic research on Western and Asian cultures and have largely neglected the African continent—the cradle of humanity, where human genetic diversity is greatest. In a recent attempt to merge developing-world politics with high-tech science, the South African Anglican Archbishop Desmond Tutu and other tribal leaders from communities that still practice a hunter-gatherer lifestyle volunteered their DNA (Schuster et al., 2010). Findings reveal stark differences between the genetic make-up of southern Africans and the genomes of Europeans, Asians, and West Africans. In due course, therefore, such results will help address the current lacuna typifying research into drugs and diseases, which almost completely ignores the genetic variation of the African population, making drugs often less effective there than elsewhere in the world and missing gene variants that make people more susceptible to specific diseases. Correlating genotype with phenotype, the natural next step, would likely uncover the underlying psychological parameters, which may aid in matching individuals to behavioral therapies.

Another fundamental change represents the imaging revolution. Magnetic resonance imaging (MRI) is arguably the most important imaging technology since Conrad Röntgen introduced X-rays in 1895. The emergence of functional MRI (fMRI) in the 1990s had a palpable impact on basic cognitive neuroscience research. Neuroimaging permitted the exploration of the cerebral underpinnings of the mental representations that mental chronometry could only infer. Historically, the microscope and telescope opened vast domains of unexpected scientific discovery. Now that new imaging methods can visualize the brain systems used for normal and pathological thought, a similar opportunity may be available for human cognition. Even back in the 1990s, two key advantages of brain imaging became prominently apparent. First, neuroimaging provided a new way of functional modularization of the hierarchy of cognitive operations. Second, even the highest stages of processing that often eluded chronometric analysis were analyzable with comprehensive reproducibility. The notion of imaging the neural substrates of mental activity, without requiring the research participant to perform an overt response or even an actual task, was a powerful factor in the resurgence of interest in higher cognition and consciousness because it permitted the visualization of the substrates of covert attentive states. The link to subliminal, unconscious, and psychoanalytic research should be obvious. (A third revolution is that of imaging genetics, which melds the two aforementioned revolutions together. This last revolution is yet to be fully realized, and we will talk about it elsewhere.)

A recent paper in *Science* (Collins, 2010b) laments that the National Institutes of Health (NIH) may be too slow to translate some scientific discoveries (Collins, 2010a). "Translation" refers to the process of taking the basic science discoveries that are pouring out of labs across the world and pushing them as aggressively as possible toward new therapeutics. Collins says that, with the availability of new technology, university scientists can work on exciting projects that would drive the front end of the "drug development pipeline." After a project has moved a few steps down that path and has been effectively "de-risked," it becomes more appealing for a commercial enterprise to pick it up and run with it, knowing that their chances of breaking even financially are constantly increasing. In the future, for example, clinicians may be able to identify suggestible patients to aid in the selection of interventions or treatment adjuncts (Raz, Hines, Fossella, & Castro, 2008), and neuroimaging may help in guiding the trajectory of behavioral treatments (Linden, 2008).

Michels explains that neuroscience is fine and good, but that any neuroscientific findings are unlikely to change the practice of psychoanalysis. I think that while such findings may have little impact on the way that practitioners of psychoanalysis are working at the moment, it is certainly likely to influence their practice in the future. "It is very difficult to predict, especially the future," said Lawrence Peter "Yogi" Berra, but science is certainly less of a sprint and more of a marathon. Michels and I have even intimated this sentiment in the conclusion of a jointly authored paper (Raz & Michels, 2007):

> In the aftermath of the human genome and neuroimaging revolutions, a future time before long may permit identification of suggestible patients and selection of psychological treatment (e.g., based on genetic screening, attentional testing, and personality profiles such as suggestibility). In such a world, neuroimaging will help in deciding which patient should be treated by drugs and which by psychotherapy, as well as providing an objective guide to the effects of treatment. [p. 180]

RESPONSE TO NACCACHE
The importance of using converging evidence

Lionel Naccache, a neurologist as well as neuroimager, offers a hopeful view of the promise of neuroimaging. While he acknowledges that "many dynamics of our psyche are not well captured" by specific experimental methods, he highlights certain strategies for exploring "streams of consciousness," which could lend insight into the free-association and cathartic outlets that Freud found therapeutic. Although fMRI is currently the chief support of neuroimaging in cognitive neuroscience, Naccache brings to our attention electrophysiological tools as a way to more accurately examine, or perhaps complement, research into mental activity. Converging evidence—using multiple technologies and different modalities to triangulate a specific issue—is the name of the game, of course. Psychological studies have sharpened and polished the kind of experimental questions that we ask. Consequently, we now have a plethora of experimental paradigms in our research armamentarium. Both behavioral scientists and neuroscientists, however, often rely on indirect inferences. Whether we observe behavior actions or measure brain activations, we further constrain the theories we seek to either support or disprove by triangulating these data and putting them together. Thus, converging methodologies guarantee that we test our hypotheses in multiple ways and circumvent the artifacts of one method by using another. Convergence of methods is especially important in neuroimaging partly because fundamental questions concerning the interpretation of fMRI results remain unanswered, partly because the conclusions drawn from fMRI studies often ignore the actual limitations of the methodology, and mostly because neuroimaging experiments are often expensive, highly specialized, and rarely replicated independently. We have been privileged to lead by example, in letter as well as in spirit, by demonstrating how to combine the temporal advantages of electrophysiological measurements with the spatial advantages of fMRI data on questions relevant to psychoanalysis (e.g., Raz, Fan, & Posner, 2005).

Beyond the methodological progress, however, we must delineate a few core issues that transcend what we can achieve with imaging of the living human brain. Brain imaging can help unravel the circuitry subserving the performance of cognitive tasks. Advances in scanning technology, image acquisition procedures, experimental design, and analysis methods promise to propel fMRI from brain cartography toward a meaningful understanding of neural organization and function. Such promise paves the road to a more scientific study of the area of neural plasticity, for example, as it arises in the study of differences among individuals in the development of the nervous system. Researchers (e.g., Michael Posner) have made special efforts to promote the study of individuality and development because imaging methods permit such studies, and individual variation represents an important way of relating brain networks to genes (e.g., Posner & Rothbart, 2007). Furthermore, neuroimaging allows for examination of sensorimotor integration, in particular how we can deal with the issue of authorship, volition, and forms of decision making in understanding how to go from brain activity to behavior. This effort may allow for a better understanding of the regulatory processes developed by the brain that are so pivotal to the control of our thoughts, emotions, and behaviors.

Functional MRI is the fad in describing brain activity. Even as we keep in mind other available tools (e.g., electrophysiology), as Naccache urges us to do, MRI is still the most important imaging advance since the introduction of X-rays and has assumed a role of unparalleled importance in diagnostic medicine and more recently in basic research. The emergence of fMRI in the early 1990s has resulted in not just a scientific revolution but also a publication explosion, with about 8 papers per day reporting on fMRI-related topics by 2007 (Logothetis, 2008). These facts are hard to ignore, and fMRI is certainly exciting—not only to neuroscientists, but also to anyone hoping to find support for theories of conscious and unconscious processing. We should refrain, however, from haphazardly translating this excitement into knowledge. Naccache is optimistic that neuroimaging technology may be fine-tuned in the near future to allow for stronger and more accurate reverse-inferencing. If his outlook comes to fruition—and I hope it does—it would certainly be an important advancement.

Naccache also targets "intersubjective cognition" as a driving force in both suggestion and psychoanalysis. We agree that response to suggestion has much to do with internalizing the culture of one's surroundings, beliefs shared by others, and expectations. Freud observed these phenomena in witnessing group inductions into hypnosis, commenting that, "it is of greatest value for the patient who is to be hypnotized to see other people under hypnosis, to learn by imitation how she is to behave and to learn from others the nature of the sensations during the hypnotic state" (Freud, 1891, p. 107). Intersubjective cognition has appeared in not only Freudian but also more contemporary psychology. For example, a collective hysteria epidemic in five Amish girls, ages 9 to 13 years, involved conversion disorder symptoms of weakness, anorexia, and motor deficits. In this instance, the girls experienced social conflicts including dysfunctional family dynamics and a temporary crisis in the local church; they were also at a fragile age where they would soon leave schooling and perform household duties under the scrutiny of the community (Cassady et al., 2005). This example highlights the importance of group influence on the presentation of symptoms that have their roots in suggestion. Such intersubjective cognition, therefore, may be a unifying factor.

Naccache, who thinks deeply about issues of mind and brain, walks the hallways of the Hôpital de la Pitié–Salpêtrière in Paris—the milieu in which Freud observed Charcot's demonstrations. It is fitting, therefore, that he is one of few researchers to study subliminal perception with neuroimaging, and his results have already helped elucidate unconscious processing (Bekinschtein et al., 2009; Gaillard, et al., 2009). In his book, *The New Unconscious* (2006), Naccache describes his attitude toward and scientific findings on the unconscious. While Naccache explains how Freud's notions of the unconscious were largely an interpretation of conscious material, he defends Freud's concept of hidden mental processes and argues that these processes can teach us much about the human psyche. Naccache's informed approach feeds on a large body of evidence, including that from a research team led by Stanislas Dehaene who has been using converging brain-research tools—spanning fMRI, electro- and magnetoencephalography, and deep-brain electrodes—to elucidate the neural underpinnings of consciousness. Forging an access to consciousness—that is, characterizing when participants are able to report on their conscious experience and when they are not—Dehaene and his colleagues, including Naccache and Laurent Cohen, have identified indices to conscious processing: physiological markers that change when a participant reports awareness of a datum (Bekinschtein et al., 2009). Moreover, when they render the same information nonconscious (e.g., with subliminal presentation), these biological signatures disappear. Proposing a theory—the global neuronal workspace theory—Bekinschtein et al. suggest that when processing information exceeds a threshold for large-scale communication across many brain areas, the network step-functions into a large-scale synchronous state wherein the biological indices become measureable. This recent theory has been positively received and embraced even by prominent scholars in different fields (e.g., Steven Pinker in the domain of language).

Naccache comments that, "since Mesmer, magnetism is never far away when mind issues are under scrutiny." It is indeed ironic that Mesmer too referred to magnetism, but times, meanings, and our scientific insights have changed considerably since the days of Mesmer. In spite of the many caveats we outline in our Target Article, fMRI is currently one of the best tools we have for elucidating brain function and formulating testable hypotheses (even if the plausibility of these hypotheses critically depends on factors such as apt experimental paradigms, adequate statistical analysis, and appropriate mathematical and hemodynamic models). Obviously, fMRI is hardly the only methodology suffering from serious limitations. For example, electrical measurements of brain activity, including invasive techniques with single or multiple electrodes, are suboptimal to describe network activity; single-unit recordings and firing rates are better for the study of cellular properties than for learning about neuronal aggregates, and field potentials share many of the same conundrums we outlined for fMRI. A multidimensional approach, therefore, is essential for the study of the human brain, and a combination of fMRI with other noninvasive techniques that directly assess the brain's electrical activity seems especially appealing (Esposito, Mulert, & Goebel, 2009).

Research into the "default-mode" brain state has taught us a great deal about spontaneous brain activity and its potential relationship to introspection and other conscious activities. We must appreciate, however, that creating a baseline condition to which we can compare our fMRI results is an exceedingly tenuous proposition. Setting aside variations of techniques such as arterial spin labeling, baseline brain activity in fMRI is a moving target; it is very difficult to pin it down (see Response to Shevrin). On the other hand, in techniques such as positron emission tomography (PET), a baseline condition can be measured accurately and used as a reliable yardstick from which to calculate deviations. Yet, fMRI, unlike PET, is noninvasive, so the specific relative merits and drawbacks of each imaging technique make the issue of convergence and triangulation of the data all the more poignant. Thus, instead of seeking specific findings using a particular technique, we should seek to obtain converging findings using diverging techniques. As a case in point, the convergence of results in research into attention and language seem especially impressive (Posner, 2003). Both these areas hold important implications for psychoanalysis.

RESPONSE TO SHAPIRO
The abracadabra of repression

Theodore Shapiro has been an inspiration, a mentor, a colleague, and a friend. Many aspects of the Target Article have been influenced by discussions and informal interactions with him over several years. Specifically, modulation of the seemingly automatic brain computation of word reading that is apparent in the Stroop effect comprised a research project that we have contemplated and carried out jointly. Since our initial theoretical account (Raz & Shapiro, 2002) and first empirical data set (Raz, Shapiro, Fan, & Posner, 2002) our results have been independently replicated and extended (Casiglia et al., 2010; Raz, 2006; Raz & Campbell, in press; Raz, Fan, & Posner, 2005; Raz, Kirsch, Pollard, & Nitkin-Kaner, 2006; Raz, Moreno-Iniguez, Martin, & Zhu, 2007; Raz et al., 2003; Sun et al., 1994). In line with his psychoanalytic outlook, Shapiro wonders whether this process parallels what occurs in the brain when certain defense mechanisms, such as repression, are at play. The concept of repression is vast, however, and defense mechanisms represent the basis for many of Freud's claims, including an unconscious forgetting that Shapiro likens to the "forgetting" of word meaning that highly hypnotizable individuals achieved in our experiments. In the Stroop paradigm, however, the "forgetting" of word meaning is the result of a direct posthypnotic suggestion to view words as gibberish and a result of words being neither emotionally laden nor anxiety-evoking—certainly far from approaching traumatic significance. As Shapiro states, it would be interesting to find a cognitive task that could elicit a need for repressing or unconsciously dealing with psychologically distressing material. Such a task would be revolutionary not only for its ability to track the defense process neurologically, but also for having passed the ethical code that upholds a "no harm done" motto, especially when it comes to potentially distressing mental material. I would like to argue, however, that such a task is likely untenable. In addition, I provide a long-overdue account of *abracadabra*—an incantational word which was first used as a charm in the second century and has since surfaced in many of our intellectual exchanges—a term of relevance to this topic.

A few problems with Freudian repression of memories

The number of contemporary experimental psychologists who subscribe to Freud's idea of repression is decreasing rapidly. Moreover, even behavioral scientists who study emotional processing in psychiatric patients seldom cite Freud (McNally, 2006). While some still think that modern research vindicates Freud's concept of repression (e.g., Erdelyi, 2006), these accounts represent a dwindling minority. In memory research, for example, we would expect that the magnitude of forgetting would be much more pronounced for individuals who are highly motivated (aka highly hypnotizable) to shunt this material out of awareness. In other words, the motivation to forget should increase the individual's ability to do so. But research studies into this issue have reported that this is hardly ever the case. Aside from two noteworthy exceptions (Moulds & Bryant, 2002, 2005), it has been inordinately difficult to support Freud's "repression" hypothesis. On the other hand, multiple studies seem to support the contrary (Korfine & Hooley, 2000; McNally, Metzger, Lasko, Clancy, & Pitman, 1998; McNally, Otto, Yap, Pollack, & Hornig, 1999; Power, Dalgleish, Claudio, Tata, & Kentish, 2000; Wilhelm, McNally, Baer, & Florin, 1996). Furthermore, even adults who report histories of having "repressed and recovered" their memories of childhood sexual abuse rarely display the predicted superiority for forgetting trauma-related information (Geraerts, Smeets, Jelicic, Merckelbach, & van Heerden, 2006; McNally, Clancy, Barrett, & Parker, 2004; McNally, Clancy, & Schacter, 2001; McNally, Ristuccia, & Perlman, 2005). Thus, cumulative findings suggest that even individuals who should be extremely motivated to forget their traumas are largely unable to do so. In addition, experiments instructing participants to discount, disregard, or generally push unwanted thoughts out of awareness often result in a boomerang effect, thereby increasing the prominence and accessibility of these thoughts (e.g., Beck, Gudmundsdottir, Palyo, Miller, & Grant, 2006; Shipherd & Beck, 1999; Wegner, 1994).

Freud's move away from hypnosis

Shapiro helps explain Freud's move from suggestion as a desire to cultivate his own psychotherapy of analysis. In other words, if Freud hoped that suggestion and hypnosis would fall short, it was not a factor of his disbelief in these practices but, rather, a tactic to distinguish and promote his theory. Shapiro points out that Freud purported to "shun" suggestion and argue in favor of interpreting the transference between patient and therapist. In examining Freud's later writings, it seems plausible that his ruling against suggestion was perhaps a ruse to make his therapy look more appealing by comparison. In discussing one patient in particular, Freud likened her increased suggestibility and strong emotions for the hypnotist to the strong transference relationship that bound her to her therapist (Chertok, 1977). In speaking of his creation of psychotherapy and use of transference, Freud says, "And it must dawn on us that in our technique we have abandoned hypnosis only to rediscover suggestion in the shape of transference" (Freud, 1916–17, p. 466). So while Freud attempted, as Shapiro points out, to strip suggestion from interpretation, this rhetoric may have represented a strategic diversion more than a bona fide stance.

Abracadabra—I shall create as I speak

The Romans believed that abracadabra had the ability to cure toothaches and other illnesses. Patients seeking relief wrote these letters in the form of a triangle on a piece of parchment and wore it around their necks on a length of thread. Today abracadabra is used as a pretend conjuring word, which also means meaningless talk or nonsense. Although the etymology of the post-classical Latin abracadabra has been the subject of much conjecture—some have suggested an origin within Latin or Greek, others a borrowing from languages as diverse as Thracian (a sparsely attested Indo-European language) and Sumerian—one large group of etymological explanations tries to derive the word from Hebrew or Aramaic. According to this proposal, a possible source is אברא כדברא (*evra kedebra)*, which roughly means "I shall create as I speak." Etymology aside, this charm against illness or evil also refers to obscure, mystificatory language—in line with what many individuals think of hypnosis and perhaps of our posthypnotic suggestion in the Stroop task.

In conclusion, empirical findings addressing motivated forgetting have largely undermined Freud's legacy of repression. Members of the psychoanalytic school often mention retrieval inhibition, elaborative reconstruction, and hypermnesia as possible mechanisms for repression and the recovery of lost memories, but extremely little, if any, empirical evidence supports repression in the first place. Instead, dozens of studies have been unable to report on a single convincing case of repression in the entire literature on trauma (e.g., Kihlstrom, 2006; McNally, 2003; Pope, Oliva, & Hudson, 1999). Thus, most traumatized individuals seem to remember their traumas well. Moreover, when trauma is forgotten, it appears to occur through processes other than repression. We—and many an informed observer—find it curious that Freud's assorted remarks about the brain have earned him undue acclaim for envisaging such fields as cognitive neuroscience, or even psychotherapeutic approaches developed in direct opposition to psychoanalysis, such as cognitive therapy. We regard Freud as an important thinker who has provided many insights into human behavior and the mind. Ideology must not trump evidence, however, and at least in the field of trauma and memory empirical findings suggest that Freud's conceptualization of repression is more of an abracadabra than a helpful explanatory notion.

RESPONSE TO SHEVRIN
Discerning dark energy from psychoanalysis

Howard Shevrin is a senior statesman of psychoanalysis and a man whose life work and accomplishments I admire and salute. In his commentary, Shevrin mentions that he would have liked to have seen a more detailed account of the evolution of Freudian technique, and we wish we had had more space to elaborate on that aspect. The historical background of Freud's move from observer of Charcot and Bernheim to his ultimate creation of psychoanalysis is a long and involved story. Freud became interested in hypnosis while personally witnessing many hypnotic inductions and experiments. In speaking of his experience, Freud wrote, "now for the first time, in the phenomena of hypnotism, it [the unconscious] became something actual, tangible and subject to experiment" (Freud, 1924 [1923]). Freud dabbled in hypnosis under the influence of Charcot, but he eventually abandoned the practice—purportedly out of embarrassment over his inability to induce hypnotic experiences in his patients (Puner, 1947). Freud originally went to Bernheim at the Nancy school to learn how to deepen his patients' hypnotic trances in order to get the patients talking about the origin of their symptoms. Instead, Freud's interactions with Bernheim paved the way toward a transition into psychoanalysis. Bernheim showed Freud that he was able to accomplish the same goals as did Charcot without a formal hypnotic induction. Bernheim's construction of hypnosis as a state of interpersonal suggestion led Freud to shift his focus from hypnotic suggestion to psychotherapy in the waking state. At that time, Freud also witnessed Bernheim remove posthypnotic amnesia by insisting a patient could recall his memories. Thus, Freud gained the evidence he needed to support the notion that behavioral therapy was sufficient to manipulate one's memory. Upon returning to Vienna, he decided to force forgotten facts into consciousness through "free association" (Chertok, 1984).

Ultimately, Freud wanted to create a psychotherapy that he could call his own, and he thus relied on his free-association and cathartic techniques to establish psychoanalysis. Although Freud opted out of hypnosis in his own clinical practice, he did continue to use suggestion to treat patients' symptoms. Even after he moved from direct suggestion to concentration-like techniques involving free association, Freud published a case in which he used direct suggestion only (Aron, 1996). Looking back on his transition from hypnosis to psychoanalysis, Freud credits the former for having fostered the latter. He says, "It is not easy to overestimate the importance of the part played by hypnotism in the history of the origins of psychoanalysis. From a theoretical as well as from a therapeutic point of view, psychoanalysis has at its command a legacy which it has inherited from hypnotism" (Freud, 1924 [1923], p. 192).

Shevrin views psychoanalysis as a comprehensive theory of the mind rather than solely a picture of the mind in conflict. He is right, of course; the circumscribed take on conflict was a mere example on our part. We note, however, that some individuals go beyond Shevrin's view and construe psychoanalysis in ways reminiscent of a theological concept or a philosophical lifestyle. Through these more encompassing views, neuroscience and psychoanalysis seem to fall on more common planes, but perhaps with less specificity than one would have desired; overarching definitions of such broad scope often cause concepts to lose their meaning (e.g., Erdelyi, 2006). The broader psychoanalytic constituents, including motivation and expectation, play important roles in suggestion and modulate brain processes. Furthermore, Shevrin and his collaborators have provided both theoretical accounts (Shevrin & Fisher, 1967) and empirical evidence (Snodgrass, Shevrin, & Kopka, 1993) that have helped shed light on the effects that dreams and extremely brief subliminal exposures hold for unconscious processing.

On the danger of using analogies and metaphors: the case of dark energy

When I was a graduate student studying the neural and psychological science of attention, I encountered Michael Posner's earlier idea of a fast-moving attention spotlight that is controlled by a specific brain network. Posner coined his idea of a "spotlight of attention" more as a helpful metaphor—an educational adage—than as a literal analogy; however, some investigators (and many students) were already busy at work characterizing the physical properties of this elusive spotlight. Years later, when Posner was my postdoctoral mentor at the Weill Medical College of Cornell University, he was surprised to learn from me how far his pedagogical metaphor reached and how many researchers who should have known better have taken it at face value. He had not imagined that people would actually design experiments and try to measure the "attention spotlight," which was only an analogy—a higher level of abstraction aimed to guide a cognitive comparison. Analogies and metaphors are dangerous and ubiquitous in the brain sciences (e.g., Malabou, 2008).

Shevrin reaches out to Marcus E. Raichle—a prominent neuroimager from Washington University School of Medicine in St. Louis—and his thought piece in *Science* about "dark energy" (Raichle, 2006), highlighting the massive intrinsic brain activity unaccounted for by responses to external stimuli. Because the brain uses only about 5% of its energy to respond to external stimuli, so goes the argument, the remaining "dark energy of the brain" may fuel intrinsic neuronal activity including mechanisms related to unconscious processes, which are harder to observe via typical brain imaging techniques such as fMRI.

Even if intuitively appealing, this specific analogy is tricky. First, the main limitations of fMRI are unrelated to physics or poor engineering and are unlikely to be resolved by increasing the sophistication and power of present scanners. Instead, they emanate from the circuitry and functional organization of the brain, not to mention inadequate experimental protocols that are often oblivious to this organization. We must bear in mind that in our present state of knowledge, it is difficult to anchor fMRI measurement relative to a true baseline level—something we can actually do with other neuroimaging techniques such as PET—and so the fMRI baseline is largely a moving target. Furthermore, we can rarely differentiate reliably between the fMRI signal of function-specific processing and neuromodulation, between bottom-up and top-down signals, and even between excitation and inhibition. In addition, it is difficult to quantify the magnitude of the fMRI signal to accurately reflect differences between brain regions, or between tasks within the same region. Second, the initial information reaching a cortical region receives context-dependent evaluation, under the influence of strong intra- and cross-regional cortical interactions (Raz, Lamar, Buhle, Kane, & Peterson, 2007). The cortical output reflects ascending input but also cortico-thalamo-cortical pathways (Raz et al., 2009), whereas its responsiveness and signal-to-noise ratio reflect the activity of feedback and, likely, information from the ascending systems of the brainstem. The regulation afforded by these systems—probably the main neural networks to underlie the altered states of higher cognitive brain functions including attention, motivation, and suggestion—is likely to affect large masses of cells and potentially induce larger changes in the fMRI signal than in the original sensory signals themselves. Third, physicists postulate the existence of dark energy to explain the accelerated expansion rate of the universe. Dark energy accounts for an astonishing 75% of the total mass/energy of the universe, and it is this disproportionate distribution of energy that seems to prompt the comparison of dark energy to the energy consumption in the human brain. Whereas in astrophysics the universe's dark energy accounts for missing energy in the mass/energy density of the universe and explains the driving force of the universe's acceleration, in neuroscience we hardly construe "dark energy" in the brain as the energy that is unconsumed by responses to external stimuli and is instead allocated to other neural processes. While dark energy in the universe is a fundamental property of space, we must not reduce this form of energy to regular energy, used for intrinsic processes. In addition, while responses to environmental stimuli are usually visible, intrinsic neuronal signaling is typically invisible. Moreover, the implicit comparison of invisible activities to dark energy implies a reference to dark matter—a concept completely different from dark energy and one that has little to do with neuronal activity.

The unconscious

Regarding Shevrin's discussion of the unconscious, we concur that the whole topic of unconscious processes is relevant to the discussion of theoretical psychoanalysis and related brain processes. We are aware, of course, of important contributions by Shevrin and his mentor Charles Fisher (Shevrin, 2003). We are also aware of other cumulative contributions, including from the labs of Eyal Reingold, Stanislas Dehaene, and the recently retired Phil Merikle. In this regard, the neuroimaging experiments of Lionel Naccache and his collaborators are especially relevant (see Response to Naccache). At the University of Michigan, Shevrin followed through with Fisher's legacy and extended it beyond subliminal explorations of perception, dreams, and fantasies (Shevrin, 2003) into a programmatic investigation of the unconscious (Shevrin, Bond, Brakel, Hertel, & Williams, 1996). Along with individuals such as Linda Brakel and Michael Snodgrass, Shevrin has taken a unique look at unconscious processes by using a mechanical tachistoscope capable of projecting an image for the duration of a single millisecond—most digital tachistoscopes use much longer delays—to elucidate how information that appears too fast to be uptaken consciously can influence behavior. His behavioral and electrophysiological data are intriguing. However, Merikle at the University of Waterloo and later his student, Reingold, at the University of Toronto used slightly different methods and reported somewhat disparate findings (Merikle & Reingold, 1998). Naccache et al. used fMRI to explore unconscious phenomena using innovative cognitive paradigms. Through imaging, it is clear that something is occurring at the brain level, but it is difficult to definitively conclude what that something actually is. Because experimental cognitive neuroscience research on subliminal perception is still in its early infancy and lacks desirable characteristics such as consistency and independent replication, we agree with Shevrin that a cohesive and converging set of findings would substantively advance the field.

Lastly, Shevrin mentions Eric Richard Kandel, a psychiatrist, neuroscientist, and professor of biochemistry and biophysics at the Columbia University College of Physicians and Surgeons, a Senior Investigator in the Howard Hughes Medical Institute, and a recipient of the 2000 Nobel Prize in Physiology/Medicine for his research on the physiological basis of memory storage in neurons. Kandel seems to represent the apotheosis of neuropsychoanalysis: a prominent professional who, on the one hand, has psychoanalytic roots and, on the other hand, pursues a cutting-edge neuroscience career. While Kandel's intellectual interests began with psychoanalysis, moved toward biology, and then back toward questions of psychoanalysis, his stance is a bit uncommon among analysts in that he is a strong reductionist. For Kandel (2009), for example, the distinction between the social and the genetic is a false one because, according to him, any long-term change in the human animal involves a change in gene expression, and short-term changes impact brain function. Thus, changes in neural anatomy occur through long-term training via synaptic changes. Moreover, Kandel posits that long-term change in the form of psychotherapy might constitute persistent functional and anatomical changes in the brain. Kandel's ideas are largely at odds with the mainstream approach common in psychodynamic circles in that he challenges the premise that social or cultural factors influence psychiatric disorders; to him there is no distinction between culture and biology—one is a coconstruct of the other—because all phenomena, including social and cultural influences, work through the brain. The latter is probably true, but archreductionism holds a potential conceptual problem in that it may be overly dismissive of factors less amenable to reductionist investigation. After all, most psychiatric diseases comprise multilevel systemic issues, and we have uncovered but a mite of the neurobiology and biochemistry underlying mental processes. Even if we had considerably more advanced physiological understanding of brain function, our behavior still thrives on interactions with people, institutions, and other elements that are outside the brain. Everything is biological, certainly, but everything is also social. We should probably pay as much attention to a new social phenomenon as we do to a new molecule.

RESPONSE TO TOREM
Suggestion undefined: on the limitations of sensitive definitions

Is suggestion a form of a placebo response/effect? (Raz, 2007a, 2007b; Raz, Raikhel, & Anbar, 2008). Beyond showing that antidepressants are comparable to placebos in many contexts within depression (Kirsch, 2010; Kirsch et al., 2008), recent studies report that the efficacy of antidepressants in the treatment of depression is due more to patients' experience of their relationship with the prescriber than to the chemical properties of the drug (Ankarberg & Falkenstrom, 2009). These findings underscore the large role that suggestion seems to play in any therapeutic setting, including psychoanalysis.

Suggestion has been defined by multiple scholars (Raz, 2007b). The quest for a universal definition of suggestion has deliquesced into some half-dozen definitions (Harrington, 2008). This trend has been a common occurrence since the beginning of systematic research in the field (Hull, 1933). Hypnosis, for example, has been defined operationally by the administration of a hypnotic induction, whereas hypnotizability has been operationally defined as responsiveness to suggestion following a hypnotic induction. The conundrum centers on findings indicating that the induction of hypnosis has little impact on responsiveness to suggestion and that hypnotizability scales, therefore, probably measure the effects of suggestion, not the effects of hypnosis (cf. Barber & Glass, 1962; Braffman & Kirsch, 1999, 2001; Caster & Baker, 1932; Glass & Barber, 1961; Weitzenhoffer & Sjoberg, 1961; Williams, 1930).

The upshot of these multiple reports proposes a high correlation between hypnotic and waking suggestibility.

Definitions can be evasive, and in the field of suggestion we find a clear dissociation between the operational definitions of hypnosis and hypnotizability in the experimental literature. The problem, however, is hardly novel: Andre Weitzenhoffer had broached it some 30 years ago (Weitzenhoffer, 1980), and although Irving Kirsch rekindled the flame almost two decades later (Kirsch, 1997), this issue has been largely dormant (Kirsch et al., 2006). As a seasoned psychiatrist with clinical expertise and an active teaching career in hypnosis, Moshe Torem must recognize that hypnosis too lacks a precise definition; however, this shortcoming hardly prevents a thriving scientific and clinical discussion.

Weitzehnhoffer's definition describes suggestion as a planned verbalization or nonverbal signal and assumes that the participant experiences the response as involuntary. Some scholars diverge from Weizenhoffer's definition, arguing that suggestion may or may not be the result of intended communication. This phenomenon of "unplanned" suggestion is congruent with the notion that symptoms can be the product of one's autosuggestions (Freud, 1888). Overt suggestion can be as powerful as its covert variations and has been shown to exert some robust effects in lab experiments (e.g. Raz, Kirsch, Pollard, & Nitkin-Kaner, 2006; Raz, Moreno-Iniguez, Martin, & Zhu, 2007; Raz, Shapiro, Fan, & Posner, 2002; Raz et al., 2003). And yet, comparing hypnotic and nonhypnotic suggestions, individuals may be more easily influenced by suggestion under hypnosis, but suggestion can also occur without any formal induction (Raz et al., 2006).

Placebos and the *DSM-V*

On occasion, even key terms are difficult to define, especially in the behavioral sciences. Take placebos, for example, a concept intimately connected with suggestion. What is the right definition for a placebo? Consider the following common options (Harrington, 2006):

a. A short-term and illusory impression of improved health that some patients experience when they take an inert substance that looks like real medicine (e.g., a sugar pill).
b. The nonspecific effects of medical treatment that, in clinical trials, must be controlled in order for researchers to assess the specific effects of new interventions, especially drugs.
c. A powerful mind–body phenomenon with a specific

"real" biology all its own, one that medicine should study and exploit.

While the latter is the definition that intrigues our generation the most, the first two are hardly meaningless: Option (b) is still the working definition of the placebo effect in medicine, pharmacology, and therapeutics; Option (a) is the prevailing definition in bioethical discussions about placebo use, as well as in explanations as to why biochemically ineffective treatments, such as homeopathy, might appeal to a vast clientele.

As another case in point, consider the draft edition of the *DSM-V*. The *Diagnostic and Statistical Manual of Mental Disorders* (*DSM-IV*; APA, 1994) is psychiatry's only official guide for distinguishing between "healthy" and "unhealthy," even though this determination is often tenuous and evanescent. Small changes in the definition of mental disorders can instigate vast, sweeping repercussions. On the one hand, a definition too permissive may mislabel healthy people—who may be better off without coming into contact with the mental health system—as "patients" and consequently offer them unnecessary, expensive, and even harmful treatments. Such permissive definitions are also dangerous because they extend the reach of psychiatry deeper into the rapidly diminishing purview of the mentally healthy. On the other hand, a definition too narrow may prevent some mentally ill individuals from getting the help they need. In addition, the *DSM* definition influences many parameters, including insurance coverage, eligibility for disability and services, and legal status, not to mention social stigma and a sense of personal control and responsibility.

The current *DSM-IV*, which came out in 1994, has been accused of popularizing disorders such as attention deficit disorder (ADD), autism, and childhood bipolar disorder allegedly because the *DSM-IV* panel of experts issued definitions that were not sufficiently judicious. In February 2010, the American Psychiatric Association published online the first draft of the next edition, *DSM-V* (http://www.dsm5.org). Allen Frances—professor emeritus and former chairman of the department of psychiatry at Duke University, and chairman of the task force that put together the current *DSM-IV*—read it and had a few concerns (Frances, 2010).

The draft of the *DSM-V* includes expansive definitions, such as the classification of "Binge eating disorder" as having one eating binge per week for three months (i.e., more than 6% of the population would qualify); "Minor neurocognitive disorder" as applying to no more than the expected memory problems of aging; "Major depression" as inclusive of the grieving after the loss of a loved one; and "Mixed anxiety depression" as indistinguishable from the commonplace pains of everyday life. In addition, ADD would become more prevalent in adults, encouraging the already ubiquitous use of stimulants for performance enhancement; "Psychosis risk syndrome" would use the presence of strange thinking to predict who would later have a full-blown psychotic episode, etc. With such liberal diagnostic definitions, many misidentified patients would receive medications that can cause enormous side effects and shortened longevity. Thus, definitions can sometimes be a societal issue that transcends psychiatry or behavioral science, and Allen Frances urges that the greater public interest be part of the risk–benefit analyses. Definitions for terms such as suggestion, hypnosis, and placebo would have to take this discussion into account. Alas, few efforts at definitions have attempted to follow this reasoning (e.g. Kirsch, 1997; Raz, 2007b).

AN OVERARCHING ARGUMENT IN FAVOR OF (AND AGAINST) BRAIN IMAGING: AN EXPERIMENTAL SELF-RESPONSE

As a member of the cognitive neuroscience community who actively uses neuroimaging in research, I maintain a critical eye on certain aspects of our discipline, including the tools of our trade. On the one hand, fMRI seems a formidable tool to unlock the mental and cognitive workings of the brain and has revolutionized the field. On the other hand, considerable controversy shrouds many technical aspects of this neuroimaging technique, and fierce exchanges appear regularly in the professional psychological and neuroscience journals. For example, a recent issue of *Perspectives on Psychological Science* (Vol. 4, No. 3, 2009) features one of the most spirited exchanges concerning statistical issues at the heart of most functional neuroimaging studies (see also http://www.stat.columbia.edu/~martin/Workshop/ECWorkshop.html).

At the center of the debate is a piece reporting that brain–personality correlations in many studies concerned with social neuroscience and related fields are "implausibly high," "likely . . . spurious," and "should not be believed" (Vul, Harris, Winkielman, & Pashler, 2009; cf. Vul & Kanwisher, in press). Vul et al. (2009) originally titled their piece, "Voodoo Correlations in Social Neuroscience" and circulated it widely in the scientific community and in the popular press prior to publication. This dissemination effectively short-circuited the peer-commentary process, giving the article a window of time without published criticism or opposition. The word voodoo, as applied to science, carries a strong and specific connotation of fraudulence (e.g., Park, 2002). Although Vul et al. changed the title and removed the word "voodoo" at the suggestion of the editor, the tenor of their piece remained a scathing view of what is loosely referred to as "social neuroscience." In addition to a vast array of responses within the same issue, a distinctive follow-up paper echoed the same fundamental arguments as Vul et al., but in a more generalized and effective form (Kriegeskorte, Simmons, Bellgowan, & Baker, 2009). It seems that we will need to replicate or at least reanalyze a large number of papers to avoid circular analysis problems. Furthermore, aside from Kriegeskorte et al., several refinements and recommendations for improvement are now available for the design and analysis of fMRI studies (e.g., Bennett, Wolford, & Miller, 2009; Lieberman & Cunningham, 2009; Poldrack & Mumford, 2009).

Many readers take these arguments as grist for the antineuroimaging mill, citing the results as evidence that cognitive neuroscience is fundamentally a shady science (cf. Cureton, 1950). I beg to disagree. Functional neuroimaging is still one of our best options to investigate the living human brain in a noninvasive fashion. Science is self-correcting, however, and these collective findings represent an inexorable natural evolution of a relatively young field. Vul et al. (2009) ruffled a few feathers, and views on how to proceed are still in flux, but when the dust settles it is going to be more difficult than it had been to make nonindependence errors and publish the results in peer-reviewed journals. That's progress, scientific progress.

Lie detection

Alarmingly, in 2009 one of India's courts found two people guilty of murder partly because of brain measurements. Accused of killing her former fiancé, 24-year-old Ms. Aditi Sharma, who had meanwhile married Mr. Pravin Khandelwal, met her ex-lover at a McDonald's where she allegedly laced his food with arsenic. After her arrest, investigators offered Ms. Sharma a neuroelectrophysiogical test to support her innocence. Upon her consent, they placed 32 electrodes on her scalp before reading the allegations to her in the first person: "I bought arsenic;" "I met my former fiancé at McDonald's," along with control statements such as "The sky is blue." Seemingly, Ms. Sharma failed the test: prosecutors claimed that her brain lit up in areas that are allegedly in line with lying. At the end of the trial, both she and her husband received a sentence of life imprisonment.

If this "sub-fMRI," electrode technology sounds obsolete, for the past several years companies have been offering fMRI-based brain scans to those wishing to prove their innocence (e.g., http://www.noliemri.com). At the moment, however, neuroimaging is a far cry from a full-proof "brain polygraph." Moreover, even the actual polygraph has hardly lived up to its original reputation of a liar-proof machine. Instead, it measures one's physiological responses to stress, such as increases in blood pressure or heart rate; thus it can produce a guilty reading on innocent suspects who are merely nervous, and it is vulnerable to well-prepared liars who can control their emotions. Although brain scanning is probably more humane than waterboarding, fMRI is neither useless phrenology nor is it—and I suspect it unlikely to mature into—a mind-reading technique. Fortuitously, earlier this year the Bombay high court granted Sharma and Khandelwal bail, citing a lack of material evidence to link them to the crime.

Conclusion

Despite its current substantive shortcomings, I hope that I have been able to adequately express why I hold neuroimaging in high regard. As Robert Michels points out in his commentary: let's assume that we have cognitive neuroscientists who really know how to conduct neuroimaging experiments properly, then what would this do for psychoanalysis? I am happy to answer this question as a cognitive neuroscientist writing for academic psychoanalysts, but I represent only myself and am hardly the plenipotentiary of the neuroscience community.

Neuroimaging is likely to remain a powerful tool—one of many—at our experimental disposal. Experiments give us a good model of specific phenomena (e.g., how memories might be stored), but this information is often less useful when it comes to broader questions (e.g., "How can I improve my memory?"). Neuroscience typically focuses on the former, whereas psychoanalysis usually targets the latter. While it may be possible to traverse a trajectory from a single neuron and its subprocesses all the way to an emergent property of an inordinately vast network of brain cells, we are really only at the absolute beginning of this incipient investigation. An ontological unity of the world suggests that if we convened all the social scientists, neuroscientists, etc., we could get an overarching explanation of how the mind works; however, at the moment, neuroscientists and psychoanalysts have little in common, even when they use the same terms. Thus we are dealing with immense epistemological diversities. Convergence may be intractable at the present time but perhaps possible in the future.

People, including scientists, often ask unscientific questions: Do you believe that hypnotic suggestion can reduce pain? Do you suppose that antidepressants can help depression? Pristine scientists, however, do not *believe* or *suppose*. Instead, they look at the data and ask whether the evidence supports the hypothesis. At least in theory, researchers' beliefs should be immaterial to the results of their experiments because science is about empirical evidence. In reality, however, an experimenter's belief may introduce a substantive bias to the interpretation of data, and sometimes even to more nuanced aspects. For example, beliefs and attitudes may bias participant recruitment and influence their expectations, affect feedback, and even subtly permeate data collection and analysis. When it comes to Freud, people usually harbor strong beliefs. I asked many a colleague from both sides of the divide, "What kind of data would make you change your mind?" While several associates danced around the answer with grace and elegance, most coy responses amounted to one troublesome sentiment: "None."

Neuropsychoanalysts must exercise prudence and desist from attempts to rehabilitate the psychoanalytic doctrine by exaggerating its compatibility with the findings of cognitive neuroscience. Profound as they may be, Freud's writings are neither the holy writ nor the apotheosis of behavioral science. They are voluminous, however, and undisciplined citing of any portion of them may support many a point (cf. Raz, in press). In science—biological, psychological, or social—we follow the findings. Evidence for repressed memory, for example, is illusory, and in the absence of scientific evidence, most cognitive scientists continue to challenge the validity of the concept of repression (see response to Shapiro). Science thrives on converging independent replications of rigorous empirical evidence, not on doctrinaire viewpoints. If compelling evidence were to materialize, scientists should be willing to change their minds. At the same time, proponents of specific claims—Freudian or otherwise—should provide compelling evidence, and everyone should be sufficiently critical to dismiss claims that already have been found to be specious (e.g., the long-discredited account of Freud's "seduction theory"). Science provides an evanescent form of truth. We never get there, but we can judge how close we are. One test that we can perform requires the convergence of evidence over multiple researchers, methods, labs, and periods. We should probably apply the same time-honored scientific principle to the study of neuropsychoanalysis.

POSTSCRIPT

While proofreading the Target Article and stand-alone responses to the peer commentaries, I came across a new paper coauthored by one of the leaders in imaging neuroscience and theoretical neurobiology (Carhart-Harris & Friston, 2010). This recent article reports how findings from brain imaging elucidate the idea that Freudian constructs may have neurobiological substrates. It proposes that Freud's descriptions of the primary and secondary processes are consistent with self-organized activity in hierarchical cortical systems and outlines how the concept of the ego seems consistent with the functions of the brain's default mode and its reciprocal exchanges with subordinate brain systems. In a nutshell, the report submits that Freud's metapsychological theory of mind fits well with a systematic view of how the brain works. In this regard, it echoes the tenor of Howard Shevrin's peer commentary—that is, calling for greater attention not to the clinical aspect of psychoanalysis but to its comprehensive theory of mind. In addition, Carhart-Harris & Friston seemingly show how contemporary neuroscience is moving toward a comprehensive theory of brain function—a theme I explored in my Target Article.

Ontologically, Carhart-Harris & Friston walk a tenuous line. For example, they provide a reading of Freud's psychology that emphasizes the primary–secondary process as a category distinction, whereas most modern thinkers would likely view it as a continuum; they conflate Freud's original concept of energy with notions having to do with statistical physics and information theory; they blur the "unconscious" with the "id"—id is unconscious, but so is the "super ego," and early memories, according to Freud—and the list goes on. At least in this ontological regard, their account is a bit weak.

Leaving aside my substantial quibbling with the neurobiology, the paper proposes ways to bridge the gap between the two models proposed. The goal, however, should hardly be bridging the lacuna; rather—to paraphrase Robert Michels—the critical question is whether either model provides a feature or a prediction that would trump the other model. What we really need is for research to elucidate whether such new models can extend our understanding and offer a critical test to support a proposed theory. Alas, this goal remains elusive.

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