What Is Neuropsychoanalysis?

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This article serves to briefly survey the relationship between neuroscience and psychoanalysis ("neuropsychoanalysis") and, at the same time, to address some of the criticisms that the field has encountered. First, the article reviews the historical foundations of neuropsychoanalysis, including both theoretical and technical questions of whether an interdiscipline is appropriate. Second, the article reviews the philosophical foundations of the field, including the position of dual-aspect monism. Third, the article examines the scientific foundations of the field, with a discussion of whether analytic work with neurological patients represents an optimal point of contact between the disciplines. Finally, the article engages with the issue of what neuropsychoanalysis is not, covering issues such as "speculation versus empirical research,"and the question of whether neuropsychoanalysis represents a new "school" within psychoanalysis.

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The first formal use of the term "neuropsychoanalysis" occurred in 1999, when it was introduced as the title of this journal. Plainly, however, the relationship between psychoanalysis and neuroscience is much older than the term. In the dozen years since the word "neuropsychoanalysis" was first used, it has been employed in a number of different ways, for different purposes, by different people.[1] This article briefly surveys some of this complexity and, in the process, sketches the intended scope of the field. In doing so, it will also address some of the criticisms that the field has encountered in the decade or so since its foundation.

There are two major limitations to this account. The first is that we can speak only for ourselves and thus describe what *we* think "neuropsychoanalysis" is—and ought to be. Nevertheless, we may claim a certain privilege in that respect, by virtue of one of us having invented the term. Second, we aim to speak only of the absolute basics of the discipline, to address only the foundational issues.

[1] There was a time when "depth neuropsychology" was the term used for the new interdiscipline (Kaplan-Solms & Solms, 2000, Turnbull & Solms, 2003), with reference to Freud's "depth psychology" (Freud, 1915).

We address the question "what is neuropsychoanalysis?" under four headings:

1. Historical foundations of neuropsychoanalysis.
2. Philosophical foundations of neuropsychoanalysis.
3. Scientific foundations of neuropsychoanalysis.
4. What neuropsychoanalysis is *not*.

Historical foundations of neuropsychoanalysis

When we speak of the historical foundations of neuropsychoanalysis, we must of course begin with Freud. In doing so, we are also addressing the question as to whether or not *neuro*psychoanalysis is really a legitimate part of psychoanalysis. The alternative view is that it is somehow a foreign body in our midst, or a deviation, or perhaps even something fundamentally *anti*-psychoanalytic.

In relation to this question, Freud's attitude to the issue is of paramount importance. If neuropsychoanalysis is legitimately part of what Freud conceived psychoanalysis to be, it places the interdiscipline of

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neuropsychoanalysis in a strong position with respect to this "parent" discipline. It was Freud, after all, who invented psychoanalysis. Happily, therefore, Freud's view on the matter was very clear and also consistent throughout his life. Freud was, of course, a neuroscientist and a neurologist for the first two decades of his professional life (Solms, 2002; Solms & Saling, 1986; Sulloway, 1979). Throughout his later psychoanalytic work, he had a specific scientific program in mind, largely continuous with his earlier neuroscientific work, albeit shaped by the limitations of the scientific methods and techniques available to him at that time (for more on this topic, see Solms, 1998; Solms & Saling, 1986; Turnbull, 2001).

Freud's program was to map the structure and functions of the human mind and naturally he recognized that these were intimately related to the structure and functions of the human brain. However, as regards the mapping of these relationships, he consistently argued that the brain sciences of his time did not have the *tools*, in both conceptual and technical terms, necessary for exploring these relationships. He therefore shifted to a purely psychological method—a shift that he reluctantly saw as a necessary expedient. Just a few quotations illustrate this position:

> We must recollect that our provisional ideas in psychology will presumably some day be based on an organic substructure. . . . We are taking this probability into account in replacing the special chemical substances by special psychical forces. [Freud, 1914, pp. 78–79]

> The deficiencies in our description would probably vanish if we were already in a position to replace the psychological terms by physiological or chemical ones. [Freud, 1920, p. 60]

> Biology is truly a land of unlimited possibilities. We may expect it to give us the most surprising information and we cannot guess what answers it will return in a few dozen years to the questions we have put to it. [Freud, 1920, p. 60).

There are many such statements throughout Freud's work. All reveal, firstly, that he viewed the separation of psychoanalysis from neuroscience as a *pragmatic* decision. Secondly, he was always at pains to clarify that progress in neuroscience would have the inevitable result that *at some time in the future* the neurosciences would advance sufficiently to make the gap bridgeable. As one of the quotes above suggests, his rough estimate was that this might happen in a "few dozen years." That was in 1920.

What were the methodological limitations that Freud encountered at that time? The main neuroscientific tool then available was the clinico-anatomical method, based on the clinical investigation of patients who had suffered focal brain lesions (Finger, 1994)—that is to say, studying how different functions of the mind were altered by damage to different parts of the brain. It was effectively the *only* method available for studying mind–brain relationships (though Freud's later years did briefly overlap with early developments in neurochemistry; see Finger, 1994).[2] However, Freud regarded the clinico-anatomical method as unsuitable for his purposes, despite having used it himself in his preanalytic work. Best known is his *On Aphasia* (1891), which demonstrates how sophisticated was his mastery of that method, and of its limitations (for a modern appreciation of Freud's early neuropsychological investigations, see Shallice, 1988, pp. 245–247).

In that 1891 book on the aphasias and in the papers that he published soon after (Solms, 2001), Freud rejected the clinico-anatomical method, as he made the transition into psychoanalysis. He did so for several reasons. First, he recognized that the mind is a dynamic entity. It was Freud's emphatic view, even as a neurologist (Freud, 1891), that the mind was not made up of static modules or boxes connected up by arrows. Instead, Freud saw the mind as comprising dynamic, fluid processes. Second, Freud observed that the mind consisted of far more than consciousness; there was, beneath consciousness, a vast substructure, the workings of which had to be explored and understood before we would ever be able to make sense of the volitional brain.

The aim of psychoanalysis then became to develop a method, and ultimately to derive from that method a theory (and a therapy), that would enable science to explore and understand the dynamic nature and unconscious structure of the mind. It is widely known that Freud then proceeded to use this purely clinical method, free from neuroscientific constraints, from 1895 or thereabouts, until 1939. This pioneering work left us a vast legacy, including a series of theoretical models of the basic organization of the mind, which we now refer to as "metapsychology."

Some psychoanalysts, misreading Freud, argue that the theoretical work of psychoanalysis must continue to remain aloof from neuroscience forever. We must avoid using neuroscientific methods, no matter how far these advance, and must cling to our exclusively clinical, psychological approach. These are authors

[2] "The future may teach us to exercise a direct influence, by means of particular chemical substances, on the amount of energy and their distribution in the mental apparatus." (Freud, 1939, p. 182). Freud 1939 not in Refs

who question "whether the study of [neuroscience] contributes in any way to the understanding or development of psychoanalysis as theory or practice . . . whether neuroscience is of value to psychoanalysis *per se*" (Blass & Carmeli, 2007, p. 34; for a similar opinion, see Karlsson, 2010, pp. 40–64). The proponents of this view appear (fortunately, in our opinion) to form a diminishing minority,[3] but we must acknowledge that there are still some colleagues who believe that psychoanalysis has nothing to learn from neuroscience *in principle*. (Oddly, however, they do seem to think that neuroscience has something to learn from psychoanalysis!)

Independently of this theoretical—or ideological—question, there remains the *technical* question as to whether neuroscience has developed sufficiently as a discipline to allow it to make an adequate contribution to psychoanalytic theory: whether the methodological limitations (and related limitations of neuropsychological knowledge that Freud referred to) still remain. Stepping back, it is clear that there have been huge technical and methodological advances in the neurosciences over the last several decades. To offer but the briefest historical summary:

Electroencephalography (EEG) was introduced around the 1930s (Berger, 1929), though it was not fully exploited until after the war. This represented the beginning of a capability, initially rather crude, to measure and observe dynamic aspects of brain activity under changing functional conditions. The later development of event-related potentials (ERPs) in the 1960s (Sutton, Braren, Zubin, & John, 1965; Sutton, Tueting, Zubin, & John, 1967; Walter, Cooper, Aldridge, McCallum, & Winter, 1964; for a recent review, see Luck, 2005) offered substantial advances over the basic EEG technique, by virtue of experimental control and averaging procedures. The recent development of magnetoencephalography (MEG) represents a further substantial advance, allowing us to study the neural dynamics associated with mental events at the millisecond level, with increasing anatomical precision.

In another domain, after the Second World War, there were tremendous developments in neuropsychology, using the lesion method in a new way that adapted its inherent limitations to the dynamic nature of the mind. Alexander Luria, in particular, developed a method known as "dynamic localization" (Luria, 1966, 1973; see Kaplan-Solms & Solms, 2000, pp. 39–34; Solms & Turnbull, 2002, pp. 64–66). This method permitted the investigator to identify constellations of brain structures that interact to form functional systems, where each structure contributes an elementary component function to the complex psychological whole. On this basis, modern neuropsychology has a well-developed understanding of most of the basic mental functions. This applies especially to cognitive functions.

Further enormous technical advances followed the advent of computerized tomography in the 1970s, which made it possible to identify the precise location of a brain lesion while the patient was still alive. This was followed by magnetic resonance imaging (MRI). And from the 1990s onward, functional neuroimaging (functional magnetic resonance imaging, fMRI; position emission tomography, PET; and single-photon emission computed tomography, SPECT) made it possible to *directly observe* neurodynamic processes under changing psychological conditions.

It is now also possible to deliver temporary, short-acting "lesions" to neurologically intact research participants—either through sodium amytal injection (which was first introduced in the 1940s) or through magnetic pulses delivered to the outside of the skull via transcranial magnetic stimulation (TMS; which has been readily available since the 1990s). Innumerable other technologies also exist, ranging from stimulation of the cortical surface in neurosurgical operations (Penfield & Boldrey, 1937; Penfield & Rasmussen, 1950), to deep-brain stimulation (Mayberg et al., 2005), through to psychopharmacological probes (Ostow, 1962, 1980), to mention only the most obvious examples.

Even this brief summary demonstrates that we *do* now have neuroscientific methods that enable us to study the dynamic nature of the mind and to identify the neural organization of its unconscious substructure. Each of these methods has its limitations, as all methods do, and there are undoubtedly many future advances to come—but the landscape of scientific enquiry in this domain has, certainly, *radically* changed since Freud's lifetime. For this reason, it seems entirely appropriate to reconsider whether we might now attempt to map the neurological basis of what we have learnt in psychoanalysis about the structure and functions of the mind, using neuroscientific methods available to us today. Freud would, in our opinion, have considered this a welcome and wholly legitimate development of the work that he pioneered, and there has been something of an explosion in the number of books addressing this issue (e.g., Bazan, 2007; Bernstein, 2011; Corrigall & Wilkinson, 2003; Cozolino, 2002; Doidge, 2008; Fotopoulou, Pfaff, & Conway in press; Kaplan-Solms & Solms, 2000; Mancia, 2006; Northoff, 2011; Peled, 2008; Shevrin, Bond, Brakel, Hertel, & Williams, 1996; Solms & Turnbull, 2002,)

still in press?

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[3] English-Speaking Conference debate, British Psychoanalytical Society, London, 2008.

Philosophical foundations of neuropsychoanalysis

If we are to correlate our psychoanalytic models of the mind with what we know about the structure and functions of the brain, we are immediately confronted with the philosophical problem of how mind and brain relate—that is, with the "mind–body problem." This opens huge philosophical questions. Are we *reducing* the mind to the brain, are we *explaining away* the mind, or are we merely correlating mind and brain? And if we are merely correlating them, what is the causal basis of this apparently compulsory correlation? Is the relationship hierarchical, whereby psychoanalysis studies mere epiphenomena of the brain? Or is the mind *an emergent property* of the brain? (Chalmers, 1995, 1996; Churchland, 1986; Searle, 1980; see also, for a basic review of these issues, Solms, 1997a; Solms & Turnbull, 2002, pp. 45–66).

It is, of course, terribly important in this field to be clear about one's conceptualization of the relationship between the mind and brain. We favor a conceptualization (shared by Freud) that we think neuropsychoanalysis as a whole may be based upon. This approach is conventionally called "dual-aspect monism" (see Solms, 1997a; Solms & Turnbull, 2002, pp. 56–58).

Freud says—very clearly in many places—that the actual nature of the mind is unconscious (for a review, see Solms, 1997a). He uses the phrase "the mind *in itself*," referring directly to the philosophy of Kant. For Kant, our subjective being, the thing we perceive when we look inwards, is not the mind *in itself*: the mind in itself cannot be perceived directly. We can only know the mind via our phenomenal consciousness, which provides an indirect and incomplete *representation* of the mental apparatus and its workings. The actual ontological nature of the mind is something epistemologically unknowable: it necessarily lies behind (and generates) conscious perception. We can, of course, *infer* its nature from our conscious observations and thereby "push back" the bounds of consciousness, which is what the psychoanalytic method seeks to do. Ultimately, however, we can never *directly* know the mind itself. We must therefore have recourse to abstractions derived from inferences and built into figurative models: metapsychology.

Similar epistemological limitations hold for the theoretical abstractions of other branches of psychology—to the extent that they too attempt to describe the inner workings of (any aspect of) the mind—even highly developed theories such as, for example, dual-route reading models (Coltheart, Curtis, Atkins, & Haller, 1993), models of multiple memory systems (Schacter, 1996; Schacter, Norman, & Koutstaal, 1998), models of divergent visual systems engaged in perception and action (Milner & Goodale, 1993), and so on. *All* of psychology is ultimately just model-building of one sort or another. It is only the scale of Freud's metapsychology that distinguishes it in this respect, from the more narrowly focused models of cognitive psychology and neuroscience. It is also (partly) for this reason that the metapsychology lacks some of the specificity of modern cognitive models. But that has no bearing on their ultimate epistemological limitations.

Freud argued not only that the mind is epistemologically unknowable, but also that it is ontologically no different from the rest of nature. Kant's view was that *everything* in the world as we know it, including the contents of our external awareness, is only an indirect representation of reality. What all scientists do is probe beyond this perceptual data to try to get a better picture of what Freud called "the real state of affairs" (1939, p. 196). This approach, we note, is common to *all* the natural sciences—often with the use of artificial perceptual aides such as microscopes and telescopes and spectroscopy machines. They are ultimately all reduced to building *models* of our natural universe, and, in this way, the mind in itself exists on the same ontological plane as the rest of nature; it is just one of the things that we perceive.

It is unquestionably significant that evolutionary selective pressures advantage organisms that develop better—that is, more accurate—models of reality. In a world without vision, the first animals to evolve organs of sight would be highly advantaged. Those that develop *better* vision—for example, binocular viewing capabilities, a lens with adjustable focus, low light detection capacities for twilight conditions, etc.—are further advantaged (for a readable account of the process, see Dawkins, 1998). And so much more are those organisms that develop multiple sensory organs, each probing and sampling (and ultimately *representing*) a different aspect of the world around them. Considered across evolutionary time, organisms have, on this basis, developed successively better perceptually derived models of reality. Thus, the human mental apparatus (if functioning normally) delivers remarkably effective capabilities for perceptually guided locomotion, action, navigation, attentional selection, object identification, and object recognition. However, the fact that the perceptual systems offer only *representations* of the world can readily be demonstrated by the remarkable errors seen in visual illusions, as well as in psychotic hallucinations and dreams.

Freud argued that the model-building of physics is no different in principle to what we do in psychoanaly-

sis—we begin with perceptions of our inner state, and then we make inferences about the true nature of the things that determine those perceptions. Our phenomenal consciousness gives us the *impression* that things are (from an external perspective) visual or auditory, or that things make us (from the internal perspective) sad or hungry, but these things are all merely *qualities* of consciousness. Our science, like all others, then strives to abstract "the real state of affairs" that lies behind them. Freud formalized all of this in his conception to the effect that consciousness has both internal and external "perceptual surfaces" (Solms, 1997a; Solms & Turnbull, 2002, pp. 18–31). The difference between psychoanalysis and the *physical* sciences is (on this account) merely the perceptual surface that we use.

Behind *both* of the perceptual surfaces lies something else ("reality itself"), which we can only build abstract models of. Forming better models of reality itself forms the goals of all science, including psychoanalytic science. This may surprise those who have forgotten the origins of psychoanalysis: for Freud, his discipline was *always* a natural science, identical *in principle* with the other basic sciences, of physical reality, such as physics and chemistry. The mind in itself, then, is not ontologically different from—and not distinct from—the rest of the universe.

In sum, Freud was a monist, from 1900 all the way through to 1939. But his philosophical position can perhaps best be described as that of a dual-aspect monist (Solms & Turnbull, 2002, pp.56–58), and so he was also a follower of Spinoza (cf. Damasio, 2004). Indeed, in Freud's correspondence he speaks highly of Spinoza (for an accessible survey, see Damasio, 2004, p. 260), while in his published work he regularly described his position in Kantian terms (see Solms, 1997a, pp. 687–689).

If the mind, in itself, is unknowable, and we can only describe it with abstract models, such as Freud's model of the "mental apparatus," then we must take full advantage of the fact that our mental apparatus can be perceived in *two different ways*. If we look at it with our eyes (via the external perceptual surface), we see a *brain*: wet, gelatinous, lobular, and embedded within the other tissues of the body. If we observe it with our internally directed perceptual surface, introspectively, we observe mental states such as thirst and pleasure.

If we accept this philosophical approach, it follows naturally that we would want to make use of *both* points of view on our object of study, as perceived externally and internally. Why would we want to exclude, *a priori*, a full half of what we can learn about the part of nature that we are studying? In psychoanalysis, we adopt the viewpoint of subjectivity, because there are things that one can learn about the nature of the mental apparatus from this perspective, things that one can *never* see with one's eyes, no matter how much you aid them with scientific instruments. The philosophical position taken by some other scientists (see Solms, 1997a, for the opinions of Crick, Dennett and Edelman, for example) *do* exclude this subjective perspective. Nevertheless, feelings exist, they are no less real than sights and sounds, and they represent a fundamental part of the mind, and they can teach us a great deal about how it works. To exclude them *tout court* is actually crazy.

The information we can glean with our external sense organs, by studying the mental apparatus in its physical realization (the brain)—is, of course, no less important. From a scientific point of view, there are actually a great many advantages that attach to studying physical objects. Some of our psychoanalytic colleagues (e.g., Blass & Carmeli, 2007; Karlsson, 2010) hold a contrary, exclusionary, position that we struggle to understand—not least because it seems irrational to deny oneself any source of useful data. Moreover, we should remind ourselves that the singular, fleeting, and fugitive nature of conscious states bestows distinct disadvantages; the more stable properties of the physical brain are more amenable to the requirements of the scientific method. Nevertheless, we reiterate that if one *correlates* the subjective experiences with the "wetware" of neurobiology, we are in a much stronger position to develop an accurate model of the mental apparatus itself. Thus, as with the moral of the blind men and the elephant, viewpoint-dependent errors are minimized. In sum, neuroscience offers a second perspective on the unknowable "thing" that we call the mental apparatus, the thing that Freud attempted to describe for the first time in his metapsychology.

Naturally, some in psychoanalysis have become anxious about how they might need to change their theories, and perhaps even their practice, by virtue of advances in knowledge that flow from such neuropsychoanalytic correlations.

Paradoxically however, for us personally, the interest has always been more in the *opposite* direction. In our early careers as neuroscientists, we became frustrated with how little we were able to learn about the essential nature of the mental apparatus and the lived life of the mind, with the cognitive neuroscience methods and theories that were available to us when we first trained (in the early 1980s). At that time (thankfully long past), neuroscience appeared to be blind to the fact that the brain was also a sentient being, capable of experiencing itself, with emotional feelings, volitional will, and a spontaneous sense of agency. The fact that

these brain "mechanisms" are endogenously driven and motivated, that they arise out of the embodied nature of the subject, substantially affects the way the apparatus operates. These are not (we feel) epiphenomena, or details, or nice-to-haves—they are fundamental characteristics of how they brain works; they are what distinguishes the brain from the lung.

Scientific foundations of neuropsychoanalysis

The empirical basis for the discipline naturally flows from the facts described in the first section of this paper, from the fact that Freud lacked confidence that the neuroscience of his time was capable of responding to the questions that psychoanalysis was putting to it.

We have argued (e.g., Solms & Turnbull, 2002, pp. 294–295) that subjective data are not, in themselves, an especially solid foundation on which to build a robust scientific discipline—given the fleeting and transient properties of subjective experience, given the fact that (by definition) such phenomena can only be indirectly reported by a single observer, and finally given that many aspects of mental life take place beyond subjective awareness. Surprisingly, even this apparently self-evident assertion has been challenged by our critics, who argue, for example that: "It would appear, according to Solms and Turnbull (2002, p. 46), that we have better access to atoms, molecules, quarks—that is, the non-perceivable perception of the world—than to our own subjective perceptual experiences" (Karlsson, 2010, p. 54). We concede that some aspects of nuclear physics and/or quantum mechanics may be difficult for the casual observer to grasp, but the combined efforts of the scientific community, utilizing multiple technical methods, together with the advantages of exteroceptive observation and the possibilities of replicable experiments, mean that physicists have achieved a remarkable degree of precision in their understanding of the world—evidenced by mathematical formulae that predict physical events and measure physical properties (size, mass, electrical charge, etc.) with great accuracy. There is no aspect of subjective mental life that *by itself* can begin to yield this level of precision.

Have there been advances in the neurosciences that might propel mental science toward the increased levels of understanding that is the goal of all science? The mind–brain problem is in some ways a more complex challenge than the problems that physics tackles. However, much has changed in the last few dozen years to move neuroscience in a promising direction. First, there have been many technical and methodological advances in neuroscience (which we have already reviewed). These in turn have led to genuine advances in our *understanding* of the mind and its workings, most notably flowing from the abandonment of radical behaviorism and the subsequent adoption of cognitive models by the psychological community. Thus, the last half-century has seen a dramatic advance in our understanding of (say) episodic memory (Scoville & Milner, 1957), visual attention (Posner, Cohen, & Rafal, 1982), executive control (Shallice, 1988), and visually guided action (Milner & Goodale, 1993), to mention but a few examples.

As we have suggested elsewhere (Turnbull & Solms, 2007, pp. 1083–1084), these findings in *cognitive* neuroscience have limited implications for psychoanalysis. Of potentially far greater importance are developments over the last two decades in the domain of *affective* neuroscience (Damasio, 1994, 1999, 2011; LeDoux, 1996, 2000; Panksepp, 1998, 2011; Turnbull & Solms, 2007, pp. 1084–1085). Also very important have been significant advances in neuropsychology, the outstanding example being the discovery of "mirror neurons" (Gallese, Keysers & Rizzolatti, 2004; Rizzolatti, Fadiga, Gallese & Fogassi, 1996), as well as recent developments in social neuroscience (Cacioppo, Berntson, Sheridan, & McClintock, 2000; Cacioppo, Visser, Pickett, 2005; Decety & Cacioppo, 2011). Finally, one should not overlook the many developments in psychoanalysis itself in the last century. Probably the most important of these is the line of "ethological" work on attachment, separation, and loss, running from Harlow (1958) through Winnicott (1960), Bowlby (1969), to Ainsworth (Ainsworth, Blehar, Waters & Wall, 1978) and Fonagy and colleagues (e.g., Fonagy, Steele & Steele, 1991; Fonagy & Target, 1996). An important turning point was undoubtedly the publication of a pair of papers by Eric Kandel (1998, 1999) that offered a number of suggestions of research topics of relevance to neuropsychoanalysis. These papers provided much-needed support for the very idea of neuropsychoanalysis—an important endorsement for the field, especially when Kandel went on in 2000 to win the Nobel Prize for medicine/physiology.

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Importantly, however, individual developments in either of the "parent" disciplines of neuropsychoanalysis do not *themselves* bridge the divide between the fields. Through the decades there have, however, been a number of bold attempts at such bridging. The works of Paul Schilder (2007), Mortimer Ostow (1954, 1955), and Edwin Weinstein (Weinstein & Kahn, 1955) serve as beacons in this regard. Unfortunately, none of these earlier attempts flourished into the fully fledged interdiscipline we enjoy today, in part, perhaps, because

each of these early attempts ran into the same difficulties (of means, motive, and opportunity) that Freud had encountered (for an interview with Ostow on this topic, see Turnbull, 2004).

In retrospect, one of the most central limitations may have been the lack of a significantly well-developed dynamic *neuropsychology.* This only fully emerged in the 1970s, especially through the efforts of Luria (1966, 1973; for review see Kaplan-Solms & Solms, 2000, pp. 26–43; Solms & Turnbull, 2002, pp. 25–27). The second transformational shift occurred with the full development of affective neuroscience in the 1990s (Damasio, 1994, 1999; Panksepp, 1998), which finally aligned neuroscience with the topics of fundamental interest to psychoanalysis, allowing the disciplines to share findings not merely in relation to cognition, but also in the core psychodynamic domains of emotion and instinctual drive. We discuss this issue in more detail later in this article.

The bridging work that catalyzed our own present interest in neuropsychoanalysis began in this context, with one of us doing relatively conventional psychoanalytic investigations of neurological patients (Kaplan-Solms & Solms, 2000). Why did this prove to be such a seminal approach for neuropsychoanalysis? First, it involved a clinical method that followed on directly from where Freud had left off. The method requires relatively modest changes in working practice, and little additional training on the part of a psychoanalyst, and yet gives direct access to the subjective mental life of the (neurological) patient in precisely the same way that psychoanalysts traditionally gather data about psychiatric (or "normal") patients.

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This ensures that we can make direct observations concerning the neural correlates of metapsychological concepts in a methodologically valid setting. All of our metapsychological concepts and theories about the structure and functions of the mind are operationalized in a *clinical* psychoanalytic setting. Analytic work with neurological patients is therefore an ideal way of ensuring that we are studying the same "things" that Freud studied, albeit from a neurological perspective.

We would like to add a second reason why clinical work in neuropsychoanalysis is best performed with *neurological* rather than *psychiatric* patients. This is due to the methodological advantage of working with patients with *focal* brain lesions. First, most of these patients are pre-morbidly "typical" examples of humanity, with (as a population) few of the potentially confounding issues of aberrant development that so often occur in psychiatric disorders (Bentall, 2003, 2009). Second, and most importantly, it enables us to correlate our psychoanalytic inferences with *definite* neuroscientific ones. Structural neurological lesions provide infinitely more precision than do psychopharmacological manipulations, considering all the interactive vagaries of neurotransmitter dynamics. Moreover, by virtue of advances in structural imaging, it is possible to identify the neural basis of the clinically observed phenomena in neurological patients with a high level of scientific accuracy—a method well-suited for establishing clinico-anatomical correlations (Heilman & Valenstein, 1979; Kertesz, 1983; Kolb & Whishaw, 1990; Lezak, Howieson, & Loring, 2004).

In sum, having researched small populations of such patients (Kaplan-Solms & Solms, 2000), we have developed a method that offers a respectable degree of experimental control, a reasonable degree of neuroanatomical localization, excellent construct validity, and a direct observational window into the subjective life of the brain in a reasonably naturalistic setting.

On the basis of this approach, we have been able to build a preliminary picture of how our most basic metapsychological concepts might be correlated with brain anatomy and with all that we know of the functional organization of the brain. To take one example, in Kaplan-Solms and Solms (2000) we describe psychoanalytic observations on a small series of patients with right parietal lesions. They exhibited a remarkable degree of self-deception, in that they were paralyzed (on the left side) but insisted that they were *not* paralyzed. In some cases they explained away their paralysis through transparent rationalizations ("I tired the arm out this morning doing exercises"), or they developed more complex delusions—such as that the paralyzed arm belongs not to them but to the examiner, or to a close relative (for examples see Aglioti, Smania, Manfredi, & Berlucchi, 1996; Feinberg, 2001; Ramachandran & Blakeslee, 1998). Cognitive neuroscientists have traditionally explained these remarkable clinical phenomena in terms of simple cognitive *deficits*—damage to inferred cognitive "modules" (for review, see Nardone, Ward, Fotopoulou, & Turnbull, 2007; Turnbull, Jones, & Reed-Screen, 2002; Turnbull, Owen, & Evans, 2005). When we studied such patients psychoanalytically, however, we observed a pattern of psychological phenomena that was not at all modular in nature and was not by any means accurately defined as "deficit." What we observed were dynamic phenomena, in which the primary interacting forces clearly revolved around *emotional* states. Moreover, these emotionally determined dynamics caused important aspects of the cognitive processes involved to become *unconscious*. By intervening psychoanalytically in these dynamics, moreover, it was possible to reverse the dynamic process in question and return the

repressed cognitions to consciousness. This empirically demonstrated the validity of our conclusions and required students of this clinical phenomenon to radically reconceptualize its nature.

Kaplan-Solms and Solms (2000) concluded that self-deception in right parietal-lobe damage might well be attributable to narcissistic defensive organizations, such that the patients avoided depressive affects, using a range of primitive defense mechanisms. This regression to narcissism appeared to be attributable to a loss of capacity for whole-object relationships (Kaplan-Solms & Solms, 2000, pp. 148–199). These patients also appeared to have disrupted cognitive processes that represent space correctly, as acquired through normal development. Of course, this begs the question of *why* these effects should be seen typically with right-sided legions. A likely explanation is that the sort of emotion-regulation systems that are mediated by the right-hemisphere convexity are lost in such patients, disrupting their ability to tolerate powerful negative affects (Fotopoulou, Conway, Solms, et al., 2008; Fotopoulou, Conway, Tyrer, et al., 2008; Fotopoulou, Solms, & Turnbull, 2004; Turnbull, Jones, & Reed-Screen, 2002; Turnbull, Owen, & Evans, 2005) These findings can be seen to confirm the relationship between realistic spatial representation (of self/object boundaries) and maturation of object relationships. It also pointed to the neural correlate of what in psychoanalysis is termed "whole-object" representation, the metapsychological foundation of mature object love.

However, while this approach of applying clinical psychoanalytic methods to the study of neurological patients has many strengths, it also has limitations. Because clinical observations necessarily involve limited experimental control and are open to confirmation bias (Kahneman, 2003), it is a relatively weak method for determining the precise causal mechanisms involved.

Experimental studies, following on from those purely clinical observations, were therefore employed to provide fuller empirical support and refinement of the above hypotheses. A series of publications (Fotopoulou, Conway, Solms, et al., 2008; Fotopoulou, Conway, Tyrer, et al., 2008; Fotopoulou, Solms & Turnbull, 2004; Nardone et al., 2007; Tondowski, Kovacs, Morin, & Turnbull, 2007; Turnbull, Jones, & Reed-Screen, 2002; Turnbull, Owen, & Evans, 2005) have now conclusively demonstrated the powerful influence of emotions and unconscious cognitions (and associated defensive processes) in the neurodynamics that underpin the false beliefs of right parietal patients. For example, such patients show excessive attention to words that refer to paralysis and disabilities, despite *denying* that they are disabled and paralyzed (Nardone et al., 2007).

These lines of work have been an important contribution to behavioral neurology, taking forward the ideas generated in neuropsychoanalysis beyond our own sphere of interest. As a result of these efforts, a psychoanalytic point of view is now included in conceptualizations of these phenomena in mainstream neuroscientific journals, and the influence and contribution of psychoanalysis to the neurosciences is spreading, apparently for the first time in history (e.g., Carhart-Harris & Friston, 2010; Fotopoulou, Conway, Tyrer, et al., 2008; Fotopoulou, Solms, & Turnbull, 2004; Fotopoulou, Pernigo, Maeda, Rudd, & Kopelman, 2010; McKay & Cipolotti, 2007; McKay, Langdon & Coltheart, 2007a, 2007b; Solms, 2000; Turnbull, Berry, & Evans, 2004; Turnbull, Jenkins, & Rowley, 2004; Turnbull, Owen, & Evans, 2005; Turnbull & Solms, 2007).

Simultaneously, psychoanalytic observations on how the mind is altered by damage to different parts of the brain has enabled us to begin to build up a coherent model of how the mental apparatus, as we understand it in psychoanalysis, is realized in anatomy and physiology, providing what we might call a new "physical" point of view in psychoanalytic metapsychology. We have made especially remarkable progress in this respect in relation to the psychoanalytic theory on dreams (Solms, 1997b, 2000, 2011), by using multiple converging methods. It has been gratifying indeed to rediscover the Freudian conception of dreams in the neurodynamics of the sleeping brain. So much so that in 2006, at the "Science of Consciousness" conference in Tucson, Arizona, a formal Oxford-Rules debate (Solms vs. Hobson) on the contemporary scientific validity of the Freudian conception of dreaming resulted in a 2-to-1 vote in our favor. While such renewed demonstrations of confidence in our most basic theoretical propositions may be regarded as merely sociological phenomena, they are not unimportant for the future viability of our discipline.

What neuropsychoanalysis is *not*

We have described what neuropsychoanalysis *is*—in terms of its historical foundations, philosophical premises, and empirical underpinnings. We turn now to what neuropsychoanalysis is *not*, by defining of some boundary conditions.

The first boundary is a methodological one. We have especially recommended the clinico-anatomical method of making *direct* psychoanalytic observations on patients with focal brain lesions, in a clinical set-

ting. However, this is just a starting point. We have pioneered an example of how such clinical observations can be extended, using experimental neuropsychological tools. We have already alluded to the multiple converging methods that were used to establish the neural organization of dream psychodynamics. But numerous other approaches are possible. Thus, to take a relatively extreme instance, one might manipulate different neuropeptides, in research participants who are *themselves* psychoanalysts, and then have them describe their subjective states, using their expertise in doing so (with reference to the theoretical concepts that we use). Approaches such as this are rather radical, but they have huge potential, and appear to be remarkably underappreciated. To take a less radical example, why do we not have systematic psychoanalytic studies of the manipulations of the different classical neurotransmitters that psychopharmacologists regularly tinker with in conventional psychiatric settings (cf. Kline, 1959; Ostow, 1962, 1980; Ostow & Kline, 1959)?

Other psychoanalytically informed neuroscience comes from the use of neuroimaging methods—for example, studying Freud's theory of mourning (Freed, Yanagihara, Hirsch, & Mann, 2009), psychodynamic aspects of confabulation (Fotopoulou, Conway, Solms, et al., 2008; Fotopoulou, Conway, Tyrer, et al., 2008; Fotopoulou, Solms & Turnbull, 2004; Turnbull, Berry, & Evans, 2004; Turnbull, Jenkins, & Rowley, 2004), or tests of Freud's dream theory (Solms, 1997b, 2000). We might wonder, of course, whether work of this sort can legitimately be called "neuropsychoanalysis," given that the data collection occurs using merely neuroscientific and psychological, rather than psychoanalytic, methods. Such work might best be described as *psychoanalytically informed neuroscience*. But does it matter what label is attached to it? On balance, we prefer to take the "broad-church" approach to this issue—such that neuropsychoanalysis represents *all* work that lies along the psychoanalysis/neuroscience boundary. It may at times involve psychoanalytically inspired neuroscience (using purely neuroscientific methods to test psychoanalytically informed hypotheses), at other times the direct psychoanalytic investigation of neurological variables (brain injury, pharmacological probes, deep-brain stimulation, etc.). What unites these approaches is that they are attempts to do neuropsychoanalytic *research*.

was "But who cares?", which sounded a bit flippant : ok as chnaged?

There is another way of doing "neuropsychoanalysis," which relies entirely on speculative imaginings, transpositions, and guesses. The classic instances of this arise from psychoanalysts reading something about the latest developments in the neurosciences and observing that the new findings are vaguely reminiscent of such and such phenomenon or theory in psychoanalysis. They then claim that this or that neuroscientific finding discloses the biological correlate or underpinning of some aspect of psychoanalytic theory. In our view, "armchair" speculation such as this does not represent the way forward for our field. The last century saw more than enough speculation in psychoanalysis, leading to the formation of multiple "schools of wisdom" but remarkably little scientific progress. There is only one way to decide between theories, and that is to *test* them against reality, in such a way that the alternative predictions can be either confirmed or disconfirmed. Freud's "Project" (1950 [1895]) was a notable early instance of such speculative guesswork, which is why he himself so strongly resisted its publication, describing it as an "aberration."

One further instance of what neuropsychoanalysis is *not* is worthy of mention. Neuropsychoanalysis is not (in our opinion) a "school" of psychoanalysis, in the way that we currently speak of Freudian, Kleinian, Intersubjective, and Self Psychology schools. Neuropsychoanalysis, we feel, is far better conceptualized as a link between *all* of psychoanalysis and the neurosciences. Alternatively, it might be described as an attempt to insert psychoanalysis into the neurosciences, as a member of the family of neurosciences—the one that studies the mental apparatus from the *subjective* point of view.

Finally, we would like to make it clear that neuropsychoanalysis (or neuroscience in general) is not a final "court of appeal" for psychoanalysis. Psychoanalysis cannot look to any other science to find out what errors it may have made in its methods, theory, and practice. This is not to say that neuroscience brings no information to bear on what may have been erroneous or misleading paths in psychoanalysis. We have been criticized (Karlsson, 2010, pp.50–51) for not offering concrete instances of such erroneous paths.

Thus, take one powerful example, there is abundant evidence in neurobiology for the existence of what we refer to as "drives" (Panksepp, 1998; Pfaff, 1999; Rolls, 1999). For some students of psychoanalysis, drive theory has been rejected as out-moded and inappropriate (Kardiner, Karush, & Ovesy, 1959; Kohut, 2009; Siegel, 1996). Do recent neuroscientific observations invalidate this conclusion in psychoanalysis? They may not, but they are highly relevant to our ongoing thinking. It may be that the term "drive" is used in a quite different way by the psychoanalytic and neuroscientific communities (Fotopoulou, Pfaff, & Conway, in press), or that the concept of drives is more relevant to some aspects of mental life than others, or perhaps that it is only the psychoanalytic *taxonomy* of

Kardiner, Karush, & Ovesy, 1959 not in Refs

still in press?

the drives that needs revision. Other interpretations are also possible. Nevertheless, it seems appropriate that the psychoanalytic community looks again at the data that led them to reject Freudian drive theory and investigates whether drives may play a more substantial part in mental life than they had previously thought.

Plainly, this is not the whole story; it is merely the beginning. Once we have started to ask ourselves these questions, based on our reading of the current state of drive theory in neurobiology, we must *test* their conclusions using our own, *psychoanalytic* techniques. This is bound to lead to new observations, not only of psychodynamic phenomena or continuities that we had not noticed before, but also of possible limitations or errors in the neuroscientific conceptions at issue. It is, after all, more than possible that behavioral neuroscientists might have missed something important about the drives, deprived as they are of much of the data of subjective experience.

Thus, in our opinion, the interface between psychoanalysis and neuroscience is a rather *dialectical* one. As analysts, we may learn something new about the brain that seems relevant to psychoanalysis. We may think about it, keep it at the back of our minds, entertain the possibility, but above all *test it* psychoanalytically, as well as investigate its clinical usefulness. In this way, the final court of appeal for psychoanalysis remains the psychoanalytic setting—psychoanalytic observations made on real human beings, in the conventional clinical situation. A similar argument might, in principle, apply to the neurosciences—though, of course, they should and would never look to psychoanalysis as their final court of appeal. The risk of reductionism seems always to go in the direction of the physical, which is itself an interesting neuropsychoanalytic phenomenon! But neuroscientists today *do* look to psychoanalysis for interesting observations and theories, which they are increasingly applying to their work. They also quite naturally adopt them where they seem appropriate (Feinberg, 2001; Fotopoulou, Conway, Solms, et al., 2008; Fotopoulou, Conway, Tyrer, et al., 2008; Fotopoulou, Solms & Turnbull, 2004; Ramachandran & Blakeslee, 1998; Turnbull, Jones, & Reed-Screen, 2002; Turnbull, Berry, & Evans, 2004; Turnbull, Jenkins, & Rowley, 2004) and then move on.

The Future

There is a long history in the sciences of remarkable creativity at the boundaries between disciplines (Watson & Crick, 1953; for a discussion of the psychological basis of such creativity, see Bowman & Turnbull, 2009). Consistent with this, our interdisciplinary field has already opened rich veins of new enquiry. Doubtless this will continue to occur, and in unpredictable ways. Nevertheless we would like to sketch a general outline that we would like the field to move toward.

Our own vision is one of collaborative investigation of phenomena of common interest, approached using the rigor that is associated with all good scientific enquiry but also respects the methodological tools (with all the advantages and disadvantages) associated with each distinct field. An ideal outcome would be for neuropsychoanalysis to avoid any suggestion of being an armchair activity, or a field that is based on speculation rather than empirical work. Moreover, we envisage an interdiscipline in which the acquisition of knowledge is bidirectional (psychoanalysis informing neuroscience, and *vice versa*), and a discipline that retains the deep respect for the subjective perspective that is the hallmark of psychoanalysis.

We are confident that this will be the outcome for our field—for, as Freud told Einstein 83 years ago, "There is no greater, richer, more mysterious subject, worthy of every effort of the human intellect, than the life of the mind" (quoted in Grubrich-Simitis, 1995).

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