Introduction

One of the pressing problems facing psychoanalysis today is its relationship with the neurological sciences. The educated public in most developed countries (and especially in the United States of America) seem to have concluded that psychoanalytical therapies, research methods and theories are - to say the least - no longer at the cutting edge of mental science. Almost all the generally recognized therapeutic, technical and scientific advances in recent years have emanated from neuroscience. How should we respond to this situation? Is the educated public misinformed? Are the recent advances in mental science not really advances at all? Are they concerned with a fundamentally different subject matter? Do they have nothing at all to teach us? Can we simply ignore them? I want to show how an historical study of the origins and early development of our discipline can shed light on these questions, and thereby guide us in our current and future efforts to grapple with this problem.

The Historical Origins of Psychoanalysis in Neuroscience

Psychoanalysis, we are told by Freud, was born in 1895 or 1900 or somewhere in between. But when we identify the birthdate of psychoanalysis in this way, we should not forget that its birth was preceded by a long period of gestation, which included two decades of painstaking neuroscientific research. During that period (1877-1900), Freud published more than 200 books, articles and reviews (Meyer-Palmedo & Fichtner 1982), including numerous significant contributions (Jellife 1937; Brun 1936; Jones 1953; Vogel 1955; Triarhou & Del Cerro 1985), some of which established Freud as a leading international authority in the specialized fields of aphasia and cerebral palsy (Stengel 1954; Vogel 1956; Russin 1968; Accardo 1982). It seems reasonable to assume, as did Freud, that psychoanalysis was, to some extent at least, a natural development from - or even the culmination of - these early neuroscientific endeavours. If that is so, it seems reasonable also to expect that an exegesis of the historical progression from Freud's neuroscientific work to psychoanalysis proper should cast some light on the nature of the subsequent (and current) relationship between the two disciplines.

It is unnecessary to describe here, in any detail, Freud's earliest histological, anatomical and pharmacological researches (see Jellife 1937; Brun 1936; Jones 1953; Spehlmann 1953; Reicheneder 1990). It is sufficient to point out that Freud gradually moved from a study of the elementary morphology of the individual nerve cell, and from the simple spinal structures of some of the lowest vertebrates, upwards through the anatomy of human foetal cranial nerve nuclei and other brainstem structures, until finally he reached the human adult cerebrum, where he tackled some of the most complex clinical and theoretical problems of human neurology and neuropsychology. It is obviously in relation to these latter problems that he began to confront the questions that concern us here and, specifically, the questions arising from the metaphysical problem of the relationship between brain and mind.

When Freud first tackled these questions, they had already been the subject of intense scientific interest in European neurology for many years. Following the phrenological researches of Gall and Spurzheim (based on the questionable method of correlating bumps on the skull with personality characteristics), the hypothesis underlying those researches (namely that human mental faculties were differentially organized
in the brain, and therefore that they could be ‘localized’ in particular areas of the cerebrum) had fallen into disrepute. However this fundamental hypothesis of cerebral localization found new and unexpected support from an authoritative source: the clinical case studies reported by Paul Broca in 1861 and 1865. These studies were based on a different and far more serviceable method: the method of *clinico-anatomical correlation*.

The method of clinico-anatomical correlation was carried over into the new medical speciality of neurology in the middle of the nineteenth century by some of the ablest practitioners of the art of internal medicine. As its name suggests, internal medicine concerned itself with the diagnosis and treatment of diseases attacking the interior of the body - diseases which could for that reason not be apprehended directly in the living patient, but rather had to be inferred from their indirect manifestations as external symptoms and signs. The internal physician had to wait for the death of the patient, and the pathologist's report, before he could determine conclusively whether the physician's diagnosis had been correct or not. With the accumulation of experience, over generations - regarding the sort of clinical presentation during life that tended to correlate with particular pathological-anatomical findings at post-mortem - it gradually

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1 Freud once remarked that these works demonstrate that he did not ‘pull psychoanalysis out of my hat’ (Brun 1936).

became possible for the internal physician to recognize pathognomonic constellations of symptoms and signs, and thereby to predict from the clinical presentation (with reasonable accuracy) what and where the underlying disease process was, and to conduct the treatment accordingly. This was the origin of the concept of clinical syndromes (cf. Charcot's ‘formes’ and ‘types’). A syndrome is a regular constellation of symptoms and signs which reliably indicates the presence of a particular disease process. By identifying the relevant syndrome, the underlying process can be inferred from the manifest clinical presentation.

Neurology became a separate speciality of internal medicine as it became increasingly evident that the brain - like all other organs - was subject to its own special pathologies peculiar to its own special tissues. It also became apparent that damage to different parts of the brain produced different clinical manifestations. This led to an increasingly complex body of specialized knowledge of the syndromes of the human nervous system, based on clinicoanatomical correlations, which led naturally to the development of specialist practitioners and researchers. The existence of this new branch of medicine was formally recognized in Europe when Charcot was appointed to the first Chair of Neurology (at the Medical School in Paris); and when the first hospital devoted specifically to the diagnosis and treatment of nervous diseases was built (at Queen's Square in London).

When Freud decided in the early 1880s to specialize in this new branch of medicine, this is what he was taught - the art of rational diagnosis and treatment of neurological diseases through the recognition of syndromes, based on knowledge obtained by the method of clinico-anatomical correlation. In fact, we are told that Freud was a particularly gifted practitioner of this art. In *An Autobiographical Study* he wrote:

> I was able to localize the site of a lesion... so accurately that the pathological anatomist had no further information to add... The fame of my diagnoses and of their post-mortem confirmation brought me an influx of American physicians to whom I lectured upon the patients in my department. (Freud 1925d, p. 12)

Freud published a series of articles at the time, attesting to his mastery of syndrome diagnosis and the clinico-anatomical method - articles which Jelliffe (another psychoanalyst-neurologist) later described as ‘models of good neurological deduction’ (1937, p. 702).

Although the method of clinico-anatomical correlation had its origins in general medicine before it was formally introduced to neurology by Charcot, in neurology it came to serve a new and radically different purpose. This shift was due to the fact that diseases of the brain, unlike those of any other organ, have immediate and dramatic effects upon the *mind*. The association between brain disease and mental disorder was recognized in ancient times. Physicians through the ages were struck by the uncanny but undeniable fact that brain damage changed the patient as a person. The classical notion that the ‘brain is the organ of the mind’ was, therefore, from the first, based on a prototype of the method of clinico-anatomical
correlation.

From 1861 onwards, this method began to be used in a formal, systematic way to map the different faculties of the human mind onto the multifarious convolutions of the human cerebral cortex. This concerted research programme, which continues to this day, was initiated by Broca's (1861, 1865) observations - mentioned earlier - to the effect that damage to a circumscribed region of the brain (the third left frontal convolution) resulted in a loss of the power of speech. On this basis, Broca localized the psychological faculty of language to a specific anatomical structure (subsequently known as Broca's area). In 1874, Carl Wernicke, who - like Freud - was a pupil of the great Austrian neuroanatomist Theodor Meynert, extended this doctrine. He discovered that, whereas the powers of expressive speech were lost following damage to Broca's area, damage to another part of the left cerebral hemisphere (the first temporal convolution - Wernicke's area) resulted in a loss of speech *comprehension*. Wernicke concluded that the cells of these two cortical areas -Broca's and Wernicke's centres - the centres for expressive and receptive language respectively - were the repositories of memory images for the movements and sounds corresponding to spoken and heard words. These images were deposited in these areas by virtue of their relatively direct anatomical connection - via Meynert's ‘projection’ fibres -with the relevant peripheral sensory-motor organs. Wernicke speculated further that the two centres in question were connected with one another by means of Meynert's ‘association’ fibres, which passed through (or under) the intermediary region of cortex known as the insula. Eleven years later, Ludwig Lichtheim (1885) - also under the influence of Meynert's teachings - further elaborated this scheme, and postulated additional centres (one each for writing, reading and 'concepts'), all of which were likewise connected with one another and (directly or indirectly) with the body periphery.

On this basis, the whole complex faculty of language was gradually localized in specific tissues of the brain. Subsequently - still under the influence of Meynert's teachings - numerous other complex psychological faculties, such as visual recognition (Munk, Lissauer) and voluntary action (Liepmann), were similarly localized in discrete cortical areas. By 1934, a few years before Freud's death, this approach to clinico-anatomical correlation had produced detailed maps of the psychological functions of the various cortical convolutions, including such complex faculties as the body scheme, emotional sensations, and the social ego (Kleist 1934). This research programme resulted in the gradual development of a further subspeciality within the new field of neurology, known as *behavioural neurology*.

We know from Freud's early writings that he was thoroughly versed in the methods and discoveries of this exciting new branch of science. In fact there is abundant evidence to suggest that the clinico-anatomical localization of mental functions was a subject of very special interest to him (see Freud 1888b, 1891b, 1893-94a). Clearly then, Freud was aware, shortly before he made the breakthrough into psychoanalysis, that there was a well-established method available to the clinical neurologist by means of which it was possible to correlate mental faculties on a rational basis with the physiology and anatomy of particular tissues in the brain. But if that was so, it raises a fundamental question: why did Freud not use...
this method to identify the neurological correlates of the psychological processes that he later discovered? And why don't we use it today?

It is undoubtedly true that Freud wanted to establish such correlations. The very existence of his ‘Project for a Scientific Psychology’ (1950a [1895]) - with its aspiration to ‘represent psychical processes as quantitatively determinate states of specifiable material particles, thus making those processes perspicuous and free from contradiction’ (p. 295) - attests to this fact. The life-long persistence of that aspiration is evident in the following quotations from Freud's psychoanalytical writings:

*It would have to be possible in some manner which cannot yet be indicated to represent [psychical] paths by organic elements of the nervous system. (Freud 1905c, p. 148)*

*We must recollect that all our provisional ideas in psychology will presumably some day be based on an organic substructure. (1914c, p. 78)*

*The theoretical structure of psycho-analysis is in truth a superstructure, which will one day be set upon its organic foundation. But we are still ignorant of this. What distinguishes psycho-analysis as a science is not the material which it handles but the technique with which it works. (1916-17a, pp. 388-9)*

*The indefiniteness of all our discussions on what we describe as metapsychology is of course due to the fact that we know nothing of the excitatory process that takes place in the elements of the psychical systems, and that we cannot feel justified in framing any hypotheses on the subject... 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... 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. They may be of a kind which will blow away the whole of our artificial structure of hypotheses. (1920g, pp. 30, 60)*

*In view of the intimate connection between the things that we distinguish as physical and mental, we may look forward to the day when paths of knowledge and, let us hope, influence will be opened up, leading from organic biology and chemistry to the field of neurotic phenomena. That day still seems a distant one, and for the present these illnesses are inaccessible to us from the direction of medicine... However much philosophy may ignore the gulf between the physical and the mental, it still exists for our immediate experience and still more for our practical endeavours. (1926e, pp. 231, 247)*

*These quotations could be multiplied.*

There appears to be a contradiction between these statements and other, oftquoted statements to the effect that psychoanalysis is a *psychological* science which must remain completely independent of neuroscience. Why would Freud look forward eagerly to the day when it would become possible to 'represent psychical processes as quantitatively determinate states of specifiable material particles’ (loc. cit.), and yet simultaneously 'completely disregard the fact that the anatomical apparatus... is also known to us in the form of an anatomical preparation’ (Freud 1900a)? A solution to the apparent contradiction is not difficult to find: clearly Freud did not conceive of the separation between psychoanalysis and neuroscience in *absolute* terms; he saw it as a *temporary* situation, arising out of the inadequacy of the neurosciences of *his time* to provide solutions to the problems that psychoanalysis was grappling with. Hence Freud wrote, on the one hand, that 'our psychical topography has for the present [Freud's emphasis] nothing to do with anatomy’ (1915e, p. 175) but, on the other hand, that it ‘will one day be set upon its organic foundation’ (1915e, p. 175, emphasis added). In short, Freud rejected the possibility of identifying by means of the clinico-anatomical method the physical correlates of the psychological processes he discovered, not because he rejected psycho-physical correlation in general, but because the *clinico-anatomical method in particular* was incapable of providing the correlations he sought.

Our question therefore becomes a more specific one: *why did Freud consider the clinico-anatomical method (in particular) to be incapable of revealing the physical correlates of the mental processes that
occupied his scientific attention from the last two decades of the nineteenth century until his death in 1939? The answer to this question, as I will now show, was that Freud believed that the clinico-anatomical method could not accommodate (1) the functional nature of neurotic pathology and (2) the dynamic nature of normal mental processes. The classical syndrome method was only useful clinically for the diagnosis of those illnesses which could be traced back to structural lesions of the nervous system, not for functional disorders; and the clinico-anatomical method was only useful scientifically for identifying the physical correlates of elementary components of the mental apparatus, not for representing the dynamics of the apparatus as a whole. And it was, Freud realized early on, the functional dynamics that mattered most when it came to the complex mental processes that interested him.

The reasoning behind Freud's objections may best be conveyed by returning to my historical account. Although it is true that the clinico-anatomical method was to all intents and purposes the only research method available to the late nineteenth-century neurologist - and especially to the neurologist interested in mental functions - it was used in subtly different ways within two distinct schools of Continental neurology. In the German school, in which Freud was initially trained, the emphasis fell squarely upon the anatomical side of the clinico-anatomical equation. According to this school, the primary aim of neurological science was to establish anatomical and physiological facts which

3 The ‘German’ school included the universities of the Austro-Hungarian empire.

explained clinical facts. Clinical material served the secondary purpose of advancing theoretical (anatomical and physiological) knowledge. Accordingly, the clinical facts were subordinated to anatomical and physiological theories.

In the French school of neurology, on the other hand, the emphasis fell very much upon the clinical side of the equation. According to this school, which collected around the personality of Charcot and the famous wards of the Salpetriere Hospital, the primary task of clinical science was to observe and describe clinical reality - to establish new clinical facts, regardless of their relation to anatomical and physiological theory. Anatomical and physiological theory served the secondary purpose of trying to make sense of clinical reality. Clinical reality was primary. The goal therefore was not so much to explain the various clinical pictures, but rather to identify, classify and describe them. The following quotation graphically illustrates the difference between these two ways of applying the clinico-anatomical method:

Charcot... never tired of defending the rights of purely clinical work, which consists in seeing and ordering things, against the encroachments of theoretical medicine. On one occasion there was a small group of us, all students from abroad, who, brought up on German academic physiology, were trying his patience with our doubts about his clinical innovations. ‘But that can’t be true,’ one of us objected, ‘it contradicts the YoungHelmholtz theory [of vision].’ He did not reply, ‘So much the worse for the theory, clinical facts come first’ or words to that effect; but he did say something which made a great impression on us: [‘Theory is good; but it doesn’t prevent things from existing.’] (Freud 1893f, p. 13)

This anecdote, one of Freud's favourites, is reminiscent of the lines attributed to Mephistopheles in Goethe's Faust (Part 1, Scene 4), which Freud also cited more than once with approval:

Grey, dear friend, is all theory,
And green alone Life's golden tree.

For Charcot, in short, neurological science was neurological nosology. Please note, however, that the term ‘nosology’ had a broader meaning for the French school than it did (and still does) in English:

The word nosological in English refers to the nomenclature and classification of disease, but it is the clinical method of investigation in its widest sense which is... [implied by Charcot:] that method of investigation which argues from effect to cause, commencing with a study of the disease at the bedside, as distinguished from the converse method of a priori reasoning, with the teachings of physiology for its basis. (Savill, in Charcot 1889, p. 9).
Charcot never tired of quoting the great Claude Bernard's opinion in this regard:

*Set up first the medical problem which arises from the observation of a malady, and afterwards seek for a physiological explanation. To act otherwise would be to risk overlooking the patient, and distorting the malady.* (Charcot 1889, p. 8)

Rich descriptions of Charcot's clinical approach can be found in a number of recent studies (see, for example, Goetz, Bonduelle & Gelfand 1995).

As is well known, during a period of study at the Salpetriere in the mid-1880s, Freud moved from being under the direct, personal influence of Meynert and other leading figures of German theoretical neuroscience, to being under the direct personal influence of Charcot, the clinician. This shift had a decisive influence on his thinking and, in particular, on his attitude to clinico-anatomical localization. The reason for the shift was simple. Freud had come to the conclusion, like Charcot and many others, that clinico-anatomical knowledge of the major syndromes of physical neurology was complete by the end of the nineteenth century (Freud 1893f, p. 19). The method had therefore degenerated into ‘a silly game of permutations’ (Freud, quoted by Bernfeld). As a result, Freud felt that he had ‘nothing more to learn from the German-speaking universities’ (Freud 1956a). What, then, did the French universities have to teach him?

Although the differences between the French and German schools of neurology complemented each other in regard to most physical neurological disorders, with the one school emphasizing the clinical and the other the anatomical side of the clinico-anatomical equation, there was one group of diseases - which were considered to fall under the domain of neurology at the time - which threw the differences between the two approaches into sharp relief. I am referring to the neuroses, and to hysteria and neurasthenia in particular, where no demonstrable lesion of the nervous system could be found at autopsy to account for the clinical symptomatology observed during the life of the patient.

The absence of a demonstrable lesion posed no serious problems for the French school: Charcot simply proceeded to describe the pathognomonic clinical syndromes of hysteria and neurasthenia, as he had done with countless other ‘nervous’ diseases. The neuroses were for Charcot, as Freud wrote at the time, ‘just another problem in neuropathology’ (Charcot 1893f). However, for the German neurologists, the problem of the neuroses was well nigh insoluble. How was one to explain in anatomical and physiological terms the mechanism of a clinical syndrome which had no pathological-anatomical basis? As a result, some German neurologists, Freud's teachers among them, developed elaborate speculative theories of the mechanisms of the neuroses, based on apriori anatomical and physiological assumptions, while others simply declared that the neuroses were not fit subjects for serious scientific attention; if there was no demonstrable lesion, there was no clinical syndrome.

During the crucial period that Freud studied under Charcot, the neuroses were the subject that most preoccupied him. Charcot's pioneering nosological studies on hysteria are very well known and hardly need to be reviewed here. Equally well known is the fact that Freud was a devoted pupil of Charcot and that, upon his return to Vienna, initially at least, Freud expounded Charcot's views whenever and wherever he could - much to the irritation of his old teachers. It is important to notice, however, that this shift of allegiances did not imply a shift away from neurology towards psychology; it applied equally to Freud's work on physical neurological disorders and to his work on the neuroses (which for Charcot, do not forget, 'were just another problem in neuropathology', loc. cit.). What occurred was not so much shift away from neurology as a shift away from German neurology.

*What can only be called a 'conversion' from mechanistic physiology to clinical medicine occurred during Freud's travelling fellowship to the Salpetriere (1885 to 1886), when he fell under the influence of the great neurologist Charcot... His contact with Charcot breathed life into his previously sterile clinical expertise.* (Accardo 1982, p. 452)
I will return to this point in a moment.

It is also important to remember that, although Charcot emphasized the priority of clinical description over anatomical and physiological explanation, he did not disregard the ultimate need for explanation. In this respect, the difference between Charcot's and Meynert's schools was only a matter of whether one started from the clinical observations (like Charcot) or from the assumptions of physiological and anatomical theory (like Meynert). Charcot was content to describe and classify the clinical syndromes of hysteria and neurasthenia as a necessary prerequisite for physiological and anatomical explanation, on the assumption that their pathological-anatomical correlates would eventually yield to advances in laboratory techniques. Charcot, no less than the German neurologists, therefore, believed that neurotic symptoms were attributable to anatomical and physiological abnormalities. He emphasized that the lesions in question were functional lesions (lesions *etat dynamique*) rather than structural ones (lesions *etat statique*); but he still believed that these functional lesions would be localizable some day, when anatomical techniques were sufficiently advanced.

When Charcot turned to autopsy studies, he found no lesion in hysteria, but, undaunted, he continued his studies in this purely anatomical perspective, searching for ‘an analogy to anatomical lesions’. (Goetz et al. 1995, p. 129)

Freud's earliest writings on the subject of the neuroses - which were essentially nosological contributions - mirrored Charcot's views closely. However, he soon extended Charcot's approach, and began to doubt whether ‘dynamic’ lesions would ever be anatomically localizable:

Neurasthenia is not a clinical picture in the sense of textbooks based too exclusively on pathological anatomy: it should rather be described as a mode of reaction of the nervous system [as a whole]. (Freud 1887a, p. 33, emphasis added)

Hysteria is a neurosis in the strictest sense of the word - that is to say, not only have no perceptible changes in the nervous system been found in this illness, but it is not to be expected that any refinement of anatomical techniques would reveal any such changes. Hysteria is based wholly and entirely on physiological modifications of the nervous system and its essence should be expressed in a formula which took account of the conditions of excitability in the different parts of the nervous system (p. 41, emphasis added).

So, whereas the first step in Freud's transition from neuropathology to psychoanalysis was a conversion from theoretical to clinical medicine, the second step was a shift from anatomical to physiological medicine. It is important to recognize once more that this was not, initially at least, a shift from physical to psychological medicine. The above quotations clearly demonstrate Freud's continued adherence to a physiological conception of the neuroses. What was at issue was not whether these were physical as opposed to psychological disorders, but rather whether they were disorders of physiological function as opposed to anatomical structure. So Freud was not moving away from neurology; rather he was moving away from a particular approach to neurology (the anatomical localizationist approach), and towards another (equally neurological) approach: a dynamic physiological one (Solms & Saling 1986). This alternative approach was fundamental to the dynamic school of neurology (Riese 1959), about which I shall have much more to say later.

The shortcomings of the clinico-anatomical method for Freud's purposes were thus becoming increasingly apparent. How does one set about localizing the ‘seat’ of a dynamic, functional disorder which could only be described as a ‘mode of reaction’ of the nervous system as a whole, or expressed in a formula which was based on the ‘conditions of excitability’ of the nervous system?

For Freud, the only way to understand these disorders was to approach them physiologically; anatomy (and therefore the clinico-anatomical method) had nothing to offer him here. What he was looking for was a formula which expressed the physiological laws of the functional apparatus that was disordered in these conditions. But how does one set about exploring the functional laws underlying such disorders? There were simply no physiological methods available. This is where the French approach to neurology - with its reliance on a purely clinical method - came into its own. This was the essential influence that Charcot had on Freud. Under this influence, Freud set about exploring the neuroses by means of purely clinical
methods, which, given the nature of the disorders themselves, rather than any such preference on Freud's part, inevitably meant a purely psychological method. For the clinical picture in these cases was a psychological picture.

4 The point is underlined by the fact that Freud soon made similar remarks about the need to conceptualize the movement disorders of children - cerebral palsy, an undeniably physical disorder - in dynamic, functional terms rather than static, anatomical terms. Freud (1897a) suggested that these disorders, no less than the neuroses, are best understood as reflecting the laws of a complex functional system, with a long developmental history, rather than in localizationist, anatomical terms (Accardo 1982).

5 ‘The theoretical structure of psycho-analysis is in truth a superstructure, which will one day be set upon its organic foundation. But we are still ignorant of this. What distinguishes psychoanalysis as a science is not the material which it handles but the technique with which it works… Supposing, now, that it was possible by some chemical means, perhaps, to interfere in this mechanism, to increase or diminish the quantity of libido present at a given time or to strengthen one instinct at the cost of another - this then would be a causal therapy in the true sense of the word, for which our analysis would have carried out the indispensable preliminary work of reconnaissance. At present, as you know, there is no question of any such method of influencing libidinal processes.’ (Freud 1916-17a, pp. 388-9, 436).

However, a personal preference of sorts did play a part. Whereas Freud certainly followed Charcot's advice by giving priority to clinical observation over anatomical theory, he observed in a different way from Charcot. Charcot was a visual type, he was ‘as he himself said, a visuel, a man who sees’ (1893f, p. 12). Freud, by contrast, was an auditory type, a man who listens. It is quite likely that Charcot's penchant for ordering things visually contributed to his tendency to conceptualize hysteria anatomically, whereas Freud's capacity to listen facilitated a fully psychological conceptualization. ‘There can be no doubt that the radical revolutionization of clinical perception was fully accomplished only when 'hearing' too came to be incorporated into the perceptual process’ (Grubrich-Simitis 1997, pp. 29-30).

However, Freud's writings from the late 1880s make it abundantly clear that initially he drew no distinction between physiology and psychology (Stewart 1969; Levin 1978). ‘Psychology’ for the young Freud, like so many of his contemporaries (Amacher 1965), was simply the physiology of the cerebral cortex. This is demonstrated, for example, by the following remarks on hypnotism:

The question might still be asked whether all the phenomena of hypnosis must somewhere pass through the psychical sphere; in other words - for the question can have no other sense - whether the changes in excitability which occur in hypnosis invariably affect only the region of the cerebral cortex... We possess no criterion which enables us to distinguish exactly between a psychical process and a physiological one, between an act occurring in the cerebral cortex and one occurring in the subcortical substance; for 'consciousness', whatever that may be, is not attached to every activity of the cerebral cortex. (SE 3, p. 84)

The untangling of this conflation of psychology and cortical physiology was the next step in Freud's transition from neuropathology to psychoanalysis. In this respect the theoretical influence of the English neurologist John Hughlings Jackson was decisive. It is of no small interest that this development occurred not in relation to hysteria or any other neurosis, but rather in relation to aphasia: a fully 'neurological' condition and, moreover, the very condition upon which the clinico-anatomical localization of mental functions was first attempted, 25 years before.

6 ein Seher. In an unpublished letter to Strachey (4 June 1962) Eissler remarked that ein Seher is not only a person who is attracted mainly by visual impressions but also a person who can see what is not accessible to sense perception. Eissler suggested that 'a seer' would better convey the full meaning of the phrase than does Strachey's literal 'a man who sees'.

7 Jackson's 'doctrine of concomitance' - a variety of psycho-physical parallelism- stated: 'first, that states of consciousness (or, synonymously, states of mind) are utterly different from nervous states; second, that the two things occur together - that for every mental state there is a correlative nervous state; third, that, although the two things occur in parallelism, there is no interference of one with the other… [The] mental state… arises during (not from) the
activities of the two highest links of this purely physical chain; so to speak, it “stands outside” these links’ (Jackson 1884, p. 72).

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In his 1891 monograph on aphasia Freud exposed the major fallacy upon which the entire localizationist enterprise rested, namely its conflation of psychological and physiological concepts, which led Meynert, Wernicke, Lichtheim, and the whole of the classical German school, to assume that individual ideas (Vorstellungen, ‘presentations’) could be localized in individual nerve cells (Marx 1966, 1967).

In psychology the simple idea is to us something elementary which we can clearly differentiate from its connection with other ideas. This is why we are tempted to assume that its physiological correlate, i.e., the modification of the nerve cells which originates from the stimulation of the nerve fibres, be also something simple and localizable. Such an inference is, of course, entirely unwarranted; the qualities of this modification have to be established for themselves and independently of their psychological concomitants. (Freud 1891b, pp. 55-56)

Henceforth, for Freud, ideas would be studied in their own right and in their own terms; and the relationship between psychology and cortical physiology would therefore be immeasurably complexified.

What then is the physiological correlate of the simple idea emerging and re-emerging? Obviously nothing static, but something in the nature of a process... it starts at a specific point in the cortex and from there spreads over the whole cortex and along certain pathways. (Freud 1891b, p. 56)

This was the culmination of Freud's journey away from the localizationist neurology of his German masters. Henceforth, in addition to conceptualizing mental processes in dynamic, functional terms (as opposed to static, anatomical ones) he would conceive of mental functions in purely psychological terms (as opposed to physiological ones); and, accordingly, he would recognize them as being unlocalizable in the tissues of the brain. Henceforth, for the remainder of his scientific life, Freud would speak of mental processes not as being localized in the elements of the mental organ, but rather as being distributed; that is, as being located not within but rather between the elements.

I shall carefully avoid the temptation to determine psychical locality in any anatomical fashion. I shall remain upon psychological ground, and I propose simply to follow the suggestion that we should picture the instrument that carries out our mental functions as resembling a compound microscope or a photographic apparatus, or something of the kind. On that basis, psychical locality will correspond to a point inside the apparatus at which one of the preliminary stages of the image comes into being. In the microscope and telescope, as we know, these occur in part at ideal points, regions in which no tangible component of the apparatus is situated. (Freud 1900a, p. 536)

It will soon be clear what the mental apparatus is; but I must beg you not to ask what it is constructed of. That is not a subject of psychological interest. Psychology can be as indifferent to it as, for instance, optics can be to the question of whether the walls of a telescope are made of metal or cardboard. We shall leave entirely on one side the material line of approach, but not so the spatial one. (Freud 1926, p. 194)

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But the important point to remember, once again, is that this shift towards a fully psychological understanding of the functional apparatus supporting mental processes was worked out in a neurological monograph, on a purely neurological topic (Freud 1891b). It was only subsequently applied to hysteria and the other neuroses (Freud 1893c), at which point its dependence upon the aphasia monograph was clearly acknowledged.

All of this has very important implications for our understanding of the contemporary relationship between psychoanalysis and neuroscience. Our analysis of the development of Freud's neurological views between 1885 and 1893 reveals that, first, he shifted away from an explanatory method based on
anatomical and physiological speculation, in favour of a descriptive method, based on clinical observation. Next, the inferences that Freud drew from his clinical observations became increasingly functional and dynamic, that is to say, physiological as opposed to anatomical. Finally, Freud recognized that the functional apparatus at issue in the neuroses was constructed according to the laws of psychology rather than those of physiology; in other words that the relationship between psychology and physiology was a complex one, involving relationships between multiple elements rather than simple one-to-one correspondences. But none of these shifts entailed a radical rejection of neuroscience; they represented nothing more or less than a recognition of the true complexity of the relationship between psychological events and their neural correlates - an inevitable consequence of his conversion to Charcot's dictum that the neurological scientist should grant a privileged status to clinical facts, and let the anatomical and physiological correlates of those facts fall into place behind the clinical reality, no matter how complex the resultant relationship might ultimately prove to be.

All of this places Freud not outside neuroscience, but rather within a particular tradition in neuroscience, namely the dynamic school of neurology. The dynamic school of neurology, which traces its lineage back to John Hughlings Jackson and continues as a powerful force in neuroscience up to the present day, always endorsed (and still endorses) all of the objections that Freud levelled against narrow clinic-anatomical localizationism, and it continues to conceptualize the neurology of mental processes in functional and dynamic terms, and to give pride of place to psychological methods and models.

Freud made one, final attempt in 1895 - in his celebrated ‘Project’ to represent the complex physiological correlates of his putative mental apparatus. Recognizing that the neurology of his time still could not provide any viable empirical method for determining these relationships, recognizing painfully therefore that he could proceed no further than the speculative ‘imaginings, transpositions and guesses’ of the German school (Freud to Fliess), he abandoned any further attempt to determine the neurological correlates of the functional apparatus of the mind that he spent the remainder of his scientific life elucidating. He thereby bequeathed this task to the future.

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The psychical topography that I have developed... has nothing to do with the anatomy of the brain, and actually only touches it at one point.8 What is unsatisfactory in this picture - and I am aware of it as clearly as anyone - is due to our complete ignorance of the dynamic nature of the mental process. (Freud 1939a, p. 97)

There were undoubtedly advantages for Freud in adopting a purely psychological approach. The most important of these was, of course, his subsequent ability to describe the unconscious processes which were truly causal of mental life in fully psychological terms, and thereby to arrive for the first time at an adequate understanding of them.

Many people, both inside and outside science, are satisfied with the assumption that consciousness alone is psychical; in that case nothing remains for psychology but to discriminate among psychical phenomena between perceptions, feelings, thought processes and volitions. It is generally agreed, however, that these conscious processes do not form unbroken sequences which are complete in themselves; there would thus be no alternative left to assuming that there are physical or somatic processes which are concomitant with the psychical ones and which we should necessarily have to recognize as more complete than the psychical sequences, since some of them would have conscious processes parallel to them but others would not. If so, it of course becomes plausible to lay the stress in psychology on these somatic processes, to see in them the essence of what is psychical and to look for some other assessment of the conscious processes. The majority of philosophers, however, as well as many other people, dispute this and declare that the idea of something psychical being unconscious is self-contradictory. But this is precisely what psycho-analysis is obliged to assert... It explains the supposedly somatic concomitant phenomena as being what is truly psychical, and thus in the first instance disregards the quality of consciousness. (Freud 1940a, pp. 157-158, emphasis added)

But we should not forget that Freud nevertheless looked forward to the day when it would be possible to re-translate the ‘truly psychical’ phenomena into their ‘somatic concomitants’. It is therefore of the utmost importance to note that, soon after Freud's death, enormous strides were made within the dynamic
school of neurology in precisely this respect, as a new neuroscientific method was devised which finally managed to accommodate the dynamic nature of mental life. In short, advances in the neurosciences have overcome Freud's misgivings about the method of clinico-anatomical localization. It is significant that these developments arose from the work of a clinical neurologist who had initially, for almost a decade, conducted psychoanalytical research under the direct personal influence of Freud. In the following paragraphs this phase in the

career of Aleksandr Romanovich Luria - the founder of the Soviet school of neuropsychology - is reviewed briefly, in order to demonstrate its impact upon his later thinking.

The Origins of Dynamic Neuropsychology in Psychoanalysis

After graduating in 1921 from Kazan University with a degree which was rooted mainly in the biological and social sciences, the young Luria established a psychoanalytical society in Kazan. He wrote to Freud in 1922 to inform him of this fact, and thus initiated a brief correspondence with him. (Three letters from Freud can still be found in the Luria family archives; see Van der Veer & Valsiner 1991, p. 87.) The intensity and scope of Luria's subsequent involvement with psychoanalysis are well illustrated by the wide-ranging series of reports, which he published at the time in the Internationale Zeitschrift fuer Psychoanalyse (Luria 1922a, 1922b, 1923a, 1923b, 1923d; see also Luria 1923c, 1923e). The meticulous notes upon which these reports were based can still be consulted in the Luria archives. During the 17 meetings of the Kazan Psychoanalytical Society, which were held between September 1922 and September 1923, Luria delivered 12 lectures. On 7 September 1922, he spoke on the 'Present state of psychoanalysis'. On 21 October he presented a psychoanalytic study of sexual differences in clothing, a subject to which he was later to return at the All-Russian Congress on Psychoneurology in Moscow (Luria 1923c). On 10 December 1922, he addressed the Society on 'The current crisis in Russian psychology'. He spoke twice on 18 February 1923, firstly on 'Some principles of psychoanalysis,' followed by a psychoanalytical study of Leonid Andreev's play Savva. On 5 March he spoke on 'Psychoanalysis in the light of the main tendencies of modern psychology'. Finally, on 18 March he presented a study of sleep-onset phantasies. In addition to these scientific activities, Luria analysed patients (including, incidentally, Dostoevsky's granddaughter) at the local Kazan Psychiatric Hospital.

After further training in pedagogics, Luria moved to Moscow. There he joined the Russian Psychoanalytical Society (and completed his formal studies in medicine and linguistics). He continued to play a central organizational role in Soviet psychoanalysis and to pursue his intensive psychoanalytic research programme (see Luria 1923f, 1924a, 1924b, 1925a, 1925b, 1925c, 1925d, 1926a, 1926b, 1926c, 1926d, 1926e, 1926f, 1926g, 1927a, 1927b, 1927c, 1928; Vygotsky & Luria 1925). The Russian Psychoanalytical Society had been founded in 1921. By the time that Luria joined the Russian Society, its formal

8 Freud continued throughout his psychoanalytic career to correlate consciousness (i.e. the activities of the system Pept-Cs) with physiological activity in the primary ‘projection’ zones of the cerebral cortex. The primary sensory-motor zones of the cortex were the ‘specific points’ mentioned in the quotation cited above (1891b), from which the excitation of the mnemonic traces corresponding to the simple idea spreads out over the whole of the cortex and beyond (i.e. the ‘psi’ systems of the Pcs and Ucs - the subsequent ‘ego’). Freud (1891b) described these sensory-motor projection zones as the ‘cornerstones’ of the mental apparatus (the subsequent ‘nuclei’ of the ‘bodily ego’).

9 For a full discussion of Luria's psychoanalytic career, see Leon (1982), Kozulin (1984), Angelini (1988) and Van der Veer & Valsiner (1991). The present account relies heavily upon the latter work.

10 Van der Veer & Valsiner (1991, p. 79) state that ‘it is no exaggeration to say that the institutional history of psychoanalysis in the Soviet Union was to a substantial degree determined by his efforts’.

11 The Russian Psychoanalytical Society included among its membership Sabina Spielrein, who later played a prominent role in Swiss and Viennese psychoanalytic circles (including in the conceptual development of the death instinct). She also analysed Jean Piaget for a brief period.
activities included scientific meetings, publication of a series of books entitled *The Psychological and Psychoanalytic Library*, clinical work at the State

Psychoanalytic Institute, and the running of a psychoanalytic kindergarten and laboratory. Luria not only performed administrative duties as the Society's secretary, but he also conducted clinical and scientific work. The latter included a series of studies on the dream-work, wherein he suggested dream thoughts under hypnosis and then observed transformations in the subsequent manifest content. He presented numerous lectures and frequently participated in the discussions at scientific meetings. On 29 May 1924, for instance, he presented a talk entitled ‘Psychoanalysis as a system of monistic psychology’ (this was subsequently translated into English, Luria 1925a). In the following year he spoke on ‘Affect as a non-abreacted reaction’ (26 May), ‘Experimental study of the phantasies in a boy’ (16 April), and ‘The use of experiments for psychoanalytic goals’ (12 November). On 23 February 1927 he delivered a lecture entitled ‘The experimental study of children's primitive thinking’, and on 17 March he presented a discussion of Bykhosky's book on *Freud's Metapsychology*.

Suddenly, on 7 April, less than one month after presenting the last-mentioned discussion, Luria asked to be relieved of his duties as secretary of the Russian Psychoanalytic Society. Within two years he had resigned from the Society itself. At that point he delivered a ‘penitent speech’ (Lobner & Levitin 1978, p. 19) in which he disassociated himself publicly from psychoanalysis (Pappenheim 1990), and he published an article in which he admitted to his ‘psychoanalytic mistakes’ (Kozulin 1984, p. 88). A few years later he contributed an article on psychoanalysis to the *Great Soviet Encyclopedia* (Luria 1940) in which he dismissed it as ‘a false theory’ belonging to ‘the sphere of hostile advanced [bourgeois] science’, on the grounds that it ‘biologizes the complex, historically-determined conscious state of the human being’ (p. 510). He never again discussed the subject of psychoanalysis in his published writings - apart from in his autobiography, where he described his youthful involvement with it as ‘an error.

Here, I thought, was a scientific approach that combined a strongly deterministic explanation of concrete, individual behaviour with an explanation of the origins of complex human needs in terms of natural science. Perhaps psychoanalysis could serve as the basis for a scientific reale *Psychologie*, one that would overcome the nomothetic-ideographic distinction... But I finally concluded that it was an error to assume that one can deduce human behaviour from the biological ‘depths’ of the mind, excluding its social ‘heights’. (Luria 1979, pp. 23-24)

Considering the depth of Luria's understanding of psychoanalysis, as reflected in his writings in the 1920s, this is a surprisingly naive misrepresentation of psychoanalytic theory and the aims of psychoanalytic research. It is particularly surprising in that it was precisely this misunderstanding of the nature of psychoanalysis that Luria had spent so much time and effort arguing against in the 1920s.

Luria's sudden change of allegiance can easily be explained. His (ostensible) abandonment of psychoanalysis was undoubtedly the result of political and ideological pressure, rather than of developments in his scientific thinking. The facts supporting this view are well documented in numerous places and do not need to be discussed in detail here (see, for example, Lobner & Levitin 1978; Kozulin 1984). Essentially, following increasing criticism in the scientific literature and the public press between 1924 and 1929 - including direct and personal criticism of Luria himself - ‘psychoanalysis became a scientia non grata in the Soviet Union’ (Van der Veer & Valsiner 1991, p. 78). After Luria was denounced.

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12 Jaroshevsky (1989, p. 131) claimed that Stalin's son, Vasily, was among the children attending this kindergarten.
13 The reader interested in obtaining a fuller understanding of the nature of Luria's commitment to psychoanalysis may profitably consult this work.
14 The actual wording of Luria's apology read: ‘This point of view [psychoanalysis], in its essence incompatible with Marxist psychology, was accepted in some of my works. It took me a number of years to realize that these biologizing ideas are hostile to Marxism’ (Luria 1932b, p. 72).
and ‘found guilty of ideological deviations’ (Kozulin 1984, p. 20), his resignation from the Russian Psychoanalytic Society and his subsequent public disavowal of his views were ‘the only way he would be able to continue his important work’ (Pappenheim 1990, p. 5). Many of his colleagues were blacklisted, some were even executed, and ‘those who survived lived in an atmosphere of total suspicion’ (Kozulin 1984, p. 22).

However, as numerous authors have pointed out, the apparent change in Luria's scientific direction initially amounted to little more than a change in terminology.

Published papers and official records must be taken not at face value but rather as rough material for subsequent distillation and decoding. (Kozulin 1984, p. 1.)

In the case of Luria, it is not quite clear whether his renunciation of psychoanalysis in the 1930s was a result of, or a form of resistance against, the silencing of the topic. (Kozulin 1984, p. 89)

15 The charge that psychoanalysis ‘biologized’ mental life is ironical in view of the later direction Luria's research followed. Sacks (1990, pp. 188-189) writes: ‘There is, indeed, a fascinating contrast here to Freud. Freud started as a biologist, a neurologist, and only later moved up to mental life, to the psyche; whereas Luria started as a cultural relativist and a psychologist - with a predominantly social-developmental orientation, and only later moved down into neuropsychology and biology… It was only when Luria came to grasp the biological aspects firmly - he liked to speak here of the ‘neurodynamics’ of nervous activity, as analogous to the ‘psychodynamics’ of which Freud was speaking - that he was able to achieve the twofold unity which he had so long needed and sought. It was only at this juncture that the ‘double science’ of neuropsychology came into being, as an enterprise analogous to psychoanalysis.’

16 This might explain why Luria's (1940) article on psychoanalysis combined a neutral description of its historical development and a favourable estimation of its conceptual, methodological and clinical importance, with a blistering critique of its ideological premises.

In short, Luria always managed to maintain professional integrity within his discipline, while adapting himself to the requirements of the authorities without. The subtle combination of inner autonomy and outward compliance has been a characteristic feature... in Luria's response to... Stalinism. (Joravsky 1974, p. 24)

A survey of Luria's subsequent publications clearly demonstrates that the basic conceptual and methodological debt that he owed to psychoanalysis was merely ‘buried under layers of ideological verbiage’ (Kozulin 1984, p. 1). This explanation accounts for the fact that, despite his public disavowal of psychoanalysis and all that it stood for (and it is interesting that this ‘disavowal’ took the form of a verbatim recital of the Party line), Luria continued to pursue the same scientific goals, using the same fundamental methods, in his neuropsychological period.

This is evident from the fact that, although the word ‘psychoanalysis’ no longer appeared in Luria's publications (which reported experimental studies of free association and clinical studies of the development of various mental functions in normal and abnormal children), these publications continued to be cited in Grinstein's (1956-1975) Index of Psychoanalytic Writings (e.g. Luria 1929, 1932a, 1936, 1961, 1968a). The continuity is perhaps most obvious in Luria's (1932a) book on The Nature of Human Conflicts. This book was based directly upon the researches that he had initiated while he was a member of the Russian Psychoanalytical Society; but the name of Freud and the subject of psychoanalysis were scrupulously avoided in its pages. Instead, Luria now conceptualized his findings within a Jacksonian framework - as Freud (1891b) had done before him. He particularly emphasized Jackson's theory in that:

the higher stratum of the nervous apparatus was inhibitory, restraining the primitive reactions of the older cerebral systems; this included the restraining and organizing role of the morphologically higher strata of the apparatus as well as the analogous role of the higher functional systems, creating the complex processes of biological and historical evolution. Jackson... working on aphasia, pointed out the primary organizing role played by speech on the voluntary and emotional disturbances occurring when these complex functional strata were injured. This exposition is of vital importance to us. (Luria 1932a, p. 370)

Any psychoanalyst would recognize this model as his own. Thus Michael Cole, who studied under
Luria and corresponded with him from 1962 until his death in 1977, wrote:

> Read in the proper way, The Nature of Human Conflicts is a unique source of information; but read in isolation from his 1925 article on psychoanalysis... this book seems opaque because of its many theoretical positions. (Cole 1979, p. 208)

The same could be said for Luria's other post-psychoanalytic writings:

17 Other work by Luria in the immediate post-psychoanalytic period included a field-study of cultural differences in thinking, conducted with a group of colleagues, in which Luria himself concentrated on 'visual thinking' and 'self analysis and evaluation of other individuals at various stages of personality development' (Luria 1932c, p. 242). See also Luria & Vygotsky's (1930) intriguing book on *Ape, Primitive Man and Child*, which has only very recently appeared in English translation 1992.

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> When I correlated the content and style of his writings with the general political and social controversies of the day, the otherwise disjointed, zigzag course of Alexander Romanovich's career began to make sense. His interest in psychoanalysis no longer appeared a curious anomaly... his apparent shifts of topic at frequent intervals, all took on the quality of an intricate piece of music with a few central motifs and a variety of secondary themes. (p. 198)

Other authors have recognized the continued influence of Freud in Luria's life-long commitment to the clinical method. In his autobiography, Luria identified his approach with the clinical-descriptive tradition in neurology:

> The medicine of previous years had been based on the effort to single out important syndromes by describing significant symptoms. This activity was considered essential both for diagnosis and for treatment. With the advent of the new instrumentation, these classical forms of medical procedure were pushed into the background. The physician of our time, having a battery of auxiliary aids and tests, frequently overlooks clinical reality. Observation of patients and evaluation of syndromes have begun to give way to dozens of laboratory analyses which are then combined by mathematical techniques as a means of diagnosis and as a plan of treatment. Physicians who are great thinkers have gradually disappeared. It is rare now to find a really good physician who is equally adept in observing, judging, and treating. I do not intend to underrate the role of instrumentation in medicine. But I am inclined to strongly reject an approach in which these auxiliary aids become the central method and in which their role as servant to clinical thought is reversed... In the previous century, when auxiliary laboratory methods were rare, the art of clinical observation reached its height. One is unable to read the classical descriptions of the great physicians, J. Lourdat, A. Trousseau, P. Marie, J. Charcot, Wernicke, S. Korsakoff, Head, and A. Meyer, without seeing the beauty of art in science. Now this art of observation is nearly lost. (Luria 1979, pp. 176-177)

We have seen above that Freud, too, was deeply influenced by this tradition. In attempting to understand the above passage, Oliver Sacks (1990, p. 185) wrote that he himself was 'ineluctably drawn to [Luria's] earliest enterprises - his writing to Freud, at the age of 19; his founding, with Freud's encouragement, a psychoanalytical society in Kazan; and his first book, written as a youth of 20, an appreciation and critique of psychoanalysis'. Similarly, Luciano Mecacci wrote that '[Luria's] clinical approach to the study of neuropsychological disorders undoubtedly sprang from his early experience in psychoanalysis in the 1920s' (1988, p. 268). Mecacci continued:

> Luria's involvement with psychoanalysis was deeper and more complex than he cared to show... As anyone who saw Luria at work at the Burdenko Institute of Neurosurgery in Moscow would have noted, his approach to patients was purely clinical, closer to the psychoanalytic style than that of the experimentalistic attitude towards behavior. He had no fixed schedule for interviewing and testing a patient, but he employed a freeassociation technique, selecting the questions and the test trials according to what emerged in the session. Finally, this mode of neuropsychological investigation was
unique with each patient, and might not be replicated with another patient… The neuropsychological 'portrait' that emerged from this clinical investigation fit[ted] in with the conception of the historical character of an individual's psychological life. (1924b, p. 269)

Luria's abiding interest in psychoanalysis is attested to directly by other friends and colleagues. Oliver Sacks, for example, states the following (personal communication, 17 March 1987):

*I can give you only one direct quotation from Luria bearing on his (later) attitudes to psychoanalysis. In December '75 I sent him a tape of (the verbal and vocal ejaculations of) a patient of mine with severe Tourette's syndrome. Among these, but ejaculated with such speed as to seem at first a meaningless noise, was the word 'Verboten', uttered in a harsh (indeed parodied) 'Teutonic' voice, and at times (and in a manner suggestive of) self-recrimination. This had, it later turned out, been spat out by the patient's Germanspeaking father whenever his son showed 'impermissible' tics and impulses. The confirmation of this, indeed the following up of it, was initiated by Luria's letter, in early '75, when he suggested that I study '... the introjection of father as tic'. (I will have to pull out and xerox the original letter.)*

*I think Luria said, or felt able to say, in letters a good deal that he felt (externally or internally) unable to say in print - and this made me feel that he was still, at least, sympathetic to psychoanalysis as a tool and dynamic description of value.*

This insight into Luria's private beliefs makes sense of his abiding commitment, in his later work, to the methodological and theoretical principles that originally attracted him to Freud and psychoanalysis. These principles include, amongst other things (a) the priority of psychological analysis in mind-brain correlation, (b) the single-case approach, with its emphasis upon qualitative-descriptive methods of investigation, and the method of syndrome analysis in particular, (c) the appreciation of the dynamic nature of mental life, (d) the recognition of the intimate connection between mind and body, and (e) the developmental and hierarchical conceptualization of mental structure.
However Luria was not merely a crypto-psychoanalyst, nor can all of his later work be reduced to his early interest in psychoanalysis. There is no doubt that there were other influences besides Freud upon Luria's thinking. Vygotsky is the outstanding example. Also, Luria's interest in psychoanalysis may itself have been determined by prior, more fundamental interests, which can be traced throughout his work. Luria himself suggested this in his autobiography, where he stated that he was attracted to psychoanalysis because he was searching for a psychology which could bridge the conflict between descriptive (ideographic) and explanatory (nomothetic) science (see Luria 1979, pp. 21-23; cf. Luria 1925a).23 Also, although Luria's rejection of psychoanalysis was externally motivated, his personal commitment to Marxism-Leninism was such that he may well have gradually internalized the Party line. Personal communications from some Western colleagues suggest that his mature attitude to psychoanalysis, even in private conversation, was sometimes ambivalent.24 These issues raise extremely complex questions which extend beyond the scope of the present paper. However the intricate thread of psychoanalytic influence through the ‘disjointed, zigzag course’ of Luria's career is secondary in the present context to another aspect of his work, namely, his basic contributions to neuropsychology. The following pages demonstrate that Luria's fundamental concepts and methods with regard to brain-behaviour relationships were entirely compatible with those of Freud.

It is a remarkable fact that Luria's work in this field began where Freud's left off - with a study of aphasia. He began to investigate the problem shortly after he resigned from the Russian Psychoanalytical Society25 and he eventually published his findings in a celebrated monograph, Traumatic Aphasia (Luria 1947). He introduced this work with a discussion of the historical background.

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23 In an essay on ‘Luria and “Romantic Science”’, Sacks (1990, p. 188) writes: ‘Here, it seems to me, is the key to Luria's early enthusiasm for psychoanalysis, for Freud; here, too, the permanent heuristic effect of Freud on his thought, whatever reservations and differences were later to appear. Freud offered a principle - the general principle Luria needed.’

24 Karl Pribram writes that Luria considered Freud's 1895 ‘Project’ to be ‘seminal and viable’ but felt that ‘some of Freud's later work was not of this calibre and in general wanted therefore to be disassociated from it’ (personal communication, 16 June 1987). Michael Cole writes that ‘There is no doubt in my mind that Luria was both very sympathetic of psychoanalytic ideas and critical of certain shortcomings in the enterprise’ but also that ‘there was severe external pressure to renounce Freud’ (personal communication, 1 April 1989). Jason Brown writes that Luria was ‘antagonistic [to psychoanalysis], rather like most Soviet neuropsychiatrists’ although his work ‘seems to have something in common with [Freud's] topographic theory’ (personal communication, 11 March 1992). Luciano Mecacci describes an occasion on which a reference by a Western colleague to The Nature of Human Conflicts angered Luria - ‘perhaps because it belonged to an old story’. Mecacci suggests that Luria was attracted to psychoanalysis because he wished to study ‘the human psyche through the human being in itself and not in its distinct psychological elements’. He concludes: ‘I think that Luria was not directly influenced by psychoanalysis from a strict theoretical point of view, except for what regards Freud's theory of aphasia, but this is another point’ (personal communication, 24 March 1992). Mecacci's last point is of considerable interest in the present context (see below).

25 Luria (1979, p. 136) states that he defended a thesis on aphasia for his medical degree.

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After a brief review of the classical theories of Broca, Wernicke, Lichtheim and others, he concluded that the localization of complex psychological processes in discrete brain ‘centres’ was untenable. Luria, referring to Jackson, distinguished between the localization of symptoms and the localization of functions. Psychological functions, he insisted, are not ‘lost’ with focal brain lesions, rather they are disorganized in complex, dynamic ways. He applauded Jackson's anti-localizationist, evolutionary approach to the problem - and (in a glaring omission of Freud) noted that his ideas were not taken up by his colleagues until well into the present century. However, Jackson's views, he argued, resulted in an excessive swing away from localization, to psychological and biological theories which neglected the unequal role played by different parts of the brain. Luria, in his own study, aimed to bridge the divide between localizationism and equipotentialism.

The solution which Luria (1947) proposed was almost identical to the one that Freud adumbrated in
He conceived of speech and language as products of a hierarchical functional system, subserving a psychological process, with a complex genesis and structure, which occupied the space *between* the basic sensory-motor centres. A functional system of this sort, he argued, could not be localized in the conventional sense of the word. However, its elementary component parts - the ‘cornerstones’ of the system, in the terminology of Freud - *could* be localized:

‘Localization of function’ in this case becomes another problem, viz. the problem of the dynamic distribution of functional systems in central regions of the nervous system and especially in the cerebral cortex. Instead of conceptions of ‘centres’ for complex psychic processes there arise the concepts of dynamic structures or constellations of cerebral zones, each of which comprises part of the cortical portion of a given analyser and preserves its specific function, while participating in its own way in the organization of one or another form of activity. (Luria 1947, p. 20)

Lesions within the speech field affecting the different component zones (sensory-motor analysers) lead to the characteristic aphasic syndromes. In the conceptualization of these syndromes, however, the secondary effects of the lesion upon the functional system as a whole also have to be considered. Luria went to great lengths to emphasize that this system subserved a *psychological* function, and that its symptomatology therefore required a fully psychological analysis. On this conceptual basis, he examined the structure of aphasic disturbances in large numbers of patients, and correlated the disturbances with the underlying lesions. He then proposed a new classification of the aphasias, based on a new theory of speech, in which he took care to localize only the *elementary component parts* of the functional system, and not the functional system of speech as a whole.

The similarities between this model and Freud's (as reviewed above) will immediately be obvious to the reader. Consider, for example, the striking similarity between Freud's (1900a) suggestion that we view the mind as a complex optical instrument in which psychical locality corresponds to an ideal point in which no tangible component of the apparatus is situated, and the following statement by Luria: ‘all attempts to postulate that… images, or ideas, could be found in single units of the brain were as unrealistic as trying to find an image inside a mirror or behind it’ (Luria 1987, p. 489).

However, there was one important difference between the two models. Whereas Freud believed that only the primary sensory-motor functions which lie at the periphery of the speech apparatus could be localized, Luria showed that *every stage* in the complex psychological process of speech could be localized, so long as one respected their fundamentally *dynamic* nature. He localized not only the peripheral components, but all components of the speech process, including those performed by structures lying deep within the apparatus. This represented a major advance.

The question arises as to how Luria managed to achieve this step - which Freud had always acknowledged must be possible in principle, but which he could not execute in practice. There were two essential factors. First, Luria's (1947) work was based on *a more advanced theory of the internal psychological structure of speech* than Freud's (1891b) was. The details of Luria's neurolinguistic theory itself are not elaborated here (see Luria 1976a, for a comprehensive account). However, it is important to recognize that his approach to the problem was entirely compatible with Freud's basic approach. Freud (see above) had always argued that it was essential to gain a comprehensive understanding of the internal psychological structure of any mental process, before it would be possible - or even useful - to localize it.

The second advance that Luria introduced was *a methodological* one, that is, he adapted the clinico-anatomical method to accommodate the essentially dynamic nature of the mental process. This, the reader will recall, was the second step - indeed the breakthrough - that Freud had always insisted was necessary before psychoanalysis could be rejoined with neuroscience. In view of the obvious importance of this advance for present purposes, Luria's methodological proposals will be examined in some detail below. However, the problem of speech is no longer the sole concern, for Luria applied this same method to a wide range of human mental functions (his findings are summarized in Luria 1966, 1973, 1976b, 1980).

Luria's method involved two stages, *(1) qualification of the symptom* and *(2) syndrome analysis*. He described the first stage as follows:

*In order to progress from establishment of the symptom (loss of a given function) to the localization of the corresponding mental activity, a long road has to be travelled. Its most
important section is the detailed psychological analysis of the structure of the disturbance and the elucidation of the immediate causes of collapse of the functional system, or, in other words, a detailed qualification of the symptom observed. (Luria 1973, p. 35)

The purpose of this ‘detailed psychological analysis’ is the formulation of an hypothesis as to the nature of the fundamental disturbance underlying the manifest symptom:

The investigator's immediate task is to study the structure of the observed defects and to qualify the symptoms. Only then, by work leading to the identification of the basic factor lying behind the observed symptom, is it possible to draw conclusions regarding the localization of the focus lying at the basis of the defect. (Luria 1973, p. 38)

Another passage is quoted here in which Luria describes this aspect of his method:

Symptoms evoked by disturbances of different factors have complicated structures and can have different causes. For this reason, symptoms must be carefully analysed and ‘qualified’. The ‘qualification of the symptom’ depends on a careful analysis of the patient's defects. This is the basic goal of the Soviet neuropsychologist's approach. He is never content with merely finding a certain defect... The singling out of a symptom is not the end but rather the beginning of his work, which continues in depth. He attempts the elucidation of the disturbed structure, attempting to find distinct psychological factors underlying the symptom. This must occur first, in order to make the symptom's inner structure lucid and to allow the formulation of a hypothesis as to its relationship with a local brain lesion. (Luria & Majovski 1977, p. 963)26

It is easy to see how this part of Luria's clinical method, the ‘qualification of the symptom’, coincides with Freud's psychoanalytic approach. It could be said that Luria's method is to neurology what Freud's was to psychiatry. The aim was not to identify and designate the symptom, but rather to obtain a detailed picture of its internal psychological structure in order to elucidate its psychological mechanism.

This leads directly to the second stage of Luria's method, namely, syndrome analysis.

The qualification of the symptom is only the first step in the analysis of the cerebral organization of mental processes. So that the results of this analysis... can serve as the basis for reliable conclusions regarding both the structure of mental processes and their ‘localization’ in the human cerebral cortex, the next step must be from the qualification of the single symptom to the description of the complete symptom-complex or, as it is generally called, to the syndrome analysis of changes in behaviour arising in local brain lesions. (Luria 1973, p. 38)

It is necessary not only to identify the factor underlying the disturbance of the functional system under investigation (qualification of the symptom), but also to identify which other functional systems are disturbed by the same lesion and what factors underlie those other disturbances (syndrome analysis). This enables the investigator to identify the single, basic factor which underlies all of the symptoms produced by the one particular lesion. The common underlying factor, in turn, points to the basic function of that particular part of the brain. The next step is to study the different ways in which each functional system is disturbed by lesions to different parts of the brain. Lesions in different parts of the brain will disturb the functional system in different ways. The different types

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26 Luria was, incidentally, fully aware of the fact that a psychological analysis of this sort is, of necessity, theory-driven (Luria & Majovski 1977, passim).
In this way, the component parts of each functional system are identified and localized in the tissues of the brain. This process represents something quite different from the direct localization of whole mental faculties (i.e. the classical clinico-anatomical method):

It will easily be seen that syndrome analysis sheds considerable light on the cerebral organization of mental processes and also gives considerable insight into their internal structure, something which for many centuries psychologists have been unable to do... The fact that every complex mental activity is a functional system which can be disturbed in different components and which can be impaired by brain lesions in different situations (even though it is impaired differently) means that we can get closer to the description of the factors composing it and thereby discover new ways of neurophysiological analysis of the internal structure of mental processes... From all the foregoing remarks it will be clear that the use of observations on the changes in mental processes arising in local brain lesions can be one of the most important sources of our knowledge of the cerebral organization of mental activity. However, the correct use of this method is possible only if the attempt is resisted to seek the direct localization of mental processes in the cortex, and only if this classical task is replaced by another - by analysis of how mental activity is altered in different local brain lesions and what factors are introduced into the structure and complex forms of mental activity by each brain system. (Luria & Majovski 1977, p. 42)

Luria described this method as a ‘dynamic localization’ of function, which he distinguished from the ‘narrow localizationism’ of classical neurology. The way in which his approach differs from the classical method of clinico-anatomical correlation is best conveyed by the following analogy:

Most investigators who have examined the problem of cortical localization have understood the term function to mean the ‘function of a particular tissue’... It is perfectly natural to consider that the secretion of bile is a function of the liver and the secretion of insulin is a function of the pancreas. It is equally logical to regard the perception of light as a function of the photosensitive elements of the retina and the highly specialized neurons of the visual cortex connected with them. However, this definition does not meet every use of the term function. When we speak of the ‘function of respiration’, this clearly cannot be understood as the function of a particular tissue. The ultimate object of respiration is to supply oxygen to the alveoli of the lungs to diffuse it through the walls of the alveoli into the blood. The whole process is carried out, not as a simple function of a particular tissue, but rather as a complete functional system, embodying many components belonging to different levels of the secretory, motor, and nervous apparatus. Such a ‘functional system’... differs not only in the complexity of its structure but also in the mobility of its component parts. (Luria 1979, pp. 123-124)

Freud, too, recognized that it was only possible to localize elementary perceptual processes by the classical clinico-anatomical method. The essence of Luria's advance was to devise a method which could accommodate the more complex processes, i.e. those that occur deep within the mental apparatus. This method clearly is, as Freud insisted it had to be, capable of accommodating the dynamic nature of complex mental processes.

In the period of 30 years during which Luria applied his method of ‘dynamic localization’ to human mental processes, he elucidated the neurological organization of many complex mental functions. In a book written shortly before his death, Luria (1973) summarized his findings under the following chapter-headings: 'Perception', 'Movement and Action', 'Attention', ‘Memory’, ‘Speech’ and ‘Thinking’. There is much in these chapters that is of interest to the psychoanalyst. However in the final, concluding chapter Luria wrote the following:

Neuropsychology is still a very young science, taking its very first step, and a period of thirty years is not a very long time for the development of any science. That is why some very important chapters, such as motives, complex forms of emotions and the structure of personality are not included in this book. Perhaps they will be added in future editions. (1973, pp. 341-342)

Sadly, Luria did not live to produce future editions. Moreover, after his death (in 1977) neuropsychology developed in a distinctly different direction and returned to the academic psychological
traditions which Luria (and Freud) had always opposed (see Luria & Majovski 1977). A reunion of Luria’s method with its psychoanalytic roots would extend Luria’s approach into the area of motives, complex forms of emotion and the structure of personality.

This brings us back to the present.

The Future of Psychoanalysis in Relation to Neuroscience: A Methodological Proposal

Neuropsychological methods have generally not been applied to those aspects of mental life that were explored by Freud, that is, to the deep structure of human subjectivity. This trend led Sacks (1984) to remark recently - in a very apt turn of phrase - that ‘neuropsychology is admirable, but it excludes the psyche.’ He elaborated:

Neuropsychology, like classical neurology, aims to be entirely objective, and its great power, its advances, come from just this. But a living creature, and especially a human being, is first and last... a subject, not an object. It is precisely the subject, the living ‘I’, which is excluded [from neuropsychology]. (Sacks 1984, p. 164)

Very recent years have spawned concerted attempts to redress this imbalance; there has been a flurry of neuropsychological studies of the neural substrata of emotion. However, it is difficult to relate the findings of these investigations to the field of psychoanalysis. They were conducted within a conceptual and methodological framework which is completely alien to psychoanalysis. That is, they were conducted without reference to the repressed unconscious determinants of personality, motivation and emotion. Heilman and Satz (1983, p. 1) for example, in the introduction to their book on the neuropsychology of emotion (which was one of the first in the series of recent books in this field) wrote that psychoanalytic explanations of emotion ‘do not address the physical state of the brain’ and consequently ‘are not directly relevant to neuropsychology and, therefore, are not discussed in this book’. The findings of such research can only be correlated with psychoanalysis by means of indirect translations, akin to those which were employed by Freud in his ‘Project’. This raises all the insoluble problems of a speculative methodology.

Clearly, what is required is a neuropsychological method which is capable of accommodating both the object of psychoanalytic investigation (namely unconscious subjectivity) and that of neuroscience (namely brain structure and function), and one which is also capable of correlating these two things, without violating the conceptual premises of either discipline.

What I am recommending is simply that we now use Luria’s method to study the neurological organization of those mental functions (personality, motivation and complex emotion), the psychological structure of which we have got to know so well with our psychoanalytic method of investigation. We will never know how these, the most important and interesting by far of all human mental functions, are represented in the tissues of the brain unless we investigate the relevant psycho-physical relationships directly.

In the first part of this paper we saw that Freud himself always believed that a correlative study of this sort was feasible in principle (although it was impossible in practice, given the limitations of the methods.
available to him at that time). He was well aware that every mental process must somehow be represented as a physiological process which occurs in the tissues of the brain; but he also held to the view that it was an error to localize complex mental faculties within circumscribed neurological ‘centres’. The reasoning behind this point of view has been reviewed already, but his essential objection was that human mental processes are dynamic (i.e. ‘virtual’) entities, which cannot be correlated isomorphically with the concrete, static structures of cerebral anatomy. He believed, moreover, that, even if a localization of conscious processes was possible, it would still ‘give us no help towards understanding them’ (1940a, pp. 144-5). He concluded that it would not be possible to understand conscious phenomena in neurological terms until their unconscious

\[\text{psychological substructure} \] had been laid bare. To do otherwise would be akin to trying to map out the circuitry that generates an image on a computer monitor without reference to the underlying programme of algorithms. Freud therefore devoted his scientific energies to the latter (purely functional) task, and deferred the correlative (neuro-psychological) task to future investigators, anticipating methodological advances.

In the second part of this paper I argued that the required advances have now occurred. Luria's modifications of the clinico-anatomical method (the method that Freud originally rejected) have made the correlation of dynamic mental functions with their neuroanatomical substrata possible, in a manner which accommodates both of Freud's major methodological objections. Luria's method of ‘dynamic localization’ recognizes that the underlying \textit{psychological} structure of a mental process needs to be clarified before it can be localized, and that these complex, \textit{dynamic} processes cannot be correlated isomorphically with static anatomical structures. The neurological representation of Freud's psychological concepts is therefore within reach. My methodological proposal flows from these facts.

Since Luria's method has never been applied to those deeper aspects of mental life which interest us most in psychoanalysis, and since these aspects of mental life have some unique attributes, a basic modification of Luria's method is required in order to extend it into the area of motives, complex forms of emotion and the structure of personality.

In studying these highly complicated functions, Freud discovered that they are not easily accessible to the scientific investigator. There is a fundamental difference between these functions and those of speech, calculation, perception, and so on, to which Luria's method has previously been applied. Powerful forces are at work which directly oppose the investigator's attempts to lay bare the personal, emotional and motivational contents of an individual's mind. Freud classified these powerful forces - which express themselves clinically in forms such as shame, guilt and anxiety - under the collective heading of ‘resistance’. Due to the phenomenon of resistance, even the primary observational data for a study of human emotion, motivation and personality are unusually difficult to obtain. These difficulties are compounded by the fact that the inferred \textit{causal determinants} of the primary observational data (i.e. the unconscious forces that regulate our subjective thought process) are not conscious. Freud discovered that any attempts to bring these deeper determinants to consciousness are opposed by the strongest resistances - and yet they are the very processes that are of the greatest interest to the investigator of the functions at issue. He therefore experimented with various techniques for overcoming these resistances (e.g. hypnotism and the ‘pressure’ technique), and on the basis of this experience he gradually developed the definitive psychoanalytic technique of \textit{free association}. Freud's justification for and explanation of this technique are carefully outlined in a number of places (e.g. 1900a, 1904a, 1910a, 1912e, 1913m, 1914d, 1923a, 1924f, 1925d, 1937d).

Both Freud and Luria always insisted that it was not possible to correlate a complex psychological process with its cerebral representation until the internal structure of that process was understood. Accordingly, the first step we have to take in our effort to discover the neurological organization of the human mental ‘i apparatus (as we understand it in psychoanalysis) is to dissect the internal \textit{psychological} structure of the various changes in personality, motivation and complex emotion which occur with damage
to different cerebral structures. Thereafter, the multiple underlying factors producing these symptoms and syndromes can be identified and each correlated with their anatomical 'scene of action'.

However, due to the forces of resistance described above, these factors cannot be revealed by conventional neuropsychological techniques. The psychometric tests and bedside techniques that neuropsychologists have traditionally developed for assessing the mental status of neurological patients were designed for the investigation of disorders of ‘surface’ cognitive functions - such as language, calculation and perception - functions which operate relatively independently of emotional resistances. The technique of free association was specifically developed for the elucidation of functions the internal structures of which are obscured by resistances. In order for us to lay bare the underlying psychological structure of the disorders of personality, motivation and complex emotion that afflict the neurological patient, therefore, the free association technique must be introduced into Luria's neuropsychological method.

This is not a radical departure from Luria's standard approach. But it is necessary to introduce this modification if we are going to take account of the fact that the internal structure of the mental functions that interest psychoanalysts most is largely inaccessible to conventional neuropsychological techniques. One cannot adequately elucidate the unconscious structure of a change in personality, motivation or emotion in a neurological patient by examining that patient at the bedside any more than one can do so with any other kind of patient (and still less by assessing him or her in a neuropsychological laboratory). In order to gain access to these deeper mental strata in any patient, whether the patient has a brain lesion or not, we need to get to know the patient as a person, by providing a reliable human relationship, in a professional setting, within which we can win their confidence by tact and understanding, and by gradually making them aware of their resistances. Then, having gained relatively free access to their private thoughts, feelings and memories, we may explore the way in which the internal (unconscious) determinants of those thoughts, feelings and memories unfold in the transference relationship, and test the hypotheses that occur to us in this regard in the form of appropriate interpretations. Only in this way, and by carefully observing the effects that these interpretations have upon the subsequent associative material, can we gradually elucidate the unconscious structure of the psychological symptoms that are the focus of our interest.

30 Cf. Mecacci's remarks to the effect that Luria's method is akin to free association (quoted above).

We all know that this is not the easiest way to elucidate a psychological syndrome, but we also know that it is the best available method when it comes to those deeper aspects of mental life which neuropsychology has left unexplored, but which have always been of central concern to us in psychoanalysis. In fact, the emotional resistances which conceal the internal structure of personality, motivation and complex emotion, probably explain why the neurological organization of these, the most important by far of all mental functions, has still not been systematically explored by the clinico-anatomical method. This is the scientific contribution that I believe psychoanalysis can make to neuroscience.

Ironically, we owe the development of a clinical procedure for analysing these deeper psychical strata to the fact that Freud abandoned neuroscientific methods of investigation, when he realized that they were (at that time) unable to accommodate the dynamic and ‘virtual’ nature of the mental process. Now it seems the time has come for us to re-introduce the fruits of his labours back into the neuroscientific field out of which they originally grew. In doing so - although I do not want to underestimate the enormity of the task before us - I believe that we will be able to gradually rejoin psychoanalysis with neuroscience, on a solid clinical basis, in a way that is beneficial to both fields, without ignoring any of the valuable lessons which the pioneers of psychoanalysis fought so long and hard to teach us.

What I am recommending, therefore, and what I believe will provide the essential cornerstone for a lasting integration of psychoanalysis and neuroscience on a valid basis, is a fully psychoanalytic investigation of patients with focal neurological lesions. In other words, I am recommending that we chart the neurological organization of the deepest strata of the human mind, using Luria's method of syndrome analysis, by studying the deep structure of the mental changes that can be discerned in neurological patients within a psychoanalytical setting.
This recommendation is now being put into effect by a number of small study groups, in New York, London, Frankfurt, Cologne and Vienna. Unfortunately, I do not have space here to illustrate this method by case examples, nor to demonstrate the results that it produces in terms of establishing correlations between our basic psychoanalytical functional concepts and the anatomy and physiology of the brain. Readers who are interested should consult KaplanSolms & Solms (1996, in press) and Solms (1995, 1998, 1999) for some illustrative case descriptions and preliminary reports of our findings.

I am very sorry I cannot go further into those details here. I nevertheless hope that I have managed to convey the essential points; and I hope also that I have been successful in my attempt to show that knowledge of the history and prehistory of our discipline can sometimes contribute to the solution of contemporary scientific problems of practical importance.

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Abstract
This paper begins with a review of the scientific and technical problems that led Freud to abandon neuroscience and to establish the new discipline of psychoanalysis. That review is followed by a consideration of some developments that took place in the neurosciences after Freud's death, due to the efforts of the Soviet neurologist, A.R. Luria. These developments seem to reverse the historical justification for an ongoing separation between psychoanalysis and neuroscience. Luria's early involvement with psychoanalysis is then described in detail, and its possible influence on his neuropsychological work is considered. The paper ends with the suggestion that a reintegration of psychoanalysis with the neurosciences is now possible, due to the fact that Luria has addressed all the major neuroscientific problems that Freud identified as the stumbling blocks to a viable neurological understanding of mental functioning. A methodological proposal for future neuro-psychoanalytical interdisciplinary research is advanced on the basis of these historical considerations. In this paper I want to show how knowledge of the history (and prehistory) of our discipline can sometimes contribute to the solution of contemporary scientific problems of practical importance.


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