

## From Freud to Winnicott

### Aspects of a paradigm change<sup>1</sup>

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The main purpose of this paper is to present a unified view on Winnicott's contribution to psychoanalysis. Although Winnicott is presently recognized as one of the great figures in the history of psychoanalysis it is evident that a systematic philological, historical and conceptual study of his writings is rare, in spite of some notable exceptions (see Abram 2007, 2008; Davis and Walbridge 1981; Ogden 1986; Phillips 1988). Moreover, we are still waiting for the publication of Winnicott's *Collected Works*,<sup>2</sup> and generally speaking, Winnicott research can hardly be compared with current Freudian scholarship. This situation is changing, particularly in Latin America, where Winnicott has become the most quoted psychoanalytic author after Freud (cf. Abadi and Outeiral 1997). Unfortunately, citation does not necessarily mean that the work is truly studied and understood.

My emphasis in this paper is on the nature of Winnicott's contribution rather than a focus on one or another of his many contributions to psychoanalysis. I aim to achieve this by conceptual analysis largely based on a study of the historical development of Winnicott's ideas. Winnicott himself recommended a historical approach to the understanding of his views on emotional development. In his posthumously published book *Human Nature*, after explaining some of his ideas on the imaginative elaboration of body functioning, he added:

The reader must form a personal opinion of these matters, after learning what is thought as far as possible in the historical manner,

which is the only way that the theory of any one moment [in personal development] becomes intelligible and interesting.

(Winnicott 1988: 42)

Here I suggest that 'interesting' means both personally appealing and theoretically important. Clearly this applies to any attempt to understand other parts of Winnicott's theory and indeed psychoanalysis in general:

Readers of analytic literature may easily become impatient if they take some statement of analytic theory and treat it as if it were a final pronouncement, never to be modified. Psychoanalytic theory is all the time developing, and it must develop by natural process rather like the emotional condition of the human being that is under study.

(Ibid.: 46)

It would be very tempting to try to develop this Winnicottian 'natural process' view of the origin of the scientific attitude and of the growth of scientific knowledge as Winnicott begins to do in the first chapter of *Human Nature*, but instead, I shall limit myself to applying an already existing model of natural growth of science expounded by Thomas S. Kuhn.<sup>3</sup>

There is one straightforward reason to appeal to Kuhn's theory in the present context: both Winnicott and Kuhn were strongly influenced by Darwin. Winnicott is indebted to Darwin for his view that '... living things could be studied scientifically, with the corollary that gaps in knowledge need not scare me' (Winnicott 1996: 7). Kuhn, in turn, learned from the British biologist that the growth of science is a struggle between rival paradigms for survival in scientific communities. The aim of that struggle is not towards something like the final truth, but the temporarily greater problem-solving efficiency of scientific knowledge.<sup>4</sup> This shaky goal is achieved by dramatic changes in established scientific world-views or, more technically, by Gestalt switches in scientific paradigms commonly called 'scientific revolutions.'

Following Kuhn, I shall therefore be discussing the paradigm switch introduced by Winnicott into the psychoanalytic discipline. To present this with clarity I shall start with an outline of Freud's paradigm that resulted from 'normal research,' and show how it was the emergence of anomalies that subsequently brought about a crisis that

triggered Winnicott's revolutionary research. My main thesis here is that Winnicott's research concluded by Winnicott introducing a new paradigm for psychoanalysis, i.e. new guiding problems and a new conceptual framework that would enable him to solve the anomalies he discovered. My conclusion is that Winnicott was a revolutionary thinker and that he paved a new way for scientific research and practice in psychoanalysis. Furthermore, I aim to show how he achieved a great deal of such research, without ever intending that his alternative framework or his results were 'final pronouncements.'<sup>5</sup>

I am not the first one to speak of Winnicott's paradigm and several authors have used Kuhn to study the history of psychoanalytic thought (Modell 1968; Levenson 1972; Lifton 1976). Greenberg and Mitchell view the history of psychoanalysis as

the dialectic between the original Freudian model,<sup>6</sup> which takes as its starting point the instinctual drives, and an alternative comprehensive model initiated in the work of Fairbairn and Sullivan, which evolves structure solely from the individual's relationships with other people.

(Greenberg and Mitchell 1983: 2)

The results reached by the latter two authors are, however, severely limited by the fact that they only considered the metaphysical component of psychoanalytic theory, which completely leaves aside concrete problem-solutions, i.e., the so-called 'exemplars' which are the main components of Kuhn's model of analysis.

Kuhn's concept of the exemplar is applied by Judith Hughes whose attempt is to depict 'the paradigms which constitute psychoanalytic theory' by describing the 'Freudian paradigms' and scrutinizing their 'transformation' in the work of Klein, Fairbairn and Winnicott (Hughes 1989). To that end, she not only analyzes specific theoretical difficulties in paradigm formation, but also pays attention to clinical issues and clinical practice, which forced the re-shaping of the psychoanalytic domain within the British Psychoanalytical Society. With Klein, she concentrates on the case of 'Richard,' and with Winnicott, the case history entitled *The Piggie*. 'The consulting room,' writes Hughes, 'has, after all, provided the empirical base for the psychoanalytic enterprise, and nowhere has it been more apparent than in Britain' (Hughes 1989: 176). More recently, a view congenial to applying Kuhn's approach to the history of psychoanalysis has been adopted by

Joyce McDougall (McDougall 1996: 226) and Jan Abram has referred to the Kuhn-based paradigm switch thesis as an arguable alternative interpretation of Winnicott's contribution to psychoanalysis (Abram 2008: 1205).<sup>7</sup>

In 1988, Adam Phillips approached Winnicott in a way which, although not presented in Kuhn's terms, uses a very similar language. Phillips admitted, without the ambiguities which spoil so many other accounts, that Winnicott introduced 'important innovations' in psychoanalytic practice and technique which represent, despite Winnicott's 'disingenuous' disguises, 'radical departures from Freud.' The main departure selected by Phillips consists in the observation that Winnicott 'would derive everything in his work, including a theory of the origins of scientific objectivity and a revision of psychoanalysis, from the paradigm of the developing mother-infant relationship' (Phillips 1988: 5). For Winnicott, says Phillips, the mother-infant relationship was becoming 'the primary model for the psychoanalytic situation' and the main 'source of analogy in his work' (*ibid.*: 87). Let me provide an example among many given by Phillips:

But whereas for Freud psychoanalysis was essentially a 'talking cure', for Winnicott, the mother-infant relationship, in which communication was relatively non-verbal, had become the paradigm for the analytic process, and this changed the role of interpretation in psychoanalytic treatment.

(*Ibid.*: 138)

Guided by the mother-baby paradigm, Winnicott was led to new questions and thus to new results. Examples of such questions 'rarely addressed in psychoanalytic theory' are the following: What do we depend on to make us feel alive or real? And: Where does our sense come from, when we have it, that our lives are worth living? Winnicott approached these issues, continues Phillips, by linking the 'observation of mothers and infants' with 'insights derived from psychoanalysis' (*ibid.*: 5–6). But it was not simply that Winnicott also enriched psychoanalysis with essential new insights which turned out to be incompatible with those of Freud, since they were 'rarely linked by him [Winnicott] with the place of the erotic in adult life.' For Winnicott, the 'crux of psychoanalysis' was the 'infant's early dependent vulnerability' in a two-person relationship with the mother, not 'the Oedipus complex – a three-person relationship.' Whereas Freud,

starting from the Oedipus situation, was interested 'in the adult's struggle with incompatible and unacceptable desires,' which put in danger their 'possibilities for satisfaction,' Winnicott, starting with the relationship of total dependence, treated these possibilities as 'part of a larger issue of the individual's possibilities for personal authenticity, what he [Winnicott] will call 'feeling real' (ibid.: 7). Working in that manner, and 'neglecting Freud's metapsychology' (Phillips 1993: 43), Winnicott evolved, during the 1940s, 'a powerful rival developmental theory to those of both Freud and Klein' (Phillips 1988: 97).

What have I to add to the above results? Firstly, a more systematic and precise account of the essential constitutive elements of Winnicott's paradigm and, secondly, a more detailed analysis of the process Winnicott went through in searching for these elements. To that end, I shall use, as previously stated, the word 'paradigm' not just in the common sense meaning of a model, but in the more technical sense defined by Thomas Kuhn in the Postscript of the second edition of his book *The Structure of Scientific Revolutions* (Kuhn 1970a). I shall also borrow Kuhn's general view on scientific research and on the growth of science.<sup>8</sup> In substance, I aim to produce a more accurate picture of Winnicott's procedures and contributions, which may serve as a blueprint for further research on his contribution as well as helping to avoid some not infrequent misunderstandings.<sup>9</sup>

### **Kuhn's view of empirical science**

According to Kuhn, normal, everyday science is a problem-solving activity guided by a paradigm. Scientific problems resemble puzzles in so far as they are thought of as having an assured solution within the adopted theoretical framework (Kuhn 1970a: 37). Socially important problems become scientific only after they have been reduced to puzzles, their solution depending exclusively on the ingenuity of practitioners trained in a paradigm. Scientists do not intend, and even refuse, to cope with every problem. 'Scientism' – the idea that science can solve all questions that are important for human kind – is a peculiar philosophical stance on science and not at all part of what scientists actually are aiming at.

Paradigms, presupposed in scientific puzzle solving, are of two kinds. Firstly, there are accepted examples of actual scientific practice which provide 'models from which spring particular coherent

traditions of scientific research' (ibid.: 10). In the Postscript to the second edition of his book, Kuhn calls these accepted models 'exemplars,' by which he means 'the concrete problem-solutions that students encounter from the start in their scientific education' (ibid.: 187). Secondly, paradigms are 'conceptual, theoretical, instrumental and methodological commitments that guide the scientific research' (ibid.: 42). In the Postscript Kuhn offered a more detailed analysis of this second concept of paradigm and specified that its main components are guiding empirical generalizations,<sup>10</sup> ontological models of the subject matter, authorized heuristic procedures (preferred or permissible analogies and metaphors) and finally, values or norms which define scientific practice by specific groups and provide their members with a sense of community (ibid.: 182–5). Exemplars and constellations of commitments, taken together, constitute the 'disciplinary matrix' of a scientific discipline.

Exemplars are the more important of the two. To start with, a science is not learned by becoming acquainted with verbal statements of laws or rules, but by being taught how to see new problems in the light of exemplars: 'That [scientific] sort of learning is not acquired by exclusively verbal means. Rather it comes as one is given words together with concrete examples of how they function in use, nature and words, learned together' (ibid.: 191). By saying that we learn 'nature and words' together, Kuhn implies that scientific groups with different paradigms live, in some sense, in different worlds and that they use language in essentially different ways. This in turn accounts for the incommensurability of theoretical statements and the absence of supra-paradigmatic criteria of truth and interpretation. Indeed, in order to be able to interpret a statement we must first be able to see a case of it, and this requires a paradigm for observing the case. The verbal interpretation, (being 'a deliberative process by which we choose among alternatives as we do not in perception itself') always comes second (ibid.: 194). The knowledge that is learned from paradigmatic examples is not 'explicit', but rather 'tacit.'

The change of paradigms for seeing the world is initially also a tacit, unintentional and even unconscious process. It resembles Gestalt switches, which happen 'suddenly' and 'involuntarily', and 'over which we have no control' (ibid.: 111, 194). The central aspect of Gestalt switches which are at the 'heart of the revolutionary process' (ibid.: 202) is 'that some of the similarity relations change' (ibid.: 200), which again implies the changes in the use of language. Kuhn writes:

Objects that were grouped in the same set before are grouped in different ones afterward and vice versa. [. . .] Since most objects within even the altered sets continue to be grouped together, the names of the sets are usually preserved. Nevertheless, even the transfer of a subset is ordinarily part of a critical change in the network relations among them. [. . .] Not surprisingly, therefore, when such re-distributions occur two men whose discourse had previously proceeded with apparently full understanding may suddenly find themselves responding to the same stimulus with incompatible descriptions and generalizations.

(Kuhn 1970a: 200–1)

Differences in responses to the same stimuli do not only mean that our world-view has modified, they also reveal that the world itself has suffered a change. These disagreements cannot be eliminated ‘simply by stipulating definitions for troublesome terms,’ nor can we resort to a ‘neutral language,’ for no paradigm independent of language exists. A paradigm change is, therefore, necessarily followed by a ‘communication breakdown.’ In such cases, translation from one scientific idiom to the other is a resource of dialogue, but not of consensus; moreover ‘it is threatening and is entirely foreign to normal science’ (ibid.: 203). It is clear that holding different paradigms scientists usually disagree on at least three points: (1) the list of problems that any candidate must be able to resolve in order to enter a paradigm; (2) the list of criteria for acceptable solutions; (3) the list of criteria for what exists, since, when a paradigm changes some things simply cease to exist and others start to exist. For instance, what was previously seen as a duck, was called, and has been a duck is now seen as, is called, and has become a rabbit. Under such circumstances, the procedure of translating does not lead us very far, because, according to the context, being a duck might indeed have a very different meaning from being a rabbit.<sup>11</sup>

The other important point is that science does not make progress in solving problems by applying theories and rules, but by seeing new problem situations in the light of exemplars: ‘Scientists solve puzzles,’ writes Kuhn, ‘by modelling them on previous puzzle situations, often with minimal recourses to symbolic generalizations’ (ibid.: 190). That brings us back to the thesis that scientific knowledge is embedded in shared exemplars rather than in rules, laws, or criteria of identification.

Guided by a specific way of seeing the world, scientists attempt 'to force nature into the pre-formed and relatively inflexible box which the paradigm supplies' (ibid.: 24). Kuhn adds:

No part of the aim of normal science is to call forth new sorts of phenomena; indeed those that will not fit the box are often not seen at all. Nor do scientists normally aim to invent new theories, and they are often intolerant of those invented by others. Instead, normal scientific research is directed to the articulation of those phenomena and theories that the paradigm already supplies.

(Kuhn 1970a: 24)

To summarize: in normal science scientists restrict their efforts to solve three kinds of problem: to determine significant facts, to match facts with theory and to articulate existing theories (ibid.: 34).

Why then, do paradigmatic changes occur at all? The answer is: when a crisis occurs, that is, 'a pronounced failure' of the old theory 'in the problem-solving activity' (ibid.: 74–5). Now, every paradigm is constantly confronted with anomalies, recalcitrant problems which should have been solved but were not. Usually, scientists leave such problems provisionally to the side and do not reject the paradigm because of this kind of failure. However, it also happens that some persistent anomalies may oblige a scientist to interrupt their normal research and pause over them and reasons may vary. They may become concerned about the absence of guiding generalizations, or about the impossibility of solving a particularly important social problem or a problem felt to be significant for technical and technological reasons (ibid.: 82). When anything like this happens, 'an anomaly comes to seem like more than just another puzzle of normal science' and the transition to crisis and to extraordinary science or to revolutionary research has begun. Kuhn describes the emergence of a crisis in the following way:

More and more of the field's most eminent men devote more and more attention to it. If it still continues to resist, as it usually does not, many of them may come to view its resolution as the subject matter of their discipline. For them the field does no longer look quite the same as it had earlier. [. . .] An [. . .] important source of change is the divergent nature of the numerous partial solutions that concerted attention to the problem has made available. [. . .]



Through this proliferation of divergent articulations (more and more frequently they will come to be described as ad hoc adjustments), the rules of normal science become increasingly blurred. Though there is still a paradigm, few practitioners prove to be entirely agreed about what it is. Even formerly standard solutions of solved questions are called in question.

(Kuhn 1970a: 82–3)

Finally, how are we to describe the progress achieved through scientific revolutions? The answer is that they will not be an approximation to the truth. Whereas normal science is cumulative, revolutions introduce new problem fields and incommensurable world-views. We have therefore to 'relinquish the notion, explicit or implicit, that changes in a paradigm carry scientists, and those who learn from them, closer and closer to the truth' (ibid.: 170). Scientific growth is not a process of evolution in the direction of an ultimate goal at all. In what terms then can we speak about the progress of science? Let's take an analogy inspired by Darwin: just as the evolution of species is a result of a natural selection of organisms 'more adapted' to the environment (and has no final goal set by God or by nature), so the evolution of scientific theories is a product of 'the selection by conflict, within scientific communities, of the fittest way to practice future science' (ibid.: 172). This evolution also has no final goal.

Not all sciences are mature enough to be able 'to work from a single paradigm or from a closely related set' (ibid.: 162). This kind of maturity is rather rare. Even in highly developed sciences we encounter competing paradigms at any given time (ibid.: 209). Moreover, one has to distinguish scientific communities that have achieved the mature paradigm stage from schools that are still in the 'pre-paradigm' period. During such a period individuals may be said to practise science, but 'the results of their enterprise do not add up to science as we know it' (ibid.: 163). Fact gathering, for instance, may occur, 'but it is far more nearly at random than the one subsequent scientific development makes familiar . . .' (ibid.: 15). Some data may be obtained from observation, others from experiments and still others 'from established crafts like medicine,' which is 'one readily accessible source of facts that could not have been casually discovered' (ibid.: 15). When the 'fundamental tenets of a field are once more at issue' and 'doubts are continually expressed about the very possibility of continued progress if one or another of the opposed paradigms are

adopted,' that is, during periods of revolution, scientific fact gathering usually regresses to a situation very similar to the pre-paradigmatic one. Cumulative scientific progress seems both obvious and assured only during periods of normal science (*ibid.*: 163).

### **Objections against the application of Kuhn's theory to review the history and structure of psychoanalysis**

Before applying Kuhn's view of science and scientific progress to Winnicott's contribution to psychoanalysis, I shall briefly address two possible objections to a Kuhnian reading of psychoanalysis in general. In the first place, it might be said that Kuhn's view only applies to natural sciences and therefore not to psychoanalysis, which is a science of man. This way of reading Kuhn is not without difficulties. It is true that for Kuhn it remains an open question 'what parts of social science have yet acquired such fully-fledged paradigms at all' (*ibid.*: 15). However, by saying this, Kuhn does not imply that there are no paradigm-like elements in social sciences. In fact, Kuhn observes:

. . . members of all scientific communities, including the schools of the 'pre-paradigm' period, share the sorts of elements which I have collectively labeled 'a paradigm'. What changes with the transition to maturity is not the presence of a paradigm but rather its nature. Only after that change is normal puzzle solving research possible.  
(Kuhn 1970a: 179)

Nor are we prohibited to speak of progress in disciplines different from natural sciences, or even in areas very remote from empirical research, such as theology and philosophy: 'The theologian who articulates dogma or the philosopher who refines Kantian imperatives contributes to progress, if only that of the group that shares his premises' (*ibid.*: 162). The real issue for Kuhn in discussing psychoanalysis and social sciences in general is the problem of transition from pre-scientific or pre-paradigmatic kinds of questions answering to the specifically scientific or paradigmatic way of problem-solving. This process can be studied in its own right, since it is constantly going on in several fields of Western culture. Current research 'in parts of philosophy, psychology, linguistics, and even art history' suggest, according to Kuhn, that these disciplines are looking for new paradigms (*ibid.*: 121 and 162).

In the Postscript of the second edition Kuhn stresses once again that his main theses about the structure of science and of scientific revolutions are applicable to many other fields as well: 'To the extent that the book portrays scientific development as a succession of tradition-bound periods punctuated by non-cumulative breaks, its theses are undoubtedly of wide applicability' (ibid.: 208). And he explains why it is so:

But they should be [applicable], for they are borrowed from other fields. Historians of literature, of music, of the arts, of political development, and of many other human activities have long described their subjects in the same way. Periodization, in terms of revolutionary breaks in style, taste, and institutional structure, has been among their standard tools. If I have been original with respect to concepts like these, it has mainly been by applying them to the sciences, fields which had been widely thought to develop in a different way.

(Ibid.)

As Kuhn says earlier in the text (ibid.: 92), it was indeed politics which provided him with the initial idea of revolution. What Kuhn did is nothing other than isolate features of problem-solving activity 'none necessarily unique to science' (ibid.: 209). This is why he cannot but agree with those who feel the need 'for comparative study of the corresponding communities in other fields.' The questions to be asked are:

How does one select and how is one elected to membership in a particular community, scientific or not? What is the process and what are the stages of socialization to the group? What does the group collectively see as its goals; what deviations, individual or collective, will it tolerate; and how does it control the impermissible aberration? A fuller understanding of science will depend on answers to other sorts of questions as well, but there is no area in which more work is so badly needed.

(Ibid.: 209–10)

Against my application of Kuhn's theory of scientific problem-solving to psychoanalysis it might be objected, in the second place, that Kuhn did not consider that psychoanalysis was a scientific activity at all, since, in an article written in 1970, he agreed with Karl Popper

who wrote that psychoanalysis 'cannot now properly be labelled a "science"' (Kuhn 1970b: 7).

A careful reading of Kuhn's article allows for several caveats to argue against this objection. To start with, the very phrasing of Kuhn's agreement with Popper indicates that it is restricted to the present, the implication being that though psychoanalysis is not a science now there is no reason for thinking that it could not become a science in the future. There is thus nothing intrinsically non-scientific in the project of psychoanalytic research.

This reading is confirmed by Kuhn's comparison of 'contemporary [sic] psychoanalysis' with 'older medicine' and with crafts and practical arts in general, such as astrology as it was practised in the more remote past by famous astronomers, including Ptolemy, Kepler and Tycho Brahe, and even with engineering and meteorology, as they were 'practised a little more than a century ago.' Kuhn writes:

In all these fields shared theory was adequate only to establish the plausibility of the discipline and to provide a rationale for the various craft-rules which governed practice. These rules had provided their use in the past, but no practitioner supposed they were sufficient to prevent recurrent failure.

(Kuhn 1970b: 8)

All the crafts mentioned were constantly searching for a more stable and effective paradigm. Indeed, writes Kuhn:

... a more articulated theory and more powerful rules were desired, but it would have been absurd to abandon a plausible and badly needed discipline with a tradition of limited success because these desiderata were not yet at hand. In their absence, however, neither the astrologer nor the doctor could do research. Though they had rules to apply, they have no puzzles to solve and therefore no science to practice.

(Ibid.: 9)

From this historical sketch, Kuhn extracts that the main consequence for psychoanalysis is that it is still unable to formulate puzzles of the kind which are currently being solved by normal science during normal research. The problem-situation of psychoanalysis is similar to that of medicine (engineering and meteorology in the recent past), and

to that of astrology, in earlier periods of Western culture. If, for that reason, it may be said that psychoanalysis resembles astrology, this does not imply that it must have the same destiny or that it cannot possibly come to formulate its own fully fledged paradigms to solve puzzles.

Kuhn's article contains an important remark about the similarity between the behaviour of scientists in pre-paradigmatic and revolutionary periods and that of philosophers in general. Kuhn understands that 'the reasons for the choice between metaphysical systems,' as described for instance by Popper, 'closely resemble' his own 'description of the reasons for choosing between scientific theories.' In other words, the main resemblance between paradigms consists in the fact that, in neither choice '... can testing play a quite decisive role' (ibid.:7): just as there are no second level criteria for choosing between rival metaphysical systems, there are no meta-scientific criteria for choosing between sets of scientific test-criteria.<sup>12</sup> The difference between science and philosophy is thus not a matter of decision-procedures for networks of commitments. Rather, it is due to the capacity of science to produce exemplars, that is, commonly accepted solutions of shared empirical or factual problems. Whereas philosophers remain always, so to speak, in a pre-scientific stage and never 'come down' to 'normal science,' scientists only go through this same kind of process in early phases of their disciplines or in periods of crisis. Since psychoanalysis is a new science, which is still trying to produce its full paradigmatic frame, it is only natural – and this seems to be the position of Kuhn – that it goes on making choices which are more like those which are currently practised by philosophers than like those which characterize mature sciences, and that therefore, it still lacks shared exemplars.

Now, Kuhn seems to be right as to the first point, but he is apparently wrong as to the second. It is simply not true that psychoanalysis does not have puzzles to solve. Psychoanalysis actually started (I shall come back to this point later on), by Freud's formulation of specific puzzles and by his solving them in a way that he himself and the psychoanalytic community in general considered extraordinarily fruitful in current psychoanalytic research and practice. My difference with Kuhn here is not conceptual but factual, the implication being that Kuhn was simply not familiar enough with what was and what is going on in psychoanalysis.

I trust that the way is now free to start a description of the (natural) process by which Winnicott found his paradigm related to Kuhn's

thesis. I shall proceed historically in the first place, by reconstructing the Freudian Oedipal, triangular, 'three-body' paradigm from which Winnicott started.<sup>13</sup> I shall then go on to examine the crisis that Winnicott fell into soon after he began his study of psychoanalysis. I shall suggest that this crisis was initially motivated by the result of Winnicott's observations of very early infantile psychic disturbances which seemed to go against the Freudian theory of sexuality (i.e. against the leading generalization of the Freudian paradigm). Secondly, Winnicott came to recognize that the problems of maladjusted children were not thought to be sexual and were, therefore, excluded from treatment by psychoanalysts; instead children were sent to institutions. Thirdly, related to the first two problems, the original Freudian setting showed up technical insufficiencies. In short, Winnicott's crisis was founded on all of the three main grounds stated and explained by Kuhn as the existence of a crisis.

Following the above suggestion I go on to show how Winnicott tried to find his way out of the crisis by making an alliance with Melanie Klein, but that he gradually came to the conclusion that Klein, and others (including Fairbairn), offered no solution to the observed problem areas. Subsequently, I shall re-construct the main steps of Winnicott's own revolutionary research that led him to propose a new non-Oedipal, dual or 'two-body'<sup>14</sup> paradigm, based on the infant-mother dual relationship. According to my perspective, Winnicott's main contribution to psychoanalytic theory and practice is seen as an attempt to overcome his particular crisis with psychoanalytic theory by developing a new theoretical matrix for psychoanalysis as a whole that would be capable of solving the problems which had led him and others into a cul-de-sac. It should be added that his achievement did not lose anything of his predecessors' and contemporaries' achievements that he saw as important.

### **Freud's Oedipal paradigm**

What are the main exemplars that classical psychoanalysts encounter in their formation and apply in their clinical practice? In a paper delivered in 1913 to a broad scientific audience, Freud characterized psychoanalysis by showing how it proceeds in explaining slips and dreams. Dreams, in particular, are regarded 'as normal prototypes of all psychopathological structures.' Anyone who understands dreams 'can also grasp the psy-

chical mechanism of the neuroses and psychoses' (SE 13: 172). In this statement, there is no special significance attributed to sexuality. Freud comes to that topic later on in the same paper, by saying that:

... at an early stage of its researches psychoanalysis was driven to the conclusion that nervous illnesses are an expression of disturbance of the sexual function and it was thus led to devote its attention to an investigation of that function – one which had been far too long neglected.

(SE 13: 180)

To that effect, it was necessary, in the first place, to develop the 'unduly restricted concept of sexuality, a development that was justified by reference to the behaviour of children.' Freud's final formula on the nature of neuroses was: 'The primary conflict which leads to neuroses is the one between the sexual instincts and those which maintain the ego' (ibid.: 181).

The important question is: what was the clinical material regarding the primary conflict that this formula was related to? In Kuhn's terms, what were the concrete clinical problems that the theory of sexuality was supposed to make intelligible and to solve? The unequivocal answer is not just slips or dreams, but all problems that arise for the child from what Freud called the Oedipus complex. This is the meaning of Freud's later statement, found in a footnote added in 1920 to the fourth edition of *Three Essays on the Theory of Sexuality*:

... it has justly been said that the Oedipus complex is the nuclear complex of neuroses, and constitutes the essential part of their content. It represents the peak of infantile sexuality, which, through its after-effects, exercises a decisive influence on the sexuality of adults.

(SE V11: 226, footnote)

A close study of Freud's research on sexual development leads to the conclusion, firstly, that Freud's theory of sexuality started simultaneously with discoveries in the clinical material of work with hysterics alongside Freud's self-analysis. These discoveries led and illustrated the existence of the Oedipal constellation and the theory of infantile sexuality. Secondly, the theory developed mainly by recognizing, to an ever-increasing extent, the importance of the Oedipus complex 'as the central phenomenon of the sexual period of early

childhood.’ In the same footnote that I have just quoted, Freud says: ‘With the progress of psychoanalytic studies the importance of the Oedipus complex has become more and more clearly evident.’ And he adds: ‘Its recognition has become the shibboleth that distinguishes the adherents of psychoanalysis from its opponents.’ (SE V11: 226, footnote).

By making a ‘shibboleth of the Oedipus complex,’ i.e. an ‘identification sign,’ Freud was specifying what Kuhn named the ‘exemplar’ that serves to establish the community of psychoanalysts. Freud’s identity criterion for psychoanalysis is a problem-situation that, in his opinion, has been solved in an exemplary manner by the constellation of psychoanalytic theoretical commitments. Thus, the psychoanalytic theory of sexuality reinforced Freud’s metapsychology. It was not long before Freudians started to use the Oedipus complex as a concrete rule for expelling dissident members. The example was provided by Freud’s theoretical separation from Jung. It is important to observe in the specifics of that argument that Freud’s only text in which he makes an attempt to prove the historical and material existence of the primal scene (i.e. the Oedipal situation) is ‘The Wolf Man’ which is a text directed explicitly to refuting Jung’s position.<sup>15</sup>

We have thus identified the main exemplar and the most important guiding generalization that constitutes a central part of the new ‘constellation of commitments’ by which Freud produced his revolution in the scientific research on sexuality and psycho-neuroses and created psychoanalysis i.e. the Oedipal conflict and its solution by means of the theory of sexuality.<sup>16</sup> In the constellation of commitments that constitute Freud’s disciplinary matrix there are three further elements that I wish to account for: his ontological model of man, his heuristic rules and his values. Very briefly, Freud’s ontology includes a number of suppositions, more precisely, ‘speculations’ about psychic forces and energies as well as those of the innate constitution of the mental apparatus.<sup>17</sup> As to Freud’s methodology and heuristics, they are based on the transference relationship, specific to psychoanalysis, combined with methods common to all scientific research: fact-gathering, formulation and testing of hypotheses (empirical generalizations). Freud also believed, as did all other members of the Helmholtz School of natural sciences, in some methodological tenets which, in essence, go back to Kant. Namely, that no empirical science can be complete without ‘auxiliary constructions,’ and that all explanations have to be dynamic explanations based on quantifiable forces. Furthermore, in



the case of human individuals, the interplay of forces takes place in an apparatus that is inherited and further developed. This methodological stance allowed for bold speculations which, for Freud, were based on a vast range of metaphors, taken mainly from biology and from both psychological and philosophical theories of consciousness.<sup>18</sup>

Finally, there is a set of values contained explicitly or implicitly in the Freudian paradigm. Just as any other inquiry that is guided by the scientific method, psychoanalysis is a never-ending search for empirical truth about clinical phenomena. And, as in all other sciences, the results achieved by psychoanalysis are essentially revisable in the sense that there is no final truth, no absolute true belief, since in science we can have only provisional beliefs, subject to correction.<sup>19</sup> Although he assumes a positivistic view of science,<sup>20</sup> Freud is obliged to work with heuristic speculations which are metaphysical in character, and he proceeds thus as a Kantian. Nevertheless, psychoanalysis, as a science, remains different from philosophy – in so far as it does not offer a general and final world-view but rather a way of attempting, step by step, to enlarge objective knowledge – as distinct from the arts and, particularly, from religion. As to the social utility of psychoanalysis, it is concerned with relieving unpleasure and pain caused by an excessive repression of desire, i.e. by the censored libido.<sup>21</sup>

It was within this disciplinary matrix that Freud produced a clinical psychology and a metapsychology. The first one is an empirical science that studies four main areas: sexuality, neuroses, psychic structures and social order. The second is a 'speculative superstructure' of the first. Whereas the theory of sexuality and other parts of clinical psychology may lay claims to empirical truth, metapsychological parts of psychoanalysis are introduced as mere conventions. For instance, instincts (*Triebe*) are conventions. Accordingly, metapsychology cannot be used as a foundation of clinical psychology: the only possible foundation of this kind of knowledge is clinical experience itself. Nevertheless, metapsychology was viewed by Freud as having great heuristic value through providing guide-lines for empirical (clinical) research and schemes for organizing results already obtained. To that effect, metapsychological hypotheses and speculations must be coherent with clinical experience and with conscious experience in general, as well with each other.<sup>22</sup>

Freud's metapsychology is a vast and sophisticated speculation about an unconscious scene of mental life that is seen to be inhab-

ited by entities analogous to conscious mental entities, for instance representations, impulses and desires. Mental processes that govern these entities (although they do not obey the same laws as those that govern conscious mental processes) are conceived as resulting from psychic forces that act in agreement with the principle of universal determinism. In that way, Freud transferred to the unconscious domain the general empirical, as well as metaphysical, properties of conscious states. Most of these elements, well known to the empirical psychology of his time,<sup>23</sup> are taken from the Kantian theory of subjectivity, which, as is well known to philosophers, was founded on a dynamic view of nature, and included the two basic forces of attraction and repulsion, and a theory of psychic structure. The Freudian dualism of forces appears to be an adaptation of the Kantian metaphysical dualism, and the main elements of his psychic apparatus are the Kantian faculties, now called agencies or instances for the purpose of psychoanalytic research.<sup>24</sup> Influenced by his medical training, Freud naturalized all these ingredients of the unconscious and even tried to construct a machine capable of producing the same effects as those observed in clinical practice and everyday life. In the initial version of Freud's metapsychology the machine was a biological one (cf. the Project of a Scientific Psychology). In the later version, formulated around 1915, the prevailing metapsychological model of the human being is a psychological machine, which appears to be inherited from Leibniz, Kant and others. At that period, Freud was speaking exclusively of psychic forces and of the 'psychic apparatus'.

There are several reasons why Freud's metapsychological speculations have to be carefully distinguished from his exemplar (the Oedipus complex) and his guiding generalizations (that belong to the theory of sexuality and its extensions). Firstly, exemplars are different from other commitments and, furthermore, are by far the most important elements of a disciplinary matrix. Secondly, empirical commitments should not be mixed up with ontological ones. Thirdly, these differences are important for the understanding of the history of psychoanalysis. As we shall see later, Winnicott's crisis was not triggered, in the first place, by problems related to Freudian metapsychology, but rather because the Oedipus exemplar (and theory of sexuality) did not always assist with the clinical problems that he happened to find important in his paediatric and psychoanalytic practice.

### **Winnicott's crisis**

The Oedipal paradigm proved itself extremely successful in dealing with a number of new problems, and the theory of sexuality served as the starting point for various extensions and applications of psychoanalysis. Firstly, and most significantly for psychoanalysis itself, it served to develop the theory of neuroses and of psychic disturbances in general (paranoia, homosexuality, fetishism.) Secondly, it helped in elaborating the theory of psychic development and of the structure of the psychic apparatus. Thirdly, it served as a starting point in the theory of society, religion and morals. Let me note that Freud ventured a very bold assertion about morals, namely, that 'Kant's categorical imperative is the direct heir of the Oedipus complex' (SE XIX: 167), which implies that the very essence of traditional morality was a derivative of human sexual life.

But the Oedipal paradigm was also confronted very soon with serious anomalies. Freud himself found one of them: the early pre-Oedipal relation of female children with their mothers. Melanie Klein followed this up and made the case for the existence of anxieties earlier than the fully developed phallic or genital Oedipus complex.<sup>25</sup> In the 1940s, Fairbairn added a new criticism to the Oedipal paradigm and indeed to the whole of Freud's libido theory (Fairbairn 1952).

However, according to my research, the first real challenge to Freud's Oedipal paradigm within psychoanalysis came from Winnicott. While still undergoing psychoanalytic training, Winnicott became 'astounded both by the insight psychoanalysis gave to the lives of children and by a certain deficiency in psychoanalytic theory' (Winnicott 1965: 172). He describes this deficiency in the following way:

At that time, in the 1920s, everything had the Oedipus complex at its core. The analysis of the psycho-neuroses led the analyst over and over again to the anxieties belonging to the instinctual life at the 4–5-year period in the child's relationship to the two parents. Earlier difficulties that came to light were treated in analyses as regressions to pre-genital fixation points, but the dynamics came from the conflict at the full-blown genital Oedipus complex of the toddler or late toddler age [. . .].

(Ibid.)

Winnicott makes the same point in a later autobiographical report about his learning process of psychoanalysis, phrased almost directly in Kuhnian terms:

When I came to try and learn what here was to be learned about psychoanalysis, I found that in those days we were being taught about everything in terms of the 2-, 3-, and 4-years-old Oedipus complex and regression from it.

(Winnicott 1989: 574–5)

While learning to see every psychic disturbance in the light of the Oedipus complex, Winnicott, who at the same time was a practising paediatrician, found himself in the following difficulty:

Now, innumerable case histories showed me that the children who became disturbed, whether psycho-neurotic, psychotic, psychosomatic or anti-social, showed difficulties in their emotional development in infancy, even as babies. [. . .] Something was wrong somewhere.

(Winnicott 1965: 172)

What is described here are the clinical problems that triggered Winnicott's revolutionary research, namely the disturbances which belong to the intended field of the Oedipal paradigm but which do not fit it. The Oedipal paradigm was not entirely wrong, it was even constantly confirmed, but it was insufficient; more precisely, it could not do all that Freud hoped it could do. Winnicott's first, and by far his most important, difficulty with Freudian psychoanalysis was thus about its shibboleth, not about metapsychology. In Kuhn's terms, what happened to Winnicott during his learning process is that he found a serious anomaly in the framework of the paradigm he was trained in. What is more, he found an entire field of problems that resisted the 'classical' psychoanalytic understanding and treatment.

After having made this discovery, although maybe not as a direct consequence of it, Winnicott was alone and found himself in between both the Anna Freudians and the Kleinian group post Controversial Discussions. In the 1920s and 1930s, he writes in 'D.W.W. on D.W.W' (Winnicott 1989) (Chapter 1), that the very existence of something like obsessional neurosis in a 16-month baby was simply

denied as a fact. It was rebuffed with the objection: 'But this can't happen.' Winnicott comments:

There wasn't an audience for that, because of the fact that to have an obsessional neurosis one would have to have had a regression from the difficulties of the Oedipal stage at 3. I know that I overdo the point but that was something that gave me a line. I thought to myself, I'm going to show that infants are very ill early, and if the theory does not fit it, it's just got to adjust itself. So that was that.

(Winnicott 1989: 575) (see Chapter 1)

We have thus identified the exact point at which Winnicott started to depart from Freud and initiated his revolutionary research which, as I argue here, concluded by the substitution of Winnicott's new mother-baby or two-body paradigm instead of the original Freudian Oedipal or three-body paradigm.

### **The attempt to find a solution in the 'learning area' of Melanie Klein**

Winnicott's first move, however, was to try to save the Oedipal paradigm. From the mid-1920s onward he gave 'many tentative and frightened papers to his colleagues,' in which he described samples of cases histories of emotionally ill babies 'that had to be reconciled somehow with the theory of the Oedipus complex as the point of origin of individual conflicts' (Winnicott 1965: 172). Yet Winnicott soon came to the conclusion that what he needed was a psychology of the newborn infant which would not try to reduce all problems just to 'castration anxiety and the Oedipus complex' (Winnicott 1958: 34). He felt 'that the psychology of the small child and of the infant is not so simple as it would at first seem to be, and that a quite complex mental structure may be allowed even in the new born infant' (Winnicott 1958: 34). But Winnicott did not know where to look for such a psychology. He stood quite alone, and without a guiding paradigm.

It was an important moment in Winnicott's life when James Strachey, his analyst at that time, sent him to Melanie Klein, who was also trying to apply psychoanalysis to small children (Winnicott 1965: 173). Winnicott took her a paper which presented an example of 'pre-Kleinian' child analysis which he realized on the basis of his own

analysis with Strachey. 'This was difficult for me,' remembers Winnicott, 'because overnight I had changed from being a pioneer into being a student with a pioneer teacher' (Winnicott 1965: 173).

Winnicott discovered very soon, however, that the psychology of the newborn infant he was looking for could not be of the Kleinian type. In different writings, Winnicott spelled out his main reasons for rejecting the Kleinian line of approach (e.g. Winnicott 1965: 177). According to Klein, the relevant clinical material 'either has to do with the child's object relationships or with mechanisms of introjection and projection' (*ibid.*: 174). These were 'deep' mechanisms, but, Winnicott argued, not 'early' mechanisms. As he puts it in 1962, much of what Klein wrote in the last two decades of her fruitful work may have been 'spoiled' by her tendency to push unwarrantedly the age at which deeper mental mechanisms appear further and further back. According to Winnicott, Klein made mistakes because 'deeper in psychology does not always mean earlier.' Winnicott was convinced that 'when you are going back to the deepest things you won't get to the beginning' (Winnicott 1989: 581). For instance, the talion dread and splitting the object into 'good' and 'bad' are truly deep mechanisms. Yet, the capacity to use them is not established before the capacity of using projection and introjection mechanisms, and these capacities, in turn, are dependent upon previous good mothering which, by the way, is neither a mental mechanism nor a mental phenomenon at all. Moreover, Winnicott never accepted Klein's theory of the nature and aetiology of psychosis, formulated in terms of hereditary mental mechanisms and conflicting instincts.

### **Winnicott and Fairbairn**

One might think that Winnicott should have felt himself closer to Fairbairn, who was also critical of the Oedipus paradigm. Indeed, in 1941, Fairbairn complained about the misconception of regarding 'the Oedipus situation as a psychological, in contrast to a sociological, phenomenon' (Fairbairn 1952: 36–7). In 1944, he declared that the Oedipus situation is not 'an explanatory concept,' but rather a 'phenomenon to be explained' (*ibid.*: 121).

These remarks seem to be compatible with Winnicott's findings. However, a closer examination of Fairbairn's position shows that this is not so. Fairbairn looked for causes of all pathological psychic

conditions in disturbances of object relations (*ibid.*: 82), in particular of relations with internalized objects. Schizoid disturbances, specifically, were thought of as results of introjections. As such they were viewed not as primary processes but as a defence mechanism (Winnicott 1989: 418). The question is: defence against what? Against ambiguity in object relations, that calls for the repression of the libido. The rationale for repression is not to be found in the (late) Freudian Oedipus situation, because the initial oedipal situation 'is not really an external situation at all, but an internal situation.' The fundamental difference from Klein is that the situation is not built around the symbolic mental equation 'breast = penis' and the conflict between death and libido instincts, but '... around the figures of an internal exciting mother and an internal rejecting mother' (Fairbairn 1952: 123-4). Fairbairn sums up his position in the following way:

Thus, in my view, the triangular situation which provides the original conflict of the child is not the one constituted by three persons (the child, his mother and his father), but the one constituted essentially by the central ego, the exciting object and the rejecting object.

(Fairbairn 1994 vol. I: 28)

Fairbairn's aetiology of pathological conditions is thus still Oedipal, triangular, although the triangle is defined differently from Freud and Klein. It is no more the actually lived objective Oedipal situation, as it was originally in Freud, but an 'internalized' condition. Internalization implies the existence and the functioning of mental operations and mechanisms that Winnicott came to reject, as I said above, on the basis of his clinical observations.

In 1953, Winnicott wrote a devastating review of Fairbairn's 1952 book of articles. What were his main critical points? Firstly, that Fairbairn '... starts off with an infant that is a whole human being, one experiencing the relation to the breast as a separate object, an object that he has experienced and about which he has complicated ideas' (Winnicott 1989: 416). Secondly, Winnicott criticizes Fairbairn's explanation of the disturbances found in individuals displaying schizoid features as a regressive phenomenon determined by unsatisfactory emotional relations with parents, without making clear whether '... the mother only "provokes the regression" to this early state or is the creator of it.' In other words, Fairbairn does not decide '... whether deprivation is the result of a deficiency in the mother's care

or inevitable in childcare.' It is therefore very difficult '... to work out whether Fairbairn considers this maternal failure to be truly the mother's failure or the child's projection on her of his own fate' (Winnicott 1989: 417–18). If the two are held to be the same on account of the imperfect maturity of all persons (including mothers), then it must be said that Fairbairn did not '... find the language that covers both the normal and the abnormal' (ibid.: 417). This faulty 'theoretical structure' spoils what can be learned from Fairbairn's valuable 'flashes of clinical insight.'

This is essentially the same objection Winnicott addressed to Klein, regarding the treating of early disturbances as internal mental problems without taking enough account of the actual mother-baby relationship dynamic. This difference is all-important because, in the second case, one is confronted with the additional task of defining the good enough maternal care whereas, in the first case, no such question arises.<sup>26</sup>

### **Winnicott's revolutionary research**

Winnicott did not want to abandon the efficient problem-solving procedures of classical psychoanalysis, even though they were embedded in metapsychological postulates (psychic forces and mental mechanisms) which he rejected. We have seen him saying that the existence of the Oedipus complex was confirmed. He also recognized the Kleinian theory of the depressive position as important and empirically founded, in which he saw a dual and not, as Klein saw, a triangular situation.<sup>27</sup> On the other hand, he needed, as I have indicated, a new and more powerful procedure to solve clinical problems that have their origin in the very early actual mother-baby relationship. So, how did he get out of this predicament?

One important element of Winnicott's solution came from his study of the 'environment.' Beginning in 1923 he became increasingly aware of the fact that there is a relationship between the environment and psychic disease, and, he says, this '... led to something in me' (Winnicott 1989: 576). In the 1920s and the 1930s no analyst was interested in this problem. Winnicott was even deterred from doing this sort of research by his first analyst James Strachey (1923–33), who was a classical Freudian, and later on by Joan Riviere, his second analyst (1933–8[?]).<sup>28</sup> Riviere bluntly refused even to consider



a planned paper of Winnicott's on the classification of environments. At that time, psychoanalysts, writes Winnicott, '... were the only people [...] who knew there was anything but the environment' (Winnicott 1989: 577) (see Chapter 1). Yet, Winnicott could not help but agree with those who were screaming out that a child might become ill by his father being drunk. Thus he was confronted with the following: 'How to get back to the environment without losing all that was gained by studying inner factors' (ibid.: 577).

How did Winnicott solve this? He was helped very much by an accidental factor: the war, and probably also by Clare Britton, his future wife. By being involved in the evacuation operations of small children in the London area, Winnicott was obliged, 'at last', he writes, to treat abandoned and maladjusted children.<sup>29</sup> Until then, he avoided treating such cases, remaining in line with the official position that psychoanalysis has nothing to do with 'real' situations. This is how Winnicott came to the 'original idea' of the links between the 'anti-social tendency' and 'hope' which is one of the essential discoveries of his child psychology and 'extremely important' for his clinical practice. The idea was that 'the thing behind the anti-social tendency in any family, normal or not, is deprivation,' and that hope has the unconscious meaning of 'trying to reach back over the deprivation area to the lost object' (ibid.: 577).

Having discovered the connection between maturational processes and the facilitating environment, between nature and nurture, Winnicott found himself confronted with a new task, i.e. of formulating 'a sort of theoretical basis of environmental provision starting at the beginning with 100 percent adaptation and quickly lessening according to the ability of the child to make use of failure of adaptation' (ibid.: 579). This task, in turn, required elaboration of 'dependence and adaptation theories' in a developmental and historical perspective (ibid.: 579).

### **Winnicott's exemplar: the baby on the mother's lap**

While working on the theory of the infant's relationship to what he named the 'environment', Winnicott came to two decisive results. Firstly, that it is 'impossible to talk about the individual without talking about the mother,' because, speaking the language of late Winnicott, the mother 'is a subjective object [...]' and therefore how the mother

behaves is really part of the infant' (ibid.: 580).<sup>30</sup> Secondly, that the initial mother-baby relationship is not a triangular internal (mental) relationship, but a very special kind of dual external (not mental) relationship. In 1958, Winnicott put this point in the following terms:

Any attempt to describe the Oedipus complex in terms of two people must fail. Nevertheless two-body relationships do exist, and they belong to relatively earlier stages in the history of the individual. The original two-body relationship is that of the infant and the mother or mother-substitute, before any property of the mother has been sorted out and moulded into the idea of a father.

(Winnicott 1965: 29–30)

In the beginning the father, from the baby's point of view, may or may not have been a mother-substitute. If he has, he was not there as father, i.e. as somebody endowed with properties or roles different from that of the mother. In the initial two-body relationship, the mother can be said to start off 'as a part object or as a conglomeration of part objects.' The same is true of her surrogates and thus of the father as the mother-substitute. Yet, 'at some time,' the father does begin 'to be felt to be there in a different role.' The time comes at which the individual is likely to use the father for a very specific purpose, namely:

. . . as a blueprint for his or her own integration when just becoming at times a unit. If the father is not there the baby must make the same development but more arduously, or using other fairly stable relationships, to a whole person.

(Winnicott 1989: 243)

This being so, the main initial role of the father with respect to the developing child who is no more a baby is not at all that of a partial object, but rather to ' . . . be the first glimpse [ . . . ] of integration and of personal wholeness. In favourable cases, the father ' . . . as father, not as a mother surrogate' starts off ' . . . as a whole person,' ' . . . as an integrate in the ego's organization and in the mental conceptualization of the baby' (ibid.: 243).

It is only later that he ' . . . becomes endowed with a significant part object,' (the penis) which then plays a very important role in the child's three-body relationships.

This conception of the initial dual mother-baby relationship allowed Winnicott to come to a clear-cut formulation of his paradigmatic problem. This is the point from which he started, i.e. that babies suffer from anxieties which are not to be conceived as products of putative innate mental forces and mechanisms, but, as a consequence of an external factor, albeit psychic, the early maternal failure to provide a good enough environment.<sup>31</sup> In a late text, Winnicott wrote:

To make progress towards a workable theory of psychosis, analysts must abandon the whole idea of schizophrenia and paranoia as seen in terms of regression from the Oedipus complex. The aetiology of these disorders takes us inevitably to stages that precede the three-body relationship. The strange corollary is that there is at the root of psychosis an external factor.

(Ibid.: 246)

Winnicott ends this passage, with a remark probably aimed at the Kleinians, by noticing that it is '... difficult for psychoanalysts to admit this after all the work they have done drawing attention to the internal factors in examining the aetiology of psycho-neurosis.'

By turning to 'external factors' as the cause of psychotic illness, Winnicott reversed the then prevalent tendency in psychoanalytic theory to formulate clinical problems in terms of mental mechanisms and still more radically in terms of innate symbolic equations, e.g. breast = penis, or of Lacanian symbolic castration.<sup>32</sup> Psychosis became a 'natural' process, having its causes in actual external human relations, not in inner, or still less symbolic, relations and processes. In opposition to Freud, Winnicott did not define external relations as sexual, social nor even as psychological, but rather as 'personal,' based on special forms of mutuality and intimacy between mothers and their babies. Thus, he switched to his new dual paradigm, or, as I propose to call it, 'baby-on-the-mother's-lap' paradigm.<sup>33</sup> From that new perspective of clinical experience, situations causing schizophrenia cannot be seen as triangular:

Just as a study of psycho-neurosis leads the student to the Oedipus complex and to the triangular situations that reach their height in the child at the toddler age and again in adolescence, so the study of psychosis leads the research worker to the earliest stages of infant life. This implies the infant-mother relationship since no

infant develops outside such a relationship. (It involves the idea of dependence prior to the establishment of the operation of mental mechanisms of projection and introjection).

(Winnicott 1965: 131)

What Winnicott is rejecting, in this and many other texts, is the very idea that early infantile schizophrenia and paranoia can have anything to do with triangular or three-body relationships. The only facts that can possibly be potential causes of psychic disturbances of the kind mentioned are related to the not good enough early mother-infant relationship at a time when, for the baby, there is no awareness of father, and therefore, there cannot be any third. This is why Winnicott states that schizophrenia is '... a sort of environmental deficiency disease' (Winnicott 1958: 162).<sup>34</sup>

Here we come to the crux of the matter: the psychology of a newborn is essentially different from the psychology of adults and even from that of young children. Not only does the theory of sexuality not apply, but also the Freudian metapsychological approach cannot be incorporated. A baby's life and his 'unconsciousness,' if there is something like that at all in a baby, cannot be described in terms of mental forces and processes. In particular, his needs have to be distinguished from desires, which are mental states, as well as from drives or instincts, which are putative or actual biological entities, with or without a mental, 'psychological' or conscious-like counterpart. Such mental states and processes are not there at the beginning. An individual's life develops out of something else, namely, out of an early psycho-somatic partnership established by the imaginative elaboration of body functions, instincts, sensations and feelings, which requires maternal care in order to succeed. In Winnicott, the binomial nature and nurture has taken the place of the classical polarity between an instinct-driven subject and its objects.

Yet, in a way, Winnicott was going back to Freud, since he saw no meaning in talking about Oedipus in terms of partial and internal objects. In *Human Nature*, Winnicott treats Freud's Oedipus complex as part of the problem of '... management of the first triangular relationship, with the child power-driven by newly established instincts of genital quality characteristics of the 2-5 year period' (Winnicott 1988: 49). Thus there is no substance in the frequently repeated statements that Winnicott is fleeing from the erotic into infancy (cf. Phillips 1988: 152). Winnicott is not fleeing from anything, on the contrary,

he is confronting the problem traditional psychoanalysis is trying to escape, namely the fact that Freud's theory of sexuality implied in the Oedipal situation does not account for disturbances which arise in the dyadic relationship between mothers and their babies. None of the later efforts to extend the Oedipal situation and sexual theory related to it (theories rejected by Freud himself, Otto Fenichel and Anna Freud, among others) produced the desired results. These extensions were theoretically degenerative, if not meaningless:

I think something is lost if the term 'Oedipus complex' is applied to the earlier stages, in which there are only two persons involved and the third person or part object is internalized, a phenomenon of inner reality. I cannot see any value in the use of the term 'Oedipus complex' where one or more of the trio is a part object. In the Oedipus complex, for me at least, each of the three of the triangle is a whole person, not only for the observer but also and especially for the child.

(Winnicott 1988: 49)

Winnicott did not just retain Freud's late Oedipus complex, he even developed it further, by introducing, for instance, a new explanation of the origin of the fear of castration. This fear, says Winnicott, '... becomes welcome as an alternative to the agony of impotence' which characterizes the genital phase of sexual development where '... the child's performance is deficient, and the child must wait (till puberty as we know) for the ability to act out the dream' of genital relation with the mother (ibid.: 44). I want to emphasise that it is a serious, though widespread, error to think that Winnicott flees from sexuality to early infancy. What he demonstrably does is to place each of these developmental moments into the appropriate stage in the process of personal growth. Thus he makes it clear and precise how the environment impacts on the individual at each stage of early development (Winnicott 1958: Chapter 13).

### **Winnicott's main guiding generalization: the theory of maturational processes**

The guideline for Winnicott's treatment of psychosis is his theory of emotional or personal development:

To examine the theory of schizophrenia one must have a working theory of the emotional growth of the personality. [. . .] What I must do is to assume the general theory of continuity, of an inborn tendency towards growth and personal evolution, and to the theory of mental illness as a hold up in development.

(Winnicott 1989: 194)

Here Winnicott is describing two things: his main scientific problem – infantile schizophrenia – and the theoretical tool he uses to solve it – his theory of maturational processes or personal growth. In the study of schizophrenia, this theory has the same paradigmatic role as that held by the theory of sexuality in the study and treatment of psycho-neuroses within Freud's three-body paradigm:

Also, I can say that the statement of infantile and child development in terms of a progression of the erotogenic zone, that has served us well in our treatment of psycho-neurosis, is not useful in the context of schizophrenia as is the idea of a progression from dependence (at first near-absolute) towards independence [. . .].

(Ibid.: 194)

Like Freud's theory of sexuality, Winnicott's theory of dependency (from dependence towards independence) is an empirical generalization and not a metapsychological speculation (Abram 2007: 130–47). It was initially constituted from clinical material in relation to deprived children and developed by application to the study of two-body relationships.

The theory of emotional growth stands at the very centre of Winnicott's theoretical matrix and represents one of his main contributions to psychoanalysis. In almost every article Winnicott consistently returns to the main problem of the ' . . . treatment of psychiatrically ill children, and the construction of a better, more accurate and more serviceable theory of emotional development of the individual human being' (Winnicott 1986: 84).<sup>35</sup> Curiously enough, in the secondary literature, this theory as such has received little attention, being simply forgotten or viewed as trivial and reducible to psycho-analytic common sense.

### **Other components of Winnicott's paradigm**

In order to complete this schematic re-construction of Winnicott's paradigm, I will now examine his ontological model of man, his heuristics and the values he stressed: items which, according to Kuhn, must be present in the disciplinary matrix of any science.

Firstly his ontology – Winnicott's theory of personal growth is based on a new view of the human being. Winnicott defines psychoanalysis (perhaps in an unexpected and seemingly old-fashioned way) as 'the study of human nature' (Winnicott 1988: 1). What Winnicott has in mind is the assumption that '... fundamentally all individuals are essentially alike, and this in spite of the hereditary factors which make us what we are and make us individually distinct' (Winnicott 1964: 232–3). At face value, this assumption seems to be more philosophical in kind than biological. This impression is strengthened by Winnicott's subsequent commentary:

I mean, there are some features in human nature that can be found in all infants, and in all children, and in all people of whatever age, and a comprehensive statement of the development of the human personality from earliest infancy to adult independence would be applicable to all human beings whatever their sex, race, colour of skin, creed, or social setting. Appearances may vary, but there are common denominators in human affairs.

(Ibid.: 233)

The common denominators identified are of two kinds – structural and developmental. The structural are that 'The needs of infants and small children are not variable; they are inherent and unalterable' (ibid.: 179). This same thesis is expressed in the following way:

The essential needs of the under-fives belong to the individuals concerned, and the basic principles do not change. This truth is applicable to human beings of the past, present, and future, anywhere in the world, and in any culture.

(Ibid.: 184)

As to developmental common denominators, they are obviously the invariant features of human personal growth. There is a straight connection between the two kinds of denominators, since needs

are essentially related to the tendency towards integration, that is, to growth.

It is no surprise that some commentators interpret Winnicott's concept of human nature as a return to essentialism.<sup>36</sup> But this point should not be overdone. Human nature is something which, in spite of being invariable, has a beginning. 'The only certain date of which is that of conception' (Winnicott 1988: 29). It is not easy to ascertain the correct meaning of what Winnicott is saying here. One possible interpretation is that human nature is not a Platonic essence, but the invariant structure of a particular kind of temporalization which manifests itself as a human being, who, as Winnicott puts it, '... is a time sample of human nature,' just that. Where does this process of being start from? The answer is that it starts from 'not being', 'from nowhere', 'from aloneness' (ibid 131).<sup>37</sup> Next we may ask where does the process go? The answer is the same – to 'not being', to 'nowhere', to 'aloneness.' Winnicott states that 'The life of an individual is an interval between two states of unaliveness' (ibid.: 132). The important thing to notice here is that these two states of unaliveness, which are the extreme points of the human life interval, belong to human nature and can even be experienced. 'The experience of the first awakening gives the human individual the idea that there is a peaceful state of unaliveness that can be peacefully reached by an extreme of regression' (ibid.: 132). If this is so, then human nature is, in itself, the negation of any fixed essence. The only thing a human being can have, as a time sample of human nature, is his history, that occurs due to the tendency '... to begin to exist, to have experiences, to build a personal ego, to ride instincts, and [...] to have a self that can eventually even afford to sacrifice spontaneity, even to die' (Winnicott 1958: 304). '... Natural death follows as the "final seal of health"' (Winnicott 1988: 12).

This is the main ontological hypothesis presented by Winnicott. Elsewhere, I have tried to show that Winnicott's argument is in close agreement with Heidegger's concept of the human being as happening-in-the-world of a being-to-death (Loparic 1995 and 1999b). Be that as it may, one thing is certain: there is a great difference between Winnicott's concept of human nature and Freud's naturalistic concept of the mental apparatus driven by instinctual forces. The latter concept, as I have said, is taken from modern empirical psychology and, in the last resort, from the modern philosophical concept of a naturalized subjectivity.



As to heuristics – Winnicott continues to accept the Freudian method of research, i.e. the clinical setting and work in the transference. However, he modifies its meaning, by allowing for the occurrence, in the clinical setting, of regression to dependence. Moreover, Winnicott does not allow for any kind of metapsychological speculation and prohibits going ‘behind’ phenomena by means of metaphors. His view of human nature is based on a very general hypothesis concerning the development of the human capacity to live an experience, rather than a metapsychological structure and functioning of something like a ‘psychic apparatus.’

As to his values – they can be divided into the theoretically and practically significant. Theoretically, Winnicott sees psychoanalysis as a science, which has to test its hypotheses and to obey the verdict of observed facts.<sup>38</sup> As any science, psychoanalysis must be formulated so that it can be submitted to public discussion by psychoanalysts, by other scientists in the related fields, such as child psychiatry and paediatrics, and by the public in general. In so far as practical values are concerned, Winnicott takes into account unduly censured sexuality (Freud) and intrapsychic pain caused by internal conflicts (Klein, Fairbairn). Yet he thinks that by far the most severe suffering is that which arises from unmet needs that originate from the infant’s predicament at the beginning of his life, i.e. the need for the continuity of being. Paradigmatic examples of this kind of pain are described as ‘unthinkable agonies,’ unthinkable, because they precede the time the baby is able to have any mental representation, and agonies, because they imply a lack of a good enough holding environment in which there is a struggle for the continuity-of-being. These troubles are ‘early’ but not ‘deep,’ because they originate in the two-body relationship, before the existence of any representation structure in the human baby (Winnicott 1989: 581).

### **A comparison between the paradigms of Freud and Winnicott**

Both Freud and Winnicott agree that psychoanalysis is a science, not a craft, art, philosophy or religion.<sup>39</sup> Neither classifies it together with ‘mixed disciplines’ like astrology or alchemy. Both conceive psychoanalysis as a problem-solving activity, guided by concrete clinical problem-situations and their solutions, completed by an addi-

tional theoretical framework. Whereas exemplary problem-solutions are considered to be beyond question in normal research, they are not viewed as having an unlimited heuristic power. Both thinkers concede that new exemplars might be needed to complete the psychoanalytic picture of psychic diseases and to promote further research.

However, Freud and Winnicott disagree as to which problems are exemplary for psychoanalytic research and as to what empirical generalizations are to be taken as guiding lines. Freud made normal psychoanalytical research possible by demonstrating, through his work with the hysteric, that all psychopathological situations relate to Oedipal conflicts and by interpreting this situation in terms of his theory of sexuality. Winnicott, beginning his study of psychoanalysis in the 1920s, found that he could not see things exclusively in that way. He concluded his work by viewing the mother-baby situation as exemplary, a result which in turn forced him to develop a theory of emotional growth, that is, of nature and nurture. This is, in essence, the paradigm change which accounts for the difference between the Freudian Oedipal, triangular or three-body psychoanalysis and Winnicott's mother-baby, dual or two-body psychoanalysis.

There are also radical differences with regard to theoretical commitments. Whereas Freud, following the Kantian tradition, admitted a number of speculative auxiliary suppositions that he used to formulate his metapsychology, Winnicott decidedly rejected such a mode of theorizing and limited his explanatory hypotheses to the experiences of persons in treatment, in particular babies and young children. Winnicott does not allow for the reduction of personal 'subjective' phenomena to apply to the point of view of the patients' consciousness nor, even less, to that of an observer. He wants it the other way round: to make sure that these points of view, though external to the phenomena themselves, capture the patient's way of being and experiencing, even if this patient is a newborn baby. This is not always possible. In such cases, the analyst must stop trying to know what is happening 'behind the scene,' he must refrain from making metapsychology and from theorizing, which in clinical terms means that he must give up interpreting and even saying anything whatsoever.

Thus, both Freud and Winnicott set limits on our possibility of actually knowing 'unconscious phenomena.' But they deal with this fact differently. Freud permits himself to speculate, that is, to project

to the unconscious the properties, the dynamics and the structures of conscious subjectivity. And on the contrary, Winnicott, based on his experience with mothers and their babies, understands that such a procedure is not legitimate, because it makes us think of babies as being adults and forget what happened during the process of emotional growth. Winnicott's baby is a human being, yes, but not the one who can be thought of in terms of conscious mental phenomena. Seen from the vantage point of Winnicott's theory of emotional growth, Freud's theoretical errors come from the incorrect view that what is beyond consciousness may be conceived of as being similar to consciousness, as 'un-conscious.' What, in babies, is beyond consciousness is not just primary processes, which have nothing to do with anything like conscious forces and mechanisms. The baby's experience of the continuity-of-being is something very different from any state of consciousness. Thus, the true philosophical difference between Freud and Winnicott is that whereas Freud still thinks in terms of the theory of subjectivity, initiated by the seventeenth century's philosophers and represented paradigmatically by Kant, Winnicott in contrast thinks of human beings in an entirely different theoretical key, which has much affinity to Heidegger's fundamental ontology, as presented in *Being and Time* (Heidegger 1962).<sup>40</sup>

### **Winnicott's heritage**

Although the evidence is clear that Winnicott has introduced a new paradigm, the question remains as to whether he also instigated a revolution? Kuhn distinguishes between 'major revolutions' and 'small scale' revolutions. A scientific revolution being '. . . a special sort of change involving a certain sort of reconstruction of group commitments' it need not be 'a large change, nor need it seem revolutionary to those outside the single community, consisting perhaps of fewer than twenty-five people' (Kuhn 1970a: 181). It seems to me that there are more than just twenty-five psychoanalysts in the world who would be willing to declare themselves ready to do 'normal science' within the two-body paradigm proposed by Winnicott and these could appropriately be called Winnicottians. We are thus in a position to declare that a truly Winnicottian international community is beginning to arise, which could very well prove to make real

contributions to present day psychoanalytic research and practice as a whole.<sup>41</sup>

There are some standard objections, frequently repeated but never really argued, against the possibility of creating a Winnicottian Research Community or a Winnicottian School in psychoanalysis. One such argument is that Winnicott was not a man of institutions. This is simply not true, as can be seen from Winnicott's many engagements in institutional matters (see Chapter 3 and Chronology). Winnicott's protest was not against psychoanalytic societies but, rather, against societies and theories that were turned into propaganda machines and instruments of indoctrination. What he fought for was open scientific research and discussion within and between psychoanalytic societies.

The second argument, defended for instance by Charles Rycroft, states that Winnicott was '... too idiosyncratic to be readily assimilated into the general body of any scientific theory' (Rycroft 1985: 114). Phillips in a sense echoes Rycroft when he says that, 'Winnicott did not become systematically coherent at the cost of his own inventiveness' (Phillips 1988: 99). However, if we seriously consider the reconstruction of Winnicott's paradigm offered here, this objection is far from doing justice to Winnicott's oeuvre and reveals not so much Winnicott's theoretical laziness as that of his objectors. Winnicott certainly did value his own inventiveness, nevertheless his main task as a psychoanalyst and paediatrician was not to cultivate and develop his originality but to help psychotics and deprived children. In order to do that he had to proceed in a methodical, coherent way. In other words, scientifically, and therefore he could not afford simply to be creative. That would mean being intrusive. Indeed in many situations he simply had to wait, that is, to sacrifice his own creativity in order to facilitate the patient to be creative. Winnicott needed, of course, to use much of his inventiveness in order to give a scientific format to this simple conclusion, but after that, he had to apply it and help his patients invent their lives. Like so many others, Phillips is confusing different aspects of Winnicott's work and personality, to the damage of an understanding of both.

Thirdly, it is said that Winnicott did not want to become a 'master.' He certainly did not want to master-mind people by telling them what to do and what to think. Nevertheless he developed an extraordinary capacity to make his own ideas public by writing, lecturing and teaching. In *Therapeutic Consultations*, for instance, he explicitly

addresses the problem of training psychoanalysts in his technique of squiggle games. The basis for this training is 'a long term psychotherapy of individuals' (Winnicott 1971: 270). If this condition is not available, the teacher has to consider whether the candidate possesses a certain number of 'desirable qualities,' specified either by orthodox or Winnicott's own psychoanalytic theory and practice. Once the choice of a good candidate is made, the teaching of the technique of therapeutic consultations can begin. For this purpose, the case histories described by Winnicott in considerable detail '... may prove to be good teaching material' (ibid.: 9). Winnicott thus assumes the teaching role, with, however, the following caveat:

It would be from my point of view a satisfactory outcome if the material could be used for criticism and I would much prefer this to the alternative whereby what I have described here might simply be imitated. As I have already stated, the work cannot be copied because the therapist is involved in every case as a person, and therefore no two interviews could be alike as they would be carried through by two psychiatrists.

(Winnicott 1971: 9)

In the same vein, Winnicott points out that his case descriptions reflect his own personality, without forgetting to point out that his personality is not the only 'constant factor' in this kind of research, since in doing it he has had one constant companion:

The only companion that I have in exploring the unknown territory of the new case is the theory that I carry around with me and that has become part of me and that I do not even have to think about in a deliberate way. This is the theory of emotional development of the individual which includes for me the total history of the individual child's relationship to the child's specific environment.

(Ibid.: 6)

The cases presented in *Therapeutic Consultations* are, therefore, neither 'fruits of chance' nor 'genial insights of a creative psychoanalyst', but rather they are essentially illustrations of theoretical perspectives developed by Winnicott during years of dedicated scientific work and of a personal technique based on this perspective (ibid.: 215, 218, 220).

Winnicott compares his positions as a teacher of therapeutic consultations to that of the 'cellist who first slogs away at technique and then actually becomes able to play music, taking technique for granted,' and who is moved by the wish 'to communicate with those who are still slogging at technique, at the same time giving them the hope that will one day come from playing music' (ibid.: 6). Winnicott hates the idea of being 'simply copied,' but he does want to teach what he knows in order that other people might create their own capacity to acquire knowledge and to do psychotherapeutic work by themselves. It would be better, admits Winnicott, '... if the student could gather the material for himself or herself from personal contact with children instead of reading my descriptions.' But he knows very well that this is not always possible, especially for students who are starting to learn (ibid.: 11).

What we have here is a very subtle presentation of the learning process involved in the squiggle game (derived from the spatula game) that takes the personal dimension into account, while nevertheless recognizing that teaching is founded on a pre-existing theory, i.e. the theory of emotional development of the individual, which is the 'backbone of all the work described here.' Winnicott has essentially written a textbook on the technique of therapeutic consultations, based on his theory of emotional development. Not only can Winnicott's theory and technique be taught, it also seems to me that he wants it to be taught to the beginner analysts. In essence, Winnicott subscribes to the general view that there is no other way to become a scientist other than within a scientific tradition.<sup>42</sup>

I have tried to show that in Winnicott's work there is a constant, long range and carefully conducted scientific effort to solve a clinical problem: that of nature and the aetiology of psychotic disturbances. I am quite ready to admit that his solution to this problem has left many unanswered questions. However, I understand that there can be no reasonable doubt about Winnicott's commitment to scientific research conducted in agreement with methods of psychoanalysis and, to a lesser degree, of ordinary paediatrics and psychiatry. I would suggest that neither of the latter two disciplines are in much better shape than psychoanalysis. In all of them rival theories are struggling for survival and all continue to remain in, what Kuhn would term, a 'pre-paradigmatic phase.' In other words, they are all undergoing, more or less, frequently smaller or greater revolutions – the kind of activity that is generally known as normal 'scientific research.'

## Notes

- 1 This is a revised version of 'Winnicott's paradigm outlined' published in 2000, which was based on my 'Madeleine Davis Lecture' for the Squiggle Foundation, delivered by kind invitation of Jan Abram who was in her final year as Director of the Foundation.
- 2 See D.W. Winnicott Publications 1931–2002 in the Appendix, which indicates where to find the complete bibliography of Winnicott's papers and books.
- 3 As is well known, Kuhn himself leaned heavily on psychology and sociology (especially on L. Fleck's theory of scientific communities [1935]) as well as on some philosophical sources (Wittgenstein's philosophy of language) in framing his view of science and scientific research. It could be a rewarding exercise to re-examine and eventually to complete Kuhn's theory of science by taking into account Winnicott's views on the genesis and the function of intellectual and other mental processes in human life.
- 4 In 1990 Kuhn characterized his position as a 'sort of post-Darwinian Kantianism' (Kuhn 1990: 12). For comments on the resemblance between Darwin's history of life and Kuhn's history of science, see Hodge and Radick (eds) 2009: 165–6 and 172.
- 5 Winnicott strongly criticized a similar claim of Riviere's as regards the Kleinian development of psychoanalysis (Winnicott 1987: 35, 97).
- 6 In this passage and elsewhere, Greenberg and Mitchell prefer, for reasons which are not quite clear to me, to use the later Kuhnian term 'model' instead of the original term 'paradigm.'
- 7 For other accounts of the development of Winnicott's ideas cf. Greenberg and Mitchell (1983) and Jacobs (1995).
- 8 In 1989, Holton and his collaborators introduced the concept of the 'solace paradigm' in an attempt to solve the problem of human need for 'consolation', particularly urgent in our epoch which is 'overwhelmingly nihilistic'. In this context, Winnicott's concept of the transitional object is treated as a 'very important sub-class of solacing objects' (Holton et al. (eds) 1988: 62), the elements of 'transitional relatedness' being 'no less ubiquitous in life than are elements of the Oedipus complex' (ibid.: 88). Though I agree that Winnicott's transitional objects are an important component of his new paradigm and that this paradigm is no longer based on the Oedipus complex, I cannot follow Holton and his group in the attempt to embed this concept in the solace paradigm of their own, presented as an 'enlargement' of the scientific world-view by a 'multi perspective' strategy, which combines scientific, philosophical and even theological backgrounds. There is little doubt that philosophy and theology have been and continue to be influential in framing scientific world-views, but I cannot see any value, just as Freud and Winnicott did not, in mixing up science with these two disciplines. Holton's concept of paradigm does not square with what we know about paradigms in

*scientific* disciplines, but rather portrays what happens in philosophical and theological disputes about fundamentals.

- 9 Let me give an example. Dodi Goldman (1993) puts much emphasis, as many other commentators do, on the personal factors in Winnicott's procedures. For example he writes: 'By temperament, Winnicott was more an innovator than a curator. He needed to seemingly destroy certain facets of psychoanalytic theory so as to re-create them in his own image: Only then could theory feel real to him' (Goldman 1993: 132–3). And: 'Winnicott's original contributions to psychoanalytic theory are best understood, therefore, as efforts to re-create for himself, in a personal way, aspects of theory that he has imaginatively destroyed.' (p. 133). As I see it, Winnicott did not 'destroy' psychoanalytic theory and practice driven by needs flowing from his temperament. He rather developed and modified it, in such a way, however, that 'bridges that lead from older theory to newer theory' are kept 'open' (Winnicott 1989: 256). In some cases, this was done in order to increase the problem-solving capacity of psychoanalysis, in others to correct errors ('blunders') of Freud's. One of the reasons for my use of Kuhn's theory of paradigms is that it illustrates more accurately that the kind of move practised by Winnicott is part of the common scientific practice and that Winnicott took Freud as approving and welcoming 'revolutionary' procedures in psychoanalysis.
- 10 Kuhn's term for this component is 'symbolic generalizations', which covers empirical laws and definitions of empirical phenomena.
- 11 Cf., for instance, the very special personal significance of the duck figure in the squiggle game of Winnicott with Iiro, as specified in Winnicott 1971, Chapter 1, which would get completely lost if this figure were seen as a rabbit.
- 12 At this point Kuhn agrees entirely with Heidegger who denies that there are independent criteria for choosing between competing metaphysical systems (cf. Heidegger 1961, vol. 2: 258, 264 and 290).
- 13 This is an expression which Winnicott takes from J. Rickman, who introduced the distinction between 'two-body' and 'three-body relationships' (see Winnicott 1965: 29). I wonder whether Rickman's usage was not inspired by distinction made in classical mechanics between two- and three-particle problems.
- 14 See the previous note.
- 15 Freud's coolness towards Melanie Klein can be explained in the same way. Moreover, the essential points of the debate between Anna Freud and Melanie Klein can be summed up as turning around the question of how far back are we allowed to displace the Oedipal elements of the mental apparatus? (see Phillips 1988: 43).
- 16 Freud's theory of sexuality is a result of a continuous, both empirical and metapsychological research, which extended over decades. At the beginning, it paid much attention to the problem of perversions— since Freud was standing still under the influence of Krafft-Ebbing – and to the differences



between adult and infantile sexuality, including puberty. Yet, with time, questions related directly and specifically to infantile sexuality became predominant. Some of this work appears in additions to later publications of *Three Essays*. Particularly noteworthy are sections 5 and 6 of the Second Essay, which deal with infantile sexual theories and phases of development of sexual organization (the erotogenic zones), as well as section 3 of the Third Essay, which deals with the libido theory. Among significant developments in sexual theory present in other writings of Freud's we can mention the theory of libidinal types and of female sexuality.

- 17 See Loparic 1999a.
- 18 The term 'speculation' is my translation of Freud's 'Spekulation', which is taken from Kantian philosophy and characterizes Freud's way of constructing his metapsychology. Metapsychology is the speculative part of his new science, parallel to the speculative part of physics, which includes expressions and terms like 'gravitational force,' 'particle of matter,' 'absolute space,' 'infinitesimal' etc. One main trait of Freud's speculative concepts is that they are 'conventions' ('Konventionen'), to be used not for making statements about matters of fact, conscious or unconscious, but exclusively for heuristic (problem-solving) and merely expository purposes, being 'heuristic fictions' in the Kantian sense. I guess that on this point many British Contemporary Freudians differ sharply from Freud (perhaps due to the British empiricist tradition and Winnicott's influence).
- 19 Winnicott thinks the same way since he praises Freud's openness to criticism and his readiness to abandon his ideas, whereas he criticizes the dogmatism of Klein and the Kleinians as not scientific (cf. Winnicott 1989: 460).
- 20 In 1911, Freud signed, together with Einstein and several other first rate scientists of the epoch, a manifesto in favour of the foundation of a 'Society for Positivistic Philosophy.' This document is now published in *Natureza humana*, vol. 2, no. 2, 2000.
- 21 Klein was concerned about 'psychic pain.' Winnicott, as we shall see, is concerned about real failures in human relations (which are not just 'social,' but personal, at any stage of development).
- 22 A non-coherent theory is a false theory. Since *ex falso sequitur quodlibet*, inconsistency has to be avoided.
- 23 As we know, one of the sources used by Freud in elaborating his metapsychology was the article by Theodor Lipps, a philosopher of psychology, entitled: 'The Concept of the Unconscious in Psychology', from 1897.
- 24 As Heidegger noticed (1987: 220), Freud's id is a new scientific name for unconscious *sensibility* and passions, ego for unconscious *understanding*, and super-ego for unconscious *reason*, in particular, practical reason.
- 25 As we know, Freud was not very happy about the proposal made by Klein.
- 26 In 1953, Winnicott still thought that Fairbairn was trying to take his distance from Klein. In his autobiographical report of 1967 (1989, Postscript),

- he admitted however that Klein and Fairbairn had several important things in common, but that he 'could not see that for years and years' (Winnicott 1989: 579).
- 27 On Winnicott's interpretation of the depressive position as a two-body situation see Winnicott 1965: 22, 30 and 176.
  - 28 Cf.: Chronology – It is not clear what were the exact years when Winnicott was in analysis with Joan Riviere but it's likely to be these years.
  - 29 It is interesting to note that WWI triggered a similar need for further articulation in classical psychoanalysis. The discovery of the 'war neuroses' opened the way to a series of clinical developments and to Freud's new addition to his metapsychology of the death instinct (Freud 1920).
  - 30 The same is true of transitional phenomena and has, according to Winnicott, 'quite a lot of philosophical importance.' I have tried to spell out a possible philosophical meaning of the environment as a part of the individual by approximating this idea to Heidegger's concept of man as having the structure of 'being-in-the-world' (cf. Loparic 1995).
  - 31 It may not be beyond the point to notice that Peter Sloterdijk, a German philosopher influenced by Heidegger and interested in psychoanalytic theory, also defends in his recent writings (cf. Sloterdijk 1998) the thesis that our original relationship to the external world is dual, not triangular. However, he does not conceive this relationship as the one between the baby and his mother, obtaining in the 'subjective' world, but as a pattern which is realized in couples found in very different fields of study, such as theology (relation between soul and God or the soul and the Guardian Angel) or adult sexuality.
  - 32 This tendency started with Freud's rejection of his first seduction theory.
  - 33 This image, obvious in itself, is based in particular on a particular remark of Winnicott's that the relation of a child to his mother must be such that he can feel comfortable 'on her lap' (Winnicott 1964: 133).
  - 34 This argument is parallel to the one used by Winnicott in criticizing Klein's theory of envy. Envy cannot be attributed to a newborn baby because the word 'envy' refers to an attitude, something maintained over a period of time, and to several other mental states which imply 'a degree of ego organization in the subject which is not present at the beginning of life' (Winnicott 1989: 444).
  - 35 A brief account of this theory can be found in Winnicott 1988: 8 and 101–2.
  - 36 Phillips, for instance, says that Winnicott was a 'pragmatist with an essentialist theory' (Phillips 1988: 97).
  - 37 Thus, not as in Freud who states that the individual emerges from an inorganic state.
  - 38 See Winnicott 1996: Chapter 1.
  - 39 This stance is taken by Winnicott in many texts, cf. Winnicott 1986: 13ff. and 1996: Chapters 1 and 29.
  - 40 This idea is developed in Loparic 1996 and 2001.

- 41 There are several psychoanalysts who took the same direction as Winnicott, for example Ogden (1986) and Mitchell (2000).
- 42 The same is true, for instance, of his papers on the psychoanalytic technique itself.

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