

# ON SOME CHARACTERISTICS OF THE ENGLISH SCHOOL

*Darian Leader, February 1992*

The most common is the eagerness to interpret holidays, even long weekends. Secondly there is the position in relation to theories, of which the English are suspicious. Sir Geoffrey Elton refers to them in his recent monograph 'Return To Essentials' as the "Intellectual equivalent of crack", a form of "Cancerous radiation" emanating from France, and couched in the latter's native tongue since "The absurd sounds better in French"<sup>1</sup>. The continental base of theoretical postulates is contrasted with the steady accumulation of empirical data in the Anglo-Saxon tradition, allowing the eventual proposal of explanatory theses at the top end of the pyramid of facts. Now, everyone knows that the relation of theory to 'empirical data' is not such a simple one, but it is a binary which still enjoys in both France and England a certain currency. What interests us here are less the criticisms which may be made of the foundation of the distinction than the examination of its emergence at a precise historical moment as a differential separating continental and Anglo-Saxon research. Once we have pinpointed this, we are in a better position to see what effects such dogmatism has on Anglo-Saxon psychoanalytic technique.

## **From One Controversial Discussion...**

In the Controversial Discussions which brought out the main lines of force in the British Society in the 1940s<sup>2</sup>, there was serious consideration of the relation of psychoanalytic theory to technique. One focus of these meetings was to determine the scientific status of Kleinian analysis and the results may be seen, for example in the significant changes in Melanie Klein's written style after 1946, when she is more prudent to introduce such terms as 'theory', 'hypothesis', and 'infer'<sup>3</sup>. A series of papers was to follow in the 'International Journal of Psycho-Analysis' bearing reminders of the role of evidence, proof and theory construction in psychoanalysis, none of which, however, was to attain the sharpness that characterised many of the contributions to the earlier Discussions and none of which was to evoke the remarkable statement by Freud at the Berlin Congress in 1922 on the proposed psychoanalytic essay prize. "Essays", Freud specified, "should examine the extent to which psychoanalytic technique has influenced the theory and how far these are furthering or hindering each other at the present time"<sup>4</sup>. The key to Freud's statement is that the effects of technique on theory are an open question: he is as ready to imply that technique hinders theory as the converse. One of the only members of the British Psycho-Analytic society to take such a view seriously and to develop it was Edward Glover, notably in his paper on 'The Therapeutic Effect of Inexact Interpretation' which Lacan discusses in the *Écrits* (p.593). The more dominant current followed the schema as it is set out by Winnicott, that analytic research consists of a cycle of three stages: "Piecemeal objective observation"<sup>5</sup>. This is the

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<sup>1</sup> CUP 1992.

<sup>2</sup> The Freud-Klein Controversies, 1941-45 ed. Pearl King & Riccardo Steiner, Routledge 1991.

<sup>3</sup> Cf, for example, his article "Notes on some Schizoid mechanisms", International Journal of Psychoanalysis, 1922.

<sup>4</sup> IJP, 1922.

<sup>5</sup> King and Steiner eds. Op cit, p. 89.

reason why Anglo-Saxon books on psychoanalysis, such as Harold Stewart's recent 'Psychic Experience and the Problems of Technique'<sup>6</sup> maintain a division, one half of the text for 'Technique' and one half for 'Theory', even if this latter term does not appear in the title itself, where its place is usurped by 'Experience'. Yet this sort of separation has not always been the rule, and in order to obtain the correct focus on it, we need to pass from one Society, the British Psycho-Analytic Society, to another, the Royal Society, for another form of Controversial Discussion, one about the nature of light.

### **To another...**

Newton published his first paper on the homogeneous nature of white light in the Philosophical Transactions of The Royal Society in 1672. The argument and the experiment are famous: a ray of white light is passed from a circular point source through a prism and put in a position of maximum deviation. If every part of the incident ray is equally refrangible, then in this position the refracted image must be geometrically similar to the shape of the source, ie circular. Yet this is not the case. Hence not every part of the incident ray is equally refrangible. Hence white light does not consist of equally refrangible rays. The argument together with the pictorial representation of Newton's Experimentum Crucis, was reprinted time and time again from the early seventeenth century onwards as a paradigmatic example of how to derive a theoretical conclusion from an accumulation of empirical facts, and the summary of the result I gave above may be included in such a current. Yet despite the irreproachable 'clarity' of the experiment and the 'do it yourself' advice which would accompany it, Newton's experiment at the time of its elaboration, provoked far from unanimous acceptance.

The first response was to evoke the wave theory of light as opposed to Newton's 'particle' theory, not, apparently for the sake of simply asserting the falsity of the Newtonian view but rather in Hook's words "To show Mr Newton's corpuscular hypothesis of light and colours not absolutely necessary"<sup>7</sup>. Pardies and Huygens expressed similar worries: Newton's result was posed as a necessary consequence of the data, when other interpretations could be offered at the same time. The key here, however, is to note that Newton had omitted the mathematical argument which structured the experiment, due, in part to the influence of the Secretary of the Royal Society, Henry Oldenburg. Hence the paradigmatic example of 'data to theory' itself rested on a complex theoretical apparatus which was left out of the originally published presentation. The sensitivity of the English audience to such qualifiers as 'necessary' is seen nicely in the calculated slip of the pen which occurs when Descartes' work was first done into English some years earlier: *Discours de la Methode* becomes 'A Discourse on a Method for the Well-Guiding of Reason.' The definite article of 'the Method' has become the more judicious indefinite of 'a Method'.

Another slip of the pen brings us to a second response to Newton's experiment. In the translation of Newton's Correspondence, Pardies supposedly says to Newton: "When the experiment was performed after this manner, everything succeeded and I have nothing further to desire"<sup>8</sup> but the French text shows this is not what the Jesuit

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<sup>6</sup> Routledge, 1992.

<sup>7</sup> Cf. the discussion by Zev Bechler, "Newton's 1672 Optical Controversies: a study in the grammar of Scientific Dissent" in Elkana, ed., *The Interaction between Science and Philosophy*, Atlantic Highlands, 1974, pp. 115-142.

<sup>8</sup> Isaac Newton's papers and Letters on Natural Philosophy, ed. I.B Cohen, Cambridge, Mass., 1978,

said: "L'expérience ayant este faite de cette facon je n'ay rien à dire". In other words there is no reference to the experiment having "succeeded". Indeed Mariotte was to perform the experiment around 1679 to find that it in no way validated Newton's colour theory<sup>9</sup>, a blow to its reception on the continent that would take some time to surmount. What is important to understand here is the fundamental lack of clarity of the gem of the experimental philosopher's data. Far from proving a theoretical result beyond question, the experiment itself was found to give very disparate results when performed elsewhere.

Within three decades, the opposition between continental 'theorisation' and English 'experimental method' would be established. The optical dispute was only a first step in the debate, the crucial passage being the controversies subsequent to the publication of the *Principia* in 1687 and the key second edition of 1713. Here we have a consistent differentiation of the Newtonian technique of moving from particulars of empirical observation to incorrigible general laws and then to explaining all phenomena that are seen to be consequences opposed to the continental technique of starting natural philosophy from the theoretical first principles. Hence the Newtonian fiat 'Hypotheses non Fingo'. Yet, curiously enough, in the first edition of the *Principia* there is a whole section entitled 'Hypotheses'<sup>10</sup>. By the time of the second edition, the first two of these 'Hypotheses' have changed into 'Regulae Philosophandi', three have disappeared, and numbers five to nine have become 'phenomena'. When Newton states that his laws have been deduced from careful observational data of planetary phenomena, he does not mention his refusal to accept data from Flamsteed, the Astronomer Royal, which failed to corroborate his suppositions. More grandly, the reader is informed it is now proven that planets move in ellipses, as he states in proposition 12 of book 3, yet if one turns to the exposition of the perturbation theory, one finds an explanation of why they don't. Such inconsistencies, which the industry of Newtonian research continues to study, are for a large part intended to protect the Newtonian programme from criticism, the sort of criticism which would focus on, precisely, first principles, for example: the speculative hypothesis that there exists a universal gravitation, an assumption which was, as Newton well knew, in no sense explicable in terms of mechanics, and which was little less than 'an absurdity'<sup>11</sup>. Hence the strategy of rejecting the Cartesian requirement of first principles as foundations and the consequent rejection of criticism that the theory is in contradiction with a priori principles<sup>12</sup>. While such principles are, of course, open to criticism and replacement by their contraries, as Faraday knew well, even the most basic Newtonian results which have since 'become' empirical data, such as the seven colour division of the spectrum, rest on speculative assumptions, in this case assumptions which are not unrelated to the function of identification. In an article in the *American Journal of Physics* in 1972, Biernson claimed that in fact contrary to popular belief, there are only six colours to the spectrum and he attempts to find a non-empirical source for Newton's introduction of indigo. Armstrong responding says there is no need for such a search, since in fact there are seven colours and that is why Newton saw them. Yet Newton's optical papers reveal that he was aware of only five colours, but felt compelled to add orange and indigo to his list

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p. 109.

<sup>9</sup> For these episodes and the French reception of Newton, cf Henry Guerlac, *Newton on the Continent*, Cornell, 1981.

<sup>10</sup> Cf Koyre, *Études Newtoniennes*, Gallimard, 1968, p. 317-329.

<sup>11</sup> *Correspondence*, Cambridge 1959-1977, vol 3, p.253.

<sup>12</sup> *Philosophical papers*, Lakatos, CUP, 1977, Vol I. Cf article on Newton.

to fill the gaps between the brightest yellow and red and the brightest violet and blue, since the gaps here were greater than those between the other colours. Furthermore it has been argued that Newton needed seven colours to preserve the unity of the analogy of the field of light and the seven tone division of the octave<sup>13</sup>. Both these arguments show how an empirical result is based on a failure to incorporate the function of gaps in natural philosophy.

In his history of 'English Thought in the Eighteenth Century' Leslie Stephen tells us that the English mind is "averse to a priorism"<sup>14</sup>, but as the Newtonian texts indicate, without it, there is simply no theory construction. That the elaboration of this opposition between British and French critical traditions is to be traced to the Newtonians is clear from the very different discussions of the role of hypotheses to be found in the earlier years of the seventeenth century, notably, for example in the initial reception of Cartesian philosophy by the Cambridge Platonists, thinkers who played a major role in Newton's formation. It was, after all an Englishman, Henry More, who first coined the expression 'Cartesian' in 1662, five years before the French equivalent was given currency by Graindorge in 1667<sup>15</sup>. If an empiricism was to gain predominance in the English tradition during the Newtonian years, it ignored its origins in more than the simple sense of attempting to sweep its theoretical foundations under the carpet. For, as recent debate has made clear, the very first formulations of 'British Empiricism' took their cue not from green pastures but from the pages of Gassendi; studies of Lockian epistemology trace its dependency on the version of empiricism developed by his continental contemporary<sup>16</sup>. This is another factor overlooked in the canonical separation of the two traditions.

### **English Psychoanalysis and reality**

How do such debates effect the practice of psychoanalysis? We can take two examples from the Independent tradition, clinical texts by Wilfred Bion and by Peter Lomas. In his discussion of 'Evidence' in analytic practice, Bion, acknowledging the confusion and problems generated by clinical work, makes an appeal to Bacon's valorisation of the method of "collecting axioms from senses and particulars, ascending continuously and by degrees, so that in the end, it arrives at the more general axioms"<sup>17</sup>. The key problem for the analyst, is how to link "intuitions" to "concepts" and "concepts to intuitions" within this model, given the belief, held by Bion, that the true facts and building blocks of the analytic experience are feelings and not words. Lomas, in turn stresses that "a preoccupation with theory diminishes our regard for imagination"<sup>18</sup> and he opposes theorisation to the position, which he finds attractive, that "we are unable to stand outside the world and appreciate it objectively" because

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<sup>13</sup> G. Bierson, "Why did Newton see Indigo in the spectrum?", *American Journal of Physics* 40, 1972; H.L Armstrong, "Comment on Newton's inclusion of indigo in the spectrum", *ibid* and P. Gouk, "Newton and Music: from the Microcosm to the Macrocosm", *International Studies in the Philosophy of Science* 1, 1986.

<sup>14</sup> V.I, London, 1876, p.32.

<sup>15</sup> Marjorie Nicolson, "The Early Stages of Cartesianism in England", in *Studies in Philology* 1929, Alan Gabbey, "Philosophia Cartesiana Triumphata: Henry 1646-1671" in *Problems of Cartesianism*, ed. Lennon, Nichols and Davis, Mac Gill 1982 and Michael Hunter, *The Great Instauration*, London, 1972.

<sup>16</sup> David Fate Norton, "The Myth of British Empiricism" in *History of European Ideas*, 1981, pp.331-44, Richard Kroll, "The Question of Locke's Relation to Gassendi", in *Journal of the History of Ideas*, 1984, pp. 339-59, and John Milton, "Locke and Gassendi: A Reappraisal" in *Oxford Studies in the History of Philosophy* 2, 1992.

<sup>17</sup> *Clinical Seminars and four papers*, Fleetwood Press, p.243.

<sup>18</sup> *The Limits of Interpretation*, Penguin 1987, p.42.

we are a part of it, something he takes to be directly opposed to maintaining a theoretical position. Hence Lomas' question: 'Can the psychotherapist manage without a theory?'. No doubt his hope is an affirmative, since, as he argues, the use of a theory "restricts our capacity to be open". Lomas fails to understand the difference between dogmatism and the ability to draw conclusions, a point that Lacan took care to stress when he travelled to London in 1951 to address the British Psycho-Analytic society. "Novel theories", he said, "prepare the ground for new discoveries in science, since such theories not only enable one to understand the facts better, but even to make it possible for them to be observed in the first place"<sup>19</sup>. But for a theoretician like Lomas, the choice is limited to just two positions: either one may seek the criteria in advance only if a theory's tenets can be verified by an "outside objective reality", or one may work within a "non-discursive frame of reference", letting one's "imagination dwell on the patient's words" and then conveying "whatever thoughts and images come to mind"<sup>20</sup>. The clinical response here is clear, and it characterises the English analytic tradition: rather than draw a conclusion, one appeals to a feeling. If one cannot stand "outside" the world to appreciate it objectively, one must use one's own personal response as a guide to truth. The technical name of this appeal is countertransference.

In Paula Heimann's classic formulation, this is equivalent to the analyst "using his emotional response as a key to the patient's unconscious"<sup>21</sup>, a definition large enough to encompass the projective identification proposed by post-Kleinian authors and the 'trial identifications' urged by such popular books as Casement's 'Learning from the Patient'<sup>22</sup>. The most recent monograph by a member of the British Psycho-Analytic Society, Harold Stewart's 'Psychic Experience and Problems of Technique' developing such models, informs us that when a patient would systematically fall asleep in sessions, the analyst would feel "quite comfortable and relaxed in this situation"<sup>23</sup>. But what about his identification with the patient, we may ask, an identification that Anglo-Saxon analysts are so enthusiastic about that the editors of the recent edition of the 'Controversial Discussions' instruct us to adopt this attitude even as readers in approaching their text. It will give us, we read, "the opportunity to empathise with the various speakers" and is arranged so that "the reader can imaginatively be in the situation with the participants"<sup>24</sup>! If Mr Stewart were to adopt this and his own advice, we might reproach him for having taken Lacan's remarks about the game of bridge too literally.

This reliance on countertransference is based, in one sense, on the narrow choice set out by Lomas: one may either appeal to 'outside reality' or accept the 'relativism' of remaining within the world. Now, what this perspective misses is that the rejection of the first alternative does not imply the acceptance of the second. Lacan too rejected the appeal to a zone of reality beyond the effect of dialectic, as it was propounded by certain, though not all, of the ego psychologists. But he did not follow the path of the countertransference. There was still, as he pointed out at the time, the power of dialectic, not in the sense of the imaginary tussles of countertransference, but in the dialectic of errors with which Koyre had characterised scientific thought. For the latter, we interact dialectically with error to reach new results, as seen, for

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<sup>19</sup> International Journal of Psychoanalysis, 1953.

<sup>20</sup> Ibid, p. 43.

<sup>21</sup> On Counter-Transference, in IJP vol. 31, 1950.

<sup>22</sup> Tavistock, 1985.

<sup>23</sup> Op. Cit., no. 6, p.134.

<sup>24</sup> Op. Cit., no. 2, p.3.

example, in the famous misunderstandings between Descartes and Beeckman in their dialogue on the theory of freefalling bodies. Psychoanalysis, in this sense, is perfectly scientific, because its interpretations are not 'true'.

The paradox, in the Anglo-Saxon tradition, is that despite the tendency to opt for Lomas' second principle, the first notion of 'outside reality' keeps on creeping back, as we see, for example, in the titles of such well-known books as Winnicott's 'Playing and Reality' and Rycroft's 'Imagination and Reality'. It would not have been possible for Lacan to have published 'Écrits et la Réalité' although he did write a text with the title of 'Beyond the Reality Principle'. A recent commentator on the Independent tradition claims that this is due to the cultural presence of Darwinism<sup>25</sup>, and he singles out the belief in the importance of trauma and environmental effect as a key shared belief of the Independents. So, relativist in the theory of the countertransference, realist in the theory of the cause of the patient's problems. Such a perspective exhausts itself in the elaboration of trauma theories. If problems are caused by traumas, and if no major trauma can be located in the subject's early history, the only thing to do is to invent a new category of trauma: hence the 'cumulative trauma'<sup>26</sup>, defined as the addition of all the little unhappinesses of the first years of life. Such a quantitative conception is symptomatic, and does not even match the considerations of a De Selby, who explains the fact that the night is black as due to accumulations of black air produced by industrial activities using coal tar and vegetable dyes, which leads the same De Selby to the conclusion that sleep is simply a succession of fainting fits caused by semi-asphyxiation. When Lacan praises English psychoanalysts for their 'cold objectivity' (Écrits, p. 613), it is not in reference to this passion for the causal powers of the tangible.

### Lacan in England

Lacan's warmest praises for this "*rapport véridique au réel*" are directed to the work of Bion and Rickman in the article on English psychiatry and the war, work which consisted in the study of modes of identification with the ideal and its alternatives in the group<sup>27</sup>. This perspective, which touched grosso modo on the function of the father, becomes eclipsed in their postwar theorisations, and stalls into Bion's particular version of Kleinianism to privilege the role of the mother and her reverie. This forgetting of the problem of identification is again symptomatic of the development of the English school and we find a poignant example in the juxtaposition of Paula Heimann's definition of interpretation at the Geneva Congress in 1955 and the almost contemporary remark of Lacan's at Rome. For Heimann, "The question the analyst has to ask himself is: "why is the patient now doing what to whom?"<sup>28</sup>. The answer to this will be the analytic interpretation. For Lacan: "*Pour savoir comment répondre au sujet dans l'analyse, la méthode est de reconnaître d'abord la place où est son ego - autrement dit, de savoir par qui et pour qui le sujet pose sa question*"<sup>29</sup>. In other words, Heimann puts the 'X' of the question in place of the 'why', the 'what' and the 'to whom', whereas Lacan puts it precisely in the one place that Heimann takes as a given, that of 'the patient'. To the 'full' subject of the

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<sup>25</sup> Eric Rayner, *The Independent Mind in British Psychoanalysis*, Free Associations, 1990, p.80, and p.2.

<sup>26</sup> Masud Khan, "The Concept of cumulative Trauma" in *Psychoanalytic Study of the Child*, vol.18, 1983, pp.283-306.

<sup>27</sup> "La Psychiatrie Anglaise et la Guerre" in *L'Évolution psychiatrique*, 1947, pp.293-318.

<sup>28</sup> In "Dynamics of transference and interpretations", *IJP* 27, 1956, p. 307.

<sup>29</sup> *Écrits*, p.303.

Anglo Saxon tradition, Lacan opposes the hollow of the \$, to the model of the unconscious which gives it the stuffing of instincts and internal objects, Lacan sets the empty unconscious. A curious reversal of the observation of Voltaire when visiting London in the Newtonian era: "A Frenchman arriving in London will find things very different in philosophy as in everything else. He left the world full, he finds it empty." The reference here is to the vortices of subtle matter posited by Descartes and the empty expanses of the Newtonian universe. If today something is full on the continental side of the Channel, it is less the internal world than the waiting room.